THE SOCIAL CONSTRUCTION AND RECONSTRUCTION OF A SCIENTIFIC CRISIS: A SOCIOLOGICAL ANALYSIS OF NORTHERN COD STOCK ASSESSMENTS FROM 1977 TO 1990

ALAN CHRISTOPHER FINLAYSON

The Social Construction and Reconstruction of a Scientific Crisis: A Sociological Analysis of Northern Cod Stock Assessments from 1977 to 1990

by

c Alan Christopher Finlayson

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ABSTRACT

There is, at present, a generally perceived crisis in the Atlantic Canadian fishery. From one perspective, this is nothing new as the history of the fishery can be portrayed as a long series of crises. What is new is that--with Canada's extension of its territorial limits to 200 miles in 1977--a strong, institutionalized role for science was created in the fisheries management process expressly to help avoid the "boom and bust" cycles that had plagued the fishery in the past.

This work takes the position that the descriptions and interpretations of reality offered by fisheries stock assessment science during the period from 1977 to the present can be understood as an artifact of multi-levelled, interactive social processes--that in many respects this perspective yields a more plausible explanation of scientific knowledge production than do the scientists' own reconstructions.

FORWARD AND ACKNOWLEDGEMENTS

A Reflexive Moment

Within sociology, the concept of reflexivity has gained considerable currency in recent years. On the most general level, reflexivity means that we should treat as significant the social conditions of our own constructions as well as those of the subjects of our work. If, as most of us believe, society, culture, personality, and meaning are social constructs, then it inescapably follows that our work is likewise socially constructed. Further, if we maintain that individuals, events, institutions, and so on can only be fully understood (via empirically-engaged field work) in terms of their immediate relationship to other social subsets and their more nebulous relationship to a larger social context (via empirically-grounded theory), then it would seem logical to argue that our work as sociologists can only be fully understood in a similar way. This, in essence, is the underlying rationale of the call for reflexivity.

From this perspective it is easy to see that a sociologist makes decisions (mostly unconsciously) that affect the nature and outcome of his or her work. In the following few paragraphs I attempt to make overt the most salient features of the social context within which this study has been produced.

This work was written as a Masters thesis. I began my research as a straight-forward exercise in applied theory; in this case, the social-constructivist perspective of the sociology of scientific knowledge. I selected fisheries stock assessment science as the empirical ground for purely pragmatic reasons. The fishery was in a self-proclaimed state of crisis and the question of whether or to what degree scientific error had contributed to the creation of the crisis was the subject of widespread controversy. The research station that was the base of operations for these scientists was a ten-minute drive from my office at Memorial University making the logistics and cost of the research quite reasonable.

Shortly after beginning to make the acquaintance of the scientists, I began to develop an appreciation for the human reality of the problem. In short, my research quickly ceased to be a theoretical exercise. These scientists were no longer "subjects" or "actors" but real people, many of whom I came to respect for their intelligence and obvious deep concern about the health of the resource, the well-being of those people whose livelihoods depended to some degree on that resource, as well as the crisis in their own profession.

It was the cooperation, patience and trust of the scientists who made this work possible and to whom, above all, I owe a tremendous debt of gratitude. I fear that they may feel poorly repaid. I have tried to present the people, institutions and events as objectively as possible. There are no villains in this piece. Neither are there heros. These people are all intelligent; many of them strikingly so. They are sincere; sometimes painfully so. They are surprisingly honest in their reconstruction of a personally and professionally traumatic, confusing period in their lives.

Being human, I have developed feelings for and opinions about the people who are the subjects of my study and the institution within which they work. Although some parts of my analysis are bound to irritate or even anger most of my subjects, I liked them all. Inevitably, I found the company of some to be more personally congenial than that of others. This will be most obvious in the full transcripts of the interviews presented in the Appendices. I have tried to correct for it in the body of my work.

Acknowledgements

While I am the author of record (and, as such, take sole responsibility for any errors, omissions or other blunders) this work is the product of the congruence of time, talent, and money supplied in generous measure from many sources and to these individuals and institutions belongs much of the credit for any merit the reader may find in the following pages.

First among these are the people who's faith in me never flagged, 'though sorely tested during those many years I wandered apparently lost in the wilderness: my parents, Neil and Helen, my sister Laura, more good and faithful friends than I can modestly account for--or deserve. I shall forebear to mention them by name as I would inevitably miss at least one and the hurt of that one omission would

not be justified by the pleasure that others might derive from my acknowledgment.

An immeasurable debt of gratitude is owed to the institution and people of the Memorial University of Newfoundland. Twelve years after disappearing into the terra incognito of the "real world," I was graciously accepted into the School of Graduate Studies. A bursary and a part-time job on campus and, later, a fellowship provided the essential wherewithal for me to stay warm, dry and reasonably well-fed while I tested the reality of my dream of an academic career. That this dream is now becoming reality is due in large measure to my unimpeded access to the deep pool of intellectual abilities that has collected in the departments of sociology and anthropology.

In the sociology department, my program supervisor, Ronald Schwartz, has never failed to do whatever has been required to ensure that I have had access to the necessary resources with which to do my work. Many members of the department faculty have vastly exceeded their institutional responsibilities in their unstinting gifts of time, good advice, and critical thought. There are a few people without whom I would never have made it through my first,

terrifying semester, much less accomplished the work contained in this thesis. They are (in alphabetical order): Larry Felt, Ian Gomme, Barbara Neis, Peter Sinclair, Judy Smith, and Will Wright.

Although the departments of anthropology and sociology became institutionally and administratively distinct entities some years ago, they have retained a functional association, offering many joint courses and randomly integrating the faculty within a shared suite of offices. This, in my opinion, is a very good thing as there are several anthropologists whose various contributions to my work have been of critical importance. They are (in alphabetical order): Raoul Andersen, Jean Briggs, Louis Chiaramonte and George Park.

To the Institute of Social and Economic Research (ISER) I am indebted for the substantial research grant that enabled the scope of my fieldwork to be sufficiently wide. My comrades and colleagues in the MA program were an unfailing source of solace, encouragement, and class-solidarity in our collective struggle with the oppressive forces of statistical analysis and other ritual brutalizations of graduate school. Bonnie McCay at Rutgers and Trevor Pinch at Cornell have had a significant influence on the course of my work. In fact, it was my reading of Pinch's book *"Confronting Nature: The Sociology of Solar-Neutrino Detection,"* that was the original inspiration for my research.

These past two years have been the most difficult and rewarding years of my life to date. To all of you who have made them possible I hereby acknowledge my deep gratitude and enduring appreciation. Thank-you.

Chris Finlayson
Department of Sociology
Memorial University Of Newfoundland
St. John's,
Newfoundland
Canada

May 29, 1991

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ABBREVIATIONS

DFO......Department of Fisheries and Oceans

CBC......Canadian Broadcasting Corporation

FRB......Fisheries Research Board

NAFO.....Northwest Atlantic Fisheries Organization

CAFSAC...Canadian Atlantic Fisheries Scientific Advisory Committee

ICNAF....International Council on the Northwest Atlantic Fisheries

TGNIF....Task Group on the Newfoundland Inshore Fishery

TAC......Total Allowable Catch

ICES.....International Commission for the Exploration of the Seas

EFJ......Extended Fisheries Jurisdiction

MSY......Maximum Sustainable Yield

ESY......Economic Sustainable Yield

OSY......Optimum Sustainable Yield

FPI......Fisheries Products International

NatSea...National Sea Products

NIFA.....Newfoundland Inshore Fishermen's Association

VPA......Virtual Population Analysis

CHAPTER ONE

INTRODUCTION

This is a highly critical work. It is critical of institutions, not individuals. More particularly, it is critical of an institution's tendency to develop conceptual and operational inertias that have the power to predetermine the collective reality of its individual members--to frustrate, nullify and, occasionally, subvert an individual's efforts to correct perceived errors or misdirections. The work explores the generation of consequential social forces at many levels of organization and degrees of complexity. To the extent that it offers a plausible explanation for a controversial and critically important period in the Atlantic Canadian fishery, it is a work of forensic sociology. To the extent it is an empirically-grounded discussion of theoretical issues of knowledge production, it is an attempt to construct a more broadly-applicable link between theory and praxis.

The focus of this study is the institution of the Science Branch of the Northwest Atlantic Fisheries Centre of the Canadian Department of Fisheries and Oceans. As a relatively small institution, it interacts with other, larger, sometimes competitive and/or cognitively incompatible institutions and enterprises. Among these are the professional bureaucratic structure of the Department of Fisheries and Oceans (DFO) and the political structure of the federal government of Canada in which it is embedded and to which it is responsible.

The Science Branch's primary institutional function is the provision of objective scientific advice as the basis for the rational management of the commercial exploitation of biological marine resources. Very few people in the Province of Newfoundland and Labrador would seriously suggest that it has been entirely successful in this respect.[1] In making this claim, I include many of the individual members of the Science Branch.

In the work that follows, I take the position that the descriptions and interpretations of reality offered by fisheries stock assessment science during the period from 1977 to the present (1990) can be understood as an artifact of multi-levelled, interactive social processes--that in many respects this perspective yields a more plausible explanation of scientific knowledge production than do the scientists' own reconstructions.

Obviously, my work is also a social construction of reality. I must leave it to the reader to judge its explicative merits.

Historical and Cultural Context

State-sponsored fisheries science: a history of conflict

The Canadian state's sponsorship of fisheries science dates from the creation by Act of Parliament in 1895 of the Fisheries Research Board (FRB) (chaired by the Minister of Marine and Fisheries but staffed on a voluntary basis by scientists from the nation's universities) and the establishment of a summer research station in St. Andrews, New Brunswick. The history of this relationship between science and the state documents, from the very first year to the present, an endemic structural struggle to define their respective rights and duties and to control the direction of scientific activities. This history is also one of the cognitive conflicts and contradictions inherent in the techno-utopian marriage of scientific rationality to bureaucratic rationality. The dual dynamics of this relationship, running in parallel, form the backbone of the following work.

At the interface between the political institution that is DFO and the professional institution of science there are conflicting and competing forces. The political institution of federal government operating through the Department of Fisheries and Oceans is the sole source of the Science Branch's funding and functional authority. However, the Science Branch's sole raison d'etre within that political institution is the epistemological authority derived from the putative independence of its knowledge constructions from the political concerns of the state and its allegiance to the classical norms and values of science.

In our quest to explain the Science Branch's consistent construction and persistent defense of what is now generally acknowledge to have been an erroneous description of the northern cod population dynamics, it is essential to understand something of the history of the relationship between Canadian fisheries science and the federal government. Readers interested in a more detailed description and discussion of that history than I offer below are referred to *Appendix A*.

The essential point to note is that, for over 100 years, the relationship between federally-funded fisheries science and the sponsoring government has been characterized by a struggle for control of the content of scientific knowledge production through control of the structural environment within which federal fisheries science is located. From the very beginnings to the present, we see the state's desire to assert full control and science's maneuvoring to preserve some relative measure of independence. This struggle has been variously sharp and overt, and diffused and subtle. The final resolution came when the Canadian government, anticipating the linkage of foreign policy considerations to its greatly expanded management responsibilities with a 200 mile limit, simply eliminated the last vestiges of the FRB's independence by an Act of Parliament.

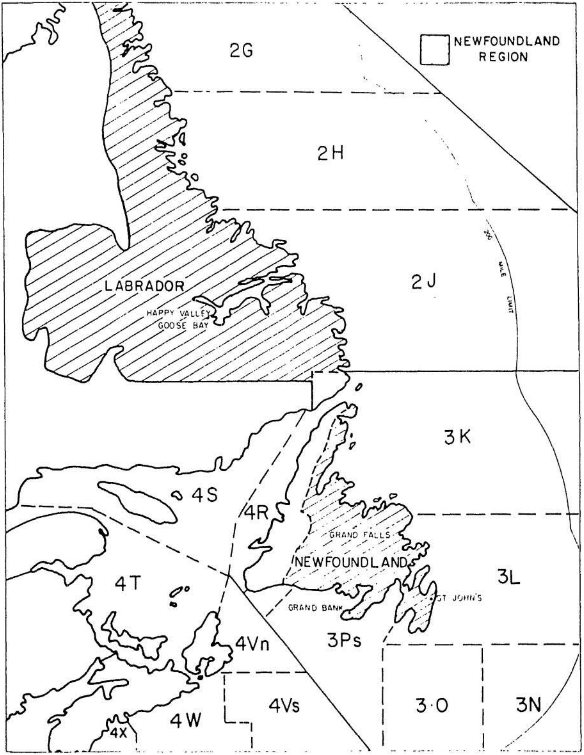
Yet, functional (or dysfunctional, depending on the perspective) vestiges of science's traditional and assumed independence remain. Foremost of these is the tendency for the individual and collective self-identification of DFO scientists to be primarily in terms of their status as scientists and only very secondarily as employees of the

Canadian government. Another survival from the years of independence is the process of peer review that governs reward and promotion. Both of these factors (discussed more fully in Chapter 5) are of considerable significance and must be remembered as we work our way toward an understanding of the general problem.

In this ambivalent relationship between science and the State, we find a dynamic capable of generating the kind of powerfully determining social forces necessary to explain the central problem.

The fish and the fishery: cause and effect

In the northwest region of the Atlantic Ocean the most important of the marine biological resources--both in terms of numbers and commercial value--is the northern cod. [Anon. *DFO Factbook*, Department of Fisheries and Oceans, DFO/4155, Ottawa 1989] This stock inhabits a vast area of the continental shelf encompassed by the Northwest Atlantic Fisheries Organization (NAFO) management areas known as 2J, 3K, and 3L or, collectively, as 2J3KL. [see Fig. 1.1]


NEWFOUNDLAND
REGION
2G
2H
2J
200 MILE LIMIT
LABRADOR
HAPPY VALLEY
GOOSE BAY
3K
4S
4R
GRAND FALLS
NEWFOUNDLAND
3L
4T
GRAND BANK
ST JOHN'S
4Vn
3Ps
4Vs
3·0
3N
4W
4X

The unequalled richness of the cod stocks was the primary reason for repeated military conflicts between nations for the control of access to these fishing grounds and the principal motive for the European colonization of the otherwise barren and inhospitable land known today as the Province of Newfoundland and Labrador. It is difficult to over-emphasize the importance of the fishery to the people of the province. While the fishery's contribution to the provincial economy, although still significant,[2] has considerably lessened in the years since 1949, when the region became the tenth province of Canada, it remains the single most powerful source of collective cultural identity for the people who were born and raised there. Most native Newfoundlanders are no more than two generations removed from direct family participation in one or more aspects of the fishery.

It is important to understand that when Newfoundlanders speak of the fishery, they mean the traditional inshore fishery; not the highly mechanised, capital-intensive offshore trawler fishery. With the exception of the addition of engines and, in some cases, depth sounders, the inshore fishermen of today ply their trade in boats and with gear not much changed for over 100 years. In the case of handlines and jiggers and multi-hook, baited longlines or trawls, the technology is pre-Elizabethan.

Thus it is that when something appears to threaten the inshore fishery--falling prices, rising costs, government regulations, or steadily declining catches--the sense of danger is shared much more widely among the population than any purely rational, economic analysis of the inshore fishery would suggest.

The depth and breadth of concern surrounding the crisis--the political, professional, and cultural stakes riding on its outcome--is difficult to imagine anywhere else in North America. Only in Iceland and, perhaps, Norway could the significance of this problem be understood in its own terms.

Background to a Crisis

There is, at present, a generally perceived crisis in the Atlantic Canadian fishery. From one perspective, this is nothing new as the history of the fishery can be portrayed as a long series of crises. What is new is that--

with Canada's extension of its territorial limits to 200 miles in 1977--a strong, institutionalized role for science was create[illegible] the fisheries management process expressly to help avoid the "boom and bust" cycles that had plagued the fishery in the past.

Recent institutional history

Some key events in this process of institutionalization were: (1) In 1977, the formation of the Canadian Atlantic Fisheries Scientific Advisory Council (CAFSAC) as a transmitter and translator of scientific information between the producers (stock assessment scientists) and the sponsoring consumers (the political management structure of the Minister's Office). (2) The transmutation in 1977 of the largely powerless International Convention for the Northwest Atlantic Fisheries (ICNAF) into the Northwest Atlantic Fisheries Organization (NAFO); (equally powerless [Harris 1990]) for the joint international management of extra- and trans-boundary stocks. (3) The creation in 1979 by an Act of Government of the Department of Fisheries and Oceans (DFO) from elements of the former Department of Fisheries and the Environment.

And yet, in spite of substantial institutional and financial commitments to the goal of creating robust stocks and a profitable domestic fishery, subsequent events have given dramatic and costly evidence that the federal government is still widely considered to be incapable of effectively managing the resource and its exploitation.[3] The *"Kirby Report"* [1983], the *"Keats Report"* [1986], the *"Alverson Report"* [1987], the *"Harris Report"* [1990], and the *"Dunne Report"* [1990] all responded to perceived crises or aspects of a perceived crisis.

In the current atmosphere of social, economic, and environmental crisis, everyone with an interest in the fishery is searching for the reason for this latest failure. Many fingers are being pointed at the traditional targets from previous crises. Among these are: overfishing (both domestic and foreign), federal mismanagement for reasons of political expediency, and over-capacity in the harvesting and processing sectors. But in the latest crisis, voices in all sectors of the fishing industry, the federal management structure, the media, and the general public are suggesting that it is science, the erstwhile saviour, that is to blame.

Recent history of crises

In 1982, and again in 1987, and 1989 a generally perceived crisis in the Atlantic Canadian northern cod fishery occasioned the formation of a federally-sponsored task force to investigate causes and conditions of the crisis and to generate recommendations for the alleviation of the crisis. The 1982 group was known formally as the *"Task Force on the Atlantic Fisheries"* and informally as the *"Kirby Commission"* in reference to its chair, Michael J.L. Kirby. The 1987 group was the *"Task Group on Newfoundland Inshore Fisheries"* (TGNIF) or the *"Alverson Commission"* chaired by Dr. Dayton L. Alverson. The 1989 group was formally the *"Independent Review of the State of the Northern Cod Stock"* and informally, the *"Harris Commission"*, its chair being Dr. Leslie Harris.

Each commission issued its findings in a report to government. These documents are widely referred to as *"The Kirby Report,"* the *"Alverson (or TGNIF) Report,"* and *"The Harris Report,"* respectively, and shall be so called in the balance of this work.

The three reports are strikingly different in almost every respect, sharing only a general sense of crisis in the fisheries and a federal mandate. And yet they are intimately linked in that the findings of the *Kirby Report* are commonly judged to have precipitated the current crisis addressed by both the *Alverson Report* and the *Harris Report*. At the heart of the matter is the deceptively simple question: *"How many fish are in the sea?"*

The Kirby Report: a crisis in the making

Broadly stated, the *Kirby Report* responded to a sharp decline in the overall profitability of the Atlantic Canadian fisheries due to a persistent "cost/price squeeze." The fishermen's and processors' operating costs were rising in the face of a steady decline in the price received for their products. The report's findings and recommendations were based upon the explicit and reiterated assumption that the resource base was strong and would continue to grow stronger under the capable management of Canadian fisheries scientists. And by far, the greatest growth in the resource base would occur in the northern cod stocks.

> "The rebuilding of the northern cod stock is expected to continue through 1987 when a Total Allowable Catch (TAC) in the vicinity of 400,000 t [metric tonnes] or more is forecast. This level is almost certainly below the maximum sustainable yield from the stock....By following a conservative rate of harvest...the eventual long-term production of the stock is thought to be about 550,000 t annually." [Kirby 1983 p. 242]

The text goes on to acknowledge a degree of uncertainty in fisheries forecasting and that these estimates, therefore, are **deliberately conservative.**

Based upon the belief that the Kirby Report's forecasts had some reasonable and valid correspondence to reality, and with the active support of the provincial and federal governments through various incentives, individuals and corporations involved in the fishery, and particularly the northern cod fishery in the NAFO management area 2J3KL, made heavy capital investments to update and expand their harvesting and processing capacities. Landings from the northern cod stocks continued to increase through 1985. However, at present, it is not at all clear whether this increase was due to a real increase in resource abundance, increased fishing effort, more efficient and effective technology and techniques, increased familiarity of the skippers and fleet managers with seasonal movements of the resource, or (most likely) some complex combination of these factors.

The Alverson and Harris reports

Contrary to DFO estimates of a 15 per cent annual rate of growth in the stock, and in spite of increased fishing effort, the total northern cod catch remained essentially static through 1987--the inshore catch declining while the offshore catch increased. [Harris 1990] People with a strong interest in a sustainable, profitable fishery began to suspect that the DFO numbers might be considerably less than accurate. Growing criticism of DFO from the inshore fishery became wide-spread public criticism and was given sympathetic coverage by the media.[4]

This generated political pressure on the federal government which responded with the formation of the Alverson Commission to investigate the causes of the decline in inshore catches. It's conclusions, as presented to the public by DFO, supported the scientific claims of an increasing resource base and concluded that the decline in catches must be due to some combination of environmental influences on the annual inshore migrations of the stock. This explanation was rejected by the inshore fishery and the public criticism and political pressure continued unabated but focused specifically on the scientific claims as to the stock's status.

In 1989 DFO issued its annual assessment based upon a revised (and ostensibly more accurate) mathematical model to generate stock estimates from research and catch data. The results--indicating that abundance had been over-estimated by as much as a factor of two--were sufficiently alarming to precipitate the latest crisis and the formation of the Harris Commission to investigate the causes of this perceived scientific error and report its findings.

A close reading of the Harris Report suggests that the DFO estimates of stock strength were based upon data, methodologies, and models of such poor or uncertain quality as to be essentially useless as a rational basis for management or commercial planning. And yet the pressure is enormous from all concerned sectors to generate legitimating ground for the strategic and tactical decisions that must be made. As a fisheries scientist said during a conversation at a recent international conference[5] in St. John's , Nfld. *"'I don't know,' simply isn't an acceptable answer."*

The Task At Hand

It is at this point that the issues and practices of fisheries stock assessment begin to get interesting from a sociological point of view.

Based upon recent research--primarily extensive unstructured interviews with the key actors in the federal scientific stock assessment and management process--I will argue that this latest crisis can be most usefully understood as a product of multi-levelled and interactive social forces and processes. This perspective diverges quite sharply from the more traditional view which holds that the "success" and/or "failure" of stock assessment science is attributable solely to the ability or inability of scientists to objectively and accurately understand, describe and predict the dynamics of external natural reality.

ENDNOTES

1. The following responses were obtained in a poll conducted on Feb. 20, 1990 for the Canadian Broadcasting Corporation (CBC) by Corporate Research Associates of Halifax, Nova Scotia. The sample was 400 individuals and the results were said to be accurate within five per cent 19 times out of 20.

In response to the question *"How would you rate the Federal Government's handling of this crisis?"*, 72 per cent rated it as *"poor."* None rated it as *"excellent,"* only 3 percent as *"good,"* 23 per cent as *"fair"* and 2 per cent had no opinion.

2. As of 1988, the fishery provided employment for 25 per cent of the workforce of Newfoundland but contributed only 15 per cent of the province's total goods production. [Mandale 1990]

3. See also Endnote 1.

4. The following excerpt from an editorial is typical of the media's treatment of the subject.

> *"On the east coast, Ottawa's flawed policies had plunged hundreds of fishing towns into crisis.*
> *"Cabinet ministers, acting on advice from federal scientists, had permitted Canadian [offshore trawler] skippers to steadily increase their cod harvests off Newfoundland's coast until the bottom fell out of established logic and Ottawa awakened to a resource crisis."*
> [The Sunday Express Feb. 25, 1990 p.6]

5. The *International Symposium on Operational Fisheries Oceanography*, St. John's, Newfoundland, October 23-27, 1989.

CHAPTER TWO

THEORETICAL PERSPECTIVE AND METHODOLOGY

I began my research from the theoretical perspective of the "social constructivist" school of the sociology of scientific knowledge. My intention was to document, discuss and analyze the activities and knowledge production of DFO's fisheries stock assessment scientists entirely from within this relatively new analytical framework. However, it soon became clear that this essentially micro-social approach to the problem was insufficient to explain the data; the empirical reality of fisheries science as I came to understand it during the course of my research.

The Dynamic Complexity of Knowledge Construction

It was clear that knowledge was being constructed as a product of dynamic, interactive, and multi-dimensional social forces. Macro-level social forces were generated within and between several national and supra-national institutions and functional structures. Primarily these were: (1) the federal governments of Canada, the United States and the European fishing nations, (2) international fisheries organizations such as NAFO and ICES, (3) the commercial fishery as a unitary national structure, (4) the public media, and (5) science as supra-disciplinary cognitive structure and process. The forces originating on this level were interactive with the demands of provincial governments, individual multi-national fishing corporations and competing sectors of the fishery--characterized by their geographic areas of operation, such as inshore and offshore, or target species such as cod, caplin, or shrimp. Micro-level knowledge construction within the DFO Science Branch in St. John's and the activities of individual scientists could be seen to be occurring interactively with all the higher levels of social organization.

Because my primary research site was located on the micro-social level within the Science Branch of the DFO station in St. John's, Newfoundland, I rely most centrally upon the insights into the production of scientific knowledge afforded by social constructivism. However, to adequately account for knowledge construction on this level, I was compelled to empirically and theoretically encompass the full range of dynamic relationships. Therefore, I occasionally borrow from other theoretical perspectives (discussed below) not normally associated with social constructivism.

Social Constructivism and Scientific Knowledge

The classical perspective

Scientific knowledge is conventionally portrayed as and believed to be an objective, dispassionate, description of external natural reality--a reality explicitly external to human social reality. In this view, the content and, by extension, the production of scientific knowledge lies beyond--and is exempt from--critical examination by non-scientists. However, relatively recent theoretical developments in the sociology of scientific knowledge allow us to approach science from a new, some would say radical, perspective.

Constructivism: the legacy of Plato, Locke, and Kuhn

In many respects the sociology of scientific knowledge is an evolutionary synthesis of aspects of history, philosophy, anthropology, and sociology. However, certain implications in Thomas Kuhn's seminal work *The Structure of Scientific Revolutions* [Kuhn 1962] have been widely cited as the conceptual catalyst for the inclusion of the actual creation and content of scientific knowledge as a legitimate site for social research.

Although for practical purposes Kuhn is well-deserving of his founder's status, from a philosophical perspective, the idea that our descriptions of natural reality are fundamentally and inevitably social constructions has been around for a very long time. Plato made this point with the parable of the cave. John Locke pronounced on the theory-ladenness of observation, albeit for his own philosophical purposes.

> *"In his 'Essay Concerning Human Understanding', John Locke argues that the only objects of human knowledge which exist are qualities, which are perceived (experienced) as ideas of sensation and reflection. According to Locke, qualities are passive effects which 'cannot be imagined to [i.e. it is inconceivable that they] subsist by themselves.' As qualities are not self-sustaining, Locke argues that 'we accustom ourselves to suppose some [unperceived] substratum wherein they do subsist and from which they do result, which...we call substance.'"* [John Locke in David Burton, *The Knowledge of Substance in the Thought of Locke and Berkeley*, Codgito (sic--it's a pun), Vol. 1, #1, Dept. of Philosophy, Memorial University of Newfoundland, St. John's 1990]

From the social constructivist view we see scientific knowledge primarily as a social artifact and a social accomplishment rather than an objective description of external natural reality. [Pinch 1986, Mulkay 1979, 1983, Knorr-Cetina 1981, 1983] The most radical treatments portray modern science as the enabling and legitimating belief system of the industrial revolution and the liberal-capitalist state.

Michael Mulkay summarized the perspective quite succinctly in *Science and the Sociology of Knowledge.*

> *"I have tried to show...that there are good grounds for rejecting this [conventional] portrayal of science. In particular, the central assumption that science is based on a direct representation of the physical world has been criticised from several directions. For instance, factual statements have been shown to depend on speculative assumptions. Observation has been shown to be guided by linguistic categories. And the acceptance of knowledge-claims has been shown to involve indeterminate and variable criteria. Scientific knowledge, then, necessarily offers an account of the physical world which is mediated through available cultural resources; and these resources are in no way definitive. The indeterminacy of scientific criteria, the inconclusive character of the general knowledge-claims of science, the dependence of such claims on the available symbolic resources all indicate that the physical world could be analyzed perfectly adequately by means of language and presuppositions quite different from those employed in the modern scientific community. There is, therefore, nothing in the physical world which uniquely determines the conclusions of that community.* [Mulkay 1980 pp. 60-61]

> *"The conclusions established through scientific negotiation are not, then, definitive accounts of the physical world. They are rather claims which have been deemed to be adequate by a specific group of actors in a particular cultural and social context."* [ibid p. 95]

In the course of the following work I will show that this cultural and social context can include, must include, the totality of the institutional and political environment in which scientists produce their knowledge. In fact, the production and content of DFO's scientific descriptions of the northern cod stock cannot be adequately explained without a fairly comprehensive understanding of the social dynamics of the Science Branch's relationship with the larger-order institutional and political environment.

> *"The revisions in the customary view of science which have been presented above enable us to reconsider the possibility of there being direct external influences on the content of what scientists consider to be genuine knowledge."* [Mulkay 1980 p.97]

> *"There is in practice a continual cultural exchange between science and the wider society. Interpretive resources enter science mainly through informal thinking, usually with only a very limited awareness of their external origins on the part of participants. They are refined and modified in the course of informal negotiation; and they are allowed into the public annals of science only after appropriate reformulation."* [ibid p.99]

Later we will see that this describes very well how both individual scientists and the institution of the Science Branch could develop commitments to a description of reality that, in some respects, actually came to invert the classical portrayal of the relationship between science and natural reality. In this case, the commitment to the idea of a strongly rebuilding northern cod stock was so powerful that it can be shown to have been determinate of data selection and processing as well as analytical methodologies. The reality of a rebuilding stock was constructed through subtle, amorphous but persistent influences on scientists and the Science Branch of the social, cultural, economic, and political concerns of the wider society to which they also belonged.

Pinch, in *Confronting Nature: The Sociology of Solar-Neutrino Detection*, identifies the concept of "symmetry" or "equivalence" as the first guiding principal of the social constructivist approach.

> *"In providing an explanation of the development of scientific knowledge, the sociologist should attempt to explain adherence to all beliefs about the natural world, whether perceived to be true or false, in a similar way."* [Pinch 1986 p.3]

Therefore, in the case at hand, we are not ultimately interested in assessing the relative accuracy of the work of DFO stock assessment scientists but rather in understanding the social forces that impinge upon the production of their knowledge and the social conditions that create judgements of "right" and "wrong" by outside groups and individuals with interests in the fishery.

Controversies and crises are the most productive sites for social-constructivist research as it is during these episodes that the actual content of knowledge and the rules under which it is created and accepted or rejected are in open, conscious debate. The key actors in the controversy are readily identifiable, accessible for interviewing, and are usually quite few in number. [Kuhn 1962, Pinch 1986]

The task of a sociologist is to be able to show that such apparently immutable, monolithic concepts used by scientists to evaluate the validity of their work such as "repeatability", "refutation," "calibration," etc., are in fact extraordinarily flexible and that their actual definitions and applications are regularly negotiated among scientists. Thus, scientific knowledge can be seen as a flexible, relativistic creation of scientists rather than an unquestionably "true" description of natural reality. [Pinch 1986]

Other Supplementary Theoretical Perspectives

The limits of constructivism

A critique of social constructivism is offered by Rob Hagendijk in his article *"Structuration Theory, Constructivism, and Scientific Change."* Hagendijk presents what he sees as the weaknesses of constructivism in order to argue the superiority of structuration theory as an analytical tool for revealing the social forces and process by which one of several competing theories are established as "true." I, however, have chosen to use his critique, not to reject constructivism, but to locate the boundaries of its utility as applied to my research.

> *"Two ideas distinguish the constructivist approach. First, constructivism holds that scientific knowledge is constructive rather than descriptive . . . . Scientific 'facts' are created by scientists and should be analyzed accordingly. Second, constructivism argues that (social) structure is at best a consequence but never the cause of what people do. Structural social factors or conditions are therefore dismissed as inadequate analytical categories for the understanding of scientific work."* [Hagendijk in Cozzens and Gieryn 1990 p. 44]

While I agree with the constructivists that *"structural social factors"* **per se** are indeed insufficient for a deep understanding of the construction of scientific knowledge, I also agree with Hagendijk that, with respect to the present problem--and very likely others--these factors must also be adequately accounted for in order to produce a complete description and satisfactory explanation of the fisheries stock assessment knowledge construction of DFO science and scientists. To this end, Hagendijk makes my point as well as his.

> *" . . . . So much emphasis is placed on the 'negotiability' of scientific knowledge and research that is becomes impossible to analyze what is beyond negotiation or manipulation for certain people at certain particular times and places, and why this is so. If everything is constructed, what makes some constructions more tenable than others? To deal with this question it seems unavoidable that structures must be invoked that go beyond the situation in which knowledge claims are being negotiated . . . ."*
> *" . . . . Constructivism allows us to understand *how* these scientists reached a given agreement, but it does not allow us to understand *why* they reached this particular agreement and not some other one . . . ." [ibid pp. 49-50]*

Boundaries as an analytical tool

As mentioned above, in the course of my research and analysis I found it necessary to account for the relationships between institutions and structures located at differing levels of social organization. However, these entities appeared as distinct and stable only within the context of a given issue or at a particular moment in time.

As time flowed and issues changed these entities could become frustratingly chimerical.

Consequently, I found it necessary to confront the concept of boundaries and the questions of how to cope with their elusive plasticity and how they could be usefully incorporated in a sociological study of science. In this case I was concerned less with the normative/cognitive boundaries within science itself such as pure/applied, biology/medicine etc. than boundaries between science and other social structures and institutions such as science/technology, science/politics, science/economics, or at the highest level, science/society. In pursuing this matter I found several of the articles in *Theories of Science in Society* to be of considerable value. [Cozzens and Gieryn eds. 1990]

The concept of boundaries is of critical importance in the sociology of science for purposes of both description and analysis. They permit us to describe relationships between actors and/or groups in terms of transactions involving knowledge, power, and material resources--to locate these transactions in time and space, to see directionality in these transactions and, therefore, to detect and describe hierarchies of relationships. In turn, these constructions permit us to analyze and explain these relationships and, thereby, construct and assign meaning.

Boundaries and other, similar distinctions originally arise because of some instrumental functionality intrinsic in the distinction. From the perspective of the originating person or group it is useful that such a boundary be constructed. From any given perspective it is common for boundaries/distinctions to vary in relation to the particular interests at stake. However, boundaries may well persist, outliving their original function and become somewhat misleading--in some instances, deliberately so--in which case they take on a new, disingenuous functionality.

In the present case, the boundary between science and the political/bureaucratic structure of the state is of critical importance. Throughout the work that follows we will repeatedly see that the location of this boundary is not fixed but can vary greatly and be the subject of heated disputes. In general, it can be said that is in the state's interest to enlarge and weaken the boundary while it is the interests of science to closely circumscribe and strengthen it.

Patronage and power

Another theoretical perspective which I have found to be of value in understanding the diversity of social forces that can impinge upon scientific knowledge construction--that of resource and power relationships between science and other social institutions and structures--is offered by Cozzens and Gieryn in their introductory text.

> *"An understanding of the complex associations that make up scientific patronage is surely near the core of a theory of science in society....*
>
> *"The relationship between patronage and the autonomy of science is center stage in several of these essays....These thoughts demand revision of the idea that scientists enjoy autonomy from political and economic forces swirling outside their laboratories, because such cloistering is essential for objectivity and truth. Laboratories* are *political and economic forces,* as Westrum *forcefully reminds us, and scientists' autonomy is an illusion perpetuated by the misbelief that neither money or power is a prerequisite for 'big' science. Both power and money come with strings attached."* [emphasis in the original, Cozzens and Gieryn 1990 p. 5]

Modern scientific knowledge production, on any significant scale, cannot exist independently of a market for that production. The time when a self-funded scientist such as the 16th century astronomer and Danish nobleman Tycho Brahe could produce substantial work is long gone. In the present atmosphere of increasing political volatility in Canada--characterized by parties' and politicians' apparent willingness to respond to short-term interests of momentarily powerful social entities, and reactions by politicians (individually and collectively) to the changing political environment--money for science must become ever-more directly linked with political/pragmatic objectives. Politically motivated interests may well notice and seek to exploit "legitimate" scientific debate on a given issue for self-interested political/economic ends. The bind that this can create for science is succinctly stated by Hagendijk.

> *" . . . . Boundary maintenance and collaboration are important in maintaining the distinct identity of science. On one hand scientists have to maintain their scientific integrity and trustworthiness; on the other hand, they depend on their nonscientific environment for support and legitimation . . . ."* [Cozzens and Gieryn 1990 p.58]

Authority and investments

This leads us to a related but distinct theoretical concern; that of scientific authority and its construction, negotiation, maintenance, and defence. In the present case, the outcome of the debate between DFO science and other outside interests (and, later, within the Science Branch as well) as to the "true" status of the northern cod stocks was by no means inevitable. The current labelling of the earlier perception of a still-growing stock as "wrong" was the result of a complex set of negotiations between competing interests with differing kinds and amounts of authority at their disposal and with differing objectives. It is perfectly conceivable that under different circumstances, the scientists' original perception would have been authoritatively vindicated and the debate about the state of the stocks settled by a definitive closure.

It is possible to postulate another reason for the persistence of "erroneous" knowledge in stock assessment science. This has to do with the costs and complexity of the production of that knowledge. Unlike knowledge produced by the humanities or the social sciences, knowledge production in the life sciences, and especially in large-scale marine biology, requires the establishment and support of a complex human and technical apparatus--research scientists, technicians, administrative support personnel, laboratories crammed with sophisticated, expensive equipment, computers, telecommunications equipment, specialized research vessels and aircraft--all coordinated and supervised by a hierarchy of managers.

The knowledge produced by this system is, to a great extent, validated simply by virtue of its production. The results of an impressive deployment of resources are imbued with the power and authority of the institution capable of mobilizing such resources. This effect is perhaps even more pronounced within the sponsoring institution than without it. Secondly, having made large investments in the production of knowledge, and having originally certified it as valid, the institution will not lightly decertify its validity. Such knowledge is energized with an inertia in rough proportion to the institutional investment in its production.

From this perspective, DFO's resistance to the initial, external criticism of its construction of reality in 1984-85 is not problematic but, rather, perfectly normal. What is remarkable is the fact that such a relatively brief period of time--five years--was required to attenuate the prevailing epistemological inertia and begin the process of reconciliation of conflicting cognitive models and the reconstruction of a more broadly-shared reality.

Another dimension of authority is the competition between scientific organizations engaged in similar work.

In the following passage the author is referring to scientific work conducted within academic institutions but his claim can be usefully extended to include state-sponsored scientific activity as well.

> *"Most of the time such institutionally supported scientific work is conducted in direct competition with that of other teams of scientists in parallel institutions engaged on the same or similar work; this competition constitutes a struggle for authority or mastery of the scientific field in question in every sense of that term."* [Redner 1987 p.97]

Applied to the current problem, we can hypothesize that DFO scientists may have conceptualized their work as being in competition with that of other fishing nations' scientists (most prominently Norway and Iceland) with respect to providing the scientific knowledge and advice necessary for the masterly rebuilding of a depleted resource and the sustainable rational exploitation of that resource. This is the guiding vision of techno-utopianism, a vision which had not previously been realized on any significant scale. This perspective yields yet another possible explanation of the persistent optimistic interpretation of ambiguous results and the strong reluctance to consider alternative interpretations.

Methods

My work relies primarily on the transcripts of extensive interviews with fisheries scientists and their managers in the political/bureaucratic hierarchy of DFO and, additionally, on related government and academic publications and media accounts.

All interviews (except where explicitly noted) were conducted under a self-imposed set of rules and procedures. Each interview (including follow-ups) began by asking permission to tape record the session. All tapes were labelled, dated, and safely stored to serve, if needed, as the reference for any questions of context or accuracy. At any point during the interview the subject could request that specified information be placed off-the-record or the tape recorder turned off.

Subsequently, the subject would receive a verbatim transcript of the interview and be requested to make any corrections, clarifications, amplifications, additions, or deletions that he or she felt were appropriate. Any published quotations or references to information acquired during the interview would be from the subject-edited transcript. Further, prior to the publication of the present work, no other person would be permitted to quote from or refer to material contained in the interview without written permission from both the subject and myself.

I felt that these precautions and guarantees were appropriate given the highly controversial nature of the subject, the sensitivity of some of the information and opinions offered during the interviews, and the vulnerability of some of the subjects to--possibly quite severe--repercussions.

It is worth noting that after a general description of my research, the reasons for my interest in this issue, and an explanation of my interview protocol, no one refused to be interviewed or my request to tape the interview. All subjects agreed to speak on-the-record and for personal attribution. In only one case was I asked to turn the tape off for a brief period. Nor do any of the subjects' self-edited transcripts contain any substantial revisions or deletions.

There are two possible reasons for their openness. One, is that I had seriously overestimated the controversial nature of the research area. I think that this is quite unlikely. The second is that the people whom I interviewed were and are deeply concerned about the fisheries--the relative health of the biological resource and the welfare of the men and women, the communities and corporations, whose wellbeing is intimately entwined with that of the fish stocks. Many of my subjects are dismayed that this latest crisis happened in spite of their best efforts and are actively searching for the reasons. A few have suffered both professionally and personally because of their perceived role in the apparent over-estimation and mismanagement of the stock. For a variety of reasons, these people took a genuine interest in my research and were willing to contribute their version of events and perceptions of the issues, often quite emphatically.

Several reviewers of earlier drafts of this work have suggested that I reduce the length of my quotations from the interview transcripts. I have chosen to ignore their advice for the following reasons: First, the most common criticism levelled against an author by a quoted subject is that their words were *"taken out of context."* By "bookending" a crucial passage with some of the preceding and subsequent conversation I attempt to make clear the context of a particular question and answer. Although I have included the full transcripts in the Appendices, I felt that it was unrealistic to expect the reader to flip back and forth. Second, it was important to me that I share these pages as fully as possible with the people who are, in a very real--if unusual--sense, co-authors of this work. I am frankly uncomfortable with my power as an author to decide what these people can and cannot say. The lengthy quotations are my way of abdicating this power so that my co-authors may speak directly to the reader.

CHAPTER THREE

THE ROLE OF SCIENCE

At the heart of this controversy is the generation by fisheries scientists of current stock population estimates, predictions of the effects on stock populations of exploitation and management variables, and the consequent issuance (through the Canadian Atlantic Fisheries Advisory Council (CAFSAC) of their findings in the form of yearly catch quota recommendations and long-range forecasts. Based upon these recommendations and with consideration given to various social, economic, and political factors, the federal Minister of Fisheries sets yearly Total Allowable Catch (TAC) quotas for commercially exploited species/stocks and develops longer-range management strategies. Based upon the yearly TACs and official predictions, individuals and corporate interests in the harvesting and processing sectors make tactical operational and strategic investment decisions.

The above process is predicated on the obvious assumption that fisheries science is capable of producing quite precise assessments and projections that are and will be of practical value to, and consistent with, the subjective experience of its two principle clients--the policy and planning sector of the DFO Ministry and the commercial fishing industry.

In fact, the entire institutional structure of DFO and the process of scientific stock assessment and advice/input to the formulation of fisheries policy and the planning of resource exploitation is based upon the widely-shared paradigm that the natural universe and perceived sub-systems are a product and process of linear dynamical interactions that are governed by "natural laws" and, as such, are ultimately knowable and, therefore, manageable.[1]

Assumptions and Expectations at the Third Law of the Sea Convention

The foregoing paradigm informed Canada's position at the Third Law of the Sea Convention when it argued in favour of the extension of the boundary of its control over marine resources from 12 to 200 miles.

> *"...from the Canadian point of view, the 2J3KL cod stock had been seriously overexploited....Thus the required management policy seemed obvious--rebuild the stock. In fact, of course, Canada had insisted on implementing this policy before invoking the EFJ [Extended Fisheries Jurisdiction], as shown by its*

actions in 1975." (At the Third Law of the Sea Conference and at the June, 1975 ICNAF meeting to set the 2J3KL quota) [Munro 1980]

Canada's arguments in favour of the 200 mile limit were presented in the powerfully persuasive language of science and, as such, were accepted as rational. They were "true" because they were believed to be grounded in scientifically mediated empirical reality. Further, the Canadian negotiators were emphatic, explicit, and--ultimately--convincing in their insistence that their motives were essentially altruistic and not expansionist.

> "*Canada's argument was stated to be a functionalist one; that is, jurisdiction would be extended for certain specific purposes where it was necessary to manage resources or protect the environment, and the extent of that jurisdiction would be coterminous with management or protection needs. Moreover, it was argued, the coastal state would be carrying out these functions as a 'trustee' or as a 'custodian' for the international community.*" [McRae in Canada and the Sea, 1980]

Canada's position was as follows:

1.) Joint international management of commercially important stocks through the agency of ICNAF had been and would inevitably continue to be a failure. This was theoretically informed by Hardin's thesis of "the tragedy of the commons" and empirically supported by the fact that, in spite of falling catches since 1968 (see Table 3.1), the northern cod quotas set by ICNAF had been hugely in excess of what was capable of being caught by a large and powerful fleet (see Table 3.2). [Regier 1978, Munro 1980]

> *"In terms of the promise of additional harvests, the 2J3KL stock complex overwhelms all else by virtue of its size and its over-exploitation between 1956 and 1976."* [Munro 1980 p. 27]

Table 3.1

Harvests of Cod in ICNAF Sub-Areas 2J3KL, Selected Years 1956-75

(All catch figures given in thousands of metric tonnes)

		Distant Water Nations		Canada		
	Total	Harvest	% Share	NF Insh.	NF Ofsh.	Other
1956	300.5	117.1	39.0	172.1	2.3	8.7
1960	393.6	228.9	58.2	157.3	2.5	4.9
1964	562.0	420.5	74.8	131.5	6.7	3.3
1968	783.2	659.8	84.2	101.0	20.2	2.2
1972	454.6	388.1	85.4	62.3	3.8	0.4
1973	354.5	310.0	87.4	42.7	1.4	...
1974	372.6	336.5	90.3	35.2	0.9	...
1975	287.5	245.0	85.2	41.1	0.9	0.4

Source: ICNAF, *Statistical Bulletin* 1975 [in Munro 1980]

Table 3.2

Total Allowable Catches and Actual Harvests of 2J3KL Cod, 1973-75

	1973	1974 (thousands of tonnes)	1975
ICNAF TAC	665.5	656.7	554.0
Harvest	354.5	372.6	287.5

Source: ICNAF, *Redbook* 1978 [in Munro 1980]

2.) Canada was the most proximate sovereign State to the resource and had a dominant, historically-grounded interest in the long-term viability of the resource. The northern cod fishery was one of the basic engines of socio-economic activity in Atlantic Canada and the fundamental *raison d'etre* of the Province of Newfoundland and Labrador. [Munro 1980, Atlantic Report July, 1990] *"Canada's position on the fisheries was that the coastal state ought to have the responsibility for* managing *species harvested near its coasts..."* [McRae, Donald M. in Canada and the Sea 1980]

3.) Canadian fisheries scientists had earned an international reputation for excellence. [Regier 1978]

4.) Therefore, Canada had the right, the incentive, and the capability to responsibly, rationally, and effectively manage its adjacent marine resources. [Kirby 1983]

5.) Further, under exclusive Canadian management depleted and, perhaps, endangered stocks would be rebuilt to and maintained at historical levels (see Table 3.3). Supporting sustained catches higher than historical levels was considered to be a strong possibility. [Kirby 1983]

Table 3.3

Projected 1985 TACs for 2J3KL Cod at Different Rates of F.

Fishing Mortality Rates (F)	Projected TAC (thousands of tonnes)
F=0.10	307
F=0.16	402
F=F0.1=0.20	442
FMSY=0.35	523

Source: ICNAF Redbook 1978 and A.T. Pinhorn, DFO St. John's [in Munro 1980]

(NOTE: Pinhorn was and remains one of the most respected and influential scientists working on matters pertaining to the Northwest Atlantic fisheries. As such, his predictions would have been accepted as highly credible.)

> "*To put these TAC levels into perspective, it can be noted that a difference of 57,000 tonnes of groundfish landed and processed per year in Newfoundland is a difference of 1,000 man-years of employment in the processing sector.*" [Munro 1980 p. 26]

6.) Finally, exclusive Canadian management would not only bring long-sought stability and sustained prosperity to the Canadian fishing industry but would also (in accordance with the provision of the Third Law of the Sea convention that required a state to make available to other nations resources surplus to that state's needs) bring these same, if somewhat lesser, benefits to the industries of other nations that had traditionally fished on Canada's continental shelf. [Regier 1978]

In reviewing this argument, we can discover the seeds of the current fisheries crisis and the foundations of the institutional structure and process that would later result in the penetration of powerful social forces deep into the heart of fisheries stock assessment science. The first point was grounded in a broadly-shared, quantified reality. The second appealed to generally accepted principles of the rights and legitimate interests of sovereign states. The third established Canada's unsurpassed expertise in fisheries science and, therefore, its eminent qualifications to rationally exercise its sovereign rights and interests. The fourth is a logically persuasive recapitulation and integration the first three points. The fifth and sixth points, while seeming to flow smoothly from the foregoing, are in retrospect, the "bridge too far."

Techno-Utopianism and Fisheries Management

The fact that no state had ever attempted (much less succeeded) to establish a long-term sustainable fisheries management regime on such a large scale did not, at the time seem to be a significant problem. It was, after all, merely a matter of scale. Canadian scientists believed that the theory of fish population dynamics was reasonably well-understood. What had prevented rational, sustainable management in the past had been lack of authority, control, and resources. And now they were to be given all three. So it was not considered to be unrealistic, or even overly-optimistic, to project and promise such specific and substantial results, both to the Canadian fishing industry and to foreign nations such as Spain and Portugal which had relied heavily on the fisheries of the North West Atlantic for more than 400 years.

The fundamental assumptions are clear:

1.) The dynamics of the marine ecosystem are those of the classical post-Newtonian scientific paradigm: the universe is mechanistic and deterministic and its workings are governed by a few fundamental and unvarying Laws.

2.) The marine ecosystem and its perceived sub-systems (in this case commercially valuable fish stocks) are fundamentally robust. That is, they are relatively insensitive to small perturbations and tend to seek natural equilibrium states.

3.) These natural equilibrium states are determined by relatively few significant variables. In this case, fecundity, recruitment, natural mortality, and fishing mortality.

4.) These variables are knowable and their effects on the stocks are linear and predictable...i.e. that a 50 per cent increase in the spawning biomass will produce a 50 percent increase in fecundity.

5.) Science-based management can manipulate some of these variables (primarily fishing mortality) and monitor the others to effectively control the system and produce (within certain broad limits) equilibrium states in general harmony with human needs and desires.

6.) Having rebuilt the stocks to the desired level, they could then be maintained at that level by relatively minor adjustments in the TACs, thereby bringing long-sought-for stability to the fishery and its dependent socio-economic structures and institutions.

The reasons for this faith in the ability of Canadian fisheries science and management to deliver the promised abundance and stability are best expressed in the words of some of the principals involved.

In 1988, at the request of the then Minister of Fisheries and Oceans, Thomas E. Siddon, Dr. Leslie Harris (then president of Memorial University) formed and chaired the panel that produced the *Independent Review of the State of the Northern Cod Stock* [Harris 1990]. The "*Harris Report*" as it is commonly called was highly critical of DFO policy and practice and focused particular attention on what it deemed to be the inadequacies of the process and product of stock assessment science. The following is excerpted from the transcript of a taped interview conducted with Dr. Harris.

> *"They [DFO] had set out in 1977 with a very optimistic world view. That if you do thus and so, the stock will grow at this particular rate. . . . The great excitement that came with the 200 mile economic zone and the possibilities that that opened up; finally we've got it under our control, finally we can manage it, finally we know what we're doing, finally we have the power to do what we want to do."*[2]

Jim Roache is Director of Communications for the Newfoundland Region of DFO. He was recently assigned to this position by the Minister's office in Ottawa specifically to manage the regional response to the Harris report. This took the form of rapidly escalating criticism of DFO from all interested parties and nearly universally negative media coverage of DFO's role in the fisheries.

> *"When we went to a 200 mile limit, people in general, and I think industry as well, felt that our ship had come in, that the time was at hand when we could catch as many fish in as many different ways as we wanted. Throw in as much technology and as much capital as we liked, making it as labour-intensive and as capital-intensive as we wanted. Pulling out all the stops in marketing the product. It was a gold rush kind of mentality. It was going to be a boom as opposed to the historic bust."*[3]

Bernard Brown, for many years the only public relations person in the DFO Newfoundland Region and now an assistant to Roache, concurs.

> *"The 200 mile limit. That's what started the bonanza attitude. It was El Dorado again. The Canadian offshore boys got into the fishery and started landing all the fish here. The processing industry went right through the roof. It was fabulous. For two or three years."*[4]

The critical point is that Canadian scientists genuinely believed that, given the opportunity, they could provide the necessary advice to rather rapidly rebuild the northern cod stock and then maintain it in approximate equilibrium. Based upon this confidence, the Canadian state, in return for international recognition of the extension of its fisheries jurisdiction to 200 miles, assumed stewardship of the resource on behalf of all interested nations.

Very specific benefits were promised which, in turn, created specific expectations. To fulfil its domestic and international commitments, the state created DFO in its present form and undertook to provide it with the necessary human and material resources. In short, the state, DFO, and many individuals in these institutional structures had a substantial investment in the idea that the stocks would respond in predictable (and predicted) ways to science-based management strategies and practices.

The Bursting of the Bubble

This widely shared and deeply felt belief, that the job was do-able and the expected results attainable, was to inform both the federal and provincial governments' fisheries policies until profoundly shaken by the 1989 northern cod assessment. More properly called a "reassessment", it concluded that the exploitable biomass (fish aged four years and older) had not grown five-fold since 1978 as previously believed but only about three-fold and was now static [DFO/4396 1990] or, possibly, in decline. [Harris 1990]

Further, if the stock size had been seriously overestimated, then the dependent quotas, set to achieve a target fishing mortality (expressed numerically as some value of "F" such as $F_{0.1}$ of roughly twenty per cent of the exploitable biomass had, in fact, resulted in annual removals by fishing of one third or more of the available population. [Alverson 1987] If this was true, then--not only was the stock much smaller than had been thought--its ability to reproduce itself had been weakened, perhaps dangerously so. [Harris 1990]

What is $F_{0.1}$?

DFO adopted the $F_{0.1}$ rule as the guiding principle for its management regime of fishery resources inside the new 200 mile limit. It is used to express both target fishing mortality and subsequent estimated actual fishing mortality where "F" simply means fish caught by commercial activity and the following numbers are meant to indicate the relationship of the weight of the fish caught to what is thought to be the weight of the total catch-able population also known as "exploitable biomass." When the number following F is in subscript, as in $F_{0.1}$ the number is a function of the returns of some unit of fishing effort in relation to stock size (discussed more fully below). When the number is in normal script, such as F=.20 or F.20, it is a straight percentage of what has been estimated as the weight of the exploitable biomass.

This rule had been developed in ICNAF as a more conservative replacement for the concept of Maximum Sustainable Yield (MSY) as a management goal. MSY is a strictly biological concept and refers to the amount of fish that can be removed from a fish population without driving it into decline. It has generally been superseded by such multi-variate concepts as Optimum Sustainable Yield (OSY) and Economic Sustainable Yield (ESY) which claim to include various social and economic factors. [Munro 1980]

As defined in a recent DFO publication:

"...$F_{0.1}$ is the level of fishing effort at which adding one more boat would result in increasing the total catch by only 10% as much as the very first boat to fish that stock....$F_{0.1}$ is a useful idea in fisheries management because it does two things the old 'maximum sustainable yield' did not. It takes some account of the economics of fishing and it leaves a wide margin of

biological safety." [Fo'c'sle, *The Science of Cod*, p. 22, DFO 1988]

In practice, fishing at the $F_{0.1}$ level will remove a larger percentage of the fishable stock from short-lived species than from long-lived species. For northern cod, fishing at the $F_{0.1}$ level means annual catches of about 20 per cent (also expressed as F=.20 or simply F.20) of the exploitable biomass defined as fish aged four and older. It should be noted that the "exploitable" biomass is different from, and can be considerably larger than, the "spawning" biomass. This is due to the fact that, while young cod begin to aggregate with the adult stock at about age four, they do not reach sexual maturity until age six to eight depending upon a number of environmental variables.

Some idea of the strength of the belief in the benefits of Canadian control and management and the magnitude of the expectations thereby created can be seen in the following excerpt from a report prepared by Gordon R. Munro for the Economic Council of Canada in 1980. Its title alone, *A Promise of Abundance: Extended Fisheries Jurisdiction and the Newfoundland Economy*, bears eloquent testimony to the point.

"*There were exceptions [to the* $F_{0.1}$ *management goal] one of these being the 2J3KL [northern] cod. The level of fishing pertaining to this stock complex was deliberately set below that corresponding to the* $F_{0.1}$ *rule in order to quicken the pace of resource investment....To be more precise, in the case of 2J3KL cod,* $F_{0.1}=0.20$ [i.e. 20 per cent of the catch-able stock]. *Present management* [1980 quota] *calls for* $F=0.165$ *. . . .* [or 16 1/2 per cent. The implied precision is significant.] *If, in fact, this management strategy were to remain unchanged, the resource would be roughly within 5 per cent of equilibrium by 1985, thus implying an equilibrium TAC of roughly 385,000 tonnes. . . . Fisheries and Oceans has, at the time of writing, now published three sets of projections, the first appearing at the end of 1977, the other two appearing at the end of 1978 and in the spring of 1980. Consider the 1985 TACs for 2J3kl cod as projected in these three publications: 294,000 tonnes (1977) 402,000 tonnes (1978) and 365,000 tonnes (1980).* [NOTE: The actual TAC in 1985 was 266,000 tonnes although the fleet was able to land only 232,000 tonnes]

" *. . . . It can be said further that, even if the biologists' estimates were perfect,* **it is certainly possible that the actual TAC will prove to be higher than projected.** [emphasis added] *The present management policy is designed for rapid investment in the stock. This implies . . . a very conservative program during the adjustment phase. There is no necessary reason, however, why such a highly conservationist policy should be maintained into the mid to late 1980s when the adjustment phase should be drawing to a close.*" [Munro 1980 pp. 25, 26]

What Happened?

In the following pages I will show that, when faced with ambiguous or inadequate data, many of the DFO scientists charged with the responsibility for assessing the size of the northern cod stocks (the inclusion of numerous caveats in their published results notwithstanding) regularly interpreted the data in the most optimistic possible way. Thus, the quotas recommended through CAFSAC to the Minister--while thought, at the time, to be very conservative--are, in retrospect, seen as having been dangerously high.

Another way to state this is that, in the absence of any hard evidence to the contrary, there was no reason to disbelieve that they had been, and continued to be, successful in doing what they and the Canadian government had set out to do in 1977--rebuild the stocks and maintain them at a healthy, stable level that would produce substantial and sustainable economic yields for the Canadian fishing industry and its dependent social structures.

This interpretation of events is strongly supported in the Executive Summary of the Harris Report:

> *"During the next seven years [1978-'85] the euphoria that had been engendered by the declaration of the exclusive economic zone was reinforced by the steady growth of the stock, by continually improving catches, and by the belief that the $F_{0.1}$ objective was, indeed, being met. In those circumstances, scientists, lulled by false data signals and, to some extent, overconfident of the validity of their predictions, failed to recognise the statistical inadequacies in*

their bulk biomass model and failed to properly acknowledge and recognize the high risk involved with state-of-stock advice based on relatively short and unreliable data series.

" . . . . **it is possible that if there had not been such strong emotional and intellectual commitment to the notion that the $F_{0.1}$ strategy was working,** *the open and increasing scepticism of inshore fishermen might have been recognized as a warning flag . . . ."* [Harris 1990, Executive Summary pp. 2,3 emphasis added]

Further, in addition to the inertia of expectations, other powerful social forces combined and conflicted to the extent that--for the period under discussion, 1977 to the present--the content of fisheries stock assessment science can be better understood as a product of complex and interactive social forces rather than an objective description of natural reality.

ENDNOTES

1. This fundamental assumption may well be seriously flawed. A number of recent works--both theoretical and applied--following from the provocative implications of chaos theory (also known somewhat less dramatically as nonlinear dynamical systems theory) claim that this is, in fact, the case. The interested reader is referred to the following selection of these works.

Alden, Robin: *The voice of the responsible groundfisherman: Listen*: [editorial] in *Commercial Fisheries News*, February, 1991

Bak, Per and Chen, Kan: *Self-Organized Criticality*, in *Scientific American*, January 1991

Briggs, John and Peat, F. David: *Turbulent Mirror*, Harper & Row, New York 1989

Finlayson, Alan C.: (forthcoming) in *Maritime Anthropology Studies (MAST)*

Gaskill, Herbert S.: *A Model of the Northern Cod Stock*, [draft] forthcoming

Gleick, James: *Chaos: Making a New Science*, Viking Penguin, New York 1987

Mandelbrot, Benoit: *The Fractal Geometry of Nature*, W.H. Freeman, San Francisco 1982

Smith, Estellie: Chaos *in Fisheries Management*, in Maritime Anthropology Studies, Vol. 3 No. 2 1990

Wilson, James A., et al.: *Managing Unpredictable Resources: Traditional Policies Applied to Chaotic Populations*, in *Ocean & Shoreline* Management, #13, 1990

Wilson, James A., et al.: *Chaotic Dynamics in a Multiple Species Fisheries* [sic]: *A Model of Community Predation*, in *Ecological Modelling* [forthcoming]

Wilson, James A., et.al.: *Management of Chaotic Fisheries: A Bio-economic Model*, Proceedings from the Symposium on Multispecies Fisheries, Sissenwine, M. and Dann, N. eds., International Council for the Exploration of the Seas (ICES) forthcoming

Wilson, James A. and Roy, Noel: *Constraint-Induced Chaos in a Multispecies Fisheries Model*, notes (unpublished) for a presentation to the Journees du Groupe de recherche en economie de l'energie et des ressources naturelles (GREEN) at the Universite Laval, October 27, 1989

2. From an interview with Leslie Harris conducted on August 29, 1990 in St. John's. The full transcript is Appendix G.

3. From an interview with Jim Roache conducted on July 24, 1990 in St. John's. The full transcript is Appendix N.

4. From an interview with Bernard Brown conducted on August 3, 1990 in St. John's. The full transcript is Appendix B.

CHAPTER FOUR

ERROR, UNCERTAINTY AND INTERPRETIVE FLEXIBILITY: THE CRITICAL NODES OF SOCIAL CONSTRUCTION

There was--and to a somewhat lesser extent still is--an unusually high degree of interpretive flexibility in the scientific assessment of the northern cod stocks. This was permitted by uncontrolled sources of error of unknown magnitude (and errors from unknown or unexamined sources) in the raw data combined with substantial uncertainties as to the relative robustness of the statistical procedures used to process this data and ambiguities and biases characteristic of the assessment methodologies employed. Given the strong individual and institutional commitments to a rapidly rebuilt stock supporting high levels of sustained yield, as discussed earlier, it is not at all surprising that, until confronted with significant substantive criticism from highly respected and credentialed peers, the most optimistic possible interpretation prevailed. Further, there certainly were incentives for those committed to the "promise of abundance" to preserve this interpretive flexibility by discounting or dismissing potentially contrary data sources and resisting the implementation of more rigorous analytical procedures.

It is quite telling that the eventual reassessment of DFO's data and methods (which led to the radically reduced 1989 assessment of the stock size) was not initiated by people or processes internal to DFO science but by political pressure brought by outside interests--primarily the inshore sector of the fishery. Through the 1980s, their scepticism of DFO's knowledge claims grew to become direct charges of scientific incompetence and mismanagement of the resource. The relationship between fisheries science and the inshore and offshore sectors of the fishery is discussed more fully in Chapter Six.

Independent Review: A Chronological Account of Criticism and Rebuttal

The Keats Report

In 1986 the Newfoundland Inshore Fisheries Association (NIFA)[1] responded to the growing discrepancy between its membership's perception of the stock's condition and that of DFO by commissioning three biologists from the Memorial University of Newfoundland to conduct the first independent review of DFO stock assessments. Their report, *A Review of* *the Recent Status of the Northern Cod Stock (NAFO Divisions 2J, 3K, and 3L) and the Declining Inshore Fishery* (also known as the *"Keats Report"* after its principal author) was highly critical of DFO's data sources, statistical procedures and conclusions. One simple, but powerfully suggestive example (Table 4.1) will suffice.

Table 4.1

Original Current-Year Biomass Estimates for the Years 1977, 1979, 1981, and 1983 and the Subsequent Revisions of Those Estimates in Following Years

YEAR	1977 Biomass	1979 Biomass	1981 Biomass	1983 Biomass
1980	8470	12070		
1981	5639	10880		
1982	5482	10466	13684	
1983	5211	9320	11863	
1984	4968	8211	10238	15531
1985	4616	7371	8589	11413
1986	3857	7353	8243	10970

[from Keats 1986]

The table shows the original biomass estimates (in metric tonnes x 10^{-2}; add two zeros on the right) for the years 1977, 1979, 1981 and 1983 and the regular downward revision of these initial estimates through the years 1980 to 1986.

Hindsight: the retrospective convergence of statistical variation

This revision happens via a process known as retrospective population analysis, cohort analysis or hindcasting. The problem is that, in a given assessment year, the total fishable biomass is comprised of fish from age four up to age twenty or so. All fish from a given year's spawning season are presumed to have the same birthday and are referred to as a "year-class." It is not until all the fish of all year-classes present in that assessment year have either been caught and accounted for in commercial catch surveys or died from natural causes that the final, most accurate, estimate of the total biomass of that assessment year can be made. Because cod are relatively long-lived fish, this means that quite a long time-series of data is required before statistical variance begins to converge to an operationally meaningful level. In practical terms, it will be at least five years from now before we can know with any useful degree of probability how many fish are in the stock today.

DFO's own original estimates and subsequent revisions, as presented by Keats, clearly illustrate this problem and show that the DFO assessment has systematically and consistently exploited the interpretive flexibility of unknown (or known but unpublished) confidence intervals to make optimistic assessments.

Notice how the numbers increase from left to right but decrease from top to bottom. The original yearly estimates show a steady increase in keeping with DFO's position that the stock was rebuilding as desired and predicted. The subsequent downward revisions, driven by statistical variance converging with reality, do not support the claim of a rebuilding stock. They do, however, support my claim that sparse and indeterminate data subjected to analytical methodologies of dubious rigor permitted an unusual degree of interpretive flexibility and that this opportunity was invariably exploited to produce scientific and, therefore, supposedly irrefutable, evidence as to the effectiveness of DFO's management strategy. It would be interesting to continue this time series for more recent years. But in the course of the extensive research for the present work, I have not encountered one single example of a northern cod assessment being subsequently revised *upward.* Keats reached a similar conclusion.

"While the lack of confidence limits do not permit an estimate of the precision with which biomass has been estimated, there is a much more serious problem with

uncertainty related to assessment methodology. Biomass estimates for any given year in the past are continuously and consistently revised downward each year as the F values upon which they are based are revised upward. For example, the 1977 biomass estimate has been revised down to about 45% of what it was estimated at in 1980 (I only have data going back to 1980 at this time). The same trend applies to 1979, 1981, 1983, and presumably the years which I have not examined in detail. **This means that the biomass is consistently overestimated in each assessment year, by as much as 55%! This has resulted in a considerable and consistent overestimate of what the $F_{0.1}$ catch should be, with the result that we have taken consistently from 1.5-3 times the revised $F_{0.1}$ catch since 1977."** [emphasis added Keats 1986]

What this means in plain English is that since 1977, instead of realising the $F_{0.1}$ management goal--catching about 20 per cent of the fishable stock; a level, it was generally agreed, that would allow the stock to rebuild--the catch had been somewhere between 30 per cent and 50 per cent of the stock. This rate of exploitation did not support DFO's contention that the stock was growing but tended to confirm the inshore sector's perception of the state of the stock as, at best, static or, possibly, in decline.

Thrust and parry: science defends its claims

The response from DFO was to dismiss the Keats Report as superficial--it was researched and written in only four weeks--and axiomatically biased; pseudo-science written to support the political actions of the Newfoundland Inshore

Fisheries Association (NIFA). However, NIFA and a growing number of other grassroots organizations and individuals representing the interests of the inshore fishery refused to be moved from their position and, in fact, actually increased the volume and severity of their criticism of DFO's claims. Their persistence in attacking DFO science (widely regarded as among the best in the world) and their growing public support in the popular media compelled the federal Minister of Fisheries--then Tom Siddon--to direct the formation of the Task Group on Newfoundland Inshore Fisheries (TGNIF) in August of 1987.

Bernard Brown, a former journalist and a member of the DFO Newfoundland Region Communications Division for nine years, recreated his actions and perceptions during the early and mid-1980s.

> *"I tried to talk to our people when the massive criticism first hit, before the Alverson Commission was appointed. It went right over their heads. [They felt that...] It had nothing to do with science, so who cared? I tried to tell them that it isn't really DFO science that's being criticised. It's the fishermen realizing now that all that stands between them and disaster are political decision-makers so they, i.e. fishermen, decided that they have got to get into the political process and start hammering the government. And either through a lucky shot or some very shrewd thinking, the pressure point they picked to hit was the science.*
>
> *"DFO is so proud of its science. And we have done a lot of good science. So to come and hammer our*

strongest point, our little area of purity, it was devastating to our scientific people.

"It was puzzling to the senior managers and politicians. Why are they attacking our science? That's the one thing we do right! We could take criticism of our management decisions because we were used to that but to come and condemn our science!

"It's hard to exaggerate the first reactions of our scientific people. They were puzzled, upset, angry. For a while they were just like children. The shock was horrible. Here we were being attacked in the one area in which we thought we were unquestionable. We were used to criticism from all over on our management decisions because you can never please all the competing interests. But we always thought that the science was the one pure area, free from political interference."[2]

The Alverson Report

Although the Task Group (TGNIF) was convened by the Minister of Fisheries and Oceans, the political director of DFO, its membership was comprised of fisheries scientists from the United States, the United Kingdom, and Canada--people with no direct connection to the Canadian government and possessed of the highest possible credentials and reputations. This was essential if the Group's findings were to have any credibility with the growing number of increasingly vocal critics of DFO science and management. On the other hand--unlike the Keats Report--because of their eminence in the international community of fisheries scientists, any criticism in their findings could not be lightly dismissed.

The wording of the final report was cautious and, in places, equivocal. Nevertheless, a careful, informed reading shows that the Task Group's conclusions were not substantially different from those of Keats: chronic, overly optimistic interpretations of data of questionable validity had resulted in a persistent underestimation of fishing mortality and a concomitant over-estimation of the rate of growth of the biomass since 1977. Further, it was a matter of interpretation whether the stock had experienced any significant growth since 1982.

> *"Selection of the VPA methodology and analysis is to some extent a value judgement taking into account the data available and the underlying assumptions, and may vary between scientists. Thus, a plausible range of possible F values for 1986 can exist."* [Alverson 1987, emphasis in the original]

Bernard Brown reconstructed his interpretation of the Alverson Report from the perspective of a public relations professional.

> *"Now to anyone who read the report closely and read it with an open mind, from the point of view of, 'maybe our critics have got a point,'--if you read the Alverson report carefully from that point of view, I think the signs were in there that the problems were worse than stated in the report.*
>
> *"First of all the report was a horror as a piece of writing. It's interesting to read the Keats Report by*

some people at MUN which was done at the request of Cabot Martin and his people [NIFA]. You [referring to the interviewer] read that. And that was part and parcel of the whole effort that went on for a year or so of criticising DFO science that eventually led up to the appointment of the Alverson group to review our scientific effort.

"That thing done by Keats really set off a little firestorm of criticism. Our scientists ridiculed it--Who are these people? They aren't fisheries scientists. They don't know fuck all. They [the Keats Report authors] were absolutely ridiculed. My own feeling is that they did a neat little piece of work. Real neat.

"Then you read the Alverson report. As I said, a real horror. God what a struggle trying to read it. And I got the sense, that, while Alverson was asked to go and do an objective evaluation of DFO fisheries science, he was most reluctant to come out and be critical. So I have a feeling--and this is purely a feeling based on the tone of the thing and so on--that he could of been, had he been willing, a good deal more critical of our scientific effort.[3]

Following my interview with Brown, I took a second, much closer look at the Alverson Report and related documents. I was intrigued by Brown's suggestion that TGNIF may have privately reached more critical conclusions than were obvious from a cursory reading of their publicly published report. Credible, although inconclusive, inferential and circumstantial evidence in support of this hypothesis can be found within the Report itself. A comparison of the public interpretations of the Alverson Report--presented by DFO in a publication entitled *"The Science of Cod"* (recently removed from circulation)--with the actual contents of the Report. Finally, there is the fact that the internal release of the report was closely followed by a switch to a much more conservative assessment methodology.

Text and data: a case study of interpretive flexibility

The greatest insight into the actual opinions of the Task Group is to be found in the substantial appendices which contain the raw data from which they worked. It is characteristic of scientific culture that assessments of professional values such as "objectivity, integrity, and honesty" are applied much more rigorously to data than to the textual interpretation of that data. While the Task Group fully exploited the interpretive flexibility of the data in the text of their report, the data remained inviolate from conscious manipulation.

As with the Keats Report, one simple table of figures (extracted from the Report's appendices) speaks more clearly than the text. Table 4.2 contains data supplied to the Task Group by DFO but also includes the results of an independent reanalysis by the Task Group of the earlier generations of data from which these figures are derived. Table 4.2 reveals a pattern of consistently optimistic exploitation of the interpretive flexibility of indeterminate data and methodology. In this instance the numbers are estimates of fishing mortality.

Table 4.2

DFO Current-Year Estimates of Fishing Mortaltiy (F) for the Years 1975-1986 and Subsequent Revsions of Those Estimates

Fishing Year	Estimate of F the Following Year	DFO Revisions of F in Subsequent Years				Task Group Estimates of F ( )
		1984	1985	1986	1987	(1987)
1975	0.50	1.03	1.04	1.08	1.09	(1.08)
1976	N/A	1.14	1.12	1.23	1.17	(1.25)
1977	0.40	0.63	0.64	0.53	0.54	(0.55)
1978	0.27	0.45	0.50	0.51	0.53	(0.54)
1979	0.20	0.45	0.49	0.48	0.50	(0.52)
1980	0.17	0.26	0.29	0.31	0.32	(0.34)
1981	N/A	0.24	0.28	0.32	0.35	(0.38)
1982	0.225	0.27	0.34	0.35	0.39	(0.44)
1983	0.225	0.22	0.29	0.32	0.37	(0.45)
1984	0.23	----	0.23	0.27	0.32	(0.43)
1985	0.25	----	----	0.25	0.28	(0.45)
1986	0.21	----	----	----	0.21	(0.40)

[from Alverson 1987 p. 81]

Notice that for the years 1975-76--when Canada was arguing its case for a 200 mile zone of exclusive jurisdiction--the first-year-after estimates are quite high; in the region that indicates an over-exploited, declining stock. Even so, later revisions have concluded that the actual value of F was 100 per cent higher. The first year of Canadian management was 1978 with the TAC set at 135,000 mt to limit the fishing mortality to no more than 20 per cent of the fishable stock or, as expressed in the above table, F=0.20. The fact that, in subsequent years, the fishing mortality for that year was revised upward by 100 per cent means that the size of the stock had been seriously over-estimated--that the actual catch for 1978 of 138,500 mt was approximately 40 per cent of the biomass; a level generally accepted to be inconsistent with the goal of rebuilding the stock.

Also notice that the Task Group's estimates of F are consistently higher than those of DFO, particularly for the more recent years 1982-1986 where DFO's seemingly inevitable upward revision--through hindcasting of F in light of longer time series of data--has not yet fully developed. What this chart is saying is that the Task Group found no reason to believe that DFO's then-current estimates of fishing mortality and biomass would prove to be any less optimistic (or, in other words, any more accurate) than in previous years.

In discussing this problem the Alverson Report stated,

> " . . . *after a large decline in the 1960's and early 1970's caused by heavy fishing, the stock indeed increased after 1977, and has continued to increase since 1982, though probably only very slowly. . . .*
> *"There are several possible reasons for this. First, as a result of the consistent over-estimation of the current stock size, the fishing mortality actually exerted has been consistently in excess of target mortality.* [emphasis in the original, Alverson 1987 p. 61]

This is understatement verging on disingenuousness as one can see from the following chart, Figure 4.1, included as Figure 7 in the Appendices of the Alverson Report. [Alverson 1987 p. 94]

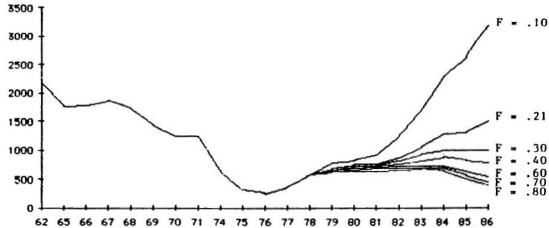


Figure 7. Biomass trend for northern cod stocks, using various assumed F values.

First, one can see that even at a fishing mortality of F=0.30--a figure lower than DFO's own revised figures and considerably lower than the Task Group's estimates--the stock biomass is shown as having doubled from the 1977 low of about 500,000 mt to roughly 1,000,000 mt by 1984 but had since remained static. Using the Task Group's estimates of F=0.40+, the picture is of stock biomass peaking at about 800,000 mt in 1984 with a slow decline thereafter. Simply stated, the claims in the text are not supported by the data in the appendices. The Report's executive summary, which is the only section likely to be read by someone unfamiliar with the specialized language of stock assessment, is even more misleading and milder in its criticism.

> *"Estimates of the growth of the total stock may have been overly optimistic, and although we conclude that the total stock has increased since 1977, it has not reached the expected levels. Nevertheless, it currently appears to be increasing, but at a slow rate." [ibid pp. 1-2]*

> *"The assessment methodologies employed by the fishery centre at St. John's should lead to reasonably accurate estimates of stock abundance. The CAFSAC estimates of fishing mortality in 1986 fall within the (0.2-0.4) range of estimates supported by the data, but at the lower end of the range accepted by the Task Group . . ."* [ibid p. 3]

Claiming in 1987 that the 1986 CAFSAC estimates are "within the range of estimates supported by the data" albeit at "the lower end," is another clear misrepresentation of the Task Group's true opinions on the matter. First, "the range of estimates [0.2-0.4] supported by the data" is, in terms of practical consequences, huge. The terminal-year difference in total biomass estimates is as much as 800,000 mt or nearly four times the total northern cod TAC for that year. Figure 4.1 (above) shows this in graphic form. Second, it is obvious that the Task Group found no reason to assume that the recent DFO/CAFSAC estimates of F would prove to be any less optimistic than those in the past. If the pattern held true, after a period of reanalysis in light of a lengthening time-series of data, the estimate of terminal F for 1986 would stabilize somewhere between F=0.3 and F=0.4. In the main body of the Report the discussion turns to the technique called Virtual Population Analysis (VPA), the method of hindcasting used by DFO and CAFSAC to estimate fishing mortality.

> *"The estimates they [sic] provide, however, only really have these virtues [relative objectivity] for years sufficiently far in the past for the method to have effectively converged to the correct answer.*
>
> *"In the case of 2J3KL cod, this convergence process takes about five years and the estimates of fishing mortality rate, population size and biomass in the most recent fishing years depends to an ever increasing extent on the value of fishing mortality used for the last year (1986) of the analysis. Figure 7 [Figure 4.1 above] illustrates for a range of values of assumed fishing mortality from 0.10 to 0.80, the nature of the dependence of biomass on these inputs."* [ibid p. 33]

In other words, in the Task Group's judgement, claims made about the value of F for a given fishing year derived from the VPA method have little statistical validity or operational legitimacy until five further years worth of data have been collected. By the Group's own admission, its carefully worded, marginal endorsement of the CAFSAC/DFO claims in the executive summary is practically meaningless.

Dr. Jean Jacques Maguire, a fisheries biologists and stock assessment scientist with DFO since 1977 and the chair of the Canadian Atlantic Fisheries Advisory Council (CAFSAC) since 1989, reflected on DFO's history of over-estimation of fishing mortality.

> *"One of the characteristics of the technique which we use, which is sequential population analysis [another name for VPA analysis] on which you've probably read, is the further back you go, the more confidence you have in your assessment. It's called a 'convergence'.*
>
> *"When we extended jurisdiction in 1977, we said, there were all those big foreign trawlers out there. Fishing mortality must have been very high. We've kicked them out and replaced them with large trawlers but much smaller and many fewer of them. Fishing mortality must have gone down. If fishing morality is down, stock size is higher.*
>
> *"We held that belief for five, six, seven years. But as time passes, you do the assessment and you estimate the fishing mortality is 0.2. So that was in 1980. You do the same assessment in 1981 and you estimate that the fishing mortality was again 0.2 but when you look back, you see that it was 0.25 for 1980. Whoops! What happened there? After a few years, you*

look back and you do the assessment in 1985 and you see that for the first part of the 1980s the fishing mortality was about 0.4. So you say, why would it be 0.2 today? There's no reason for it to be 0.2. The boats are fishing as hard. They're out there as long. Their efficiency has probably increased. Which we didn't take into account. And there's no reason for fishing mortality to have decreased. So it must be 0.4. And of the alternative explanations, 0.4 was totally acceptable.[4]

The above analysis of the actual content of the Alverson Report (and reconstructed perceptions of primary sources), while by no means conclusive, does tend to lend support to the hypothesis that the Task Group's actual assessment of the Science Branch's performance was considerably more critical than the impression conveyed by their final report. Further, their data--particularly that contained in the appendices--does not support DFO's subsequent public claims that the *"credibility of DFO scientific advice was not questioned."* [Fo'c'sle 1988] This will be more fully discussed in the following section, *"The Science of Cod."*

An alternative explanation

In the above discussion, I have argued that the textual construction of reality presented by TGNIF was driven by the inertia of macro-level institutional commitments to a strongly rebuilding stock resulting from rational, scientific management. However, as is often the case when dealing with less than fully determinate evidence, there is at least one other quite plausible micro-social explanation of the TGNIF's ambiguity and reluctance to clearly state what they--as eminent fisheries scientists--felt to be serious weaknesses in DFO's data bases and stock assessment methodologies. This is precisely derivative from their status as eminent colleagues of the people whose work they had been called upon to review.

While it is true that the TGNIF membership was not institutionally affiliated with DFO or the Canadian government, by virtue of their specific expertise they were members of the same, relatively small, international community of fisheries scientists as their DFO peers. In a Chapter Five, I establish the fact that most fisheries scientists' professional identity and advancement derives primarily from their membership in and standing within the national and international community of their peers and only secondarily as employees of a particular institution. This being the case, the Task Group's reluctance to publicly criticise their peers, and possibly, in some cases, close friends, would have been entirely understandable.

Which of these possible explanations (or some other) is "true" is not of critical importance to this work. What is important is that I have shown that the construction of reality presented by TGNIf in the text of their report was by no means determined by the underlying data. I have established that, in fact, the data were quite indeterminate and interpretively flexible. Therefore, the explanation for the construction presented in the TGNIF report cannot be found in the data itself but must be located in the surrounding social order. I have identified two plausible sources (one macro-level, the other micro-level) of sufficiently powerful social forces to account for the textual content of the Report. In this case the interests of both social sites were very similar although for very different reasons. The state would have been interested in defending the credibility of science as a source of both domestic and international legitimation for its exercise of the fiduciary responsibilities inherent in a public resource. The interests of DFO would have been served by defending its institutional authority and prestige--both in relation to its institutional competitors for state resources and in relation to the fisheries management structures of other fishing nations. The individual members of TGNIF and the Science Branch would have been interested in preserving their professional relations as members of the international community of fisheries scientists and in avoiding public scrutiny of what they felt to be a purely internal matter. It quite likely that all of these considerations (and, perhaps, others) contributed interactively to the construction of reality defended--albeit somewhat ambiguously--by the TGNIF Report.

"The Science of Cod"

Turning now to an inspection of "The Science of Cod"--DFO's direct public response to the Alverson Report--I will show that it contains misleading simplifications and misrepresentation of the Task Groups's findings. Additionally, it is now known that the work of TGNIF did, in fact, catalyze vigourous and, in some instances, hostile debate within DFO and within the Science Branch in particular. This debate was to result--less than one year later--in the radically reduced (by about one-third) 1989 estimate of the northern cod biomass by CAFSAC. It is important to note that this was the first reduction in current-year estimate since the advent of the 200 mile limit in 1977. And yet, in *"The Science of Cod,"* there is only the most perfunctory sort of acknowledgement of the uncertain and highly debateable nature of DFO's stock assessments. The dominant tone is one of objective, scientific authority.

The publication is introduced by a letter from Eric Dunne, the Director General, Department of Fisheries and Oceans, Newfoundland Region. It says, in part:

> *"Fisheries science is vital to the well-being of the industry. The scientists and technicians involved have dedicated many years to the pursuit of information on fish and their environment and are world experts in their field. They have made a substantial contribution to the rebuilding of overfished stocks as evidenced by the* **five-fold increase in northern cod since 1976....**
>
> *"As part of the work of TGNIF, the size of the stock in 1986* was estimated. *Dr. Alverson noted in a CBC interview on November 20 that 'it's rather amazing that we (TGNIF and DFO) are as close to each other as we are.' The Task Group estimated rather fewer older cod (aged 7 years and older) and rather more cod aged 4-6 years.* **The difference in numbers overall was about 4 to 5%.** *The Task Group estimate of the weight or overall biomass of the stock in 1986 was about 11% lower that the Canadian Atlantic fisheries Scientific Advisory Committee (CAFSAC)* estimate.
>
> *"It is reassuring that the conclusions of the Task Group and CAFSAC about northern cod are quite similar with respect to the present stock size and the causes for the decline in the inshore fishery since 1982.* **The credibility of DFO scientific advice was not questioned**" [emphasis added Fo'c'sle Vol 8, No. 2, "A Special Science Edition" DFO Feb. 1988]

If one were to read this without having also closely examined the Alverson Report, one would quite naturally conclude that the Task Group supported the claim of a five-fold increase. But that was not the case at all. Notice that the claim is made *prior* to any mention of the Task Group and its findings. The confusion was purely intentional as the following discussion with Dr. J.J. Maguire will show. Maguire is chair of CAFSAC, the structure that mediates between the sites of scientific knowledge production and the political leadership of DFO.

[all emphasis added]

Q: Are you familiar with the history of the Alverson Commission and the Alverson Report?

A: Yes.

Q: Until some recent discussions, I was under the impression, as perhaps most outside observers were, that the first critique of the way science was doing its job came with the [1990] Harris Report. Because the version of the Alverson report that was made public, to the extent that it was critical, it was very mild and, **in public, it was called a vindication by the scientists.**

A: **Yes.**

Q: I've been told by several sources that the original, the first draft of the Alverson Report was considerably more critical and the lessons made public in the Harris report were originally....

A: That I don't know. That I don't know. But the Keats Report was the first one. That was commissioned by NIFA. And that was not taken very seriously at the time.

Q: By science?

A: By DFO science. Yes. Because the analysis was somewhat naive, I think, and because **it was easy for us to discount it. The TGNIF report, in my eye, the way I read it...I don't know if I've seen the same version,** but most likely...was critical. It was critical. Maybe it's the broadcasting of it that was more positive. *But I think it was critical.*

It must be remembered, as well, that there were not that many conclusions that could be reached. The TGNIF report suggested, when you look at it, that the difference between the TGNIF report and the CAFSAC assessment was much greater than between the Harris report and the [1990] CAFSAC assessment. The Harris report and the CAFSAC assessment are essentially the same. They're bang on. They're saying exactly the same thing. While the TGNIF report was saying that CAFSAC [through 1987] has over-estimated stock size.

Q: But, as I recall reading, the difference was something like five percent.

A: It was more than five percent because our assessment at that time was for a fishing mortality of about 0.2. And theirs, their range, was from 0.2 to 0.4. And they picked in the middle, 0.3. So there's a much broader range. And the difference was quite large.

Q: What I'm thinking of is not the original [Alverson Report] but the DFO report called "The Science of Cod" and the first page was about how DFO had estimated that the stock had grown 5.5-fold and this independent review had concluded that it had only grown five-fold but really, that's pretty close and really we're doing a terrific job.

A: That was the interpretation that we wanted to give it. If you look at it from a different perspective, the assessment that TGNIF did showed that there was about a third less cod than we said. *And the Harris report says exactly the same amount. So for TGNIF, that's the interpretation.* For broadcasting, for publicity, the way we decided to use it.

Q: Where was that decision made? That certainly would have been made at a level higher than the Science Branch.

A: Probably. I don't know. I don't know.

Q: And yet Science is taking the public heat for that.

A: I wouldn't say that it was higher than Science Branch.

Q: No?

A: I think if....Again, I don't see ourselves having done much different with the Harris Report than we did with the Alverson Report. We used the Harris Report almost the same way saying "Look! Harris reaches almost the same conclusion as we did." That's the way we used it. Except that the noise that was generated has been much higher because the stock status that was estimated was much smaller.

Q: Perhaps the trigger was that the Alverson Report did not result in a direct reduction in quotas whereas the Harris Report did.

A: From the handling of it, as well, there's another slight difference. Which is that the Alverson Report was presented, more or less, by DFO and the Harris Report was presented by Harris.

Q: So there was more of an opportunity to manage the presentation of the Alverson Report?

A: I think so. [All emphasis added][5]

The following excerpts from *"The Science of Cod"* are illustrative of the specific ways in which DFO chose to interpret the TGNIF findings. The voice is one of unitary scientific authority when we now know that within the Science Branch, individual scientists and factions of scientists were--by that time--fiercely disputing the quality of the data bases, the reliability of the analytical methodology and the validity of the interpretations. Additionally, in this publication, DFO made claims--

particularly about the competence of its science and effectiveness of its management--that the reader was clearly intended to take as having some basis in the Report's content when this was not, in fact, the case.

> *"The most obvious reason for low inshore catches would be a shortage of fish. Yet,* **every calculation of the abundance of northern cod shows that the stock is still growing."** [emphasis added *Fo'c'sle* 1988 p. 1]

This was not true. Several of values of F that TGNIF concluded were within the range of possibility supported by the data described a static (F=0.30) or slowly declining stock (F=0.40). (See Fig. 4.1)

The following description (also from "*The Science of Cod*") of the scientific stock assessment process assumes and exploits the long-standing popular construction of scientists as humble, altruistic seekers of truth and the doing of science as the rigorously dispassionate process by which the truth is revealed.

> *"How can we be sure that the people who set fishing quotas know what they are doing? Sure, management decisions are based on scientific advice, but how do we know the advice is sound?*
>
> *"The answer lies in the nature of science itself. . . . [ibid p. 20]*
>
> *"Scientists are methodical. They value only what they can measure. Guesses, hunches, impressions, rumours, pet theories, likes and dislikes--all these*

things the rest of us find so absorbing must be avoided by a scientist. He puts them aside and looks for facts.

"To be any good as a scientist or advisor, the DFO biologist must be neutral, objective, and professional." [ibid p. 26]

"Science is a curious trade, because scientists thrive by giving away the results of their work. A fisherman who did that would be bankrupt in a season.

"Scientific knowledge is like a huge pool which belongs to everybody and which grows as new knowledge is added. But not just any new information is dumped in. Scientists are cautious, sceptical folk, and each new contribution to the pool of knowledge is closely examined by other experts in the same field.

"It's a process of quality control like fish inspection." [ibid p.20]

"In this process, sloppy work soon gets discarded. And the same strict standards apply whether the information is some new discovery or just raw data . . . .

"In the case of stock assessment, the peer review process is complex. Each of the steps involved is a safeguard against poor research or hasty conclusions." [ibid pp. 20, 21]

Social constructivist studies of the actual production and content of scientific knowledge have amply demonstrated this to be a very effective mythology by which science has created and preserved its position of epistemological privilege and by which scientists have created and preserved a special social and institutional status. However, from the constructivist perspective, the reality is considerably different. In recent years, a few scientists have come to share some aspects of this perception of their activities and knowledge claims. Others still adhere firmly to the orthodoxy. As one scientist said in reply to my questions on this subject "*The truth is discovered, not negotiated.*"

As the atmosphere of growing scepticism--in some cases outright rejection--of DFO's knowledge claims spread from its origins in the inshore sector of the fishery to include influential members of the public and the media, DFO can be seen as having had excellent reasons for appealing the legitimacy of its claims to the established mythology of science. By this point many of the individuals felt, quite correctly, that the institutional credibility of DFO was at stake and that belated public acknowledgement of the substantial uncertainties inherent in stock assessment--and the sharp internal debates engendered by these uncertainties--would be interpreted by the political sector, the fishing industry, the media and the general public as an admission of incompetence and failure. Their fears were not wholly unfounded as this was precisely the reaction that followed the publication of the Harris Report exactly one year later.

Bernard Brown's reconstruction of these events is quite similar to Maguire's in substance though considerably less circumspect.

> *A: I look at this from the point of view of my job, which is a PR hack. And when the whole racket started, when the Alverson Commission was appointed, everybody was in a quandary. How are we going to stop all this criticism? My advice was, and I exaggerate to make a point, go out on our hands and knees and say [to the fishermen], please forgive us. We've done the best we can but we realize we have to do a lot better. Work with us and help us. Instead we took the Alverson Report--which quibbled with our science but didn't condemn it--we took that and ran all over saying "look, aren't we great!"*
>
> *Our scientists were saying that since '77 the northern cod stock had increased five-and-a-half fold. And they were saying all sorts of other things around that basic central fact. So our scientist were saying that our fisheries science effort and our fisheries management effort, based on our science effort, has been a rip-roaring success. Where else on the face of the earth have we gone from a situation like we had in the late sixties and early seventies where we bloody near wiped out the stock, to a point where we now have this huge stock of fish out there?*
>
> *And essentially they were telling the inshore fishermen who were creating all the uproar about the destruction of the stocks, that you don't know what you're talking about.*
>
> *Q: So you counselled humility and they responded with arrogance.*
>
> *A: Precisely so. And from a public relations point of view that was a fundamental mistake and we're still making it.*
>
> *The fishermen were basically understanding of the fact that we were doing our best. All they were telling us was that our best, because of the difficult nature of the science, was not good enough. And they didn't expect us to become good enough overnight. They*

wanted us to admit that our science wasn't good enough and to make fisheries management decisions with that understanding in mind. Not to keep gambling on the optimistic side--that we were right in our science. That's what they were telling us. "Quit gambling. Quit pretending that you know more than you know."

They basically had the same understanding, that it was an extremely complex business and that all of our calculations had huge levels of uncertainty.

Well this [DFO's public interpretation of the Alverson Report] was a complete put-down of all the criticism that our scientists had been getting. Trouble was, over the next couple of years, the inshore fishery got even worse. So we end up a year and a half later with another independent review [Harris]. It would never have happened of course if the scientists, a year or so after Alverson, hadn't started to realize that their own numbers were wrong. And a good deal more wrong than Alverson was saying. In other words, they started to get a handle on the numbers for the first time since '77.

That's what's happened in the last couple of years. Cod being a seven to ten year-old fish, it takes a decade to get a handle on a stock in terms of assessing it. Granted, we've had fisheries science going on in this province for a long, long time but full-blown stock assessment has only been going on on the northern cod since about '77. So they're just starting to get a handle on it. Particularly with a little kick in the ass with all the criticism that forced them to be a little more careful in their research.

They came to realize a year or so ago that they were very seriously out. And as soon as that dawn started to break, the people in Ottawa reacted with another full-blown review of fisheries science.

It's not funny for the poor bloody scientists. They've been crucified through all of this. Really quite unfairly when all is said and done. You can go and quibble at some of their behaviour, their arrogance in their belief in the correctness of their own knowledge. But they were really trying and, god damn it, they're only people and they have been left to hang out to dry.[6]

The final paragraph of "The Science of Cod" recapitulates DFO's claims and dismisses those of its critics in an unequivocal statement.

> *"The Department of Fisheries and Oceans prides itself on world-class scientific capability. The unprecedented rebuilding of the northern cod resource since 1977 is ample testimony to sound management practices based on good scientific advice. Having nurtured the resource to a good stage of health overall, the department is now setting out to enhance that all-important achievement by addressing more intensively and more comprehensively other problems in the fishery."* [Fo'c'sle 1988 p. 29]

Less than one year later it was a matter of general consensus both within and without DFO that none of this was true. Further, my research has shown that at the time it was written, few people within the DFO Science Branch would have been willing to individually make such unqualified claims. From my present perspective, I suggest that the content of this publication is best understood as an argument in defense of DFO's institutional legitimacy rather than a statement of scientific knowledge claims.

The Harris Report

In spite of DFO's self-serving management of the presentation of the Alverson Report, the rising tide of criticism abated not at all. Irrespective of whether or not the northern cod stocks were, as the most radical critics claimed, in a state of critical decline, it was unquestionably true that the institutional authority of DFO was in such a state.

The political dimensions of a scientific crisis

The allocation of scarce resources--quotas for specific species--among competing sectors of the domestic industry and the issuance of specific allocations to the fleets of other nations in support of foreign policy objectives is done by the Minster of Fisheries, a political (and often politicized) entity. It has been said of justice that, not only must it be done, it must be *seen* to be done. Similarly, not only must the Minister's allocations be fair and reasonable, they must be seen to be so. Failure to meet this requirement subjects the Department, the Minister and the Prime Minister of the government in office, to unacceptable and unrelenting political pressures and public criticism. To avoid this and to promote general acceptance by the competing interests of the allocation decisions, the Minister enlists the power and prestige of science as the objective legitimating authority for the year's quotas and other management measures. However, if the credibility of the scientific description of the resource base is seriously damaged, then the Minister's claims to have made the decisions on the basis of principles of fairness and equity, even if true, will not be accepted. The result can be a political crisis.

The foregoing summarizes the position of the then-Minster of Fisheries, Tom Siddon, in the spring of 1989 when it became abundantly clear that his department's official construction of reality was passing beyond criticism and becoming the object of ridicule and contempt. The last straw, albeit a very heavy straw, was the release of the 1989 CAFSAC assessment of the northern cod stock. This was based upon a revised data-weighting methodology and modelling technique and concluded that previous assessments had over-estimated the size of the biomass by approximately one-third--essentially the same conclusion drawn by both Keats and Alverson.

The Harris Commission: mandate and membership

Once more a special commission was established but this time, with a significant difference. The Alverson

Commission's mandate was primarily to investigate the reasons for recent declines in the inshore sector's catches with the object of negating the public criticism and political pressure emanating from that source. The Northern Cod Review Panel was given considerably more scope. It's terms of reference were straightforward.

> *"The panel will consider the scientific advice provided by the Department of Fisheries and Oceans since 1977 on the Northern cod stock and the current state and size of the stock, and make recommendations regarding stock assessment methods and means with a view to better forecasting the size, growth potential and behaviour of the stock in the future."* [Harris 1990 p. 11]

The minister appointed Dr. Leslie Harris, then President of Memorial University of Newfoundland and a historian, as chair of the Northern Cod Review Panel. Former members of the TGNIF recruited by Harris included Alverson and John Pope, a highly respected stock assessment specialist from the United Kingdom.

The Harris Report was explicitly and extensively critical of DFO's pre-1989 stock assessment science and expressed continuing reservations about its current data bases and methods. The fact that the only members of the Harris panel with any real depth of experience in stock assessment science were veterans of the TGNIF is highly suggestive. It lends considerable support to the theory that the Task Group's findings were, in fact, considerably more critical than they were either inclined or permitted to reveal in their public report.

An introductory summary

In the following section I will review the Harris Report with reference to its identification of critical inconsistencies, conflicts, uncontrolled variables, and lacunae in the data collection, mathematical manipulation, and analysis of the fisheries stock assessment process. These points are of interest in that they represent what I call "critical nodes" of opportunity for social input to what has been portrayed as, and believed to be, an objective product of rational science.

The discussion of the Harris Report can begin with a synopsis of its findings. The goal of the DFO management plan during the period of 1977-1989 was to set quotas that would result in the harvest of no more than 20 per cent of the stock in a given year. As discussed earlier, this is usually expressed as $F_{0.1}$ or F 0.20 and is called the "fishing mortality rate." It was assumed, and is still assumed, that the stock could sustain this level of fishing pressure and continue to grow. The calculation of the rate of fishing mortality is of critical importance in that it is derived from total commercial landings from a stock and used as an "indicator of abundance." Very roughly speaking (and discounting natural mortality which is assumed to be a constant), if during a given year X tonnes of fish have been removed from a stock at a rate of $F_{0.1}$ (or 20 per cent of the fishable stock), then the total biomass of that stock must have been 5 times X.

Clearly, a reasonable degree of accuracy in the estimation of total biomass is essential to the effective management of a stock. But the mortality rate indicated by the revised 1989 modelling methodology (F 0.45) implied that the stock biomass was little more than half as large as had been previously thought. If the newly minted assessment was a better approximation of reality--and the Harris Report cautiously concluded that it was--then, for fishery managers, independent fishermen, corporate entities, their employees and stockholders alike, the real effect was as if millions of fish weighing hundreds of thousands of metric tonnes had really disappeared. And rather than being fished at levels of sustainable growth, the stock had been fished at levels that pointed toward commercial extinction. This, in a nutshell, was basis for the perception of a crisis in the northern cod fishery.

Critical nodes: the sites of social construction of scientific knowledge

In its introduction, the Harris Report enumerates the significant sources of uncertainty and error in the stock assessment process.

> "...*a wide variety of factors come into play, all of which have the potential of altering the hoped for results. These factors can include an unpredictable and highly variable physical environment, wide swings in the numbers of young fish annually recruited to the stock, extensive and incompletely known interactions among different species occupying similar territories, the proper reporting of fish catches and the subsequent utilization of available information in sufficiently sensitive and rigorous statistical models.*" (Harris Report pg. 1 1989)

What, exactly is to be assessed?: defining the boundaries of a stock

The Harris Report begins by examining the validity of the conception, measurement, and management of the 2J3KL northern cod as a single stock. It concludes that this is a highly dubious assumption that arises primarily from the convenience of ignoring evidence to tha contrary. For instance, the stock in 2J3KL

> *"...is comprised of a complex of rather discrete sub-groups . . . . Whether or not the spawning sub-groups constitute genetically separable stocks is unknown. . . . there is no evidence that the 2J3KL cod population necessarily recruit young exclusively from the spawning stocks in 2J3KL management divisions . . . . [and there is some evidence of] inshore stock(s) which is/are separate in a genetic and/or behavioural sense from the offshore stocks."* [Harris 1989 pp. 6-7]

In the face of such uncertainties--uncertainties which would be difficult, time-consuming, and very expensive to resolve--it is statistically and bureaucratically convenient to simply ignore these complex, elusive, and confusing variables and proceed as if the stock were a closed system unity. The problem with this is that from the very first step of the assessment process it can be seen that there is no clear understanding of just what is being assessed. Any final figures derived from a process with such a shaky foundation are likely to be more representative of wishful thinking than operational reality.

Models

The first, and most obvious, opportunity for social forces to impinge upon the creation of scientific knowledge is the selection and operation of mathematical stock assessment models. Questions must be asked as to how and for what reasons the constructor of a model selects from the range of possible input data, how the "quality" and relative statistical weighting of that data is determined, how and why the data is subjected to certain mathematical procedures and not others, how correction factors are determined and applied, how the model constructor's work may be subject to influence from the prevailing norms, values, and theories of scientific peers, etc. Other questions must be asked as to who exactly chooses among possible methodologies based upon what legitimating authority and under pressure from what competing social groups. My research enables only a very general discussion of this aspect of the problem. The above noted questions regarding model construction and implementation should be addressed by further, more narrowly focused research.

In the course of our interview, Harris reflected on the dual nature of cognitive models. On one hand, they can be seen as concrete abstractions of the current prevailing cognitive paradigm of the fundamental dynamics of a given system. They are useful tools in theorizing those dynamics and organizing and evaluating the interactive effects of human social activity with a natural system. In this way models are thought to be useful in extending our understanding of natural reality and provide a basis for the rational management of social interactions with the natural world.

On the other hand, because they incorporate basic assumptions about the nature of reality--and because they are, of necessity simplified abstractions--models tend also be determinant of cognitive reality. This occurs through models' origin in, and support of, prevailing paradigms at the expense of alternative constructions of reality. This is achieved by a model's power to frame the questions that can and cannot be asked, its intrinsic definition of data as "relevant" or "irrelevant," and its strong tendency to determine the interpretation of ambiguous data. Harris noted these problems and discussed them in terms of historical examples.

> *A: The danger in all modelling, in my view, is that you become trapped by it to some extent. It's self-fulfilling. You're dealing with data which are manipulable and variable and uncertain. You have a variety of ways that you can interpret the data. If you've got a model that you believe in you will interpret the data in a way that makes the model work. I don't think there's any dishonesty in this, as such. . . .*
>
> *When I was talking to fishermen and fishing groups, I used two or three analogies to try and explain this phenomenon, which I think is universal and has occurred throughout the whole of the history of science and technology.*

A simple example is, perhaps, the Copernican revolution. You have a couple of thousand years of people looking at the earth as the centre of the universe. The mind set is there, firmly fixed. There's no question about that whatsoever. So you see all this other data, the orbits of planets, and it doesn't fit. But what you do instead of saying "our premise must be wrong because these orbits are impossible," you say "we have to find a fancy way of modelling to prove or to show that these sorts of orbits can be created with the earth still at the centre of the universe."

So you have brilliant minds devising weird mathematics to show why planetary orbits are the way they are [Ptolemaic cosmology]. Defying all logic but very seriously presented until Copernicus comes around and says "Look. You've got it all wrong. Let's suppose that the sun is the centre of the universe. All these orbits suddenly work." Well it's the same with this fish model or any other any other model.

Take William Harvey and the circulation of the blood. People had been cutting open cadavers for years and years and years and looking at the circulation system. Looking at the veins and the arteries. Looking at the whole system. But they couldn't admit what their eyes saw because they had a conception of the heart which indicated that it was more than a pump.

Q: So theory and expectations can overpower data?

A: Exactly. And I think that's the danger of all modelling and it's a danger when you have a particularly unsophisticated model. And I think the model that was being used, the bulk-biomass method, is essentially an unsophisticated, primitive model.[7]

Terms of reference: the reification of language

Another critical issue is the decision to express stock strength and make management decisions primarily in terms of biomass rather than population. This is probably related to the fact that the largest source of data used by DFO is from the commercial, offshore sector of the fishery and--since all transactions involving the catch are in terms of weight--that it is far easier methodologically to conceptualize the stock as a biomass rather than a population.

Besides the sheer volume of data available from commercial landings, this source has other attractive aspects. It is free of cost at the source and it tends to exhibit less variability than other data sources. But the choice to treat the stock as a biomass can result in a high degree of uncertainty as to the actual numbers and reproductive potential of the stock. Two hundred thousand 10 kilogram fish have the same biomass as one million two kilogram fish but the two populations have very different implications for resource management.

Further complicating the picture is the fact that the fishable stock is comprised of fish ranging from age four up to a few venerable twenty-year old fish. Each age group is referred to as a "year-class" and is identified by the spawning season from which it arose i.e. the 1986 year class will be four years old in 1990.

Data as a source of interpretive flexibility

The Harris Report identifies five primary sources of raw data for stock assessment: 1) catch data from both the inshore and offshore sectors, 2) catch per unit effort (CPUE) data from the offshore sector, 3) research vessel (RV) data, 4) age-length and age-weight samples of the catch, and 5) on-board DFO observer data on by-catch, discards, and operational methods of the offshore sector. Each of these sources can also be seen as a critical node for potential social inputs to the data.

1.) catch data

Catch data is supplied voluntarily by the commercial sector to the scientific sector. In addition to the possibility of a large discrepancy between the two sector's concepts of this data's value and the importance of its accuracy, it is further conceivable that the commercial sector may have incentives to under-report or otherwise manipulate the data before transmitting it to the scientific sector. This would have been a more significant source of error and uncertainty when DFO's independent observers were only occasionally present aboard the offshore trawlers. Since 1990, nearly all boats over 100 feet in length--whether domestic or foreign--fishing inside Canada's 200 mile zone, have had a DFO observer aboard.

2.) CPUE data

Catch-per-unit of effort, (CPUE) data is used as an "index of abundance" under the assumption that a given unit of fishing effort, expressed as some unit of purposeful fishing activity (time that the net is in the water and fishing or days at sea for instance), will produce more or less fish in relation to the stock's relative abundance. This does not take into account the influence of such variables as changes in technology and technique that improve efficiency, general changes in the relative skill of the skippers, changes in fleet management strategies in response to market conditions and/or DFO management decisions, adaptations to a changed regulatory environment, unusually good or bad weather, and possible changes in the stock's patterns of behaviour due to fishing pressure and/or significant environmental influences.

Most critically, it does not take into account the fact that northern cod are not randomly distributed but are a densely schooling species, especially during the spawning season when the stock concentrates on a few, well-known and relatively shallow banks. Concomitantly, northern cod are not randomly hunted. Using very powerful fish-finding sonar, the skipper of an offshore dragger will not shoot his net until he has located a sufficiently large school of fish. If it is a very large concentration of fish and--as is usually the case--the skipper is an employee of a multi-ship corporation, he will contact headquarters by radio giving the estimated size and exact location of the school to the director of fleet operations who will then vector in one or more additional ships. In this respect, modern corporate fishing of northern cod is more like mining a vein of ore than hunting wild game.

Irrespective of these serious deficiencies, until the 1989 CAFSAC assessment, considerably more weight was given to the CPUE data as an indicator of abundance than to the scientific survey data collected by DFO's research vessels (see below). The reasons for this appear to be quite simple, very human and very unscientific. Massive amounts of data were available from the offshore trawler fleet at no cost. The data showed little internal variability. Most importantly, the CPUE data could be interpreted to confirm the previous descriptions, predictions and expectations of a healthy, growing stock and the fundamental soundness of DFO's management strategy.

3.) RV data

Research Vessel (RV) data attempts, through random or stratified sampling, to construct a statistically projected portrait of the stock. Sampling sites are selected in the management area and a net is towed for a given length of time (usually one-half hour) at selected depths on specific courses called transects. The fish caught are counted, measured and weighed. The total swept area of the sample is known, as is the percentage swept of the total management area. A simple multiplier factor will then yield a portrait of the entire population.

One might assume that data from this source would be the most rigorous and least ambiguous. The data is collected directly by scientists through research designed expressly for that purpose. In fact, scientific population research surveys do not appear to be any less susceptible to social inputs than other data sources. There are significant sources of error--and, therefore--interpretive flexibility, in RV sampling.

The first is that, by normal standards of statistical validity, the population is hugely under-sampled. Research vessel operations are very expensive and time-consuming and must be negotiated in competition with other demands for available financial and human resources and ship time. Thus, while continuous, year-'round sampling could conceivably provide realistic data, the operation--through 1990--was limited to a one month-cruise in the fall of each year. The timing of the cruise and the ability to adhere to the sampling plan are influenced by the prevailing weather. The consistency of the quality and quantity of the human resources and sampling gear is unknown. And the validity of a projected population portrait derived from such a small slice of space/time is the subject of considerable debate. *"It's like trying to tell the population of St. John's by counting the people in one house,"* said a fisherman on a recent episode of "On Camera" on CBC-TV. *"It is highly unlikely that we would miss any large concentrations of fish,"* said a DFO biologist on the same program. *"It's like trying to count moose at night from a helicopter,"* said Mac Mercer, then Director of the Science Branch.

Some practical examples of the sources of error and indeterminacy in RV data were given by Chris Lang, an electronics engineer with DFO working to develop hydro-acoustic technology for incorporation in the research surveys. He began by describing a purely social dimension of data collection resulting from the professional/bureaucratic differentiation between the technicians who design the equipment and collect the data and the research scientists who analyze the data and incorporate it in the assessment models. It should be noted that hydro-acoustic sampling is still in development and the data is not yet used in northern cod assessment. However, many of the issues raised by Lang are more broadly applicable to current assessment methodologies.

> *Q: Do you go out on the cruises?*
>
> *A: Once or twice a year. Somebody has to go. Our job is to get this data on computer tapes of higher quality than the scientists can deal with so we can stay ahead of them. So that their problem isn't us.*
>
> *Q: Is this kind of like a game?*
>
> *A: Not really. It's CYA...cover your ass. That's really our job. If the limitation becomes the quality of the data or the quantity of the data, then it's obviously our problem. There's a lot of analysis techniques that have to be developed to interpret the data. It's still too raw.*
>
> *Q: Who does that?*
>
> *A: Someone else. I calibrate the equipment so in that sense I can bump things up by 20,000 tonnes here and 20,000 tonnes there or bump them down by 20,000 tonnes.*
>
> *Q: How?*
>
> *A: I could just cheat on calibration if I wanted to. I could say that this many blips means that many fish when in fact it only means half that many fish.*

Q: How do you do your calibrations?

A: Honestly.[8]

We turned to the subject of social inputs to, and theory-ladenness of, research design and data analysis.

A: And that problem [theory-ladenness] represents itself in perhaps 25 layers before some number comes out of a survey. For instance, you're trying to measure the spatial distribution and concentration of fish but you only have one boat and one transducer. And there are survey designers who say you're going to drag it across this path and then you'll go up there and drag it and then go down there and drag it here so that we can look at it all and get something out of the whole thing.

If you had the luxury of 25 ships you could go through the one area at the one time and gather the stuff in parallel and then you could structure your analysis a whole different way. But...The fish could be chasing your boat around, for example. They might like the sound of it.

They're tracking some stock and all of a sudden you get a blip in life. Something might be wrong, right? So you go back and do it again. I don't know if I believe this or not. Well maybe I screwed this up. There's some amount of evidence to suggest that I did and some to suggest that I didn't. Or, maybe I screwed that up or...there's always subjective inputs all along the process.

I guess that there are social inputs in that scientists read the newspapers and they know that they need to have more fish than they are saying that they have. If he's stuck with a question with a fifty-fifty answer, he's going to take the one that gives him the answer he's looking for. If you wanted to insure that a truly objective job be done, you should lock the scientists up somewhere, don't let them read any newspapers, don't let them talk to anybody.[9]

In the following passage, Lang and I discussed his initial experiences with a new, much more technically sophisticated and sensitive hydro-acoustic system. It shows that increased sophistication and sensistiviy per se do not guarantee a reduction in the indeterminacy of data. In fact, it can have quite the opposite effect by revealing sources of variability and error hidden by the relative crudeness of the previous equipment.

A: There are some things that are not clear about the data that we get with an acoustic survey now...that we have to pin down by catching fish with a trawl whereas you could measure them directly with a multiple beam system.

Q: So you'd be able to see individual fish?

A: Yes. Then you could scale the cloud on the basis of the measurements that you make on individual fish. But there are some things that are unknown. Not so much on the implementation of the technology but in how to interpret the data and how to remove some of the biases that show up.

Q: What sort of biases?

A: Well it's easier to get good quality measurements on fish that are bigger and to reject smaller fish because the quality of the measurement is not good so you tend to bias the population that you are characterising on the high end of the size of the fish that are in the cloud.

Q: So do you drag a trawl through a population at the same time that you are scanning it to calibrate the equipment?

A: No you sample a population alternately. Within the parameters of the survey design, you periodically stop surveying and pull the survey equipment in and let the *trawl out, tow it for half and hour, pull it in and let the survey equipment back out.*

Q: So you're not fishing the same fish that you are surveying?

A: In a global sense only.

Q: Wouldn't it be better for calibration to be beaming down onto a population just before you dragged a trawl through it?

A: A trawl isn't a real good sampling tool for fish in that respect because you're looking down from a survey transducer at a depth of five to ten meters clear to the bottom whereas a trawl can only sample some subset of what's right in front of the net. There's something to be said for what you suggest but there's a long way to go yet before that's necessary. The calibration is not that precise yet anyway.

Fish look very different with behaviour. The aspect of a fish changes its acoustic signal, its reflectivity, tremendously. So if you are sampling a population that for some reason is more vertical in the water or making vertical depth changes as you are measuring them, or if you are measuring a population that's turned broadside to you and swimming on the level...even though it's the same population of fish, they will look very different acoustically. So it's not an exact science by any means. There's a lot of work to be done.

Q: Last time we were talking you mentioned error bars and it wasn't clear to me whether you were talking about errors in the TAC or in the whole population.

A: Well just from an acoustic point of view, you can have as much as one quarter as much peak response from one population. So you could survey one population and have a certain beam response and another time you could get as low a one quarter of the same population if they were all hanging around with their noses up. So there's that level of uncertainty.

Q: So you're saying that with the technology and the techniques that you are using now you could be dealing with levels of uncertainty as much as 75 per cent one *way or the other simply due to variabilities of fish behaviour?*

A: Yes. Just on that one alone. Just on behaviour.

Q: So in your opinion the behaviourial unknowns are the biggest source of uncertainty in the data?

A: With the possible exception of survey design.[10]

Later I discussed the problems of data acquisition and analysis with Dr. Jake Rice, then head of the Science Branch's Groundfish Division. He suggested a higher-order concern with the increasing sophistication and sensitivity of data acquisition technologies having to do with the sheer volume of data generated by these systems. Rice said that the only choice they have is to increasingly automate data manipulation and analysis but that this inevitably means that the technology begins to dictate the questions that they ask.

Until recently, the assessment modellers themselves apparently harboured reservations about the relative accuracy of RV data in that it was often discounted in relation to CPUE data as an independent indicator of abundance--that is until the 1986 RV survey which showed a 150 per cent increase in the biomass from the previous year. [Harris 1990] This astonishing increase was apparently accepted as reality and unproblematically incorporated in the assessment. Perhaps no other piece of evidence so convincingly illustrates the ability of established belief to overpower the supposed objectivity of the scientific method, not to mention common sense. It is probably fair to suggest that if the 1986 survey had indicated a proportional decrease in the biomass, the results would have been immediately identified as anomalous and rejected.

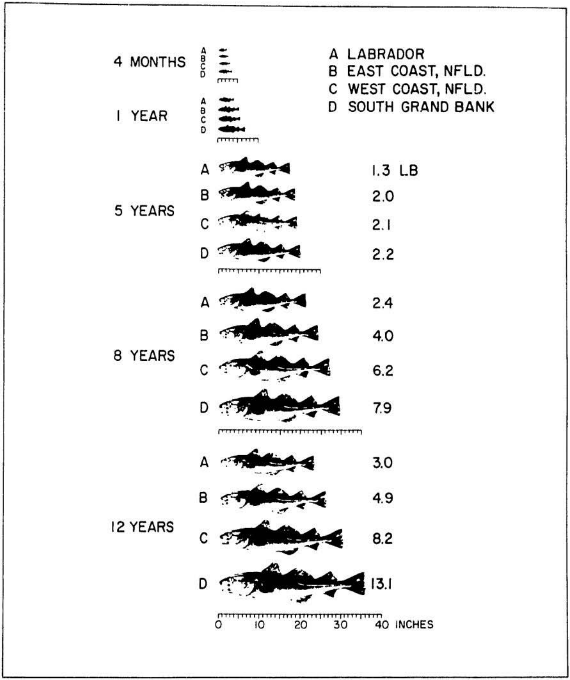
Of this event, Harris says,

> "...the 1986 survey values which were incorporated into the earlier RV survey calibration have now been shown to be an artifact of resource availability *[a disproportionately high number of fish just happened to be in the survey areas]*, probably brought about by a change in the timing of the 1986 RV survey. . . . *Whether or not the 1986 survey data should have been suspect and ignored...is a value judgement which is easier to make in retrospect than it might have been in earlier years.*" [Harris 1990, pp. 73, 74 emphasis in the original]

4.) Age-length and age-weight data

Age-length and age-weight samples would seem to be a more straight-forward proposition. It is presumed that growth-rate of age classes is an index of environmental pressures on the stock. More is better, less is worse. And yet the bulk of this data is collected ashore on commercially landed fish and it is possible that some of the same variables that can influence CPUE and catch data are present in these samples.

Further, the growth rates can vary dramatically within the 2J3KL zone by factors as high as 300 percent. Figure 4.2 (Figure 20 in Harris) taken from *"The Science of Cod"* shows the observed length-and-weight-at-age differentials for fish from various regions. At the extremes of the northern cod range, Labrador and the South Grand Bank, the average eight-year-old fish will vary from 17 inches and 2.4 pounds to 30 inches and 7.9 pounds. This disparity increases with age. A 12 year old fish will average 22 inches and 3.0 pounds in the north and 36 inches and 13.1 pounds in the south. It is not clear from my research how and to what extent this regional differential is incorporated into the assessment of the northern cod biomass. As a possible critical node for social input, this data source must be much more thoroughly evaluated.



Lengths and weights of cod (gutted, head on) of the same ages from various Newfoundland and Labrador areas. (From Templeman 1966 Marine Resources of Newfoundland).

5.) commercial observer data

The Harris Report is not clear whether by "commercial observers" it is referring to scientists placed aboard the vessels or independent technicians working under contract to DFO. It is also not clear whether participation by any given vessel was voluntary or mandatory nor is it stated what the frequency and distribution of commercial observations might be and what, if any, controls are placed on the possible variables in their observations. A skipper may well alter his methods and practices in the presence of an outside observer. In any case, the Harris Report is inclined to discount the value of this data source because,

> *"There is some question as to how effective these data have been in augmenting the scientific database because of the quality of the observers and the lack of data processors and computer time for its analysis."* [Harris 1989 p. 10]

Due to a recent expansion in the program, data from observers on the offshore trawlers may be somewhat more robust than was previously the case. Every foreign ship fishing inside Canada's 200 mile limit is now required to carry a DFO observer aboard and most of the Canadian offshore trawler fleet is monitored by observers. While this may encourage more accurate reporting of fishing results, it can only exacerbate the above-noted problem of growing backlogs of data awaiting manipulation and analysis.

A concrete example of this problem was the recent announcement that the Science Branch had finally been allocated the necessary human and financial resources to begin an analysis of the contents of a three-year backlog of cod stomachs. While it is known that an understanding of the food and feeding relationships of cod with other species in the eco-system is an absolutely fundamental requirement for an understanding of cod population dynamics, tens of thousands of cod stomachs had--for years--been carefully collected, labelled and filed away in freezers for lack of analytical resources.

6.) other factors

The Harris Report concludes its discussion of the data sources by noting that data on environmental influences--relative abundance of both food and predators, changes in ocean currents, water temperature, salinity, oxygen content, and other oceanographic and meteorological factors--although available, *"have not as yet been used to adjust population estimates in providing advice to the government..."* [Harris 1990 p. 11]

There are several plausible explanations for the omission of these data sources from stock assessments. One, as in the case of cod stomach contents, is a simple lack of the requisite resources for their systematic incorporation. Being simple, this explanation is also simplistic. It begs the general question of the criteria determining the federal government's allocation of resources to DFO, DFO's allocation of resources to the Science Branch and the Science Branch's final deployment of those resources. This too is a problem deserving of more thorough research.

Methodology of stock assessment

The methodology used by DFO to generate its stock assessments is known variously as sequential population analysis, cohort analysis or Virtual Population Analysis (VPA) "tuned" by RV- and CPUE-derived "indicators of abundance." VPA involves tracking and estimating the annual mortality of each year-class of fish. By counting the number of 1982 year-class fish caught in each of the successive years until no more 1982 fish are caught and adding to this the estimated number of 1982 fish that died of natural causes, one can--by 1995 or so--know about how many fish were in the 1982 year-class.

The initial weaknesses in this method are the uncertainty of the rate of natural mortality and its assumption that all year-classes of fish are proportionally represented in commercial concentrations. Natural mortality may be affected by oceanographic changes, variations in the food supply, increases or decreases in the predation by other species, and disease. And yet, in the assessment model, natural mortality is not a variable but a constant--assumed to occur at an annual rate of 20 per cent for all year-classes. This is, of course, a practical necessity as there is no known technique for monitoring natural mortality. Another problem is that boats operating under an enforced quota have a strong incentive to discard the smaller, less valuable fish--landing and reporting larger and older fish--introducing an unknown, uncontrollable bias to landings surveys. With nearly 100 per cent coverage of the offshore fleets by DFO observers, this may now be less of a problem than in the past. However with today's very powerful and sophisticated fish-finding sonars, a skipper may be able to pre-sort his catch to some extent by bi-passing concentrations that seem to contain a high proportion of small fish. None of these factors, or other possible variables, were controlled for in the VPA--or even very well-understood. Again, the reliance on the commercial catch as a data source was questionable.

To convert this "hindcasting" to a current stock assessment DFO applies a calculated value of fishing mortality (i.e. F=0.20). If, for example, in the 1987 fishing year, 50,000 mt of 1982 year-class fish are caught weighing an average of 1 kilogram each and--if the value F=0.20 is correct--then the total biomass of 1982 fish at the beginning of 1987 (and accounting for a natural mortality of 20 per cent) was 250,000 mt with a total population of 250,000,000 fish. Performing these calculations for each year-class present in the commercial catch will yield figures for the total fishable biomass and total fishable population.

The VPA-based, F-derived figures for total biomass and populations of all year-classes are then "tuned" by the introduction to the model of RV- and CPUE-derived indicators of abundance, both of which are subject to the sources of error and uncertainty noted earlier. To add to the confusion, the RV and CPUE data often show conflicting trends.

> *"The picture painted by RV data shows a noticeable decline in the number of fish in the population since 1985, while the commercial fishing database suggests a considerable increase in the number of animals in the*

population. Which to pick?" [Harris Report 1989 p. 27]

Which indeed! The current DFO solution to this problem, to simply average the two, shows no real confidence in either source but rather the hope that the errors and anomalies of each indicator will somehow negate each other. At present, this technique does have the happy result of showing an essentially stable stock. The end result is the official DFO estimate of total biomass and population and forms the basis for the setting and allocation of quotas.

Summary and Analysis

The findings of the Northern Cod Review Panel were presented to the public by Harris and copies of the report were widely circulated. Prior to its release, its contents were the focus of intense speculation in the public media and, after it became available, subjected to months of public analysis and debate.

Two things seemed to be abundantly clear. The first was that DFO's claims--prior to 1989--of steady stock growth were, in fact, not true and that since sometime around 1984, the stock had remained at best, static or, very likely, experienced some degree of decline. This erroneous perception of the stock was the result of consistent and persistent under-estimations of fishing mortality and consequent over-estimations of abundance. Severe and socio-economically punishing reductions in the TAC were necessary to correct the situation.

With this in mind, the Harris Report concluded that the most recent (1989) CAFSAC estimate of fishing mortality of F=0.44 with a TAC of 235,000 mt was *"most probably in the right domain,"* but that an immediate reduction of the quota to achieve a real fishing mortality of F=0.20 (a TAC in the vicinity of 125, 000 mt) *"would precipitate social and economic repercussions of a particularly drastic nature."* As an interim measure it suggested a 1990 TAC of 190,000 mt (F=0.30) but cautioned that this *"may not serve to reverse the trend of a declining spawning stock but may rather contribute to further decline."* According to figures gleaned from a recent DFO publication, this reduction of 45,000 mt would mean a loss of about $26.6 million to the industry in direct landed value, $66.6 million in processed product value and 1,035 Person Years of employment. [Dunne 1990]

Reviewing the Keats, Alverson and Harris Reports, it is immediately apparent that all three investigations reached remarkably similar conclusions. This, in spite of the fact that they had differing institutional sponsors, differing mandates and differing human, financial and temporal resources with which to work. This congruency of findings strongly suggests that their constructions bore a closer correspondence to natural reality than did that the claims of DFO.

Having established the general context, I am now prepared to state and examine in the following chapters the central sociological problem: why did DFO science--working from the same data bases as Keats, Alverson, and Harris and with vastly superior resources of every kind--persist in the construction what is now accepted as an erroneous reality and why did it defend that construction against competing alternatives to the point where its epistemological authority and operational effectiveness in the management process were severely compromised?

In the preceding chapters I have outlined the multi-levelled historical, institutional, and cognitive dimensions of the problem--both identified and hypothesized the social dynamics that animate this structure--and located important aspects of the problem in time and space. The essential features are as follows:

Science and the state have struggled to define the terms and conditions of their relationship from the first year of the creation of a formal institutional association. This relationship temporarily stabilized and harmonized around the issue of extending Canada's exclusive jurisdiction for the management of marine resources to 200 miles. That essential harmony was preserved (at least with respect to stock assessment) as long as all the consumers of scientific knowledge found its construction of reality unproblematic. I have shown that this construction fully exploited the interpretive flexibility inherent in a very inexact branch of science and that the direction of the interpretations was driven by strong institutional commitments to a very specific reality and a techno-utopian vision on the part of individual scientists as to the possibilities for effective, rational management presented by the 200 mile limit.

The initial challenge to this construction arose from a previously unrelated source--the traditional inshore fishery. Their challenge was formalized in the Keats Report but was initially rather easily discredited and dismissed by both science and the state. However, the inshore sector refused to accept their marginalization and began to actively and effectively exploit their cultural and grass-roots political power. It was at this point that the relationship between science and the state became increasingly problematic.

In its public presentation of the Alverson Report, DFO showed an apparently united front in defense of their claim of a robust, rebuilding stock. However, in the process of conducting their enquiry, the Alverson group seems to have triggered a crisis within the Science Branch itself as to the validity of their assessment data and methodologies and, therefore, their claims. The result was the crucial reassessment in 1989--which is thought to have employed more realistic data fed into a more rigorous model. This reassessment concluded that the stock was not growing but was stable at a biomass somewhere between two-thirds and one-half of DFO's previous claim.

To the consumers of scientific knowledge and related interests this appeared to be clear evidence of scientific incompetence. The inshore sector widened the scope of its attack to include not only the Science Branch but DFO as a whole, the federal government, and the offshore trawler industry. They charged the scientists with incompetence, the political/bureaucratic elements of DFO with mismanagement of the resource, the federal government with irresponsibility in permitting this situation to develop, and the offshore trawler industry with rape of the fishery through overfishing and ecologically destructive methods and technologies.

While the public focus of the crisis was on the status of the northern cod stocks, the real crisis was one of authority and legitimation--the institutional and political authority of the federal government, the epistemological and professional authority of science and scientists, the cultural authority of the inshore fishery--and the struggle for legitimation of each of their respective, conflicting, cognitive orders and constructions of reality. From the perspective of the state, DFO and science had failed in their primary function; the provision of authoritative, unproblematic legitimation for the political management of a public resource. From the perspective of science, the state had failed to accept the experimental, probabilistic nature of scientific knowledge construction. Instead, the state had attempted to exploit the epistemological authority of science to legitimate a politicized socio-economic fisheries policy. From the perspective of the inshore fishery--and much of the general public--both science and the state were committed to a construction of reality that favoured the interests of the offshore trawler industry at the ruinous expense of the traditional fishery.

This was the situation faced by the Harris Commission when it was formed by the of Minister of Fisheries in 1989 and accounts for the broadly-inclusive terms of reference which established the scope of Harris's enquiry and directed its activities. In its findings, the Harris Report generally confirmed the Science Branch's revised assessment of the stock's status. It's criticisms largely applied to pre-1989 assessments and it made a set of specific recommendations for future improvements. It passed the notice of the critics that most of these recommendations were for programs and lines of enquiry that the Science Branch was on record as having identified as things that it would have already done or been doing given sufficient budgetary commitments from the state.

From the perspective of this work, the primary value of the Harris Report is as a guide to the nodes of opportunity resident in scientific data and assessment methodologies for the intrusion of social forces into the construction of scientific knowledge claims.

In the following chapters I will turn almost exclusively to my interviews with the key actors in this debate. The points of contention and the theoretical issues will be examined from empirically-grounded points of view at various levels of social organization. We will begin at the micro-social level in a reconstruction of a fundamental conflict between two small groups of scientists within the Science Branch--a conflict that had enormous macro-social consequences. To adequately explain and understand the origins and nature of this conflict, we will move to a larger level of social organization and examine issues of professionalism, reward, and promotion as they apply to scientists embedded in a political bureaucracy. The next stop is an examination of the curious functional/dysfunctional epistemological relationships between fisheries science and the inshore and offshore sectors of the fishery. Finally, we will conclude with a more lengthy and empirically-grounded discussion of the relationships between science and the state.

ENDNOTES

1. "NIFA is a coalition of people who fish for cod or work in or own inshore fish plants or simply care about Newfoundland's environment and inshore fishing communities."

The above is from an open letter included in NIFA's public relations materials, dated November 27, 1989 and signed by the organization's president, Cabot Martin of whom we shall hear more in Chapter Six.

2. From an interview with Bernard Brown conducted in St. John's on August 3, 1990. The full transcript is Appendix B.

3. From an interview with Bernard Brown conducted in St. John's on August 3, 1990. The full transcript is Appendix B.

4. From an interview with J.J. Maguire conducted on October 28, 1990 in St. John's. The full transcript is Appendix J.

5. From an interview with J.J Maguire conducted on October 28, 1990 in St. John's. The full transcript is Appendix J.

6. From an interview with Bernard Brown conducted on August 3, 1990 in St. John's. The full transcript is Appendix B.

7. From an interview with Leslie Harris conducted on August 29, 1990 in St. John's. The full transcript is Appendix G.

8. From an interview with Chris Lang conducted on March 4, 1990 in St. John's. The full transcript is Appendix D.

9. Ibid.

10. From and interview with Chris Lang conducted in St. John's on June 27, 1990. The full transcript is Appendix D.

CHAPTER FIVE

IRRATIONAL DYNAMICS IN A RATIONAL CONTEXT

Both science and the state bureaucracy are classically rationalist institutions with structures which are intended to insulate the process and production of these institutions from the irrational forces of individual and collective human social reality. That the state bureaucracy is, nonetheless, capable of producing stunningly irrational results is commonplace knowledge. That this is also true of science is less well-known. Legendary battles have been waged between eminent scientists with competing knowledge claims; battles that--in some cases--have survived the deaths of the originating individuals.

What I will discuss in the following chapter are the irrational social forces that are generated--and can powerfully impinge upon scientific knowledge production--when science is embedded in the state. In the first section we will get an unusual look--through the words and reconstructions of two of the principal actors--into the heart of a virulent micro-social conflict between factions of the Science Branch. In the second section I will identify a plausible structural origin for this conflict and argue that, at least in the case of fisheries science, the institutional marriage of science and the state is fraught with irreconcilable differences. Finally, I will propose that these differences are the source of social forces that have had a significant impact on the production of northern cod assessment knowledge claims.

Stock Assessment Science and Tribal Warfare

Thus far, we have seen that--with the extension of Canada's management authority (and responsibility) to 200 miles--firm institutional and individual commitments were made to the re-creation of an abundant, rationally managed and, therefore, reasonably stable resource. Against this backdrop, other social processes were also to have a significant input into the creation of scientific knowledge claims. Of these, the most archetypically human (and, therefore, the most interesting) were conflicts and occasional outright warfare between a shifting cast of social groups. The membership in and organization of these groups was not stable but varied in relationship to the specific issues and over time. In many instances, individuals and group sub-sets were simultaneously locked in a bitter dispute over one issue while firmly united on another.

To attempt a holistic, synchronistic exposition of this complex interplay of forces and interests would be hopelessly confusing. For the purposes of this work, I have selected one particularly consequential conflict as illustrative of the tribal dynamics that may operate behind the facade of dispassionate scientific rationality. It is worth noting that--according to the primary sources--this conflict is by no means anomalous, has yet to be fully resolved, and continues to contribute to the production, negotiation, and presentation of scientific knowledge.

The data is presented in the form of excerpts from interview transcripts with two of the principal antagonists. Their reconstructions of the issues are so radically divergent as to be mutually exclusive. At issue was access (or denial of access) to data among research scientists responsible for northern cod stock assessment at the Department of Fisheries and Oceans (DFO) research station in St. John's, Newfoundland. As this internal conflict escalated, factions formed around the nuclei of a few highly-respected scientists on each side.

My position is that their clashes are better understood as a form of tribal warfare than as a normative scientific debate. The consequences of their struggle were non-trivial in that they can be shown to have prolonged what is now seen as a persistent over-estimation of the northern cod stock by as much as a factor of two. [Alverson 1987, Harris 1990]

Macro-level consequences of a micro-level conflict

This delay in the reinterpretation of available data and the ensuing revisions in assessment techniques contributed significantly to the profound sense of biological, social, and economic crisis when these changes were finally made. The new perception of the state of the stock precipitated drastically reduced quotas, the idling of offshore trawlers, the closures of processing plants, and the wholesale lay-offs of workers.

The consequences of their conflict--and that of the schismatic forces that they represent--were not merely "academic" but of enormous socio-economic significance to the Province Newfoundland and Labrador. The Harris Report provides a quantitative description of the people dependent upon the northern cod.

> " . . . . *In actual numbers, this means approximately 8,100 full-time fishermen, 8,200 part-time fishermen, and 18,600 plant workers for a total contribution to employment of 34,900 which does not include deep-sea fishermen and plant workers from the south coast communities that also depend, in part, upon access to northern cod.*
>
> *"In a province where the unemployment rate is 16% [and the population just over 500,000], some 35,000 jobs is a matter of very great consequence . . . ." [Harris 1990 p.40]*
>
> " . . . . *For the vast majority of the communities in question, northern cod was the only reason for their existence and northern cod remains the only substantial economic basis for their survival. And this is a simple statement of fact . . . .*" [Harris 1990 p. 21]

The federal government's stated policy regarding the setting of Total Allowable Catch (TAC) quotas for northern cod was that they should be set to achieve a target fishing mortality of $F_0.1$ or approximately 20 per cent of the fishable stock biomass. The eventual acceptance of the revisionist analysis of stock assessment methodology and results implied--if the $F_0.1$ policy were, in fact, to be adhered to--a decrease in the TAC from 235,000 metric tonnes in 1989 to 125,000 mt (approx.) for 1990. The estimated socio-economic impacts would have been as follows:

1) A decrease in employment of 2,530 person-years. [But note that in an industry characterized by a high percentage of seasonal and part-time participation, the real number of households affected would be much higher.]

2) A decrease in landed value of $65 million dollars.

3) A decrease in product value of $162 million dollars. [Dunne 1990]

From these figures it is clear that the stakes in this conflict were considerably higher than those typical of classical Kuhnian[1] scientific conflicts--not, perhaps, for the scientists but certainly for the individuals, families, and communities dependent upon the fishery and, to a lesser extent, for the socio-economic fabric of Atlantic Canada.

I have chosen to characterize these dynamics as "tribal" for two reasons. The first being that this is precisely the word that one of the most active and opinionated of the scientists used to describe an internal dispute in which he had been involved and about which he still feels quite strongly. Second, most of these conflicts are explicitly presented by the participants as cases of "we versus them, good guys and bad guys." The antagonists appeal for favourable judgement by references to well-established traditions of belief and behaviour. The problem in this case is that the respective traditions are not necessarily those we associate with science and are themselves mutually antagonistic.

Data wars: the old guard vs. the young Turks

Prior to the Third Law of the Sea Convention and the subsequent 200 mile limit, the role of Canada's fisheries scientists was quite limited and straight-forward. They addressed themselves largely to single-species descriptive biology. The microscope was the ubiquitous research tool. Some basic work was done to locate offshore populations of commercially valuable species and roughly describe their movements. It was a small community of scientists; relatively sheltered from the winds of politics, clubby, and comfortable. Such controversy as there was, was purely internal and took the form of traditional academic disputation. I should note that the above characterization is not a product of focused research but is my interpretation of reminiscences and passing remarks by a few of the older and retired scientists.

In the course of the Law of the Sea negotiations, it became increasingly evident that Canada would soon extend its fisheries jurisdiction to 200 miles. If so, the government would also assume formal responsibility for the stewardship and rational management of the marine resources in a vast and poorly-known area. It was clear that the federal government's fisheries research program had to be significantly expanded and restructured and that the fulfilment of these new responsibilities would require the services of a new kind of scientist. The territory in question was simply too great, the species too numerous, and their populations too large to be investigated and understood with the traditional tools and techniques of classical marine biology.

Dr. Edward "Sandy" Sandeman joined the Department of Fisheries research station in St. John's, Newfoundland in 1953 and, later, served as Director of the Science Branch until his retirement in 1986. Thus, his career spanned the transition from classical descriptive biology to the current emphasis on quantitative, statistically-derived population dynamics. He describes his recollection and interpretation of this period:

Q: It's my impression from reading the chronology of DFO science, the history beginning with the little station at St. Andrews [New Brunswick] up till now, that you were the Director [of the Newfoundland Region Science Branch] during a particularly crucial phase in the transition of scientific activity...the paradigms under which it was conducted.

A: That might be so. I don't see it quite like that because I think the really crucial change actually took place back in the 'fifties with advent of landmark books by Beverton and Holt [1957] and Ricker [1948]. It was during this period that the focus of fisheries science changed to a mathematical approach and the modern science of fisheries population dynamics really took off. This was really quite a difficult time for those in fisheries science because they were neither trained or even had an aptitude for this new discipline.

Fisheries scientists of that era were trained to taxonomy and the microscope, and it was a difficult challenge to change from biology to mathematics. In their university training, persons who tended to be non-mathematically inclined turned toward something like biology. They chose something that didn't require a mathematical background and now found that the calculator had to displace the microscope which previously was their major tool. That was a major challenge at the time and one which has continued to influence the relationships between scientists even to this day.

When I joined the station in 1953, a major priority was on exploratory fishing, defining where the fish were, and trying to understand their basic biology. Because you can only apply mathematical techniques once you know the population characteristics...the growth rates, the mortality rates and that sort of thing. The fishery was in an expansion phase and the expansion was outstripping the science. Because there was no shortage of fish. There was no need for conservation. At least that is the way that the Canadian fishing industry saw it.

The push didn't really develop until 1970 when most of the ICNAF community started to realize that there were problems. That gross over-fishing was taking place. That there was just too much effort no matter *what mesh size you used. And I guess really that's when our scientists were forced to become much more mathematically oriented, and to use the tools of population dynamics. As we ventured into the realms of population dynamics it became evident that we had to get people on staff who were trained in more than biology. Preferably a combination of biology, mathematics, physics, and computer science.*

Yes particularly computers. We required people who were versed in computers and who were prepared to use them rather than shy away from them as many of the older "biologists" [quotation marks in the original] were prone to do. Who were...well, the modern fisheries biologist as opposed to the one who was trained only to classical biology and to the microscope.[2]

The seeds of conflict: one side of the story

I asked Sandeman if the classically trained biologists had resented and/or resisted the introduction of these new techniques, this new conceptual approach to their field. I had frankly expected that they would have. His answer was surprising (but see below for a radically different perspective). I reproduce the exchange in transcript format to preserve the context, flavour and nuances. I have edited the transcripts to eliminate redundant or digressive material. In some cases the digressions would occupy many transcript pages before returning to the subject of the issues discussed here. I have chosen not to indicate these deletions with ellipses for the sake of readability. In making these editorial decisions, I have given a great deal of attention to accurately representing the subject's point of view. By referring to the full transcripts in the Appendices, the reader may judge to what degree I have succeeded.

In the following exchange we see Sandeman recreating the conflict in social terms and arguing his position by appealing to the normative traditions of the political bureaucracy. He denies that the conflict was in any meaningful respect about the cognitive content of science. In fact, what we are seeing is the normative values of science being dominated by their embeddedness in the professional structure of state bureaucracy.

Q: During this transition was there any resistance from, for lack of a better word, the old guard, the old microscope biologists, to the introduction of these new techniques?

A: It wasn't the techniques. The problem was data. You had guys who had worked for 15 years on a given species; had worked hard, spending many days at sea or in the field, to assemble a data set, which they were looking forward to working up and publishing. [These] papers would not only enhance their scientific reputations but, because of the reward system that was in place within the service, would also likely lead to promotions and financial rewards.

As well, I think it is important to realise that these people, who were now in the middle-management category, also had administrative responsibilities which ate into the time that they had available for their research function. With the new emphasis on "consultation" [quotation marks in the original] within the department, more and more of their time was being *devoted to attending meetings. Meetings with fishermen and industry as well as the continual round of departmental and international meetings such as those of ICNAF or later those of NAFO and CAFSAC.*

This gave rise to a situation where many of the older scientists, the ones who had worked hard to assemble useful databases that they had all sorts of plans to use, got more and more involved in meetings and less and less time was available to do the research, analysis, and writing up that they wanted to do.

At the same time, you now had the newer generation of fisheries scientists who were entering the field who were anxious to apply their newly-learned techniques and, indeed, had been hired because of their capability in this respect. It was the task of the Director and his management team to try to encourage harmonious working relationships between the old and the new so that joint papers became the accepted norm.

In this there were many success stories, but also there were several failures. Clearly, good cooperative ventures are more a function of the personality of the scientists concerned than institutional regulations, and personality disharmony occurred more frequently than one would wish. My impression is that these conflicts were more frequent when the new scientist was a recent PhD. graduate who still considered that he or she knew everything.

They are starved for data. Wanting the data. And yet unprepared to see the other side of the story and not prepared to take the trouble, I guess, to accept the fact that experience usually has something to offer and that cooperation in this sort of situation is almost always superior to an antagonistic approach.

Yes there was a clash for data. There probably still is, and there probably always will be. The guy who's invested 15 years of his life knows that his advancement is dependent on publishing and he's got this data that he wants to publish. He doesn't want to release it to someone else. Okay, usually he'll do a joint paper if it's applying new techniques and they're working on the same data and they've got a nice team going. Yes. But if they can't get that team going then you've got friction. And that friction is likely *to be relatively common when you have situations when recruitment to the service occurs in spurts with relatively long pauses in between. This is not only a problem of the laboratory in St. John's but it is everywhere.*

Q: I ask this question because I have had sources from the younger scientists' side...

A: You'll always get that.

Q: ...telling me that they had data withheld from them, that they were denied access to...

A: Oh, they will!

Q: And some of them have tended to paint it in terms of scientific irresponsibility and outright malicious withholding of...

A: It's possible that there is some malicious withholding but I think you have to see both sides of it. Our promotional system is totally dependent on two things; published papers and international recognition. If you become chairman of an international commission or chairman of a large scientific body or something like that, you get credit for that. But you get most credit for papers published.

Q: And probably papers published are among the criteria for the selection of chairpersons of these bodies.

A: Well, to some extent that's also true. In fact, that is the main criteria I guess. You've got to be well up in the field before they select you. So, you know, these guys have an investment of time in it. The young guys don't realize that, I don't think, in most cases.

Number one, they don't look at their promotional problems. They aren't worried about promotion. The world is theirs! The fact that in our promotional system...and it's worth your studying it because it's a very important part of a research scientist's thinking. There are certain levels....Do you know the system?

Q: Only very roughly.

A: Well, I think you should know the system because it really gives an insight into why you get these problems. [The DFO reward and promotion system is discussed more fully later in this chapter.]

Now the average Research Scientist 1 who comes in doesn't think of the system. Doesn't think that some of these guys who are at the top of RES 2, their only way of getting further is to get some of these papers out that they've been collecting the data for for years! That they've tried to write up and they just don't get a chance! So there is a conflict there.

But my advice to any young person who's coming in that has got ideas is to do joint papers! I mean anyone....And Ram Myers is a good example. Almost every paper that he's done is a joint paper with someone! He applies his techniques and uses someone else's data and assembles a joint paper. And both people get the credit for it then. Maybe not as much as the first person, who is usually Ram. But his publication record is superlative!

Q: And yet there are still, from what I understand, echoes of hostility bouncing around the walls of DFO as a result of his contributions to the Alverson Commission.

A: There may be hostility. I don't know. But I expect some hostility. Some of it's plain jealousy!

Q: Because he's the one who did the reanalysis of plotting growth rates to population density. Or did Scott Aikenhead do that with him?

A: I don't know. I've been gone for three years and I'm not right up to date.

Q: But this was back in '86.

A: Well, I left just before the Alverson group came on, I guess. And I don't know who did the work but I expect some jealousy.

Q: From what I understand, this paper was the first suggestion that the data...or the extrapolations and the conclusions reached from the data about abundance and growth rates...were seriously flawed. That, in fact, growth, in terms of total biomass, had not been *as great as hoped for and predicted. And that very simple things such as the fact that cod in large numbers grow more slowly than cod in small numbers....That they had been projecting growth rates based upon growth rates observed during the depleted years of the early and mid-'seventies. And as the stock rebuilt, growth rates tailed off as the population density increased and this led to a serious revision in the estimation of the total spawning biomass. Are you familiar with this?*

A: No. I'm not familiar with that. I haven't made a point of keeping up with the 2J3KL stuff which is what this was. But it doesn't surprise me that there was a little bit of enmity there or rubbing the wrong way.[3]

Sandeman's reconstruction serves very well to create a sympathetic understanding of his perspective and that of his paradigmatic peers. (Note that there is no apparent acknowledgement of any consequential interactivity between this view of data as the stuff of which careers are built and the more traditional, scientific view of data as the stuff of which knowledge is built.) The older, more experienced scientists are portrayed as wise in the ways of the professional bureaucratic world. They know how the system works and accept it. Uncomplaining, they do good work under often difficult conditions. Above all, they are tolerant and understanding of often over-zealous, sometimes thoughtless youth. After all, they too were once young scientists.

The seeds of conflict: the other side of the story

It comes as a real shock to find that the one young scientist whom Sandeman singled out as a paragon of what the relationship between the younger and older scientists should be, Dr. Ransom A. (Ram) Myers, is Sandeman's, and the older scientists', most savage critic. Myers readily acknowledges the socio-political motivations of the opposition in their withholding of data but claims that the real heart of the conflict was, in fact, scientific. His attack is grounded firmly in the cognitive/normative traditions of classical science and his reconstruction argues that the opposition's construction of the conflict is best understood as an attempt to divert attention from their scientific negligence or incompetence.

I began my interview with Myers with a blunt question as to why the stock assessment scientists had, apparently, so badly over-estimated the abundance of northern cod. What went wrong? Again, I present the data as lightly edited transcript.

> *A: There was a group of people who did not want others to have a close look at the data. It was very subjective. Virtually nothing was published.*
>
> *Q: Who were these people?*
>
> *A: You want names? Dick Wells. He's dead now. They were convinced that the stock was going up. Honestly,*

completely convinced. There were other people...I was outside of the assessment process. People who were not within a small group were very much discouraged from examining the data. There's a long history of that.

Q: So stock assessment was run as an exclusive club?

A: No. It was through CAFSAC. But if I wanted to model the distribution of fish in relationship to temperature, this was fought very hard.

Q: Why?

A: Paranoia.

Q: But if they were convinced they were right, who were they scared of?

A: I don't know. But what went wrong with the process--why the mistakes were made--was this exclusive attitude to examining the data. That, and some sociological reasons. The group dynamics of the process. It's very unscientific. There's a local group, none of whom have Ph.Ds.

Q: These were people who had been hired under Sandy's directorship?

A: Yeah. Some even before.

Q: Under Wilf Templeman.[4] *Going back that far?*

A: The key thing to understand is that it conformed to what people, some people, wanted to believe. It's a little more complicated than that but I don't feel it's a lot more complicated than that. In '87, I was asked by someone on the Alverson Commission to examine the data because I had developed new mechanisms for evaluating research survey data. It had to be done quickly. I concluded, in about four days--given access to the data--that their claim that there was an increase, from the research surveys, was simply false. For various reasons.

Q: Could you be more specific?

A: [Long technical discussion omitted; a critique of previous methodology and synopsis of Myers's reanalysis. See Appendix M for the full text.] *I did* *an independent analysis of the data--that the stock simply was not increasing at that time.*

Q: So at that point you were not working for DFO?

A: I was working for DFO. But simply because you work for DFO doesn't mean you're allowed to examine data.

Q: So it took Alverson coming in from outside to force them to let you see the data. To crack the safe for you?

A: That's right. And this was just to do my job. And it has created an enormous number of problems for me. There are people who just hate me for doing that. In retrospect, with a lot more data now, it's abundantly clear that it was true. After the Alverson Report, things were restructured and they realized that fishing mortality was basically twice what they thought.[5]

After a long digression on other topics we returned to the subject of the control of access to data as a strategy to support prevailing knowledge claims and to thwart possible challenges. Myers describes the consequences of another scientist, Scott Aikenhead, being denied access to data and having his results suppressed by the same group that had blocked Myers.

A: Mac Mercer [who succeeded Sandeman as Director of the Science Branch] *allowed the power blocs [to continue]. He didn't allow the data to be accessed freely. And that was a very serious mistake. Let me explain one simple consequence of that.*

When the Kirby Commission made their report, they projected an increase in cod based on their remaining at the same weight [at age] per cod as when the 200 mile limit was imposed. It turns out that cod growth is strongly related to density. The more cod there are, the smaller they are. [This is critically important where a stock assessment is expressed in terms of "biomass" or total weight.] *This was, in fact, noticed several years before [the synthesis of the Kirby projections]. But it was never reported. It was forbidden for that person to publish anything. That person was Scot Aikenhead.*

Q: So the knowledge was there but it was suppressed?

A: Yes. And for no good reason.

Q: By who?

A: In that case, Dick Wells. And Sandy Sandeman was director and allowed it to happen. Mac Mercer, to his credit, tried to change things but didn't try hard enough. But that wasn't the only case. Derek Ross was here and he was forbidden access to data. Jake Rice was, for years, forbidden access to the data he was hired to work with. This was before he became management level. There were all kinds of examples of that.

Q: Would you characterize it as a case of the old guard versus the young Turks?

A: Yeah. And Jake Rice became an old Turk [sic] just like that [snaps fingers]. It was an amazing transition.

Q: After he became management?

A: Yeah. We're basically a tribal society and once you become a member of a tribe, the tribe is all-important. In this case the cod assessment biologists were the tribe and they were certainly protected which was pretty foreign to me. Through all of this I remained an outsider.[6]

Myers vividly describes the powerful, usually hostile, social forces that are confronted by anyone who espouses a critical or competing knowledge claim. In this instance Myers was presenting his work to the members of the Alverson Commission--but also in the presence of his scientific peers at DFO, his direct boss, and representatives from the higher levels of the federal bureaucracy that employed him. Many of the people in the room had the power to make Myers's professional life as a DFO scientist thoroughly miserable; perhaps to end it. The pressures to recant or modify his iconoclastic analysis must have been enormous. In Myers's words:

> *A: Some times you have to go in there and slug it out. This is an important issue. People's livelihoods are at stake.*
>
> *During the Alverson Commission, I sat around a table when I was giving my reanalysis. And there was the Director of the lab, directors from Ottawa. Everyone involved in the process. I was presenting this report to John Pope and John Poole. And, basically, I said the cod population hadn't changed in the last six years and that the fishing mortality was at least double of what they were claiming. All my co-workers were there and everyone of them, without exception, violently disagreed with my analysis. Without exception.*
>
> *It began with Dick Wells saying, "Well, you really can't expect us to say anything different. We've gone through the process and this is the CAFSAC document and this is what we've concluded. Therefore, you can't expect us to say anything different." Which is an incredibly anti-scientific approach to the topic. You're talking about something that has more to do with tribal societies....But all science might be like that.*
>
> *Q: So in your opinion, a lot of this talk about the difficulties of building a new science is a cover up or a way of explaining the failures of the past?*
>
> *A: Well, I think that a lot of the failures of the past were tribal in nature. That has nothing to do with science. Except scientists are human like everyone else. These people, generally, do not publish in the peer-reviewed journals. There was almost nothing from*

this group of people doing the work that was published in open literature.

Q: And yet these were the people who decided what was done by who and where?

A: Yes. More than they should have. The Director was reluctant to exercise his full authority.

Q: And their relative authority was perhaps a function of their long tenure and institutional inertia?

A: Yes.

Q: May I speculate that these people were by-and-large Newfoundlanders and that the younger, more academically credentialed people were by-and-large come-from-aways?[7]

A: Yes.

Q: So there was resentment of all these college-educated mainlanders who were coming in and trying to tell them how to run their fishery?

A: I think it was much more personal than that. To be fair, there were Newfoundlanders who fought long and hard. There was George Winters who wrote that paper saying northern cod assessment was not worth a rat's asshole. So I don't think it's fair...there's a bit of that but that's not the whole story.

Q; I'm trying to see as many people on different sides of this issue as possible. I'm going to be seeing Sandy [Sandeman] in a week or so.

A: Ask him why data was not allowed to be analyzed when he was director. And give him the example of the growth rate/population study that Dick Wells kept the data out of.

Summary and Analysis

In the above transcripts we have two fisheries scientists reconstructing the same social dynamic process in radically different ways. Even taken at face value, the mutually exclusive interpretations of a single series of interactions are revealing. Whether or not one version or the other is "true," is irrelevant from the social constructivist perspective. What is relevant is that these were/are two scientists deeply involved with the central function (stock assessment) of a powerful state institution (DFO)--and that the development of the conflict, and its quasi-resolution, had large-order, macro-level socio-economic consequences.

Having earlier established the unusual significance of this debate, let us return to a closer analysis of the content of the transcripts.

What is immediately striking is that this not really a debate about the validity of knowledge but a dispute over property rights--in this case, data. But, in this case, there is no commonly-recognized statutory authority or body of common law to guide mediation and closure. At its most fundamental level, the conflict is over whether property rights of any kind can, or should, be attached to scientific data; particularly to data collected at public expense in support of public resource management.

Notice that when I first suggested the possibility of conflict over scientific methodologies between the "old guard" and the "young Turks", Sandeman immediately accepted the suggestion of conflict but denied that it had anything to do with scientific issues. Instead, he reconstructed the problem as purely sociological.

From this perspective, data is treated as the raw material from which professional careers are built. A scientist's relative prestige in the community of his or her peers is a function of the number of papers published and appointments to coveted positions in scientific organizations. In turn, these critical variables determine a scientist's location and movement in the bureaucratic hierarchy of DFO--variables which are very pragmatically expressed in terms of money and power.

Data can also take the form of a negotiable commodity, where an older scientist with an accumulation of unpublished data can form a partnership with a younger scientist who has a command of the latest--and, presumably, most prestigious--analytical tools and techniques. Credit is then shared, either equally or as first and second authors, according to previously agreed-upon terms of the partnership. It is noteworthy that, in discussing this issue, Sandeman never once links this problem with what we generally assume to be the central function and concern of scientists; the production of knowledge. There is no suggestion that this conflict over access to data may have impeded or distorted the process of stock assessment.

In the final analysis, if data is permitted to be treated as property, the balance of power lies in favour of the owner of the data. The most powerful and elegant analytical tools are useless unless they can be applied to a data set. From this we can see quite clearly why older DFO scientists (who may have, to varying degrees, felt threatened by the brash young Ph.Ds with their advanced degrees, complex mathematics, and computer skills) would have a great deal to gain from the privatization of data. Further, we can see why, as an older and classically trained biologist and Director of the Science Branch, Sandeman was deeply sympathetic to and supportive of their perspective.

Turning to Myers's reconstruction, we note that there is no disagreement between Myers and Sandeman as to what happened--the "facts" of the case. Data collected by public servants at public expense was privatized with the sympathetic sanction of the then-Director of the Science Branch, Sandeman, and that this practice continued--albeit, unsanctioned--under his successor, Mac Mercer.

However, to establish his, and others', right of access to the data, Myers invokes the ideal standards of science (openness, intellectual rigour, and objectivity), compares his adversaries against these standards, and finds them greatly lacking. He interprets the proprietary treatment of data as a strategy to protect deeply-held personal and institutional beliefs; that DFO had been, was then, and would continue to be, successful in fulfilling its promises of a rebuilt and robust northern cod stock. His critique of their behaviour and individual competence as scientists is constructed as a normative scientific argument.

For the purposes of making this particular point as clearly as possible, I have extracted the following quotes. They are out of context and out of order but, I believe, accurately and fairly represent Myers's position. The full transcript of the interview can be found as Appendix M.

Myers states his hypothesis:

"They were convinced that the stock was going up. Honestly, completely convinced. The key thing to understand is that it [the assessments] conformed to what people, some people wanted to believe. It's a little more complicated than that but I don't feel it's a lot more complicated than that."[8]

He presents data in support of the hypothesis:

"There was a group of people who did not want others to have a close look at the data. It was very subjective. Virtually nothing was published. People who were not within a small group were very much discouraged from examining the data. There's a long history of that....if I wanted to model the distribution of fish in relationship to temperature, this was fought very hard. [Referring to work done that contradicted the prevailing view]...it was never reported. It was forbidden for that person to publish anything. That person was Scot Aikenhead. But that wasn't the only case. Derek Ross was here and he was forbidden access to data. Jake Rice was, for years, forbidden access to the data he was hired to work with. This was before he became management level. There were all kinds of examples of that."[9]

Finally, he discredits the opposition by attacking their credentials and competence as scientists and charges that they avoided forums where their knowledge claims could be challenged:

"There's a local group none of whom have Ph.Ds. These people generally do not publish in the peer-

reviewed journals. There was almost nothing from this group of people doing the work that was published in open literature."[10]

What we have seen here is a case of scientists in conflict but not, in the classical Kuhnian sense, a *scientific* conflict. Indeed, even the very nature of the conflict is a subject of contention.

The two individuals (and, presumably, the groups they represent) are struggling to define the parameters and terms of the debate in a way that will favour their discordant constructions of the issue. And yet, the very existence of this conflict--much less its resolution (if ever)--has triggered large perturbations in the production of scientific knowledge within DFO and has been the precipitating causal factor in the creation of a crisis of confidence--on the part of the consumers of its stock assessments and advice--in the validity of DFO's knowledge claims.

Sandeman suggests that this is not an isolated or unusual incident.

"This is not only a problem of the laboratory in St. John's but it is everywhere. When I was acting as Director at the Lab in St.Andrews, N.B. [New Brunswick] I saw the same thing there and in fact I remember one young fellow in one of the labs in the Maritimes who

wrote something, I think, like 18 papers in his first one or two years. He mined the data that had been collected by others, ignoring any plans that they may have had to use it and what was achieved? A series of rather superficial papers as well as a group of disgruntled older scientists who felt betrayed."[11]

It is worth considering (as we shall do in the following section of this chapter) whether this kind of conflict may be characteristic of institutionalized, bureaucratically-directed science. In this case, the root cause of the problem seems to be that the criteria governing the reward and promotion of individual scientists is not very strongly correlated with their production of useful, empirically robust knowledge. It is nicely ironic that quantitative measures of productivity--numbers of papers published--appear to be an unproductive technique for evaluating the substantial, practical contributions of the producers of quantitative knowledge.

The DFO Structure of Reward and Promotion as an Impediment to Useful Knowledge

In the preceding section, we saw Sandeman invoke DFO's established evaluative criteria for reward and promotion of its scientists as a rationale for the proprietary treatment of data. Here, I will document and discuss other ways in which criteria for reward and promotion tend to deflect DFO scientists from their mandated mission.

"Mandate:

To ensure that the highest standard of scientific information is available to the Government of Canada for use in developing policies, regulations and legislation regarding the oceans and aquatic life, and to other government departments, private industry and the public for use in planning and carrying out aquatic activities" [DFO/4155 1989 p. 8]

Notice that this mandate is unequivocally utilitarian and that it is two-fold in nature. The primary mission is the provision of useful descriptions, assessments, and predictions to the Minister of Fisheries and Oceans as a factual basis for rational resource management policy and practice. The secondary mission is to produce and disseminate this knowledge as a service to the consumers and users of aquatic resources. The largest and most important of these consumers is the fishing industry.

The industry's relative health and stability--and that of its dependent and related socio-economic structures--is significantly affected by: 1) the consistency (or lack thereof) of federal policy and regulations 2) the ability to make realistic fishing and business plans based upon reasonably reliable resource projections and 3) the degree to which the relationship between the industry and DFO is characterized by a free exchange of useful information and mutual respect. The degradation or absence of any of these factors will, of necessity, have an adverse affect on the profitability of independent fishermen, corporate fishing interests, and the quality of life of thousands of individuals and families in hundreds of communities.

Thus, it is of considerable importance to understand how, and to what degree, the criteria by which DFO scientists are rewarded and promoted may conflict with their fulfilment of their institutional mandate.

I return to the interview with Sandeman for a description of the DFO scientific hierarchy and an exposition of the mechanisms which determine an individual scientist's location in that structure:

> *"Now the way it works is that there are basically four levels. The RES 1 level--which is the Research Scientist One level--is the recruitment level. A young Ph.D. You have to have a Ph.D or the equivalent to get into the RES scale. So the young Ph.D coming in would normally be an RES 1. And if he's publishing reasonably during the first two or three years, it's almost automatic...three or four years...that he moves up into the RES 2 scale. And the RES 2 scale....Most young scientists don't recognize this. They don't think about it. But the RES 2 scale is figured as the scale that most...the average scientist will reach the*

top of. And not everyone will go on to the three or the four scales.

"Approximately 60 per cent of the population of research scientists are in RES 1 and 2. In order to get up to the RES 3 scale, which is the next level up, you've got to have a very good publication record. It's only 32 per cent of the total population of research scientists in Canada [who] can achieve that scale. So you know there's competition to get there. And the competition is extremely vigorous! It is!

So that you have to have, number one, a good publication record and, number two, usually you have to have something else--like chairmanship of something or you're really top of your field in something--in order to get into that scale. And then the fourth scale, which is only 5 per cent of the research scientists' population in Canada, is the top scale. And that is reserved, really, for people who are the best. The Rickers and people like that become RES 4s. In Newfoundland we have, I think, one. In Nova Scotia there are possibly two or three.[12]

The important point here is that the reward and advancement of a DFO scientist is determined exclusively by his or her performance as measured against traditional scientific/academic standards; number of publications in peer-reviewed journals and relative reputation within the international community of fisheries scientists. It is largely irrelevant whether or not a scientist's work follows from, or even acknowledges, the institutional mandate. There is no consequential credit accrued for contributions to organizational function or for work in establishing, maintaining, or improving relations with the client groups. There is no incentive for tackling problems of particular interest to the Minister's office or to industry, in preference to problems that the scientific community considers to be more interesting and worthwhile.

Henry Lear is a native Newfoundlander who grew up in Port de Grave on Conception Bay, fishing with his father and grandfather. His family has been fishing in Newfoundland for over 300 years. He is also a fisheries biologist who spent the first 22 years of his career working out of the DFO research station in St. John's. His deep and abiding concern for the welfare of the inshore sector led him to work on problems of practical interest to inshore fishermen. Additionally, because of his cultural roots, Lear was often called upon to represent the Department in meetings--sometimes quite confrontational--with various fishing industry groups. The bulk of his career as a DFO scientist was devoted to fulfilment of the Department's institutional mandate and to the defence and furtherance of its institutional interests in relation to the fishing industry. In retrospect, he describes the consequences this had for his professional standing within the institution.

> *Q: That's exactly what Sandy [Sandeman] pointed out. That because the promotional and reward structure at DFO is so heavily weighted in favour of publishing, that you see data as your investment and your life's work.*

A: That's all you have. It's not a level playing field. This is the problem. I suffered from the same thing. You're so tied up in doing your job that you just don't have time to publish. You just can't concentrate and focus on getting the publications. You're the one [speaking of himself], if there's a brush fire, you get called out. You're the one who's got the experience and you've always been there and it's so easy, right?

And you hire someone, it isn't just Ram [Myers], it could be anybody. They're brilliant and they come in and you've got this wealth of data you haven't published and they say, "Well, this is not right. This demands publication." So you hand it over and they get half a dozen papers and next thing you know they're two levels ahead of you. And you say, what am I doing? I'm only a slave! And this is where the problem lies.

People who are working very, very hard--working overtime without getting any pay or anything else--were not getting any type of reward. Not even promotion. Whereas somebody'd come in who was quite free to take the data. You were giving them a free ride. It was sort of creaming off in a way from someone else's life. This is the crunch. That is the problem.

Q: So the structure does not encourage cooperation.

A: You can call it the structure. But I think the reward system for research scientists doesn't allow that. You get penalized.

I spent a lot of time talking to fishermen for example. It was interesting. I came from fishermen and I could easily talk to them and I enjoyed that. Carrying them information and discussing things with them. But in the end, it didn't do anything for me. People were just passing me by. So that's just one example. Now we've come to a crunch where we've got to have people talking to fishermen, interacting and liaising and all that stuff. When I was doing it, it was nothing!

Q: J.J. Maguire is quite concerned about this. Although there's a lot of talk about increasing the communication with the clients, there are still no points for it, no institutional rewards. When I talk to other people about this problem, your name often *comes up as an example of someone who has suffered because of this.*

A: Well, I have no one to blame, only myself. I knew what the rules were and because I cared, I suppose, I suffered. I'm not blaming the system. I'm not blaming anything. I knew the rules and because I felt a certain way, a certain dedication, whatever, that's what I did. Dick Wells did the same thing.

Q: What happened to you?

A: Well, I just never got the publications to get upgraded, pure and simple. No one caused me to suffer. The rules were there on the page in the book. You had to have a certain number of publications--which I didn't have--and there was no way I was going to get them, doing the type of job I was doing. So it was a vicious cycle and I was party to the cycle because I enjoyed what I was doing.

I thought at the time, and I still believe, that I was doing a good job. But because I did a good job, and enjoyed doing it, and kept doing it--which only ground me a little farther down--it was counterproductive to my own career interests.[13]

I discussed this issue with Dr. Jean Jacques (J.J.) Maguire, chair of CAFSAC:

Q: How are you going to deal with the resistance of research scientists to spending their time in ways that there are no points for within the internal structure of DFO?

A: It's a very serious problem as a matter of fact. You need to find points. Simple. It's as simple as that. I don't know how to do it and it's very difficult. We want to reward people who communicate and exchange and do stuff like that.

We say that out of one side of the mouth and then when it comes time to look at promotions, we say this one's got 15 primary publications this year. You've got one. Forget it boy. You've met with fishermen.

You've met with broadcast people, radio people, university people. You've met with all of these people. But what do you have to show for it? Nada! You're out.

We've got to change that and I don't know how to do it. I don't have a clue. But it must be recognized.

Q: There's a second, more serious problem in the surprising inability of stock assessment science to produce practical or useful knowledge--knowledge of the requisite precision--to fulfil the needs and expectations of the management structure and of the planning needs of the commercial industry.

A: You've mentioned something, it's the closeness to the clients. I think that's what went wrong. We distanced ourselves from the clients...from what we were supposed to do. And we came to be seen as an impediment for the industry.

Q: And, in my limited experience, this attitude is more common than not. Particularly because of the evaluative and reward...

A: ...structural appraisal system. You're totally right. To me, that's not easily solvable. Very difficult to solve. But I agree with the perception that if you're too close to the fishermen you start to see things their own way. And you lose....

What it boils down to right now is that we've got clients, and we're producing stuff that's totally useless to them. We've got no links to the clients. If we want to continue to do that, that's fine. But we're going to be out of business. If we want to stay in business we better get closer to the clients. It's straight free-market economical forces.

If I'm close to the inshore fishermen and you're close to the offshore fishermen, we're going to argue and we're going to reconcile our perceptions some way. And what's going to come out of it is going to reflect a little bit of both. So I don't see a big problem with that.[14]

I discussed the same problem with Sandeman who--as a former Director of the Science Branch--has a more applied perspective on the conflict between service science and "real" science:

> *Q: You've talked about stock assessment science in relation to values and norms, evaluative traditions that are internal to science. But DFO science exists to some extent--at least in the minds of the bureaucratic and political structure and the corporate sector--as a service industry. That's the public justification for the rather large amounts of public money that are expended on it.*
>
> *Then the question arises, if science can't provide us with knowledge of the degree of certainty we need....From the corporate point of view, they need to make five- and ten-year plans--to construct and amortize plants and trawlers over a considerable period of time based upon the projections of what their allowed catches are going to be for that period. The political sector has to make management decisions based...They expect to be able to use science as the legitimizing or justifying ground for their decisions. And if it's unreliable or unpredictable then they are in trouble. Is there any recognition of this within the scientific community and, if so, how do the scientists feel about it?*
>
> *A: There is certainly very strong recognition of the basic fact that we're a service--amongst the administrative side of it--because we are continually having to justify this, that, and the other thing in order to get funding.*
>
> *As far as the scientists are concerned...You've talked to Larry Coady and Mac [Mercer] so you've gone through our review system, right? Our review system tries to bring our scientists into contact with the fact that we are a service organization. That we've got certain things that we've got to do.*
>
> *And my own guideline, as a director and as a research manager, was always that we try to spend approximately 80 per cent of our time on the service*

function. Research towards service. There are some scientists, you'd let them go much higher than 20 per cent. But on an average, 20 per cent of the time is devoted to things that are "may pay offs." Real research. They're not the things that we have to do every day to provide our assessments. To provide our projections. Nor are they things which are keyed to just improving our techniques. They are research lines which are interesting. Which may pay off or may not. We don't know. I think much of the work that Ram's [Myers] doing is of this type. Not all of it. I think he spends 70 per cent of his time in straight service work. Service to others. But a good scientist should be able to spend at least 20 per cent of his time on long-term work which may or may not pay off. In addition to the service.

So I think a director has to recognize that. You won't need scientists just to do the service work. Scientists have to have more than just the service. Especially when, if you are a stock assessment scientist who has to produce his stock assessment twice a year....I mean, it's a relatively mundane job. Reading your otoliths. Getting your age distributions. Getting your weight/length curves and all the things you need for the stock assessment. You've got to have that extra 20 per cent to follow up lines that look interesting and to do the other things. Nevertheless, I think that everybody in St. John's recognizes the service side of it because of the review system that we have that forces that.

The system has some extremely good features about it. The main feature that I think is good is that you can get a scientist who's on his own. He has no empire under him or anything like that and he's earning as much money as the Assistant Deputy Minister. If he's a top-notch scientist he's working on his own at the bench. Maybe with one technician. And publishing. And publishing first-class stuff. The system allows that and is tailored to allow that. So it means that you don't have to spend your time in administration and build up a pyramid so that the more people you get under you the more promotions you get which is the standard civil service way. That is a big strength.

I think that if there is a weakness, the weakness is that there's not enough brownie points, for lack of a better word, given to contributions made to the *organization. You get a fellow like Jake Rice who....You know him. You've talked to him so I can use his name as a type example. A guy who is a program head. Who is chairman of this and chairman of that. He's a super chairman. He's got a broad spectrum of interests. So he's doing all sorts of things of value to the organization and maybe not publishing as many papers as he would like. You do get an imbalance. The guy who's giving himself to the organization and his papers are suffering.*[15]

Summary and Analysis

Maguire's assessment of the problem is surprisingly blunt and pessimistic. From the scientists' point of view there are no extra "*points*" awarded for directing one's research towards problems of interest to the client groups--and there are actual disincentives for time and effort spent in service to the institutional (non-scientific) goals of DFO. Lear's experience establishes this point very firmly. Most research scientists see such work as time-consuming and a distraction from the kind of publication-oriented research that will count towards promotion.

Additionally--by becoming too directly involved with the client groups--a scientist can easily become entangled in the political, economic, and social aspects of fisheries management. This exposes a scientist to charges of subjectivity and the suspicion among peers that his or her work is polluted by non-scientific considerations. Such a judgement would be the kiss of death for any hopes of promotion.

As a result, while the scientists may be doing *"good science"* according to the norms and traditions of their peers, they are *"producing stuff that's totally useless to [the client groups]."* [Maguire above] Therefore, it should not have been too surprising that--in the storm of criticism of DFO science that followed the release of the Harris Report--few individuals from the client groups (the federal government and the commercial fishery) felt inclined to come to the defense of the Science Branch. From the point of view of the consumers of scientific knowledge, there was little of any real worth to defend and, in fact, much to criticise. Still, the scientists felt that they had been abandoned, even betrayed, by their bureaucratic masters for reasons of political expediency and vigorously, but ineffectually, protested what they perceived to be shabby treatment.

The following is from an article in a St. John's newspaper headlined *"DFO scientists fuming, say Valcourt*[16] *will not answer morale concerns":*

> *"The Professional Institute of the Public Service of Canada, the association representing the scientists, wrote to the minister on March 6 and outlined problems in the department.*
> *"The scientists accused provincial and federal officials of spreading misinformation about DFO research and called the department 'negligent in allowing this climate of disinformation to flourish.'*
> *"The letter said public hostility was being stirred up against the scientists, who were being called incompetent.' The DFO researchers called the situation 'volatile and dangerous.'*
> *"Since the release of the Harris Report last month, the mood among the fisheries scientists is reported to have worsened.*
> *"'The tone of the report was condescending to DFO scientists,' said Paul Howard, a PIPSC spokesman in Ottawa.*
> *"'They have been working very hard to demoralize researchers,' Ms. Craig said. 'They don't seem to have any understanding or appreciation about research.'"*
> *[The Sunday Express, April 22, 1990]*

Having been both a scientist and, later, a manager of scientists, Sandeman's exegesis of the problem is more equivocal and contains the internal contradictions and conflicts that are at the root of the problem. As a manager he claims that there is "*a strong recognition amongst the administrative side*" that their institutional mission is the provision of services to their clients. As a scientist, though, he tends to view the service work as a necessary evil which must be done in order to secure funding for the *"real research."*

As a manager he sees the annual program review as a useful way to remind the scientists of *"the basic fact that we're a service organization..."* As a scientist, he somewhat ruefully continues *"...that we've got certain things that we've got to do."* Not, necessarily, things that they want to do. The task most central to their mandate, stock assessments, is dismissed as *"a relatively mundane job,"* for which scientists need to be rewarded by being allowed to pursue *"research lines which are interesting."*

Finally, one should note that, in Sandeman's view, the *"better"* the scientist, the more freedom should be permitted in the selection of research topics. Remember that the determination of who are the *"better"* scientists is not connected to their contributions toward the achievement of institutional goals. *"Better"* scientist are those who publish regularly on *"interesting"* research lines. So--not only is there a strong incentive for scientists to maximize their personal rewards and professional autonomy by avoiding *"mundane"* service work whenever possible in favour of *"interesting,"* publishable research--but we can assume that the bulk of the unrewarded service science is assigned to and performed by those scientists least-esteemed among their peers.

In the next chapter I will shift and broaden the field of enquiry to examine the nature and dynamics of the relationship between the Science Branch and the commercial fishery. We have seen that there is a serious discrepancy between the institutional mandate of the Science Branch to produce useful knowledge for the fishery and the actual direction and production of its activities. In this chapter I have shown that this is due, in part, to fundamental conflicts between the evaluative criteria for reward and promotion within the Science Branch and the institutional goals and responsibilities of DFO as an agent of the state.

ENDNOTES

1. From Thomas Kuhn, author of the widely influential work, *"The Structure of Scientific Revolutions."* [Kuhn 1962, 2nd edition 1970]

2. From an interview with "Sandy" Sandeman. The full transcript is Appendix O.

3. Ibid.

4. Wilfred Templeman; a native Newfoundlander who was widely regarded as the Dean of Newfoundland fisheries scientists. Templeman's career of significant research and publication began in the 1940s as a biologist at the Bay Bulls research station, included many years as Director of the Science Branch, and ended only with his death in the mid-1980s.

5. From an interview with Ram Myers conducted in St. John's on August 28, 1990. The full transcript is Appendix M.

6. Ibid.

7. "Come-from-away" is a Newfoundland colloquialism for anyone who is not native-born. It is all-inclusive as compared to "mainlander", a term usually applied to Canadians from other provinces.

8. From an interview with Ram Myers conducted in St. John's on August 28, 1990. The full transcript is Appendix M.

9. Ibid.

10. Ibid.

11. From an interview with Sandy Sandeman conducted in September, 1990. The full transcript is Appendix O.

12. From an interview with Sandy Sandeman conducted in St. John's, September, 1990. The full transcript is Appendix O.

13. From an interview with Henry Lear conducted in Ottawa, January, 1991. The full transcript is Appendix K.

14. From an interview with J.J. Maguire conducted in St. John's on October 28, 1990. The full transcript is Appendix J.

15. From an interview with Sandy Sandeman conducted in St. John's, September, 1990. The full transcript is Appendix O.

16. Bernard Valcourt; then Minister of Fisheries and Oceans.

CHAPTER SIX

IS THERE A PLACE FOR FISHERMEN IN FISHERIES SCIENCE?

The recent development of Environmental Impact Assessment and Social Impact Assessment processes is--among other things--an attempt to synthesize a progressive, productive relationship between scientific knowledge and other forms of knowledge. As anyone who has participated in such a process or followed its media coverage knows, the social and cognitive dynamics of these fora are often adversarial. This is inherent in the structure of the process and its goal--which is to achieve a negotiated resolution of the disparate, usually contradictory, sets of norms, traditions, and values that inform the cognitive realities of the concerned social groups.

This has an analog in the relationship between DFO stock assessment scientists and the commercial fishery--particularly the inshore sector--and opens another level of analysis for understanding the persistent resistance of DFO science to directing its activities in the interests of the fishery and to do so in a context of open communication and cooperation.

As we shall see, when discussing this aspect of the problem it is necessary to distinguish between the offshore trawler industry and the traditional inshore fishery. In the course of earlier chapters, I showed that the original challenge to the Science Branch's construction of the stock's status came from the inshore sector of the fishery--and that this sector was able to mobilize and sustain sufficient cultural and political resources to force a genuine and substantive internal reevaluation of scientific stock assessment. Whether or not this reevaluation would have occurred without this external pressure--and, if so, whether it would have occurred sooner or later--is a point that will forever remain moot. What can be established, and usefully questioned, is that the offshore sector did not join in the criticism of DFO's construction of reality until the critical reassessment of 1989 precipitated drastically reduced quotas for the 1990 offshore fishing year. Even then their perspective was not congruent with that of the inshore sector but diametrically opposed. They argued that DFO had been right the first time; that there were plenty of fish out there and that this was supported by the operational reality of their skippers.[1]

To more fully explain the persistence of DFO's pre-1989 construction and its vigorous defence against the challenges of the inshore fishery, it is important to recall from earlier chapters that, by far, the greatest source of assessment data is the offshore fishery. Although the inshore fishery accounts for one-third to one-half of all landings from the northern cod stock, it has been routinely ignored by the Science Branch as a valuable or valid data source. Why this should be is a question well worth pursuing and one which will provide us with yet another source of illumination of the central problem.

We can begin by noting that conflicts between the cognitive reality of inshore fishermen and federal scientists are not new.[2] In fact, deep resentment--of the history of domination of DFO's resouces by the interests of the offshore sector and dismissal of the legitimacy of the inshore fishermen's knowledge and ways of knowing--undoubtedly accounts for some of the tenaciousness and determination with which they pressed their attack on DFO science. Although they have won a tactical victory in their battle with DFO, the strategic balance of power, from an epistemological perspective, remains heavily in favour of science. As we will see in the following pages, DFO has learned to respect the political power of the inshore fishery but is still intensely sceptical of the value and validity of their cognitive reality.

The inshore fishery and its advocates claim that the accumulated knowledge of hundreds' of years of fishing these waters is resident in the tens of thousands of currently active inshore fishermen. Further, they claim that this knowledge is of at least equal validity to that of the scientists when applied to questions relating to the abundance and behaviour of the northern cod stocks and should, therefore, be incorporated in the annual assessments and the dependent setting of quotas.

The relative merits of this argument (although fascinating and well worth detailed study) are not of immediate interest to this work. What is of interest is that it exists and is being formulated as a conscious and concerted challenge to the position of epistemological superiority claimed by, and traditionally accorded to, scientific knowledge. The institutional and individual responses by science and scientists to this challenge are of interest, as are the cognitive structures and language that they muster in their defense.

When examining this issue, it is important to make the distinction between the scientific response and the bureaucratic response as well as the distinction between the scientists' conceptions of the inshore and offshore fishermen as potential sources of valid knowledge.

The Perspective of the State: The Political Validity of the Inshore Fishery

To the bureaucratic management of DFO, the claims of fishermen, particularly the 20,000 or so inshore fishermen, pose a political problem. Not only are they--and the voting-aged members of their families--a significant political force but, as noted in the introduction, the inshore fishery occupies a position of disproportionate cultural significance to the polity of Newfoundland. Issues of concern to the inshore fishery cannot be ignored or dismissed by a bureaucracy sensitive and/or vulnerable to political forces. This sensitivity can be projected downwards through the bureaucratic structure of DFO to the scientific level. While active research scientists are not directly vulnerable to political forces, they are inevitably aware that their associated institutional authority and their access to human, material, and financial resources is, to some degree, dependent upon their institution's political strength. In general, their scientific assessment of the worth of inshore fishermen' knowledge claims is tempered with an understanding and acceptance of their political power as expressed through a sympathetic media and the ballot box.

The case of the offshore sector is quite different. Here the power is not resident in the fishermen, of whom there are relatively few. Nor is it resident in public opinion or the voting population, which tends to see the interests of the capital-intensive offshore fishery as in fundamental conflict with those of the inshore. The power of the offshore sector resides in the two controlling capital corporations, Fisheries Products International (FPI) and National Sea Products (NatSea), and is exercised through the traditional corporate mechanisms of campaign contributions and direct interactions with individual members of state institutions, both elected and un-elected. The former tends to involve negotiations for favourable policy decisions or resource allocations in return for corporate decisions of socio-economic benefit to an elected representative's district.[3] The latter is through formally established processes for institutional/industry information exchange and negotiations, usually on matters of operational policy and practice. These fora generally include the top managers from the Science Branch and, often, research scientists as well.

The Scientific Perspective: The Epistemological Validity of the Offshore Fishery

The attitudes of individual scientists toward the inshore and offshore sectors are equally distinct. This may be due, in part, to their relative power to intrude on the normal routine of scientific activity. More importantly, I believe, it is due to their relative standing with scientists as sources of valid, valuable knowledge.

We have seen that of the two sources of raw data--research vessel surveys and commercial catches of the offshore trawlers--the vast majority comes from the later source. Although it contains some known and suspected qualitative problems, it's sheer quantity mitigates heavily in its favour--and the inherent biases are thought to be amenable to compensatory statistical procedures. The data are highly concentrated--originating from 50 or so trawlers--making collection easy and, not least, it is free for the taking.

There are other aspects of the offshore fishery which tend to render the data it generates more acceptable to science. First, it is perceived as recognizably rational. The trawler fishery is pursued systematically with uniform technology and techniques. The fleet effort is deployed and controlled on the basis of operational principles developed from an accumulating, well-documented, statistically accessible data base. The results are evaluated objectively by the directors of the corporations. In these respects the offshore fishery shares a great deal in common with science.

By contrast, the inshore fishery is not seen by most scientists as a valid knowledge source. Simply from the point of data collection, it presents huge logistical problems. Thousands of inshore boats are dispersed among hundreds of communities--often quite remote and inaccessible from DFO's base of operations in St. John's. There is an acutely problematic diversity of technologies and techniques. Operational strategies are self-directed and tend to be based upon orally-transmitted accumulations of traditional knowledge which is largely opaque to statistical analysis. Because the inshore fishery is the foundation of rural Newfoundland society--and because each individual fisherman is so deeply embedded in his community--results are not evaluated "objectively" but as an irreducible part of an individual's social and cultural reality.

Science Vs. the Inshore Fishery: An Empirical Account of a Struggle for Constructive Authority

It is surely significant that the crucial reassessment of data sources and analytical methodology was not initiated by people or processes internal to DFO science, but by political pressure from groups of independent inshore fishermen and their supporters--whose perceptions of the state of the stock were at considerable variance with those of DFO. Specifically, while the DFO yearly assessments had been regularly confirming their own predictions of a steadily growing stock (the expected results of adherence to the $F_{0.1}$ management principal), the inshore fishery had been experiencing ever-lower landings since 1982.

A quantitative recent history of the inshore fishery

Initially, the inshore sector had greatly benefitted from the 1977 200 mile limit and the near-exclusion of foreign fishing fleets from this zone. Inshore catches had historically averaged around 150,000 mt until 1960, when technologically-advanced foreign factory freezer trawler fleets began to heavily exploit the northern cod in NAFO zones 2J3KL. As the offshore catch rose from 301,500 mt in 1960 to 708,000 mt in 1968, the inshore catch fell from 157,000 mt to 101,000 mt in the same period. In retrospect it is clear that the stock (which had historically supported a combined inshore/offshore sustained catch of 250,000 to 300,000 mt) was being dangerously over-exploited. Inshore catches continued to fall to an all-time low of 35,100 mt in 1974 while the offshore catches, in spite of increasing effort, also began a steady decline to 100,000 mt in 1977--the last year of effectively unrestricted foreign fishing.

With the advent of the 200 mile limit, the inshore fishery continued its recovery from the 1974 low--landing 81,000 mt in 1978 and 113,000 mt in 1982. The now largely Canadian offshore fleet also made gains during this period, its catches rising from 57,100 mt in 1978 to 116,000 mt in 1982. After that year, however, the experiences of the two sectors began a consequential divergence. Inshore catches fell every year until, in 1986, the sector landed only 72,000 mt. Meanwhile, the offshore landings rose to 252,000 mt in the same period. [Keats 1986, Harris 1990] As early as 1982, and in spite of a relatively good year, the inshore fishermen and plant owners who processed their fish thought they recognized the beginnings of a depressingly familiar sequence of events and began to question the accuracy of DFO's assessments. In particular, they were concerned by the unusually high percentage of small fish in their nets and the fact that the Total Allowable Catch (TAC) (supposedly set in accordance with the $F_{0.1}$ rule to realize the goal of rebuilding the stock by removing no more than 20 per cent each year) had been raised from 135,000 mt in 1978 to 260,000 mt for the 1983 fishing year. This was in spite of the fact that in no year since 1978 had the combined inshore and offshore fleet landings met the quota. [Alverson 1987]

Questions and answers: the opening round

DFO responded to these concerns in a 1983 pamphlet entitled *"Trap Cod: Some Facts About Unpredictable Catches and Small Fish." [DFO 1983]* The tone of the response--and DFO's conception of its relationship with the inshore fishery--is made clear on the first page, headlined *"Questions and Answers."* The explicit assumption is that, while fishermen have "questions" or "concerns" or even "demands" for information, DFO has the "answers." The text states that there are many biological, behaviourial, and oceanographic factors that can contribute to the variability of inshore catches and create the *appearance* of abundance or scarcity. Although one of these factors is, of course, stock size, this was no longer a possibility as *"The size of the northern cod stock is currently estimated to be about 1,500,000 t (metric tonnes), which should provide good catches inshore."* [DFO 1983 p. 10]

Bernard Brown, long-time information officer for the DFO station in St. John's, summed up the prevailing attitude quite bluntly: *"...essentially they were telling the inshore fishermen who were creating all the uproar about the destruction of the stocks, that you don't know what you're talking about."*[4] But note that by 1986, DFO's revised estimate of the 1982 stock had fallen by more than 30 per cent to 1,097,000 mt. [Keats 1986]

As offshore catches continued to rise while inshore catches fell, the men and women of the inshore sector grew increasingly sceptical of DFO's claims. By 1986 they had become loudly and publicly critical and refused to accept DFO's explanations as valid. Nevertheless, DFO maintained the official position that the management goal cf $F_{0.1}$ was producing the predicted, desired effect. The stock was increasing on schedule and the reduction of inshore landings must be due to other factors such as reductions in effort or environmental changes affecting the annual summer inshore migration of the northern cod.

The metaphysical origins of the inshore challenge

In the section above, we saw that the operational reality of the inshore fishing community was at considerable variance to DFO's science-based construction of reality: the inshore sector was landing progressively less--and smaller--fish while the offshore trawlers' catches were continuing to increase. Inshore fishermen began to claim that the stock was in danger--that the scientific description of a healthy, growing stock must be wrong--and that the northern cod quotas, particularly those for the corporate offshore fleet, should be immediately and significantly reduced. The official response from DFO was to dismiss the inshore sector's perception of the stock's status as an artifact of resource availability: the stock was healthy and continuing to rebuild but, for reasons probably related to changes in the ocean climate, the cod were simply not migrating inshore in their usual numbers.

There was, however, a deeper metaphysical basis for the inshore community's increasingly militant position. Bernard Brown offers his thoughts on the development of the disparate, conflicting constructions of reality by DFO and the inshore sector.

> *A: You can go back to time immemorial. There have always been fishery failures. Sometimes localized to one bay, sometimes the entire East Coast, the South Coast, wherever. The fish failed for a year or two, or even three or four. It made for tough times. When it was bad enough government would step in with some little bit of assistance to help people stay alive. Not on today's scale. But it really didn't mean too much because people lived off the land and off the sea anyway. But the important thing is that the people understood that it was a natural thing. The fish failed. They didn't understand why. They just understood that they did. But they knew that the failure would only last for so long. The fish would come back. That was as certain as God. The fish will come back.*
>
> *So there was never despair among the people and never a reason to blame anyone for it, government or anyone else. It was a natural thing. And of course, there was only an inshore fishery. They always knew that they could not fish out the sea. They couldn't destroy the resource. And I doubt that anyone even had a concept of destroying the resource. It wasn't even imaginable.*
>
> *But come the 'fifties, the offshore fishery started. And it was a European fishery. The northern cod landings peaked at something over 800,000* tonnes *in the early 'seventies. But over that period, people began*

to realize--and I think it took until the early 'eighties before most people in the inshore knew--that an irrevocable change had taken place. That now you could have a fishery failure that was not a natural thing but caused by the fishermen themselves. Now they could have a failure and, maybe, the fish would not come back. And that gives you a totally different inshore community.

They have a new understanding of fishery failure. Instead of saying, "Never mind. The fish will come back," what stands between them and permanent failure, is a few politicians in Ottawa.

Q: Would you say this new understanding was the beginning of the serious criticism of DFO science?

A: I wouldn't say it was the beginning, but that's when it became mass criticism. Almost like a revolution. I would say that the mass criticism from the inshore that hit DFO three or four years ago was qualitatively different than anything that had gone before. Almost the whole inshore rose up and said, "DFO, you're blowing it." And it was different in that they concentrated on the science.

Now a few mistakes and a few bad decisions could cause a failure that was not natural but man-made. Now there could be a failure and the fish wouldn't come back. Now there was someone to blame. And this was utterly different than anything they had known before.[5]

Two solitudes: fisheries scientists and inshore fishermen

Earlier in this chapter, I claimed that DFO's bureaucrats and scientists each viewed the problems of their relationships with the inshore and offshore sectors of the fishery quite differently. In the following section I will present data in support of this claim. I also suggested that the scientists' on-the-record evaluations of the potential contributions of fishermen's knowledge to the scientific assessment process might well be tempered with an awareness of their political power and DFO's vulnerability to that power. This consideration did not apparently restrain Dr. Edward (Sandy) Sandeman who retired as Director of the Science Branch in 1986 after a 30-year career as a fisheries biologist.

He begins by making a functional argument for Science's neglect of the inshore fishery in favour of the offshore as a data source Sandeman then switches to an epistemological argument to flatly dismiss inshore fishermen as a valid source of knowledge.

> *Q: Let's go back a bit to the discussion we were having about the demands from the consumers of scientific knowledge to participate [in stock assessments]. Especially from the inshore, there's a litany of criticism. That science doesn't listen to our knowledge. That they don't value our knowledge. The inshore crowd feels pretty ignored.*
>
> *And then let's couple this with your observation that the scientists who attempt to address these issues, these concerns, and attempt to participate more fully with the fishermen, become less than optimally productive as scientists. I'd be interested in your thoughts on this general subject.*
>
> *A: Well, you've got several questions there, though they are all related.*
>
> *There is a fundamental reason why, to a large extent, we ignored the inshore cod fishery. The reason*

being that it was an extremely difficult to study. The variability was such that--for meaningful estimates of stock abundance--you had to study the whole coast of Newfoundland. That's a very large area. Whereas, if you leave it until the fall/winter period and you do the work offshore, with the vessels that we now have, you can at least get your estimates of abundance within some sort of error bars that are at least acceptable. But to do that within the inshore area is an impossible task! So we tended to downplay the inshore area. It was just too big an area to cover with the people that we had. When the fish went offshore into concentrations, we could much better devote our time on those concentrations. So you're quite right. We did ignore that inshore area to a large extent.

Now, the other part of it is the potential knowledge to be gained from inshore fishermen. We continually get blamed, for not using this fund of knowledge. I have some very definite views on this which are not necessarily supported by my colleagues. I think the inshore fisherman has very little to contribute to the solution of the fundamental problems of stock assessment. There are a few exceptions. There are a few fishermen who think, and see beyond the bounds of their local interests. But the comments of the vast majority are self-serving and extremely restricted in geographical range.

For the most part the majority of them have a litany of mumbo-jumbo which they bring forth each time you talk to them. About where the fish are and why they're not here. They relate it to things like the berries on the trees. Sometimes observations of that sort have some value such as "When the wind is such-and-such a way, you get catches." That's acceptable.

When I was going around trying to understand a bit more about Newfoundland and the fishery, I just got completely turned off by inshore fishermen and their views. Because they were totally unscientific! And you'd try to get them to approach it from a scientific viewpoint and they would say, "yes," they'd be happy to help. But in many cases they couldn't write--in the old days, they can now--so they couldn't keep a log book for you. You'd pick the one or two better ones and they might keep a log book for so long and then they'd [say], "B'ye, it's just too much trouble! I just can't help you any more. Sorry." It was just *banging your head on a brick wall. So I tended to downplay inshore fishermen as being useful to the scientific process.*

There are some who are different. I worked on shrimps for a time, which tends to be inshore fishermen in bigger boats. Most of these guys are the best inshore fishermen. Because they're the ones who have the gumption to get the boats somehow. They're not content just to go out to set the trap the same place their father set it before and, if the fish don't come, complain. And I certainly got on very well with most of these guys. They were prepared to think a bit.

They still didn't read and write, many of them. And that made it difficult to communicate by writing. Writing is so important. Very little of what we do is spoken. It's all writing. But on the whole they were they best and I could get on with them and I could work with them and I found it valuable. And they helped me a lot. But the average inshore fisherman, no b'ye, I just don't think so.[6]

While Bernard Brown, a native Newfoundlander, is not a scientist--as a long-time information officer for DFO with a background in journalism--he is well-able to assess the prevailing attitudes of the scientists. It is likely that his cultural rootedness predisposes his sympathies in favour of the inshore fishermen's perspective. With that caveat in mind, Brown claims that the opinions of Sandeman are, in fact, generally representative of those of most DFO scientists.

A: I've been watching the fish coming ashore from the 65-foot otter trawlers--the guys that are going off the Virgin Rocks--and if there's one fish in a hundred that's longer than 18-22 inches, that's about it. In other words, they're getting a lot of small fish out in deeper water and that's not a good sign. Now, I'm talking like a fisherman, the kind of stuff that the scientists absolutely disparage.

Nevertheless, in all of this our inshore fishermen have been proved to be right. Unless our scientists are going to turn around a couple years from now and say, "We were right after all. The stock did grow five-fold." Which would destroy any shred of credibility that they have left. We [DFO] were saying the stock has grown five-fold and the fishermen were saying, "You're out of you mind." They were right. But I still don't see any evidence among scientists that they're any more prepared than they ever were to go out and listen to fishermen.

And it's apparently a matter of the difficulty of dealing with the kind of information and evidence that fishermen have, the so-called anecdotal stuff, which you can't quantify very well and analyze very well. Certainly can't computerize very well. So you just don't want to deal with that kind of messy information. They won't even call it data, as a matter of fact.

Q: There's simply no cultural support or established mechanisms within science for incorporating traditional knowledge.

A: The department's trying to force it to a certain degree but I don't know how much of that's public relations work as opposed to a real effort.

Q: But, even if there were a genuine interest in incorporating traditional forms of knowledge, it's difficult to see how they could be translated into the language of science--mathematics--or conceive of science learning to speak another language.

A: Yes. But that's only part of the problem. The other part is attitude. If the scientists really feel, as a lot of them do, that the fishermen have bugger all to offer....

[long discussion of the new log book program to assess inshore fishing effort. Brown notes that this is being conducted by the Statistics Branch, not the Science Branch and feels that this is a missed opportunity to get scientists and fishermen actually talking to each other.]

> *I think it's a real problem that the fishermen and the scientists operate in isolation from each other. [NOTE: When they do have personal contact, it is almost invariably in the context of conflict and antagonism.] How the hell can some guy become credible to you if he's just some asshole out in a boat, believing what his grandfather believed? If you're a scientist and you know the truth?*[7]

An integrative perspective

Henry Lear is a native Newfoundlander who grew up fishing with his father and grandfather but who also has spent the last 20-plus years with DFO as a fisheries biologist. As one might expect, Lear's perspective on the issue is quite different from that of his fellow scientists; undoubtedly due to his much deeper, personal understanding of the inshore fishermen's cognitive reality. Notice that he makes a clear distinction between the possibility of active involvement of fishermen in the scientific process and the incorporation of fishermen's knowledge in that process. Lear sees no real functional impediments to the collection of scientifically-acceptable data from the inshore fishery. Unlike most of the rest of his colleagues, he sees fishermen's knowledge as, potentially, being of great value. It's actual incorporation, however, is impeded by difficult--but not necessarily insoluble--methodological problems. This raises the question of whether other scientists' seemingly rational constructions of the difficulty of collecting and incorporating valid, useful data from the inshore fishery are, in fact, grounded in cultural attitudes and prejudices.

> *Q: There's a lot of talk now about trying to incorporate fishermen's knowledge into the scientific assessment process. The inshore logbook program is one example of the attempt to do this. But when I speak to scientists privately, there is a wide range of opinion about whether this is A) possible and B) a good thing. What are your opinions?*
>
> *A: Back in '86 I think it was, we looked at the situation in a little technical report we did for the Director General. As one of the first recommendations, we said it was of paramount importance to include catch and effort data from inshore fisheries into the assessment process. And really that's what counts. You have to have some measure of your catch rates in the inshore fishery to know what you're dealing with. Just looking at pure catch is not enough. And you don't have to give every fisherman a logbook. You take half a dozen in La Scie and half a dozen in St. Anthony and a sample from other major fishery centres. That'll pretty well give you a fix. That'll tell you what's going on.*
>
> *The one about the local knowledge, the anecdotal information and the historical...I don't know what you call it. The folk memory if you like. I think this is valuable--extremely valuable. But the problem--and I've thought about it a lot--is how in the name of God do you quantify it?*
>
> *Because of our training--our Western thought, if you like--everything has to be analytical, structured, logical, clear. We don't have the scope for intuition that the Eastern philosophies would allow. This is the problem I've seen with this type of information. And there's a gold mine there! Or there was at one time. A lot of it has been lost.*
>
> *I remember making an observation. It was a good 20 years ago when people were leaving--going to Toronto*

and then coming back. For three or four hundred years, we learned from one another. It was an oral tradition that was passed down. And all fishing methods were orally transmitted. It was a continuous chain. But I think the chain was broken in the 'fifties and 'sixties. People went away. And then some of them came back but the information flow was sort of truncated.

And they went and set gillnets in a place where you wouldn't set gillnets. Or they'd set gear in a place where only one fisherman could fish--or only four or half a dozen fishermen could fish--because there were certain unwritten rules that said you set your gear parallel on the slope. And another guy coming behind you sets in a certain way. It was the sociological side of fishing I guess. It allowed for the maximization of a piece of ground.

Because you can take the best piece of ground in Conception Bay, take five gillnets, and you can ruin it for everybody and you won't catch fish enough for brewis[8] for yourself. For the simple reason that they're not set right.

You look at your catch/effort and you say, "I've got five gillnets out there and I only caught ten fish." His grandfather would have taken those ... five gillnets and set them and probably have got twice as many fish. Because he knew the way that the fish moved around that piece of ground in response to the way the wind was the day before.

I grew up setting line trawls around Bell Island and Kelly's Island and you didn't always set the trawls the same way every day because you knew that the fish were deeper or shallower depending on the way the wind was the day before.

So intrinsically we were using [changes in water] temperature. We couldn't detect the temperature but we knew that the water moved back and forth and around the ground. We knew we had to go deeper if there was a northeast wind. Because you had an influx of water coming in that forced the warm water down and your cod went down another five, six, ten fathoms probably. And when the wind went southwest, you'd go shallow again, because your warm water on the surface got swept back out again and your bottom water up-welled and the fish came up the slope.

And how do you work that in to a catch per unit effort? We talk about the technology change with the offshore draggers--that they became so efficient that we couldn't account for it any more. The catch rates were going up and up and up and yet the stock was staying level. But then you come to the inshore and you have to look at the sociology as a technology.

Q: So you're suggesting that the opposite has taken place in the inshore? That knowledge has been eroded?

A: It could be in some cases. No, I think it's balanced out. I have to qualify that one. I think it's balanced out now. Where most people have sounders so they can actually go along the slope and see where the fish are. Or they can look at a trap before they haul it. And the fish are just not there any more. But I have heard a lot of fishermen complaining that you get some fishermen going out who don't know what they're doing and putting gear on the ground and ruining it for anybody else. Because you just can't just set your line trawls, your gillnets, across the ground.

There's one other thing I think we've missed. When I was growing up in the 'fifties, everybody had a trap boat and was their own boss. But gradually, in Port de Grave, we got away from that and got into longliners and instead of waiting for the fish to come in, we went out after the fish. So that's a whole new development there. But it's really not new. Except that they go to the Virgin Rocks now. My father and grandfather and great-great-grandfather went up to the Labrador. Or they went to Cape St. Marys. It's not really different. They're returning to a cycle that was there for many years.[9]

Structural impediments to cognitive/cultural relativism

Dr. Jake Rice is one of the new-style, statistically oriented fisheries biologists. He was recruited by DFO

Science Branch from a university position where he taught and conducted research on quantitative population biology. At the time of our interview, Rice was Head of the Groundfish Division (which included all northern cod research among other things) and had also been given the responsibility of Acting Head of the new five-year, $50 million, northern cod research program. This is the scientific component of the federal government's response to the fisheries crisis; a five-year, $600 million package called the Fisheries Adjustment Program.

In our conversations, Rice often expressed a keen awareness of the cultural conflicts inherent in the institutional interface between science, the state structure, the capital-intensive offshore fishing industry, and the traditional inshore sector. Shortly after our interviews, Rice sought and received a transfer to DFO's research station in Nanaimo, British Columbia on Canada's west coast where, as he said, he is involved with the supervision of research on *"everything except groundfish."*

We discussed the evident problem of reconciling the cognitive reality of inshore fishermen with that of fisheries scientists.

Q: One of the most common criticisms I hear from the public about science is that you just hide away up in the White Hills and we never see you except when it's to tell us bad news. When we first met in that meeting with Mac [Mercer] and Peter Shelton, Barbara [Neis] asked a question about the place of traditional knowledge in the process of resource assessments; whereupon Mac launched into a long story about a scientist who had spent too much time with fishermen and come to a bad end. The way he told it, the story clearly had a moral--and that moral was--that it was not only a waste of time for scientists to spend time with fishermen, but that it was potentially dangerous.

A: I can't recall that story exactly.

Q: The point was that this person had misplaced sympathies which were very human and perhaps understandable but--not only had he neglected his duties as a research scientist--in the end, the conflict between the two cultures had, in some sense, destroyed him. The moral of the story was very clear. Don't fuck around with fishermen and if you do, look out! I was very surprised at the edge buried in that story.

A: Mac, at the time that you talked to him, was not a completely objective person. He had, as it afterwards turned out, a quite legitimate fear for his own neck.[10]

But certainly, in the time I've been with the department, there's been a long history...well, I can't say long history because it hasn't been that long...but going hand in hand with spending a lot of time dealing with the inshore fishermen, is a really severe case of burnout. And a great deal of frustration. Not with the system for discouraging you from doing that. I certainly have...If I went back and went through my book I probably went to 15 inshore fishermen's meetings in the two years I was Head of Division of Groundfish. That's not a great record but it's not a bad one either. As Division Head, I wasn't always the preferred...If it was about particular species, they'd want the specie biologist responsible for it. And that's where the burnout came.

Henry Lear is a classic case. A really excellent cod biologist and son of a fisherman who is 77 [years old] and still out more days than not. The Port de

Grave Lears. He became the person the department would send to every hostile meeting of inshore fishermen. It's a really difficult position to be in. They're often angry about advice you never gave. Decisions that aren't based on the advice you did give. Or you can only tell half the answer because the other half is still being debated in Ottawa for its political sensitivities.

I, and no other scientist in the Department that I know of, have never been asked to lie. But we certainly have, at various times, been discouraged from revealing the whole truth. Every government has to do that to its civil servants. You can't have everything that's going on in the halls of government ending up in the newspaper the next day. You have to allow the people whose job it is to make policy [to] talk about what the advice is, what it means, come to the conclusions and make the policy.

When it gets awkward is when you have a northern cod assessment done in January and revealed in the middle of May. That's a very long hiatus. Not to lie but simply say "Yes, I know what the results of the assessment are but I'm not at liberty to discuss them." Dealing with fishermen's groups a lot you can't avoid finding yourself in situations like that. That context of things is really a recipe to burn somebody out.

I don't know who Mac was talking about but certainly Henry is the example I've seen--and it wasn't that anything bad happened to him. He left Newfoundland. He's still with the department. He has a very good job he's happy with.[11] *But he was a real loss to Newfoundland because he was a good biologist and so deeply rooted in the inshore fishery that he could go down to any dock in Newfoundland, be accepted as someone who would understand them, and come away having understood what they had to day.*

Q: So his burnout was due to the conflict between his native culture and his adopted culture as a federal scientist?

A: To the extent that any case like that has a one sentence explanation, yes. Roughly that.

It wasn't just that he had a party line that he had to toe. It was that he was really at a loss. He *believed as much as any of us that the stock was in good shape but the inshore fishermen were not catching fish. Now, people are saying, in hindsight, that the inshore fishermen's low catches were the first sign that the scientists were wrong.*

The fishermen's inshore catches were completely incompatible with what we now view as the trajectory. The stock built until around '84, stayed stable to '87 and then dropped probably 15-20 percent with the really poor recruiting year-classes we've had coming in. So it went up, went flat and now it's down. The inshore went up, dropped a lot, was down for two years, went up and has been climbing slowly ever since. This year the projections are that it's probably going to be the best year in 20 years for the inshore. So the inshore catches are not tracking what we calculate as the total stock trajectory.

A lot of his burnout was that he could relate to these people, he could share the pain they were going through, and, as a scientist, he didn't have any answers! At that time we believed that the stock was still increasing and we weren't right. It wasn't. But the stock wasn't collapsing. At that time, when the inshore catch was going to hell, the stock was maintaining a stable state. The years it has declined are the years that the inshore fishery has gone up.[12]

Constructing the validity of the primary data source

Rice turns to a discussion of the scientific interaction with the offshore industry. Notice that--while science's relationship with the inshore sector is reconstructed in terms of irreconcilable conflict--scientists' relations with the offshore fishery is portrayed as one of increasingly fruitful cooperation. As discussed earlier in this chapter, the corporately-structured industry shares a functionally similar approach with science to the collection, documentation, and evaluation of information and knowledge.

> *A: Industry, whether it was vested self-interest or not--and I say "vested interest" because industry was quite concerned with what a low influence the CPUE data had on this year's assessment--has been incredibly cooperative in making available to us really detailed records of their best skippers. The skippers' personal log books. Not the required information that goes to Statistics Branch but what every skipper keeps.*
>
> *They have come to us saying, "Tell us exactly what you want and we'll provide it." They will try to match vessels. Because both FPI and NatSea have vessels that are the same in everything but name, but they may differ in the time that certain pieces of technology were introduced. "We'll try to match skipper expertise, we'll give you two identical vessels, and we'll give you the skippers' histories and the time at which certain pieces of technology were introduced." It was this trip that they first used the SCANMAR sensor to say where the trawl doors were. They're providing all this information to us and they've come through with what we've asked. "Tell us what piece of technology you're interested in and we'll give you the data to refine what effect that technological change had on your CPUE index."*
>
> *We haven't solved the problem of getting effort really reliably down, but boy, has industry shown an incredible willingness to make available to us the information that may help us do that. It's a non-trivial analytical job to go through all the data--but just to have it offered that freely shows a real act of good faith on the part of industry.*[13]

Neutralizing the opposition: cooption or marginalization

The conversation then turned to the subject of DFO's institutional response to the inshore fishery's persistent demand for the inclusion of its operational reality in the scientific assessment process. That response, mentioned earlier, is the logbook program. There has been some speculation, articulated by Brown above, as to what extent this effort derives from a sincere interest in the data and to what extent it is a public relations initiative aimed--not so much at the inshore fishermen, but--at the general public who share a sense of cultural solidarity with the inshore sector.

Another plausible interpretation of this program is as a strategy to neutralize the cultural/cognitive authority of the inshore sector's challenge. This could be accomplished in one of two ways. First, wide-spread participation in the logbook program would bring the inshore fishery into cognitive congruence and complicity with science. Second, failure to participate in the program--whether by individuals or by the inshore sector as a whole--could be argued as illustrative of the fundamental irrationality of the inshore sector and used to question the sincerity of their expressed interest in participating in the assessment process. From this perspective, the logbook program is a very effective piece of work. Any outcome serves to enhance science's claim to epistemological authority.

> *A: The final index we hope to have very soon is the inshore. We have the logbook program which, like any big program, has had a rocky start but each year it looks better. And one of the things that we're getting with the [additional] northern cod resources [$50 million over five years] is a dedicated biologist to spend the whole summer going from community to community--whether it's the logbook program or some mutational form of the logbook program.*
>
> *But this will be a person devoted to spending the whole summer dealing day to day on the docks with the inshore fishermen and spending the rest of the year converting what he collects into some sort of an index which will start off with equal weighting in the assessment process: i.e., it will have just as much chance of influencing CAFSAC's view of the stock as any other index does. We hope to have that person staffed by October so they can spend the winter getting to know the fishermen and the associations. Send them around to the winter meetings and stuff.*[14]

In attempting to assess the sincerity of DFO's commitment to this project it is worth noting that, from an institutional perspective, the resources allocated (one full-time biologist) are relatively insignificant--especially so when one compares them against DFO's own assessment of the relative complexities of the inshore fishery versus the offshore fishery.

> *"The annual challenge of estimating stock assessment--estimating the abundance of each commercial*

fish stock--one good measure is the relation between catch and effort....

"In the offshore fishery this is fairly straightforward, so DFO uses catch/effort data from the offshore as one of its sources in estimating the abundance of different stocks.

"It would like to use similar data from inshore, but the inshore fishery is hard to get a handle on. Thousands of fulltime and part-time fishermen in many hundreds of boats, using different kinds of gear, chasing different species at different seasons, with different priorities and different levels of effort and different approaches in all the different bays -- it's a bewildering picture.

"It's like a jig-saw puzzle in which all the pieces keep changing shape and colour. Ben Davis [the biologist] will have his hands full." [DFO *Fisheries News* Vol. 1 No. 2 Spring 1991 DFO]

The message seems to be that such a puzzle is clearly insoluble from any rational perspective. Collecting catch/effort data from the offshore fishery is characterized as "straightforward," but considerable pains are taken to establish the fact that data collection from the inshore fishery is too logistically and methodologically complex to justify the allocation of more than token institutional resources.

Bureaucratic utopianism: and the scientist shall lie down with the fisherman

Turning now to the bureaucratic perspective, J.J. Maguire, chair of CAFSAC, constructs the problem and the solution very differently than do any of the scientists (with the exception of Henry Lear) or their spokesmen. To him, the problem is one of social and cultural impediments to meaningful communications--the creation of a shared context for a substantive reconciliation of currently disparate cognitive realities.

Maguire sees the key to unlocking this problem as lying with those who were responsible for creating it in the first place -- the collective membership of DFO Science Branch and the management structure. Their institutional assumption of epistemological superiority placed fishermen, particularly those from the inshore sector, in a position of inferiority where it was conceptually impossible for them to be a source of valid knowledge. This, quite naturally, alienated the active participants in the fishery from the institution that, in many significant respects, dominated their operational reality.

When the 1989 reassessment concluded that DFO's previous descriptions of the stock had been seriously flawed, the public credibility and legitimacy of the DFO/CAFSAC knowledge claims were compromised while those of the inshore fishery were proportionally enhanced. The institutional reaction of the Science Branch was largely defensive. That of the bureaucratic/political management structure--at least as articulated by Maguire--is more interesting.

As discussed earlier, the legitimacy of the bureaucratic/political management structure of DFO is derived and evaluated quite differently than that of science. From Maguire's perspective, as a mediator of competing interests, what was a crisis for science and a victory for the inshore fishery, is seen as having sufficiently reduced the disparity in epistemological status between the two groups that it was now possible to realistically consider restructuring the relationship.

> *Q: There's been a lot of agitation for the need to include so-called indigenous knowledge, fishermen's knowledge, in the assessment process. This has a lot of political and cultural currency at the moment. It seems that there's some resistance within Science to this idea. That's understandable because the language of science is mathematics. Even if there were a willingness on the part of science to incorporate this knowledge, it would be very difficult. It's like speaking Mandarin Chinese and English. They're two different systems of knowing. Different evaluative traditions that seem almost mutually exclusive.*
>
> *A: It depends how you perceive yourself. I think, for a very long time, we perceived ourselves as holding the true picture. You, the inshore fisherman, have got your perception. You, the offshore fisherman, have got your perception. We see the big picture. You see only part of the picture.*

And, because we thought we saw the big picture, we thought we didn't have to explain too much to you what you were seeing. Or to reconcile what you [the fishermen] were seeing and what we were saying. We thought it was good enough to be somewhere in the middle of you two [the inshore and offshore sectors]. And that we didn't have to explain.

But if you change your position--or your point of view, or your perceived role, and if your role now is one of counsellor, of advisor, a useful counsellor and advisor--if you're an inshore fisherman and you tell me that you observe this, the cod not coming inshore, whatever, my first reaction is going to be well, I'm going to dream up an explanation. "Dream up" not having a negative connotation, but I'm going to try to think what the reasons are and to offer you that explanation. And I'm going to hope that you're going to be satisfied with that.

But we've got to do more than that. And the difference of language shouldn't be that much. The onus is on us to be understood. It's more difficult for us to be understood. Because it's easier to talk about "RV" instead of research vessel surveys. It's easier to talk about "CPUEs" instead of catch-per-unit-of-effort. "Non-linear least square minimization," and stuff like that. Instead of verbalizing and explaining what they are.

I think the question is not so much introducing anecdotal and local knowledge and stuff like that. The objective is to understand what's going on and to try to explain what's going on. It is to relate. It is to go out there and say "What do you see? What's your explanation of what's going on? We'll go and check it out." And we must go and check it out.

Q: But so many of the people that I've talked to--younger scientists as well as older--are either implicitly or explicitly dismissive of this knowledge. I've had people say, "How much can you learn from a bunch of stupid, illiterate fishermen? That the dogberries are heavy this year? What good is that!?" And I suppose now that Mac [Mercer] is no longer there I can say this. When I first met Mac and was talking with him he went in to a long, seeming digression of this business of scientists falling in to the trap of *spending too much time with fishermen. They lose their perspective. They lose their edge. They lose their...*

A: Objectivity.

Q: They lose their objectivity and inevitably come to a bad end. And I think he was probably speaking of Henry Lear, among others. At least that's what I've been told. But the message seemed to be directed, not simply at me but at the other people [DFO scientists] who were in the room that day. And, in my limited experience, this attitude is more common than not. Particularly because of the evaluative and reward...

A: ...structural appraisal system. You're totally right. To me, that's not easily solvable. Very difficult to solve. But I agree with the perception that if you're too close to the fishermen you start to see things their own way. And you lose....If I'm close to the inshore fishermen and you're close to the offshore fishermen, we're going to argue and we're going to reconcile our perceptions some way. And what's going to come out of it is going to reflect a little bit of both. So I don't see a big problem with that.

Q: That's in principle. But in practice there's not a fishing wharf in Newfoundland where a DFO scientist could go and not be laughed off.

A: Yes. Because what we see, what we're describing, the status of the stock, does not jibe with what people are seeing. Until now, most of what we've done is say, "Look! This is the assessment and we know it. Okay? This is it. You may not like it but this is it."

And now I think we've got to change that. We've got to go and say, "This is our best estimate of what's out there. What do you think?" And we're trying to do that now. Formally.

You may have heard or seen that we're advertising for invitations from groups to go and discuss the assessments. All the assessments. And we're going to go and say, "This is our perception. This is our best estimate. What do you think?"

They're going to tell us, "Well, that may be so but we've observed that seals have increased. We think that your perception of the inshore is wrong. Because more of the gillnetters and longliners are now fishing further offshore, 50 or 75 miles offshore on the Virgin Rocks. You're still including them in the inshore so your perception of the inshore is wrong. How much of the inshore is that?"

We're going to get these questions. And what we must do is go back next year, or in the meantime, and say, "We presented you with what we thought the stock was doing and you had questions. These are our response to your questions. Those that we could answer. The others we can't but we're working on them." Or we're not working on them. But there must be a clear, continuous exchange.

Q: But even that, although that would be a tremendous....

A: We're doing that. We're doing that with people who are directly involved with the assessments. I've done it for several groups on northern cod. The main players on northern cod, we've met with them. Individually. We haven't met with, FPI, National Sea, NIFA, inshore, and Fishermen, Food and Allied Workers, all of them in the same group. Because then they can't talk with total honesty with us because there might be something else at stake. When we meet with them individually, they have been very frank, informative and useful meetings.

Q: Isn't there still a kind of residual assumption of epistemological superiority here? That they have the questions but you have the answers?

A: No. Well, they have observations that they want us to verify, I think. But it's not done in a spirit of superiority. You can't feel that superior to these people who make their living out of it and know more about it than you do. You may think that you've got the big picture, but there's all kinds of information that they have that we don't. They have information about misreporting, about discarding, about all kinds of practices that we don't take into account. They don't exist because we haven't quantified them. So they're going to raise those points.

The Martin Luther of fisheries science: Cabot Martin

Cabot Martin is a native Newfoundlander, a lawyer, a partner in several experimental cod farming projects, and president of the Newfoundland Inshore Fisheries Association (NIFA). In this role, he is one of the most articulate, persistent, and irritating critics of DFO science and policy. Sandeman speaks for most of the scientists when he attacks Martin's constructions as irrational and informed by a hidden personal political agenda.

> *"I think they're [the inshore fishermen] being exploited right now by people like Cabot Martin. He's only got one real reason for it. He's going into the political arena before very long. And that's his way of getting there. And he's drumming up all sorts of hoo-hah one way and the other. He's always been a difficult person to get along with. If he had his way, he'd have our management system the same as in the 'States. A thought which absolutely appals me, because I think their management system stinks. Ours has got its faults but theirs stinks to high heaven.*[16]

Notice that--by ascribing cynical and self-serving motives to Martin's advocacy--Sandeman accomplishes two things; one intended and the other not. First, he attempts to de-legitimize Martin's claims by characterizing them as essentially corrupt and, thereby, to separate both him and his position from his constituency. Second, he tacitly acknowledges the considerable political power exercised by the inshore sector through its widespread support among the general public. In the following transcript of my interview with Martin, it is clear that--from the perspective of institutional legitimacy and prestige--the Science Branch has good reason to fear for its monopoly on valid knowledge.

Notice also, however, that Martin is no neo-Luddite. He does not reject the validity of science per se. In fact, he is calling for more and "better" science. In his terms "better" seems to refer to a reformation of scientific ideology so that it accounts for, and is accountable to, the larger-order social, cultural, and economic realities within which it is embedded. This strategy can be seen as a counterpoint to what DFO is attempting to achieve with the logbook program. What we are seeing is a struggle for control of the social authority for the direction of the scientific construction of reality--at least with respect to fisheries science.

> *"I don't know if it's because of the organizational structure or the type of people or the type of disciplines that are involved or whatever, but there doesn't seem to be this broad, open type of inquiry. I think that's partly due to the pressure of the political process telling the scientists what's important.*
>
> *"They're [the politicians] saying I want numbers. I need numbers. I want you to count fish and I'm going to cut off your money in other areas...or I'm not going to give you much money in other areas. And that's partly true--although down here [St. John's] they were given more money last spring and didn't bother to*

extend the scope of their inquiry to take in these other things. I suspect there's a significant amount of inertia.

"It could be that they're just shell-shocked down there. It could be that they feel criticised and under seige. Many of these people have not been trained...or nowhere in their training are social responsibilities. The scientists just took it upon themselves...I shouldn't say took it upon themselves...found themselves in this position where they had tremendous power over peoples lives. But I don't think that anywhere in their training, or anywhere in the internal culture of DFO, would you find a discussion about the social responsibility of scientists to explain and account. And I think that that's a very fundamental problem.

"And there's a whole range of issues that come out of that. The perception of the scientists and how he feels he fits into the whole range of different knowledge, of other questions. They seem to believe that they have this superior form of knowledge which is not additional to common sense or additional to the experience of people working in the industry. It's on a higher plane--somehow closer to the so-called truth.

"And the unfortunate thing is you get this tension. You get many fishermen saying, "Scientists are full of shit." By having that attitude, they tend to undermine the legitimacy of the science in the process. And that's not the answer. The answer is better science and more accountable science and more scientists.

"Maybe it's just the nature of our social organization; that people who go to university and get degrees and put shirts and ties on and work in nice offices and circulate in a social milieu that's different than most fishermen...maybe they inevitably grow apart from the people whose interests they are supposedly looking after or benefitting.

"It's perfectly possible to do internally acceptable...from a competence point of view...an acceptable type of job as a scientist and yet be totally out of context--out of step with reality. You can do that. The fact that you can do that is quite an amazing concept. I don't think there are that many

types of activities where you would get away with that. Right?

"It's almost like the inshore fishery is too complex for them to understand. The collection of data from fifty or sixty trawlers and a couple of [research vessel] cruises a year...that data base is a lot easier to manipulate and easier to handle.

"The worst thing that has happened is that they have been shown to be incorrect. I've heard scientists say, "We can't afford to know how little we know because if we admitted that then no one would listen to us." That's twisted....

"So when science is tending to put the all or nothing question to fishermen and other groups, "I'm either totally in charge or I'm not going to be in charge at all,"--most fishermen, looking at their track record, would say, "Well you're not going to be involved. If you're not prepared to be reasonable, then I can't handle it." And I think that's a great tragedy. I think that would be worse."[17]

Summary and Analysis

In this work, I have set myself the task of identifying and explaining the social forces that have shaped the construction of the scientific stock assessments of the northern cod. In the course of my research, it became clear that I would have to account for several aspects of the cognitive and political relations between the Science Branch and the commercial fishery. In the second half of Chapter Five, we examined the structural factors that tend to deflect the course of scientific knowledge production away from areas of interest or utility to the institutionally mandated client groups--the political/bureaucratic structure of DFO and the state, and the commercial fishery. Here I have shown that science and scientists have made a sharp distinction between the inshore and offshore sectors of the fishery with respect to the scientific relevance and validity of their cognitive realities.

The scientists explain their acceptance of stock assessment inputs from the offshore fishery--and rejection of the inshore fishery as a valid source of stock assessment data--in objective, rational terms. Data from the offshore sector is plentiful, dense, and efficiently and inexpensively collected. It is either generated in quantitative terms or is easily quantifiable. The relative standardization of technology and similarity of operational strategies and practices reduce the apparent number of uncontrolled variables--thereby imparting familiar form and scientific legitimacy to the data. Above all, the offshore fishery is a recognizably rational enterprise. The social organization of the two dominant offshore fishing corporations is similar to that of DFO--organically specialized and hierarchical. The performance of the fishing operations are monitored and evaluated according to shared standards of quantitative objectivity. Finally, it is important to note that when scientists speak of the offshore fishery as a data source, they do not refer to individual fishermen--but either to the two dominant corporate entities (FPI and NatSea) or to the sector as a monolithic structure. By contrast, the inshore fishery is portrayed by scientists as a hopelessly heterogeneous muddle of uncontrollable variables--individuals, gear types, fishing practices, geography--from which it would be highly inefficient, perhaps impossible, to distil any meaningful data.

This rational explanation, however, is not consistent with the findings of my research. We have seen in earlier chapters that the data from the offshore fishery is susceptible to error and bias from many uncontrolled and unknown sources. Henry Lear--the only scientist in this study with a more than superficial understanding of the inshore fishery--has made several substantive suggestions for the efficient collection of acceptable data from that sector.

A more plausible explanation of the sharply divergent attitudes of scientists to the two sectors of the fishery is to be found in an extension of our earlier characterization of a conflict as tribal warfare. In that case the conflict was internal to the Science Branch but was a war between two distinct scientific cultures for the control of knowledge construction. I suggest that the critical dynamics of the relationships between science and the fisheries are best understood in similar, but more generally applied, terms. In this case the Science Branch and the offshore fishery are similar enough in institutional and cognitive structure and function that they can be seen as sharing a broadly-defined rationalist culture with the authority and legitimacy of their knowledge constructions resident in the concept, structure and function of that culture of rationality.

The inshore fishery, however, is embedded in a distinctly different socio-economic and cognitive culture which has historically existed on the margins of the dominant liberal-capitalist-scientific society. The current crisis in the fishery has destabilized prevailing political and epistemological power relations creating opportunity for renegotiation of those relationships. The most dogmatically resistant to this possibility are those with the most to lose--the scientists. We can reasonably hypothesize a deep, tribal fear of loss of prestige and authority. Their strong commitments to the shared cognitive reality of scientific culture is threatened. The perspective of the bureaucratic/political culture is that this situation offers an opportunity to reconcile, or significantly reduce, long-standing impediments to the state's role as a mediator of conflicting and competing constituencies. The inshore sector--with the most to gain--is also the most open to a restructuring of the power relations. Again, it is noteworthy that what they have to gain are not necessarily material benefits in the form of more fish (although that is a hoped-for, long-term benefit) but an external validation of their cognitive reality and an elevation to terms of near-equality of their epistemological status in the assessment and management process.

ENDNOTES

1. The following is from an op-ed piece in the *Evening Telegram*, a St. John's daily newspaper. The author, William Cox, identifies himself as "the captain of a deep-sea fishing trawler for some 14 years." He also delivered the same message at a press conference.

> *"I've been fishing northern cod for eight years and I tell you there are more fish there now then [sic] there were eight years ago. . . .*
> *"On our last rip (Jan.26-Feb.3/90) we were fishing for cod in area 3K in water 300 to 400 fathoms deep; fishing was good. From where we were fishing we steamed south for 65 miles on a straight course. We steamed over one school of*

fish which was eight miles long. For nearly all of the 65-mile steam there were fish showing up on the sounder.

"After the 65-mile steam we were in the area where the freezer trawlers were catching 25,000 to 100,000 pounds for one-hour tows. The codfish were so large that the freezer trawlers had to freeze them separately in the fish-hold." [Cox, William in the *Evening Telegram*, Feb. 24, 1990 p. A1]

2. Conflicts between federally-sponsored marine biologists and fishermen over the construction of aquatic reality are, apparently, nothing new. Johnstone mentions a confrontation where Dr. A.P. Knight, circa 1917 *"...told a group of sceptical fishermen: 'You'd better listen; I know more about lobsters than any man alive.'"* [Johnstone 1977]

3. An explicit, and classic, example of this was the apparent deal cut between John Crosbie--Minister of Foreign Trade and Newfoundland's only representative in the federal cabinet--and Vic Young, the president of FPI.

> *"John Crosbie says he has been doublecrossed by Fisheries Products International president Vic Young and will never trust the fish executive again.*
>
> *"Mr. Crosbie said . . . that Mr. Young came to Ottawa to explain FPI's position if the northern cod TAC was going to be set at 190,000 tonnes. FPI's share of the TAC at that level would have been about 31,000 tonnes.*
>
> *"[Crosbie said] 'Based on that situation he [Young] said that they would have to close . . . at least three plants. That was repeated in subsequent meetings with our officials and with us. But if it were possible to get a larger allotment of fish...then it would be possible for one or two of the plants to be saved.*
>
> *"'In order to try to save one or two of the offshore plants, I did my best to see that we got a further allotment for the offshore companies.'"* [*Sunday Express* Jan. 7, 1990]

As it turned out, both FPI and NatSea did receive additional quotas as a result of Crosbie's effective lobbying of the Minister of Fisheries and Oceans. However, neither company deferred its planned closures of idle or under-capacity fish plants.

4. From an interview with Bernard Brown conducted in St. John's on August 3, 1990. The full transcript is Appendix B.

5. From an interview with Bernard Brown conducted in St. John's on August 3, 1990. The full transcript is Appendix B.

6. From an interview with Sandy Sandeman conducted September, 1990 in St.John's. The full transcript is Appendix O.

7. From an interview with Bernard Brown conducted on August 3, 1990 in St. John's. The full transcript is Appendix B.

8. Fish and brewis is a traditional Newfoundland dish of salt cod and hardtack biscuits stewed up together to a porridge-like consistency and served with "scrunchions"--finely diced salt pork fried crispy and golden brown.

9. From an interview with Henry Lear conducted January, 1991 in Ottawa. The full transcript is Appendix K.

10. Mercer resigned as Director of the Science Branch during the hight of the storm of criticism of DFO science that followed the public release of the Harris report. The timing of his resignation prompted considerable speculation as to whether he jumped or was pushed. Insider opinion strongly favoured the defenestration theory. However,I was unable to secure an authoritative confirmation or denial of this from a source superior to Mercer in the DFO hierarchy. Rice clearly intends us to understand that Mercer's resignation was involuntary. Rice was highly-enough placed that his claim carries considerable credibility--but it is not definitive.

11. Lear is currently working in Ottawa as a research assistant to Scott Parsons, Assistant Deputy Minister of Special Projects, who is writing a history of fisheries science in Canada since the advent of the 200 mile limit.

12. From an interview with Jake Rice conducted in St. John's on August 14, 1990. The full transcript is Appendix I.

13. Ibid.

14. Ibid.

15. From an interview with J.J Maguire conducted in St. John's on October 28, 1990. The full transcript is Appendix J.

16. From an interview with Sandy Sandeman conducted in St. John's, September 1990. The full transcript is Appendix O.

17. From an interview with Cabot Martin conducted in St. John's on March 15, 1990. The full transcript is Appendix C.

CHAPTER SEVEN

THE MACRO-CONSTRUCTION OF MICRO-REALITY

Beginning with Chapter Five, we have been exploring the social construction of fisheries stock assessments at progressively larger scales of social organization. We saw how social forces and events occurring on the micro-social level--the data wars between a few individuals in the Science Branch--are dynamically related to forces and events at other scales of organization; in this case, a large-scale socio-economic crisis which, in turn, generated political forces felt at the highest levels of the Canadian State. We saw how mid-scale social structure--DFO's reward and promotion system--can be shown to have generated social forces of sufficient magnitude to account for the data wars. We saw that relative levels of dissonance and congruence between the cognitive realities of four mid-scale social organizations--the traditional inshore fishery, the corporate offshore fishery, the scientific sector of DFO, and the bureaucratic/political sector of DFO--can been shown to have significantly affected the production and content of the Science Branch's knowledge claims.

Now we will examine issues arising at the largest scale of social organization show that social forces originating at this scale are dynamically related to events and knowledge constructions on the smallest scale--that of individual scientists.

The State and the Construction of Scientific Knowledge

At present, the Canadian state's policy and practice of natural resource management is theoretically informed by various elaborations of Garrett Hardin's thesis of "the tragedy of the commons," which suggests that any resource unprotected by property relations will, inevitably, be exploited to extinction. [Hardin, 1968] This perspective provides the underlying logic of the state's declaration of ownership of certain resources which are too vast, diffuse, mobile, or elusive to be located within the context of normal property and market relations--but which are, nonetheless, deemed to be of significant value. A liberal-capitalist state tends to assume resource property rights only under conditions of actual or anticipated market failure.

To be sure, there are dissenting and alternative interpretations of the relationships between human societies and natural resources. In *Uncommon Property*, Marchak [1989] argues that there is no theoretical impediment to successful cooperative or communal management of common resources. However, it is fair to say that, at present, this is a relatively peripheral position in resource management theory.

Having briefly acknowledged prevailing and dissenting theory, I would like to move to an exploratory discussion of the role of science and scientific knowledge--embedded in the bureaucratic/political structure of the state--as it relates to resource management and exploitation. I will show that, in the case under study, the forces generated by the tensions and conflicts inherent in the relationship between science and the state impinged in significant and specific ways on the micro-level interactions of a small group of scientists and the construction of their knowledge claims. The two most recent crises in the Atlantic Canadian fishery offer a productive and extremely relevant empirical grounding for an exploratory discussion of theoretical issues. As a prelude, it may be useful to briefly recap the earlier review of the most comprehensive documentations of these crises; *The Kirby Report* and *The Harris Report*.

The Kirby Report

The 1983 Kirby Report responded to a sharp decline in the overall profitability of the Atlantic Canadian fisheries due to a persistent "cost/price squeeze." Operating costs were rising in the face of a steady decline in the demand and price received for the products. The report's findings--and recommendations for a restructured fishery--were based upon the explicit and reiterated assumption that the resource base was strong and would continue to grow stronger under the capable management of Canadian fisheries scientists. And by far, the greatest growth in the resource base would occur in the northern cod stocks.

> *"The rebuilding of the northern cod stock is expected to continue through 1987 when a Total Allowable Catch (TAC) in the vicinity of 400,000 t [metric tonnes] or more is forecast. This level is almost certainly below the maximum sustainable yield from the stock....By following a conservative rate of harvest...the eventual long-term production of the stock is thought to be about 550,000 t annually."*
> [Kirby 1983 p.242]

The text goes on to acknowledge a degree of uncertainty in fisheries forecasting and that these estimates, therefore, are **deliberately conservative.**

This crisis was not perceived as a resource crisis but a failure of market mechanisms that, in turn, begot social and political crises. The Kirby Report recommended that harvesting, processing, and marketing of the resource be restructured to achieve the following primary objective:

> *"The Atlantic fishing industry should be economically viable on an on-going basis, where to be viable implies an ability to survive downturns with only a normal business failure rate and without government assistance."* [Kirby 1983 p.vii]

The clear implication was that, by using the fishery as an instrument of social policy, the state had upset the natural and classic workings of the market.

Consequences and Crisis

Based upon the Kirby Report's recommendations, the Canadian government restructured the offshore harvesting and processing sectors of the Atlantic fishery by combining a number of (unprofitable) small and medium-sized companies into two publicly-held "super companies"--National Sea Products (NatSea) and Fisheries Products International (FPI)--with the intention of returning their ownership to the private sector once market equilibrium and profitability had been restored. [Sinclair 1985]

Based upon faith in the success of this restructuring and the Report's predictions of an increasingly abundant resource, individuals and corporations involved in the fishery--and particularly the northern cod fishery in the Northwest Atlantic Fisheries Organization (NAFO) management areas 2J,3K, and 3L--made heavy capital investments to update and expand their harvesting and processing capacities.

Contrary to earlier DFO estimates of a 15 per cent annual rate of growth in the stock and projected increases in the quotas--the Total Allowable Catch (TAC)--the northern cod TAC was held at 266,000 mt through the 1988 fishing year. In fact, the landings were lower than the TACs set for the years of 1980-1986 even though the TACs were thought to have been set to achieve a very conservative target fishing mortality of $F_{0.1}$ or roughly 20 percent of the catchable stock. [See Table 7.1]

Table 7.1

Projected and Actual TACs for Northern Cod in NAFO Zones 2J3KL: Years 1970-1993

Year	TAC (X 1,000 mt)	Projected TAC (year of forecast) (X 1,000 mt)			
1970	520				
1971	430				
1972	460				
1973	350				
1974	375				
1975	280				
1976	220				
1977	170				
1978	140				
1979	175				
1980	180 (landings 176)				
1981	200 (landings 171)	200 (80)			
1982	237 (landings 230)	250 (80)	230 (81)		
1983	260 (landings 232)	280 (80)	260 (81)		
1984	266 (landings 230)	325 (80)	280 (81)		
1985	266 (landings 232)	360 (80)	320 (81)		
1986	266 (landings 252)		360 (81)	290 (85)	
1987	256		375 (81)	300 (85)	
1988	266			310 (85)	
1989	235			340 (85)	248-435 (89)
1990	197			350 (85)	244-466 (89)
1991					245-477 (89)
1992					254-471 (89)
1993					260-492 (89)

[From: *Resource Prospects for Canada's Atlantic Fisheries--1980-1985, 1981-1987, 1985-1990, 1989-1993*

DFO, Ottawa, 1980, 1981, 1985, 1989]

People with an interest in a sustainable, profitable fishery began to suspect that the DFO/CAFSAC numbers might be considerably less than accurate. Following the internal reappraisal of their assessment methodologies, for 1989 DFO/CAFSAC adopted a revised--and ostensibly more accurate--mathematical model to generate stock estimates from research and commercial catch data. The results (and CAFSAC's consequent recommendations for a drastically reduced TAC) were sufficiently alarming to precipitate the most recent crisis and the formation of the Northern Cod Review Panel--or Harris Commission--to investigate and report its findings.

The Harris Report

The Harris Report suggested that the DFO/CAFSAC estimates of stock strength were derived from data of uncertain or suspect quality and that these data were fed into mathematical models predicated upon highly speculative assumptions. Therefore, the point was mooted whether these estimates had any utility as a rational basis for management or commercial planning.

In a recent public discussion of the commission's findings Harris stated the problem explicitly. "*We believed our science to be much better than it was....This fancy method of counting went wrong....The data was wrong....Garbage in, garbage out is a standard formula.*" Harris concluded his presentation with the observation that "*[the] scientists are not all that credible given their performance. On the other hand, if you don't trust the scientists, who do you trust?*"[1]

From this perspective, the current crisis appears not as a resource crisis (which it may or may not be) but as a *knowledge* crisis. The simple conclusion of the Harris Report was that the empirically robust knowledge base about the fisheries resources is surprisingly small and what is known is inadequate as a basis for meaningful stock assessments and, therefore, rational management policy.

Catch 22

In this highly charged, highly consequential context, the pressure on DFO science is enormous from all the interested social groups to generate legitimating ground for the strategic and tactical decisions that **must be made.** These groups include corporate harvesting and processing interests, fishermen's and plant workers' unions and associations, federal and provincial governments, and DFO's management structure itself. As a fisheries scientist said during a private conversation at a recent international conference in St. John's , Nfld. *"'I don't know,' simply isn't an acceptable answer."* And yet there is a definite and growing dissatisfaction with the answers--to the point where the legitimacy and/or utility of scientific knowledge is being questioned as a voice in the process of fisheries management.[2]

The Problem

Although the widespread awareness of a state of crisis was initially due to the announcement of deep reductions in the 1989 and 1990 TACs for northern cod, the ensuing debates as to the relative seriousness of the problem, and remedial alternatives, have called into question the federal government's management of the fisheries--particularly with respect to its primary reliance on scientific knowledge generated by DFO for guidance of its policy and practice.[3]

Some voices in these debates are challenging the privileged epistemological position of scientific knowledge. Various political interests clearly view this as an opportunity to increase their power and influence in the fishery. [See Endnote 2] Other voices--primarily from the inshore sector--argue that the traditional knowledge of fishermen, skippers, and other members of fishing-dependent communities may be equally valid as an input to the management process. [Neis, 1990] This is, in effect, a call to qualify knowledge in terms of social relevance and social responsibility.

<u>The Structural Demand for Certain Knowledge</u>

The present system of fisheries management and exploitation planning is structured on the premise that science is capable of providing the system with quite precise and reliable assessments of stock strength as well as realistic projections of the consequences of alternative management strategies. The setting of TACs for each stock in terms of a single number for a fixed period of time is the practical expression of this premise. The question of whether or not this premise is valid is of vital importance to the socio-economic interests of Atlantic Canada. And of the Atlantic provinces, Newfoundland is most deeply dependent upon a viable and sustainable fishery.

However, in the current atmosphere of crisis, it is not the system's underlying demand for and presumption of certain knowledge that is being questioned, but the competence of fisheries assessment science and scientists.[4] A review of the electronic and print media coverage and editorializing on the crisis shows--both explicitly and implicitly--that fisheries assessment science is being blamed, in part, by both the public and private sectors for the current crisis. A further, much more crucial question is just beginning to be addressed: Is it reasonable and responsible to predicate policy development, management strategies, and exploitation structures on the assumption that science is capable of providing knowledge of the requisite precision and certainty.

The Question of Competence

The question of the competence of fisheries assessment science is subject to evaluation in a number of conflicting evidential contexts and the decisive factor is the definition of "competence." On the macro-level, the state's legitimacy as the manager of a common resource depends upon its ability to successfully mediate the social, economic, and political interests of various groups with respect to that resource. The government cites the recommendations of DFO--and, ultimately, the fisheries assessment scientists themselves--as the authoritative and objective grounding for its policy and management strategies. The generally perceived success or failure of the state's management of the resource is the evidential context for its evaluation of assessment science's knowledge claims. Thus we see that--in the state's evidential context--the answer to the question of the "competence" of assessment science hinges upon its utility in meeting the demands and avoiding the criticism of the existing exploitation structures and the social groups dependent upon the fishery.

Dr. Brian Morrissey, DFO's Assistant Deputy Minister of Science spoke to this point during our interview in his Ottawa office.

> *A: The second question is expectations of certainty for management. I would say, "Yes. There are expectations that science produce scientific predictions for the future of stock assessments." I see this not just in*

this department but in other departments where you deal with numbers.

If, for example, we were to say that the total allowable catch for, let's say, 250 thousand tonnes is possible in a given year, 250 thousand tonnes is a hard number. It's quite different from 251,000 tonnes or 249. Because you say a number, it has a precision that covers the uncertainty on which it's based.

I really haven't found a good way, in this department or in another department where I was involved in residues in food, to say, "Seven parts per million." Well, there you're drawing a firm line with a very unsteady hand. It could easily be point six or point eight. Point seven implies that it's that number and no other. It couldn't vary.

My other comment on uncertainty, particularly in this department, is that people's lives are affected by the number that's given for the TAC. And because people's payments on their gear and their boats are fixed--and are dealt with with great certainty by the bank--they are under pressure to have a catch and a cash-flow that has equal certainty.

In consequence, for them looking into the future, they are frustrated by not having consistent predictions of catch. In other words, 250,000 tonnes every year into the future. And because they are frustrated, they become angry. And when they become angry, they direct their anger at whoever seems to be frustrating them. So I would say, "Yes. There is an expectation for certainty." And we have become the focus for frustration and unhappiness when we can't provide certainty.

The big question was, "How does uncertainty affect the day-to-day management?" I'll take the lead-off from the last question. Because there's a perfectly understandable desire on the part of the fishing community and the fish processing community to have some certainty in the stock they can get, those they can take, that translates into pressure on this department to produce; A. certain numbers and B. consistent numbers over a period of time to avoid fluctuations and C. to provide increasing numbers because decreasing numbers are punishing.

Punishment to them translates to punishment for us. If we don't produce those ever-increasing numbers. So yes, I would say that uncertainty has made it more difficult.[5]

On the micro-social level--within the relatively small group of fisheries assessment scientists--theories, procedures, results, and knowledge claims are judged within a very different evidential context. The critical frame of reference is seen by the scientists to be delimited by the very academic, objective, evaluative traditions and protocols unique to science itself. From their perspective, there is no such thing as certain knowledge. All knowledge is probabilistic, rendered more or less probable by its ability to withstand disproof. Additionally, scientific knowledge is held to be objective and aloof from human social reality. This is the very essence of the scientific method. Sandy Sandeman discussed this in terms of the relationship between scientific stock assessments and the political and corporate consumers of that knowledge product.

A: They [non-scientists] don't understand the basic facts of science. They don't understand how an assessment is done. They don't understand that, in doing an assessment, there are all kind of assumptions which are there. They don't think about all these things. And when our scientists are asked to make a prediction, they make a prediction with all kinds of caveats and "if" statements surrounding them. Probability statements. If this happens and this happens then something can be expected. But if something or other happens...and so on.

And when this comes out in anything but a scientific journal, it comes out as a bare prediction. That this will happen! Never mind if, if, if, if, if! And, unfortunately, most of the trade and most of the non-scientific people, all they read is the final shortened version which says that this will happen. And it doesn't.[6]

In the course of a recent interview with Mac Mercer, then-Director of DFO's Newfoundland Region Science Branch, he made it clear that--from the perspective of science--the origins of the crisis are to be found in the social, economic, and political decisions and assumptions embedded within the policy and practice of the management and exploitation sectors. Another scientist present during the interview maintained that social, economic, and political factors are irrelevant to the conduct and evaluation of their research saying, *"The truth is discovered, not negotiated."*[7]

Complicity and Coercion: The Political Construction of Expectations and Illusions of Scientific Precision

There are several important issues which I will discuss in the context of my interviews with key actors. The first is the political/bureaucratic direction of scientific knowledge construction associated with the extension of

Canada's management authority and responsibility to 200 miles. The second is the changing nature of the Science Branch's conception of its relationship with the fishing industry. The third is the relationship between government and science with respect to the assessment of resource status and the provision of scientific advice as an input to management policy and practice. In practice, of course, these issues and relationships are interdependent and interactive. This larger-order relationship will be discussed in the *Summary and Conclusions* of the chapter.

Science, the 200 mile limit and resource projections

Having made very public, explicit, international commitments to the responsible stewardship of the offshore stocks, the political/bureaucratic structure of the Canadian state turned to its scientists for guidance and advice in the fulfilment of that commitment. In particular, the federal government--on behalf of both its own interests and those of the Canadian fishing industry--asked DFO scientists for resource projections; predictions of northern cod abundance up to ten years into the future. Most scientists now claim that they were fully cognizant of the manifold uncertainties inherent in such predictions and were extremely reluctant to make these definitive statements. When they finally did succumb to the pressure to do so, they surrounded their work with strong statements of qualification.

With Mac Mercer's resignation in 1990 as Director of Science, Larry Coady, his long-time assistant and Scientific Program Coordinator, became Acting Director of the Science Branch. (Since conducting my interview with Coady, he has been confirmed as permanent in that position.) He reconstructed the period of the extension of Canadian management authority to 200 miles and the hugely increased responsibilities and demands that placed on the Science Branch. Coady suggests that the political demand to produce projections originated in the fishing industry's need for strategic financial planning.

> *A: We were asked to provide five-year projections of stock status. We weren't able to do it. You may as well have gone out and bought a crystal ball or put on a magician's hat and pulled out a piece of paper. It couldn't be done. And we were obliged to do it anyway.*
>
> *Q: Where did that obligation originate?*
>
> *A: From fisheries management and the fishing industry who had to know. We had access to the 200 mile zone and expected to increase our presence in that zone as foreigners phased out.*
>
> *Q: So you feel pressure from the upper levels of management and Ottawa to do a job that you know can't be done?*

A: I wouldn't put it that coarsely. There was a genuine interest at that time in knowing what the future held for the fishing industry. Companies had to go out and buy ships. Should they buy ten trawlers or should they buy twenty trawlers? Where were these stocks going?[8]

Jake Rice was Head of the Groundfish Division of the Science Branch at the time of our interview. He also invokes the metaphor of the crystal ball to illustrate science's assessment of the futility of making predictions. Ultimately, however, the pressure to do so became overwhelming. The government's threat to give the job to economists--should the scientists continue to refuse--was powerfully coercive given the professional reservations of "hard" scientists as to the knowledge-value of the production of the social sciences and, particularly, economics.

A: At the time of the extension of jurisdiction, Science was asked for a bunch of projections. The economists need ten- or fifteen-year projections to look at investment patterns and rebuilding things. Any scientist would have said, "We can't look that far ahead into the future. Four years from now and we can talk about right now. Four years from now, about eighty per cent of the fish being taken by the fishery will be from year-classes we have not yet seen today. You're just gazing in a crystal ball." The scientists were told...they refused the first two times they were asked for fifteen-year projections.

Q: What years are we talking about?

A: We're talking about '76, '77, '78, in there, as we, Canada, was getting ready to extend jurisdiction. The first couple of times they said, "We absolutely can't

> *do it." But the word came back down. "We must have these projections. If you don't give them to us, we'll give the job to the economists and they'll do it."*[9]

The reluctance of science to make the projections and the current emphasis on the conditional clauses and qualifications which surrounded those projections may well be an artifact of the recent, wide-spread criticism of science. There is no doubt that--in the wake of the advent of the 200 mile limit and for ten years thereafter--there was widely-shared and essentially unquestioned belief that the northern cod stock could be and, in fact, was being steadily rebuilt from its depressed pre-1977 state. That this belief, irrespective of reflexive qualifications, was also shared by DFO science is documented by the annual current-year assessments and dependent $F_{0.1}$TACs which--until the critical 1989 reappraisal--generally followed the predicted trend of linear increases. There is good reason to believe that science had a large hand in the creation of the expectations which it is now claiming to have warned against.

Bern Brown, Public Information Officer at the DFO station in St. John's, does not claim to be qualified to judge the content of the Science Branch's knowledge production. He does, however, have the qualifications and experience to evaluate nuances of the form and style with which those claims were presented to the non-scientific individuals and groups with an interest in the fishery.

> *A: I find it hard to deal with this ambivalence there seems to be among the scientists. Because they did know damn well that the numbers that they were coming up with could be way out. Probably were way out a good deal of the time. And yet they were quite free in saying, "We're doing a pretty good job here folks." I suppose they did feel, correctly, that they were doing just about as good a job as could be done. What's caught up to them is that no one was willing to go out and try to make it clear to the fishing industry and the public how much uncertainty lay in all the science. And the way in which it finally became clear was the worst way possible for them. Instead communicating something about the uncertainties, every confidence was expressed.*[10]

Jim Roache is a career journalist and public relations professional who has, in recent years, been working for DFO in Ottawa. He was recently moved by DFO to St. John's to coordinate the regional response to the rising tide of criticism. Roache makes the same point somewhat more circumspectly.

> *A: What I see science as having done is having failed, over the last ten to twenty years, to elaborate to the public in a language that they could understand what they could expect from science. In other words, it should have been clear up front, communicated consistently throughout, that here is the job that we scientists are trying to do, here are the resources that we're allowed to do that job, here is the short-fall in those resources, here is the probability of accuracy, and therefore, every number and every option that we give the mangers is given with the caveat that*

within these conditions and limits, our best guess is X.

It really wasn't made clear that the scientific work that was being done wasn't foolproof. It wasn't really made clear that the recommendations given to the managers weren't really recommendations as you and I understand the word, but were sets of options or ranges of numbers which had certain likely outcomes attached.

Now scientists understand that to be the case and take it for granted that everybody else understands, but people don't. Had people been properly attuned to what was reasonable to expect up front, had they been attuned to the fact that there was a certain element of risk or uncertainty associated with the recommendations that the scientists were producing, had it been explained by the other parties (the managers and the politicians) that there were other variables--that the scientific output was only one of the inputs for the fisheries manager or the politician to weigh in determining the TAC, and those other considerations are equally important from other perspectives.

We all rode the wave of our own expectations. And we're now in the middle of a crisis of those expectations, not a crisis in the state of the stocks.[11]

Larry Coady was more explicit than some of his colleagues in the Science Branch in discussing the early, post-200 mile limit, expectations of the scientists as to the accuracy of their knowledge of the system's parameters and the effectiveness of their management prescriptions.

Q: I've looked at the resource prospect publications going back to the late '70s and they're quite striking. You have the bar graphs with the actual catches for the previous years on the left going up and down showing considerable variability and then the prospects on the right are these beautiful linear ascending stair steps.

A: See, that was assuming that the only change was management practice and that we had control of it. Prior to that it was more hit and miss. Preemptive sort of stuff. [12]

Another factor that contributed to the illusion of certainty and the expectations of control was the practice, until 1989, of expressing the annual assessments and quota recommendations as a single number rather than as a probabilistic range bounded by calculated confidence limits. The reason for this is to be found in the dynamics of the relationship between the political bureaucratic structure and DFO Science. The management bureaucracy of DFO was not interested in probabilistic assessments and qualitative advice. Their interests, as patrons of science, were in assessments and advice expressed with sufficient precision that they could serve as apparently objective grounding for management policy and practice. This permitted the justification of contentious decisions by the simple statement "Well, that's what the scientists tell us."

This construction is supported by Sandy Sandeman. I had asked him whether the scientists were justified in their feeling that they had been abandoned by their bureaucratic/political masters and left to bear the brunt of the criticism for the drastic reductions in the northern cod

TAC--a decision made by the federal Minster of Fisheries, not the Science Branch.

> *A: I think it's justified, yes! In the same way, we have seen that, as soon as any question comes up which has unpleasant consequences, say down-sizing of a quota, the decision makers (who are not the scientists) usually take every opportunity to "protect" themselves using such statements as, "Well, that's what the scientists tell us." However if the quota is to be raised, somehow the message seems to come through that it is by their (the managers) diligence that this is happening. Quota decisions are not scientific decisions. They are socio-economic-political in nature. And yet, when the news is bad, the answer always seems to be "Oh, that's what the scientists tell us!" It's just not true! But that's the easy way out and we've always kept quiet about it.*[13]

A somewhat less cynical reading of the relationship would be that executive, decision-making structures expect and demand that the various sources of information input to the decision-making process pre-digest their data--to present it in simplified, unproblematic form. This is, after all, their institutional function and responsibility. Ambiguity and uncertainty from sources of specialized expertise is not seen as useful from the perspective of the executive structure of a rational bureaucracy. It is then easy to understand why all the conditional clauses and phrases--the caveats, that the scientists routinely attached to their assessments, projections, and advice--were equally routinely stripped away and discarded by the consumers of scientific knowledge.

Here we have a clear example of institutional structure and mechanism forcing the resolution of an indeterminate natural reality. It is a truism that any decision is often better than no decision, but in the case of fisheries management, even this consideration is irrelevant. The policy of setting annual quotas or TACs for each managed species in each region means that a decision *must be made* and that the decision is expressed as a hard, unambiguous single number. The actual state of a stock may lie within a large range of probability bounded by large confidence limits but there is no mechanism in the present management regime to accommodate uncertainty.

Adding a further complicating factor to the dynamic, the scientists themselves had their own reasons for accommodating this demand for unambiguous advice. They had come to conceptualize their role as protectors of the resource from a rapacious, irresponsible fishing industry and did not trust the management structure of DFO to make conservative, responsible decisions. In this we find another plausible reason for science to have been somewhat less than assiduous in communicating the assumptions, uncertainties, and known sources of error in its assessments and advice. This dynamic is apparent in the following comments by J.J. Maguire, Chair of the Canadian Atlantic Fisheries Scientific Advisory Committee (CAFSAC).

Maguire explains the profound change in relationship between DFO Science and the fishing industry, particularly the corporate offshore trawler fleet, when the 200 mile limit became effective.

> *A: You've mentioned something, it's the closeness to the clients. I think that's what went wrong. We distanced ourselves from the clients...from what we were supposed to do. And we came to be seen as an impediment for the industry.*
>
> *It was before my time, but I understand in the ICNAF days, when all the foreign countries were fishing off the east coast here, the Canadian scientists were very, very close to the Canadian industry because what they had to do then was to work for a common objective--to build the resource and kick the foreigners out. That was achieved in 1977. The rebuilding of the stocks, most groundfish stocks, happened very, very rapidly. By 1980, 1983, they were rebuilt.*
>
> *The entire system I've known is that the industry is going to....Well, let me rephrase that. Don't be too close to the industry because the industry, their natural tendency, will be to over-exploit the resource. You're not at their service.*
>
> *I think what I'm trying to say is that there was some kind of a confrontation. That we're not on the same team any more. We had been on the same team for eight to ten years--working for a common goal. And now*

we were on separate teams. And we had separate objectives. The objective of the fisheries biologist was to conserve and protect the resource. And the perceived objective of the industry--mobile gear, offshore, capital-intensive--was to over-exploit the resource. That's what was being expected, I think. So that's one thing that went wrong. We distanced ourselves from our clients.

The other thing is that--because of the particular management system that we choose, which is based on Total Allowable Catch--there was more precision required of us, fisheries biologists, than we could offer. I think we thought, at least I did, naively...ten years ago I thought that our assessments were much more precise. I think that the experienced people at that time knew that they were not that precise. But when you have a management system that reacts, let me say, dramatically, to a change of five per cent--if you change the TAC, any TAC, of northern cod or any other one, by a very small margin, it's going to create big problems all the way down the pyramid. And when you realize that the precision of the stock assessment is, at best, on the order of plus or minus 25 per cent, then you realize that there's a discrepancy.

And what we were doing is that each year we were adjusting the TACs in relationship with the variability in the data. And there was total discrepancy between what the assessments that we were doing were saying and what the clients were seeing. We had two groups of clients, as well, with opposing views. The inshore seeing one thing and the offshore seeing something else. And often times we thought we were somewhere in the middle but being somewhere in the middle, you've got no one agreeing with you.

I think those are two of the main reasons. One, we distanced ourselves from our clients so they didn't see us as being helpful to them. And second, the system was expecting more precision out of us than we could offer.[14]

In the following exchange, Jake Rice has been contrasting with previous practice, the recent [1989] shift to probabilistic assessments and advice presented as a range of options having probabilistic effects. We discussed the reasons why this information had previously been expressed with misleading precision.

A: That's a very different message [the new form of advice] than scientists used to give. I agree. They used to be guilty of saying, "The number is this."

The advisory system, up until the northern cod problems, really wanted the scientists to resolve it down to a point, with the message coming back that, "If you don't do it, who's going to? Who's in a better position than you are to reconcile the conflicting information?" And that's the kind of stroking that any professional, not just scientists, [responds to]. "Who's in a better position than you to reconcile divergent information in your field of specialization?"

Q: And you never heard the bomb ticking?

A: We kept doing it. Because at the end of the day it has to be done. You can't come out and say, "The TAC is going to be somewhere between 150 and 250 thousand tonnes and we're going to watch and see how it goes and tell you half way through the season where we want to end." You just can't manage the resource that way. They need an answer.

Q: Is this a symptom of the long-standing position of privilege and authority that science has been granted and enjoyed?

A: I wouldn't put it that strongly. Again, very pragmatic people had been burned a few times. For a couple of years they said, "Here's the confidence interval that the answer lies within." And you give that to people who aren't used to dealing with confidence intervals, try to explain to them what a confidence interval is, and they say, "Oh! That means that I can take the number at the top!"

After two or three years of getting burned that way, the elder statesmen of the discipline developed the *principle that if you give them a choice, they're always going to take the most optimistic interpretation they can. So unless we believe that the upper number really is as good as the mid-point, you better give them the mid-point. And that was the reasoning behind it--certainly going back to 1982 when I joined the Department.*

At no time, then or later, did I feel that the scientists were deluding themselves about how accurate their results were. It's just that they didn't trust anybody further on in the process to take a range of options as anything other than an invitation to take the most optimistic one. And I think that fear on their part was reasonably well-founded. But in doing so, they really set themselves up.

Q: So through various pressures, some of them externally imposed and some of them internally imposed, you came to deliver your advice in a form that gave an illusion of precision that was not warranted and that you knew was not warranted. But you felt that both for the good of the resource, and for your own personal and professional reasons, that this was the best of several choices.

A: Yes.[15]

I asked J.J. Maguire a similar set of questions. He confirmed Rice's reconstruction of events and motivations.

Q: Do you think that Science itself had any responsibility in creating that expectation of precision?

A: We created it ourselves, to a point. With the help of fisheries managers. That, of course, is my biased perception.

I wasn't there so I don't know if it was an open demand, if it was implicit, if we obliged, but my guess is that we were being offered a very gratifying and important role. "Here's your role. What we have to do is very, very complicated. So please don't make it more complicated by saying that the TAC that you're proposing is not precise." It could be anywhere from

150 to 300 instead of being 200. "Don't say that, please. Help. Help. Say just one number." And I think we obliged.

And, as I said earlier, maybe we....I know that when I was doing the assessments way back, I thought that our precision was maybe plus or minus ten per cent. Maybe a little bit better. So we obliged. And we did not come out and say "This is not very precise. This is between this and there." There were other reasons for that, one of them being our perceived role as protector of the resource. If we gave a range we knew that the upper end of the range would be chosen. So we didn't know, at the time, how to present it and still have people go with the mean. Instead of going with one extreme of the range.

I think we did, yes, play a role in those greater expectations. But there were always caveats that were not acknowledged.[16]

What we are seeing in the above passages is a reconstruction of the social negotiations between science and the state that were to determine the language of expression of stock assessments and resource projections. The scientists now claim that they were well-aware that the language favoured by the state gave a misleading sense of certainty and precision to the assessments and projections. They admit, however, that for the critical period of 1977-'89, they were persuaded to accept this language by a combination of threats and flattery from the dominant political forces in the DFO bureaucracy.

It is also worth noting here that--while the scientists were quite willing to accept the offshore fishery as a primary source of raw assessment data--they were simultaneously convinced that the pursuit of rational corporate goals--profits--was not compatible with the rational scientific goal--a healthy, rebuilding stock. Although not specifically addressed in the foregoing interview excerpts, the Science Branch was worried that the corporate fishery would be able to exploit any apparent ambiguity in the stock assessments through the exercise of its political power--power based upon personal relationships between the top corporate executives and the dominant political figures at the apex of the state bureaucratic hierarchy. This concern was not without substance as is shown by Fisheries Products International President Vic Young's ability to secure an additional 6,000 mt of northern cod for his company in the 1990 quota. [See Chapter Six, Endnote 3 for details and references.]

The CAFSAC Advisory Process: Artificial Closure of Open Debates

The mechanism for distilling uncertainty and reconciling differences of scientific interpretation of indeterminate data is the CAFSAC advisory committee. The CAFSAC meetings can be likened to a cognitive foundry where individual, often disparate, constructions of reality are smelted, alloyed, and the final, authoritative, construction of northern cod reality is cast. Its existence and its workings are an expression of the state's and corporate structures' demand for regular inputs of unambiguous knowledge that can plausibly be construed as "objective and scientific" legitimation of political and corporate policy and practice.

As we will see below, an understanding of the actual dynamics of the CAFSAC process shows it to be more a forum for projecting the political interests of the state into the scientific construction of reality than the other way around.

In the preceding chapters we have seen that the data and methodologies of DFO's stock assessment science permitted an unusually high degree of interpretive flexibility and that this opportunity was consistently exploited to describe the condition of the stock in the most optimistic way possible. The VPA methodology regularly generated retrospective descriptions of fishing mortality and stock size that were considerably higher and lower, respectively, than the original descriptions of any given year's assessment (see Figs. 4.1 and 4.2). The CAFSAC process, however, continued to recommend quotas ostensibly in accordance with the $F_{0.1}$ management principal (which was thought to permit substantial growth) but based upon *current-year* calculations of biomass.

This failure to link retrospective knowledge with current knowledge permitted an operational description of the stock that reflected institutional expectations, projections, and commitments. The dependent $F_{0.1}$ TAC for northern cod remained relatively stable at a relatively high level, irrespective of the fact that retrospective analysis invariably concluded that the TAC for any given year resulted in fishing mortalities roughly twice as high as had been intended.

One avenue to understanding this curious disassociation of current assessments and advice from retrospective knowledge is afforded by an examination of the internal dynamics of the CAFSAC process. Dr. Ram Myers is a specialist in survey and assessment methodology. He offers a blunt description and evaluation of the process.

A: ... what went wrong with the process, why the mistakes were made, was this exclusive attitude to examining the data. That and some sociological reasons. The group dynamics of the process. It's very unscientific. Not in terms of the mathematics. Well, it's unscientific from my point of view.

There's a group of people that gets together and they meet continuously. And in order to make progress at these meetings, you have to accept certain things in common. Otherwise you'd be arguing about every point. This is simply the way the process worked. It almost has to because these are human beings. It's one thing to talk about perfect people but they aren't. And there are certain things that are inherent in the process of having a group of people examining things like a small society. And within that group, there are people who are very much opposed to something in the stock assessments.

One of the fundamental things to realize is that the Canadian system works by putting together a group of scientists at different levels. You try to shelter them from outside interest groups. And they try to come up with an independent decision. This process probably works better than any other process I can think of. Not that mistakes aren't made. The only interest is in people who've said something and they want what they've said to be true. It's a decision-making process without advocates, in the traditional sense.

Q: But certainly it generates advocates internally?

A: Yes. But when the quotas were generated pre-CAFSAC, when it was the old ICNAF system, there'd be different national groups arguing for different things--advocacy groups. As opposed to that, you've got a group as much as possible shielded from the outside forces.

Q: I seem to recall a note of warning in the '82 or '83 CAFSAC report...

A: George Winters?

Q: And then that voice disappeared until the '87 assessment.

A: No. That's not true. It didn't disappear. It simply...Remember, it's a consensus process. Unless you're willing to go to meetings and just slug it out....The meetings aren't over until they've come to a consensus. A decision has to be made. There's no such thing as saying, "I don't know." This is a process where saying, "There isn't enough information," is not acceptable. Decisions always have to be made. And consistently, abundance was overestimated and fishing mortality was underestimated for years and years and years.

Q: But don't most other scientific debates get resolved in a consensual way? They are debated in the journals and at meetings and the eventual resolution is a matter of consensus.

A: Not necessarily. You can have issues where a consensus has not been reached for fifty years!

Q: Ah yes. There doesn't have to be an answer tomorrow.

A: Yes. That's the big difference. For instance, interpretations of quantum mechanics. No one doubts the basic formulations but there is not really a consensus in terms of the interpretation. [At CAFSAC] A decision has to be made. A number has to be put forward. "I don't know," isn't an answer. And the person who waits longest, the person who believes strongest and is willing to stay out of town in a hotel the longest, is the one....So it's not even a consensus, it's....

Q: A war of attrition?

A: Almost. And most people are not willing to stand up and have a lot of people telling them that they are wrong. They won't do that. I don't usually go to these assessment meetings because I don't like them. I don't like the process because I get incredibly aggressive.

Q: They are probably just as happy if you don't go.

A: That's true![17]

Once Burned, Twice Shy: The Scientific Response to Political Exploitation

Yet another reason for science tc have accommodated the executive bureaucracy's demands for certified, unequivocal knowledge can be found in its dependency on the state as its sole source of operating resources and authority. It is reasonable to suppose that the state would not long continue to sanction and support the activities of DFO Science were it not responsive to the needs and demands of its patron.

What happens then when--by accommodating its patron's demands for knowledge expressed with misleading and unwarranted precision--science and scientists suddenly find themselves in the glare of the national media's spotlights being charged with gross incompetence? The answer is that science becomes a great deal more explicit in clearly communicating the uncertainties inherent in its assessments and the fact that the quotas are not set by science but are, in fact, a decision of the political bureaucracy in which the scientific advice is but one of many (and not necessarily the most important) inputs.

Coady, Rice, and Maguire all addressed this point:

<u>Larry Coady</u>: *This year, for the first time, we're saying, "We'll give you a range of options." Let's assume that this year the recruitment is high or low. Under each of those scenarios if you have a fishing mortality of 20 per cent, this is what's going to happen to the stock in the long-term. It's going to increase. If you have a 30 per cent mortality it's going to stay the same. If you have a 40 per cent mortality it's going to decrease.*

Q: So for the first time you're providing your advice in a way that makes it clear that the choices made are management's choice and not Science's choice.

A: There will always be some uncertainty attached to the advice we provide.

It ain't easy counting fish. It ain't easy and it never will be. And yet weather forecasters would probably find our track record enviable. The Economic Council of Canada would find our track record enviable.[18]

<u>Jake Rice</u>: *When J.J. Maguire had his big presentation of the assessment back in May [1990], the message he kept stressing was, "We're not going to say whether fishing mortality is .47 or .52 or .57. Relatively small nuances of a number of things can influence that bottom line. What we will say is that we are damn sure that fishing mortality is way above our target and we need to lower it. And how much we need to lower it and*

how we go about lowering it are decisions that we are consulting with you people on."

The stock is not going to collapse overnight if you keep fishing it at the same level it is. It'll collapse...you can't fish it at this level forever. But in the short-term, like 1990, 1991, the stock will survive and stay healthy and continue to reproduce itself, especially because there's some evidence of some good recruitment coming up. But the more you lower it [fishing mortality], the more it's going to rebuild.[19]

<u>J.J Maguire</u>: *Where I see it [stock assessment science] going is increased communication of the uncertainties in the assessment. Part of the reason for the shit we got was that people thought we were 100 per cent precise. So when they realized that there was a plus or minus 25 [per cent], at best, they think you're full of it. Really, you're not being very useful. So communicate the uncertainties. Be useful. Instead of being theological about what should be done, provide advice on what's feasible.*

Recognize and make it known that some of the variability in the assessments and the catch forecasts is essentially based on variability in the system. They're not real reflections of changes in stock size. They're essentially reflections of variability in the data. The difference between last year's assessment and this years' assessment is a very good example of that. Stock status is exactly the same between 1989 and 1990. Exactly the same. [Meaning, it's stable.] Except that we've done the assessment slightly differently. Which resulted in a difference of about 25 per cent [in total biomass].[20]

This tentative and highly qualitative approach to the subject does avoid the previous traps of illusory precision and unwarranted expectations but it is not without its own, unique dangers. Specifically, its knowledge content is essentially indistinguishable from the claims of the inshore fishermen that touched off the whole crisis by challenging the then-prevailing scientific constructions of increasing abundance supporting higher quotas for the offshore trawler fleet. Stripped to its bare essentials, all that science is now willing to claim is that too much northern cod is being caught and that quotas should be reduced to protect the resource--which is exactly what the inshore fishermen have been saying for some years.

Is this knowledge worth paying many tens of millions of dollars for? The following exchange with Sandy Sandeman addresses this point. His final line of defense, that fisheries science is no worse than economics as a guide to rational management, is not a strong argument for continued substantial levels of support from the public purse. In fact, it is noteworthy that many of my sources, when pushed hard on the subject of degrees of uncertainty and imprecision inherent in their work, resorted to a favourable comparison of their work with economics, weather forecasting or, in one instance, both.

> *Q: The bureaucratic structure of DFO was established in light of post-'77 expectations. That with Canadian control and good scientific management that the stocks could be rationally managed. That forecasts could be made.*
>
> *A: Not necessarily. It depends on what you mean by forecasts. Forecasts can be made....*

Q: That stability could be brought to the industry.

A: No! No one has ever, ever said that stability can be brought to the industry! I don't believe that.

Q: Not from the scientific point of view. But perhaps from....

A: Where you've got variable recruitment you can't have stability!

Q: Right! But I believe from my readings and research that this was the expectation from the corporate and political sectors....

A: I think that's probably true, yes.

Q: And a lot of the criticism that's coming from these to sectors now against Science is a result of them being disabused of this notion. Having to face facts. And they're saying, "We've spent millions and millions and millions of dollars on science which is of no apparent practical use for our needs. Our political needs or our corporate planning needs."

A: That, of course is the question. Because if you look at the stocks compared to '72 when this started, they're all way up! They've been built up! They're not continuing to be built up perhaps as well as we'd hoped....

Q: And yet these lads have just had their quotas slashed drastically as a result of what looks to them like scientific error! Screw ups! So they're not fishing for 450,000 metric tonnes this year [as predicted by DFO in the 1983 Kirby Report], they're fishing for 196,000 with the scientific advice saying that, "We got it so wrong that we think that you really should be fishing for only 125,000 metric tonnes this year." This is a shock to them. And it causes them to say, "If you boys can't get it any better than that, why should we keep forking over tens and hundreds of millions of dollars to you?" This is their perspective, not mine.

A: I can see their perspective, I must admit. I can see their perspective and it's a hard one to answer because they don't appreciate the fact that it's not an *exact science. That there are errors. But it's just as exact as any of their other economic forecasts.*[21]

The irony, of course, is that during the period when it is now thought that the errors were being made, 1977-1989, DFO Science was held in high regard--both domestically and, particularly, internationally--for its unusual effectiveness in rebuilding a devastated stock. For as long as the annual assessments reflected widely-shared expectations of a strongly growing stock and, most importantly, the dependent quotas reflected that growth, no one outside of the Science Branch, and few people within it, were inclined to inquire too closely as to the validity of the data sources and the robustness of the analytical methodologies.

Summary and Analysis

The focus of this chapter has been an empirically-grounded discussion of the macro-level social forces that have contributed to the social construction of scientific knowledge; specifically, the forces that are generated in the institutional collision between science and the state. As separate institutions, each has developed distinctive structures, values, norms, and traditions in pursuit of their institutional goals. Science seeks to defend or enhance its epistemological authority through pursuit of the "truth" while the state seeks to defend or enhance its socio-economic authority through pursuit of political power.

Here, however, we have a situation where science has become embedded in the state. The relationship is no longer one between free-standing, autonomous institutions but one where science is intended to be institutionally and functionally subordinate to the interests of the state. Science, as represented by the Science Branch of DFO, has in turn, attempted to preserve the integrity of its institutional culture and knowledge constructions while exploiting the substantial resources of the state. The strategies employed by both the state and science in pursuit of their respective goals have been both overt and covert. From this macro-level perspective we can see that an understanding of the conflicts inherent in this relationship is of considerable value in the construction of a comprehensive analysis of the process and product of scientific stock assessments for the period under study.

ENDNOTES

1. From an address delivered by Dr. Leslie Harris at the Graduate House in St. John's, Newfoundland on February 20, 1990

2. John Crosbie, the Canadian government's Minister of Trade, is also Newfounland's only representative in the federal Cabinet. Although he has no official standing with the Ministry of Fisheries and Oceans, as a long-standing power in Canadian politics, he wields considerable influence as we saw when he successfully pressured DFO to increase the quota for the offshore trawler companies in return for an apparent promise not to close a few fish processing plants in his electoral district. [See Chapter Six, Endnote 3]

The following is from an editorial in the St. John's weekly newspaper, *The Sunday Express*, December 31, 1989.

> *" . . . John Crosbie, without a doubt, has reason to view scientific briefs with a sceptic's eye. After all, officials at the Department of Fisheries and Oceans raised alarms about the health of the northern cod and urged Ottawa to lower the TAC [quota] to 125,000 tonnes only 12 months after they assured cabinet the hardy stock could sustain an annual catch of 295,000 tonnes.*
>
> *" . . . the minister insisted, 'I'm not a believer that we must slavishly follow the opinions of marine biologists.'*
>
> *"That's when the story of the old minister and the sea took a disturbing turn.*
>
> *"The minister shunned the best scientific advice federal dollars could buy. And, armed with little more than gut instincts, John Crosbie decided to tackle the mysteries of the deep blue sea alone.*
>
> *"Ottawa, he revealed, would not be strongly influenced by scientific advice when determining the total allowable catch of northern cod in 1990. Instead, John Crosbie and the federal cabinet would establish a quota that would serve Newfoundland's best interests."*

3. *The Evening Telegram*, St. John's, Nfld., Feb. 16, 1990

The Sunday Express, St. John's, Nfld., Dec. 31, 1989; Feb. 11, 16, 1990; April 22, 1990

The Globe and *Mail*, Toronto, April 11, 1989

[See also Endnote 2 for a specific example.]

4. Multiple sources including the terms of reference of the Northern Cod Review Panel [Harris 1990] and letters dated Feb. 13, March 6, and April 6, 1990 to former and present federal ministers of fisheries Siddon and Valcourt from The Professional Institute of the Public Service of Canada, representing scientists in public employment.

5. From an interview with Brian Morrissey conducted in Ottawa on November 2, 1990. The full transcript is Appendix L.

6. From an interview with Sandy Sandeman conducted September, 1990 in St. John's. The full transcript is Appendix O.

7. From an interview [not recorded] with Mac Mercer, Director of the Science Branch, at DFO's research centre in St. John's, Newfoundland; February 16, 1990.

8. From an interview with Larry Coady conducted in St. John's on July 26, 1990. The full transcript is Appendix E.

9. From an interview with Jake Rice conducted in St. John's on August 14, 1990. The full transcript is Appendix I.

10. From an interview with Bernard Brown conducted in St. John's on August 3, 1990. The full transcript is Appendix B.

11. From an interview with Jim Roache conducted in St. John's on July 24, 1990. The full transcript is Appendix N.

12. From an interview with Larry Coady conducted in St. John's on July 26, 1990. The full transcript is Appendix E.

13. From an interview with Sandy Sandeman conducted in St. John's, September, 1990 The full transcript is Appendix O.

14. From an interview with J.J. Maguire conducted in St. John's on October 28, 1990. The full transcript is Appendix J.

15. From an interview with Jake Rice in St. John's on August 14, 1990. The full transcript is Appendix I.

16. From an interview with J.J. Maguire conducted in St. John's on October 28, 1990. The full transcript is Appendix J.

17. From an interview with Ram Myers conducted in St. John's on August 28, 1990. The full transcript is Appendix M.

18. From an interview with Larry Coady conducted in St. John's on July, 26, 1990. The full transcript is Appendix E.

19. From an interview with Jake Rice conducted in St. John's on August 14, 1990. The full transcript is Appendix I.

20. From a interview with J.J. Maguire conducted in St. John's on October 28, 1990. The full transcript is Appendix J.

21. From an interview with Sandy Sandeman conducted in St. John's, September, 1990. The full transcript is Appendix O.

CHAPTER EIGHT

SUMMARY AND ANALYSIS

In the preceding chapters, I have worked within the frame of a very specific problem area--the science of northern cod stock assessment from 1977 to 1989. Now, in this concluding chapter, I would like to generalize the issues somewhat--to recapitulate my synthesis of the specific issues under study in a way that may be taken up by others and applied to other problems of state-sponsored, scientifically-mediated interactions between societies and the natural world. Finally, I will also have a few words to say about lessons learned and opinions formed regarding what I call meta-methodology--that indeterminate region between theory and the empirical ground--where the researcher negotiates the construction of reality.

Prior to doing so, it may be well to briefly review the ground we have covered to date. Structurally, the work has been organized as follows: Chapters One through Four defined the problem and established the factual and conceptual context within which I would present the data and argue my analysis. The analysis formed the substance of Chapters Five through Seven which carried the discussion through progressively higher and more inclusive levels of social organization. In the opening chapter I claimed that

> *"...this latest crisis (and the "success" and/or "failure" of stock assessment science) is better understood as a product of multi-levelled and interactive social forces and processes rather than as the ability or inability of science to objectively and accurately understand, describe and predict the dynamics of external natural reality."* [p. 17]

Next, I discussed the strengths and limitations of my primary theoretical perspective--social constructivism--and introduced several supplementary theoretical concepts which I presented as being essential to my construction of a comprehensive description and satisfactory analysis of the empirical reality that had emerged from my research.

Chapter Three presented evidence that established the development of strong commitments at all levels of social organization--from individual scientists to the Canadian state--to the idea of a rebuilt, rationally managed northern cod stock. I located this development within the context of international negotiations at the Third Law of the Sea Convention.

To pursue the argument further, it became necessary that the reader have a basic understanding of the technical content of stock assessments. This permitted the introduction of the concept of "interpretive flexibility." I showed that, for the period from 1977 to 1989, the errors and uncertainties inherent in scientific stock assessments permitted an unusually high degree of interpretive flexibility in the construction of both current-year stock assessments and resource projections. Further, through a close analysis of documentary evidence--much of it originating from within DFO itself--I established that this interpretive flexibility was consistently exploited to produce assessments that are better understood as expressions of pre-existing commitments and expectations rather than useful descriptions of natural reality.

A second and concurrent theme of this chapter was the initial appearance of a direct challenge from the inshore fishery to DFO's claims. Through an analysis of the reports of a series of commissions of enquiry, we saw a protracted re-negotiation of reality. This began with the DFO's flat dismissal of the validity of the claims of the inshore fishermen as expressed in the Keats Report and progressed to the point where the perceptions of stock status held by the inshore fishery, DFO Science, and the Harris Report were broadly congruent. And yet, the general agreement on stock status only served to reveal that the real crisis was not biological but a crisis of epistemological legitimacy and institutional authority:

> *"...the institutional and political authority of the federal government, the epistemological and professional authority of science and scientists, the cultural authority of the inshore fishery, and the struggle for legitimation of each of their respective, conflicting cognitive orders and constructions of reality."* [p. 125]

By Chapter Five we were prepared to enter into an exploration of the social construction of the cognitive reality of the scientists themselves. I showed how micro-level social dynamics could generate forces with highly consequential macro-level effects. I traced the origins of one particular conflict between two small groups of scientists to the incompatibility between their institutional mandate and their professional reward and promotion structure. By extending this analysis beyond that specific conflict, I argued that this could plausibly account for the curious but persistent failure of the Science Branch to produce knowledge of practical utility to its mandated clients; the state and the commercial fishery.

This explanation, on its own, was not wholly satisfactory. For instance, it failed to address the sharp distinctions that scientists made between the inshore and offshore sectors of the fishery and their respective status as sources of valid data. In Chapter Six I took up this problem and argued that the distinction was not grounded in a rational evaluation of the relative intrinsic merits of the data--as claimed by most scientists--but, in fact, reflected a reasonably well-founded fear on the part of science and scientists that they were facing an unprecedented challenge from the inshore fishery to their institutional integrity and epistemological authority.

Finally, in Chapter Seven, I raised the level of analysis to the broadest view of the problem and identified the embeddedness of science in the state as a source of powerfully influential social forces that could be detected as affecting knowledge construction at all levels of social organization. I suggested that the conflicts inherent in this relationship were the ultimate source of the dysfunctional dynamics that we had observed at work in earlier chapters.

In The Final Analysis

The political institution of federal government operating through the Department of Fisheries and Oceans is the sole source of the Science Branch's funding and functional authority. However, the Science Branch's sole raison d'etre within that political institution is precisely due to the epistemological authority derived from the putative independence of its knowledge constructions from that institution and allegiance to the institution of science.

We have seen that the historical development of Canadian fishery resource management policy and process has been shaped by its attempts to incorporate and integrate the cognitive contexts of two institutions with conflicting and contradictory norms. The intention was undoubtedly to use dispassionate, objective scientific knowledge to balance the social, political, and economic inputs to issues of resource exploitation and management. However, to date, this effort has not been notably successful.

Instead of achieving the desired balance, it seems that the result of the interdependence of these two institutions has been to create a perpetual conflict. Each has a kind of power that the other wants. The political-bureaucratic sector of the federal government possesses budgetary and legislative power by virtue of its control of the mechanisms of the state. The Science Branch possesses legitimating power by virtue of its association with the institution of science which, for most practical purposes, still sits atop the epistemological hierarchy.

The state's power created and enables the activities of the Science Branch. The Science Branch's power derives from its perceived ability to generate certified knowledge to legitimate the fisheries policies of the state. The problematic aspects of this relationship are resident in the fact that the two institutions have evolved highly incompatible cultures that operate in the context of disparate cognitive models of the social and natural worlds. Further, the evaluation of the two institutions' performance takes place within disparate evidential contexts.

The state's performance is evaluated by the polity within a very mundane, practical context. The continuance in power of the ruling party, and the careers of individual politicians, are weighed in the balance. As a consequence, what counts as valid knowledge in this context is also mundane, practical and, above all, non-controversial. The closer that a knowledge claim approaches the status of an incontrovertible fact, the better. The state, despite high-minded election-year claims to the contrary, has no rational self-interest in supporting the academic pursuit of knowledge for its own sake; at least not within its own organic entities. Because its performance is evaluated within this evidential context, the state tends to evaluate that of the Science Branch in the same terms.

The Science Branch, however, derives its epistemological power from its evaluation within the international community of fishery scientists. The norms and traditions of science form the evidential context. The reward and promotion of individual DFO scientists is adjudicated within this same evidential context. Work that is considered to be mundane, practical and unproblematic (such as stock assessments were seen to be until 1989) is labelled "trivial" and not highly valued. Conversely, work on problems that are not well-understood, containing significant lacunae, is referred to as "interesting" and is the path to enhanced status and material rewards within the scientific community.

There is yet another very real problem in the relationship between the state and its sponsored science that is the source of a profound ambivalence between the two institutions. This arises from the fact that the state's fisheries policy derives its credibility and legitimacy within its own critical evidential context by appearing to be in close association with science--but science derives its credibility and legitimacy by appearing to be disassociated from the state. The result is a truly bizarre relationship; one that has persisted in Canada, with periodic ruptures and reconciliations, for over 100 years. The Science Branch can only function in the state's interests to the degree it is successful in preserving its scientific credibility. However, the state will only be willing to function in the interests of the Science Branch to the degree it finds the knowledge production to be of practical value in achieving its political objectives.

Beyond Stock Assessments: Empiricising Theory

At the highest level of analysis, this work has been an attempt to build a dynamic, interactive bridge between empirical reality and theory. Useful analogies can be drawn between the stock assessment models of the Science Branch and social theory. Both are simplified abstractions of reality. As such they can be powerful tools for distilling and conceptualizing the essence of reality. However--as in the case of Ptolemaic cosmology and DFO's pre-1989 assessment models--they can also embody distorting beliefs about the nature of reality. Bodies of social theory, no more or less than stock assessment models, are manifestations of bedrock paradigms--superordinate world-views--and, as such, can be profoundly determinant of the results of their application.

It is a common characteristic of both theory and models that they are constructed on a foundation of a priori assumptions. Typically, theory is applied to empirical reality. The paradigmatic core of a theory can carry a powerfully deterministic inertia so that the results of such an application tend more to confirm the theory rather than clearly illuminate the empirical problem. Perhaps it is also necessary to occasionally reverse this order--to apply empirical reality to theory as a test of a theory's cognitive foundations.

I suggest that empirical research can be more interesting and its results more fruitful if we, as nearly as possible, first approach our ground with an empty tool kit. Leave behind our theoretical perspectives, analytical frameworks, cognitive categories. Borrow from anthropology--with deep gratitude--the ethnographic technique. Instead of us trying to make sense of the natives, let them make sense of themselves to us. Grant their cognitive and epistemological reality the same validity that we grant our own. Only then should we return to our familiar world and begin a careful process of fitting our data to our theories. Much of it may not fit our favourite perspective. Some of it may not fit any theory at all. We may find, as I did, that we have to disassemble several theories and, from their bits and pieces, rebuild a new construct uniquely suited to the empirical experience.

In conducting the research for this work, I found that there was no one "off the rack" theoretical perspective from which I could adequately and plausibly account for the empirical totality of my data, experiences, and impressions. This made my job somewhat more difficult but it also made it a great deal more interesting and enabled me to contribute a distinctive analysis toward the understanding and resolution of a problem of vital importance to the people of Newfoundland and Labrador.

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APPENDIX A

A BRIEF HISTORY OF GOVERNMENT-SPONSORED FISHERIES SCIENCE IN CANADA

The following review of the historical relationship between the federal government and fisheries science is derived largely from *"The Aquatic Explorers: A History of the Fisheries Research Board of Canada"* by Kenneth Johnstone [1977].

As alluded to in the introduction, Johnstone notes the interconnectedness of the northwest Atlantic fishery and the political and economic histories of the Canadian, US, and western European countries; and since the 1960s, of the USSR, Poland, and--until its re-unification, East German as well. He dates the beginning of this relationship as 1497, the year John Cabot returned to England from an exploration of coast of what is now the Province of Newfoundland and Labrador with his famous report stating that *"The sea there is full of fish to such a point that one takes them not only by means of a net but also with baskets to which one attaches a stone to sink them in the water."* [Johnstone 1977 p.4]

The initial awareness of the need for some measure of federal management of marine resources is credited to Pierre Fortin, a McGill-trained physician who, as Canada's first fisheries enforcement officer (1852-67), conducted the first systematic, scientific study and reporting of the state of the east coast fishery. Fortin noted general abundance in all fisheries but also occasional failures. Fortin's 1856 report correctly predicted the collapse of the whale fishery due to the introduction of harpoon guns and compared the situation of whales to that of the walrus which, once abundant in the region, had been entirely wiped out. He later recommended regulation of mesh size and other measures to protect salmon which were even then in danger of extermination.

Peter Mitchell, Canada's first minister of marine and fisheries, proposed in 1868 that the fisheries be formally rationalized and regulated under Sate authority. The Fisheries Act of that year included conservation measures such as closed seasons, licensing, closed areas, and prohibition of pollution of the fishing grounds.

Johnstone quotes from an article by James Playfair McMurrich in the University of Guelph publication *"The Week"*. After a formal nod to the practical advantages of the application of science to the fisheries (protection and development) he makes a case that science is important for its own sake.

> *"Apart, however, from the practical value the establishment of such departments would have, the scientific importance of their work should not be overlooked. Generalizations of which at present we have not he slightest inkling, might be arrived at; all departments of science would receive encouragement; a new stimulus to science would be aroused in our country and the present ban under which science lies would be removed.*
>
> *"But in this search for practical discoveries let not pure science be neglected. Though apparently valueless at the time, it will yield abundant fruit in the future, not only by becoming in its turn capable of direct application, but also by establishing a starting point where new investigation may branch out in the yet undiscovered realms."* [Johnstone 1977 pp. 24-25]

McMurrich's call was joined by the Rev. Moses Harvey, secretary of the Newfoundland Fisheries Commission, in a paper presented to the Royal Society of Canada in 1892.

> *" . . . the writer wishes to point out the desirability of establishing a Biological Station for the study of Ichthyology and Marine Biology in all their branches . . . . The scientific and practical should be so combined to render it a Fishery School . . . .*
>
> *"The interests of pure biology, as a science, would be served by such an institution . . . . If we want to increase the qualities of our food fishes, our lobsters and oysters, all our operations must rest on a scientific foundation, and all our regulations of our fisheries must have their basis in a scientific study of fish-life. Failing such accurate knowledge, our legislation regarding the fisheries will be largely groping in the dark; and all efforts for their preservation and improvement will come short of the objects aimed at."* [ibid p. 25]

Dr. E.E. Prince, a specialist in fish embryology from Glasgow, Scotland was appointed minister of fisheries in 1893. In his first annual report he wrote:

> *"There is a growing feeling in our country, which in so many respects has taken a leading place among the nations in regard to fishery matters . . . [that it] should take a position of equality with other countries in the furtherance of marine and freshwater biological research . . . . [these researches] all end in supremely practical results, and bear directly upon the welfare and prosperity of the great fishing industries . . . . Legislation has often been hazardous on account of this lack of ascertained fact and the existence of contradictory opinions. Primarily, a marine station would be a centre for investigation and research for the promotion and diffusion of knowledge. Without interfering with this first and most important work, such a station might also be a school for teaching and for scientific study. . . . ."* [ibid p. 26]

In May of 1895, Prof. A.P. Knight of Queen's University wrote to the secretary of the Royal Society suggesting that the Society officially approach the minister with a request to establish a research station and noted that Canadian marine biologists were travelling to Woods Hole and European stations to work.

> *"It seems too bad that her biologists should be compelled to expatriate themselves in order to gratify so harmless and ambition as that of adding a little to the sum of human knowledge."* [ibid p. 27]

Continued pressure was rewarded with the passage in Parliament of an act establishing a floating research station to be staffed by scientists on leave from their universities and administered by a special board consisting of a representative of the Dept. of Marine and Fisheries an representatives from all the supporting universities.

The first Board was chaired by Dr. Prince (Minister of Marine and Fisheries) with the other eight members all being distinguished academics. According to Johnstone they immediately recognized the need to establish their legitimacy in two separate, and possibly conflicting, evidential contexts--political and scientific. Funded with a one-time appropriation of $15,000--$5,000 for construction of the research station and $10,000 for five year's operating costs--there must certainly have been conflicts over the allocation of available resources to satisfy the demands of the two evidential contexts.

On one hand, they would have needed to justify this expenditure of public funds--and any further funding they may have hoped for--by the production of results that were seen as useful by the federal political institution. On the other hand, as eminent scientists, they would have also felt the need to conduct research that would command the interest and respect of their academic peers. This pressure to satisfy the demands of disparate evaluative criteria would be instantly recognizable to the scientists working for DFO nearly 100 years later.

> *"As it prepared to launch its investigations into the fisheries of Canada, the Board was faced from the start with two major tasks: it had to prove its value to the Canadian government as an instrument of research in aid of the Canadian fisheries, and it had to prove to the scientific community that it could operate a valuable laboratory for biological and fisheries research . . . .*
>
> *"Prince made himself chief propagandist with the government for the work of the Board . . . and he performed a similar role with the Royal Society . . . . But it was the scientific papers that proceeded to flow from the summers at the movable station that persuaded the scientific community that it was a valid and important instrument in the development of the science of ichthyology. Similarly, many of the subjects of the papers were matters of practical importance dealing with problems that faced the Canadian fishing industry and thereby justified the enterprise in the eyes of Parliament and successive administrations."* [ibid p. 30]

Johnstone notes that the choice of the first site for the station was a subject of conflict. The Board chose St. Andrews, New Brunswick for reasons of scientific interest while the auditor general objected on the grounds that the Parliamentary appropriation was for a station on the Gulf of St. Lawrence. The Deputy Minister of Fisheries replied that, since it was a floating station, it could be towed anywhere in the Gulf. Dr. Prince, reporting to the Royal Society, justified the St. Andrews site in terms of scientific priorities. In understanding the early resolutions of conflicts in favour of science, it is important to know that Prince personally conducted research at the station as well as being Minister of Marine and Fisheries.

In 1902, the Board approached the government for a 50 per cent increase in the annual allocation of operating funds and additional money for a number of substantial capital expenditures. Further, they proposed that these allocations be turned over to the Board en bloc to be dispensed as deemed necessary. The available evidence suggests that the research programs conducted through 1904 were designed by Prince and the Board to impress the government with the practical value of the station and justify requests for more liberal allocations of funds.

The choice of a site for the first permanent station was subject to the same sorts of pressures as the mobile station. While St. Andrews was again the scientists' choice, they created the appearance of evaluating several other possible sites before drawing up a lengthy justification for St. Andrews. A perusal of research conducted in following years shows increasing emphasis on programs of scientific interest as opposed to practical or commercial value.

In discussing the conflicts between the Board (all were scientists and only one, Prince, was a govt. employee) and their political masters, Johnstone says

> *"The two objectives which both boards undertook to achieve, one of independent aquatic research and the other of providing answers to the practical problems of the fisheries, required that they do a nice balancing act, with the pole tilted now one way, now the other.....In their own university departments the members of the board were laws unto themselves, respected for their scholarship and achievements, and not at all prepared to have their decisions reviewed by 'bureaucrats' unfamiliar with biological matters....[but] They understood very well that the government would expect to see some tangible results from the sum that it was spending, modest though it was." [ibid p.72]*

> *"From the beginning, Prince and his colleagues on the Board insisted that pure and applied science went hand-in-hand: that there could be no valid applied science without the basic knowledge furnished by a total study of the environment. This view was to be*

repeatedly challenged over the succeeding years, but it was never abandoned by the Board." [ibid p. 74]

Although the 1912 Act of Parliament that replaced the original Board of Directors with the Biological Board gave the new Board a great deal more financial and program autonomy, the work began to move more in the direction of practical and applied science.

Through the early 'twenties, the work became increasingly applied with extensive work on lobster farming and the solving of technical problems in the processing of different species for the market. Cold storage and freezing technology was developed that would later fundamentally reshape the industry.

There was a significant turn of events in 1924 when the Department succeeded in staffing the new fisheries technology research station in Halifax under civil service rules so that all its scientific staff were federal employees as opposed to Board-sponsored researchers who were volunteers from various universities. Through the Second World War and up until at least 1947, the Halifax station focused nearly exclusively on applied science such as product processing and handling technology and methods.

Following the end of WWII, the rapid development of a mechanized offshore fishery made obvious the need for an international forum to control the potential for the overfishing of commercially valuable stocks. Rather quickly it became necessary for Canada to develop a much broader and deeper expertise in marine sciences in support of its anticipated role in the creation of the organization which was, in 1949, to become the International Commission for Northwest Atlantic Fisheries (ICNAF).

Two points are of importance. The first is that the limit of Canada's authority extended only twelve miles from shore. The second is that the furtherance of Canada's interests in these negotiations was seen to depend on the degree to which it could support its arguments from a position of scientific authority. In this, the situation much resembles that of 30 years later when Canada made its case during the Third Law of the Sea Convention for the extension of its exclusive jurisdiction to 200 miles.

The objective of establishing the international prestige of Canadian fisheries science justified greatly expanded scientific research activity. This fitted well with the interests of the burgeoning Canadian fishing industry which saw a useful role for science in enhancing its productivity for domestic consumption and competitive position in foreign markets. In 1972, Alfred Needler, a former member of the Board, recalled this period.

"I think you might say from the years 1945 to about 1960, and even to 1963 and 1964, research was the magic word in government finance. Research, on the whole, received more assistance than anything else. It was allowed a higher rate of increase . . . . The activity [at St. Andrews] expanded very quickly from a budget of $55,000 or so in 1941 to I suppose over half a million in 1954, maybe more than that. I don't recall exactly. That was one thing.

"There was a tendency in the early days of the Board's history for industry to be very sceptical of the value of any research being done . . . . People had the feeling that they were doing research but it wasn't being appreciated, or it wasn't being applied, and while they believed in what they were doing, they felt a feeling of frustration. Well, sometime during this period . . . the balance swung the other way, so that industry in the early 1950s was wanting more things to be done than the Board was able to do . . . . This was really quite a definite change." [ibid p. 185]

In 1971 the Trudeau government created the Department of the Environment which was to oversee all natural resource management activities. Chairman of the Fisheries Board, J.R. Weir was clearly concerned about the potential of political objectives to dominate and distort scientific activity as the State increased its structural and financial control of fisheries science. He wrote:

"There is an ever-present danger that public policies and goals may be guided in the future by the most pragmatic and expedient calculations. Scientists have been increasingly subjected to public criticism for being too abstract . . . .

In the absence of any accepted mechanistic approach to the setting of goals and priorities, and to evaluate research, forums must be sought to use this expertise to capacity, and to bring them in concert with information users. Provided that objectives are in harmony with national goals, it is imperative that project managers and scientists on site have freedom to plan and execute their project operations so that their *resourcefulness will not be constrained."* [ibid pp. 299-300]

The Trudeau government actively pursued a policy of centralization of authority and, in 1973, control and operation of fisheries research was finally removed from the Board and given to the Department of the Environment. Former chairman Hayes construed this change as deriving from the logic of bureaucratic rationality when he wrote:

> *"In a late 1972 restructuring of Environment, the FRB lost its independent status and was brought into line authority, reporting to the new assistant deputy minister for marine and fisheries . . . .* **The government simply cannot contemplate the control of policy and funds by any but its own employees.**" [ibid p. 307 emphasis added]

APPENDIX B

Interview with Bernard (Bern) Brown, DFO Information Officer Conducted in St. John's, Newfoundland August 3, 1990

A: ...looking back on it now, their report was almost the worst thing that could have happened given what we know about the state of the northern cod stock. Because the damn Alverson report basically gave our fisheries science a clean bill of health.

Our scientists were saying that since '77 the northern cod stock had increased five-and-a-half fold. And they were saying all sorts of other things around that basic central fact. So our scientist were saying that our fisheries science effort and our fisheries management effort, based on our science effort, has been a rip roaring success. Where else on the face of the earth have we gone from a situation like we had in the late 'sixties and early 'seventies where we bloody near wiped out the stock, to a point where we now have this huge stock of fish out there. And essentially they were telling the inshore fishermen who were creating all the uproar about the destruction of the stocks, that you don't know what you're talking about. The Alverson Commission confirmed virtually all of that.

They said, "Well, you're a little bit out on your calculation of how much the stock has grown since '77. You say it's grown about five and a half fold. We think it's only grown about five fold." Well, shit! That's a quibble, right? And it had a few other quibbles about our methodology. But whatever criticisms that were in the Alverson report at least opened criticism or disagreement with our fisheries scientists. But they were not great big substantial problems. They were little matters of adjustment here and there.

Now to anyone who read the report closely and read it with an open mind from the point of view of, maybe our critics have got a point--if you read the Alverson report carefully from that point of view, I think the signs were in there that the problems were worse than stated in the report. First of all the report was a horror as a piece of writing. It's interesting to read the Keats report by some people at MUN [the Memorial University of Newfoundland] which was done at the request of Cabot Martin and his people. You [the interviewer] read that...And that was part and parcel of the whole effort that went on for a year or so of criticising DFO science that eventually led up to the appointment of the Alverson group to review our scientific effort.

That thing done by Keats really set off a little firestorm of criticism. Our scientists ridiculed it--who are these people? They aren't fisheries scientists. They don't know fuck all--they were absolutely ridiculed. My own feeling is that they did a neat little piece of work. Real neat.

Then you read the Alverson report. As I said, a real horror. God what a struggle trying to read it. And I got the sense, that while Alverson was asked to go and do an objective evaluation of DFO fisheries science, he was most reluctant to come out and be critical. So I have a feeling, and this is purely a feeling based on the tone of the thing and so on, that he could have been, had he been willing, a good deal more critical of our scientific effort. But having said that, he did say flat out that basically DFO scientists are doing a damn good job.

What did our people do with that? They ignored even the quibbles that were in it. And I suppose that they ignored them because they were quibbles. They went out to the public and said, "Look! Alverson has confirmed that we're doing a damn fine job in science. They think we're out maybe five per cent on our estimate of the growth of the stock but basically Alverson is saying that we're great guys."

Well, this was a complete put-down of all the criticism that our scientists had been getting. Trouble was, over the next couple of years, the inshore fishery got even worse. So we end up a year and a half later with another independent review.

It would never have happened of course if the scientists, a year or so after Alverson, hadn't started to realize that their own numbers were wrong. And a good deal more wrong than Alverson was saying. In other words, they started to get a handle on the numbers for the first time since '77. That's what's happened in the last couple of years. Cod being a 7 to 10 year old fish, it takes a decade to get a handle on a stock in terms of assessing it. Granted, we've had fisheries science going on in this province for a long, long time but full-blown stock assessment has only been going on on the northern cod since about '77. So they're just starting to get a handle on it. Particularly with a little kick in the ass with all the criticism that forced them to be a little more careful in their research.

They came to realize a year or so ago that they were very seriously out. And as soon as that dawn started to break, the people in Ottawa reacted with another full-blown review of fisheries science. It's not funny for the poor bloody scientists. They've been crucified through all of this. Really quite unfairly when all is said and done. You can go and quibble at some of their behaviour, their arrogance in their belief in the correctness of their own knowledge. But they were really trying and, god damn it, they're only people and they have been left to hang out to dry.

But here comes the Harris panel and that's when the bottom fell out. By this time our people knew how badly they'd been out on the numbers and of course Harris went out and redoubled all the criticism from the fishermen and the industry generally. Took a good hard look at our science, using our own information.....It's like walking up to a baseball player, saying you can't hit the god damn ball, hitting him over the head with his own bat and saying, here's how you do it. And what made the Harris report so much more critical of our scientists was basically another year or two of findings by those same scientists.

So in a way Harris confirms that our scientists are doing good work but that good work is only beginning to bear fruit in terms of the correct assessment of fish stocks. Harris stated that plainly. It does take a decade or so to begin to get a handle on a cod stock. But the previous decade's work resulted in some very wrong numbers. Harris concluded that the stock had only grown by something like two and a half times. How did I get into all this?

Q: I asked you what DFO was like when you first joined the department [9 years ago].

A: DFO has always come in for a fair bit of criticism all the time. You're always arbitrating among these competing interests. Because you occasionally make a mistake and you are occasionally caught quite plainly making a decision for a political reason. But that's just the stuff that goes on in government.

It's a little bit more so in fisheries because people are so dependent in a fishing area. But it's only with this problem in the last three or so years that we've had this constant concentrated criticism. And of course it just feeds on itself. The Department reacted to criticism with the Alverson Report and then the Harris Report and now it's reacted again with the five-year 580 million dollar fisheries adjustment program. It just goes on and on. Naturally enough. The government couldn't just sit back and say, "Fuck 'em! Let them [fishermen and fishery-dependent communities] starve."

Q: The irony is that the northern cods stocks probably have increased significantly since '77. So the crisis is really due to unrealistic expectations based on previously faulty projections of the stock's increase.

A: I will buy the unrealistic expectations line as long as we realize where they came from. I think that there is an effort now by the government, without being too open about it, to pretend that the expectations of the fishery have always been unrealistic. Which is not the case. There were unrealistic expectations but both federal and provincial governments were more than willing to pour oil on that fire.

Q: When the Kirby report was published projecting TACs of 400,000 tonnes by '89, I assume they were relying on DFO data?

A: Madness! Of course that's when the province jumped on the bandwagon and licensed fish plants and boats left right and centre.

Q: So they created a fishery with an economic structure that depended on the availability of steadily increasing numbers of fish and when they didn't show up...

A: The province was very much to blame in all of that. The 200 mile limit. That's what started the bonanza attitude. It was El Dorado again. The Canadian offshore boys got into the fishery and started landing all the fish here. The processing industry went right through the roof. It was fabulous. For two or three years. And then of course we got a market down-turn and some currency value shifts and the bottom came out of her just as fast.

[break, next side of tape lost due to operator error.]

A: I think that a major pattern of mistakes that's been made at DFO is that our senior managers making decisions at the Ottawa level, have ignored certain things that they knew in their decision-making process. The senior people, and I'm talking at the Assistant Deputy Minister level and just above and just below, including the Minister, have always known, for example, that any given assessment in any given year of any given fish stock can be out easily by as much as fifty percent.

Q: And that's still the case?

A: Yes. That's still the case. It could easily be more. Now that's one hell of a level of variability. Politicians have to deal with the real world. They've got to deal with more than scientific calculations. Just the same, we would not be in the political pickle that we're in now if they'd taken a little more account of that sort of factor and been a little more conservative in their fisheries management.

Q: And been a little more brave and honest and said, "We really don't know...."

A: Speaking as someone in the communications business, a glorified PR hack, that's been their major bloody mistake. An unwillingness to come clean. And it's still our major mistake. But the politician, as an animal, that's the way he's bred. That's the kind of person that gets into politics and particularly that ends up being successful in politics. Getting up to the level of Minister of Fisheries. That's the nature of the beast. With rare exceptions.

We had a minister in the 'seventies and 'eighties, Romeo LeBlanc, and I remember Romeo from my time in the media primarily, who tended to be far more open. Also, another pattern of mistakes in Fisheries and Oceans over the last seven or eight years, particularly under the Mulroney government. And that may merely be coincidental. I'm not saying it's because it's a Tory government. We have not had anyone in that department with a basic philosophical grounding or approach to the damned industry. You always have this problem of management making decisions on things other than dollars and cents. Keeping Bung Hole Tickle alive. That sort of thing. We've not had any ministers who based their decisions on any philosophy, however vague.

By contrast, Romeo LeBlanc, who was there from about '74 or '76 up to about '82, had a philosophical grounding about the way he approached fisheries management from the minister's level. He was regarded, rightly, and still is today, as a fisherman's minister. That's not to say that he ignored the processors. Obviously you've got to have the processors to process the fish and get them to market. But his first concern was the poor bloody fishermen and the fishing communities. Now that doesn't mean that he was always right in his decisions but it does mean that there was some sort of consistency in the way he approached the management of the fishing industry. There was some predictability that people could work within. So Romeo had a rock to stand on and was less open to all the pressures. Everyone since has been more open to pressure because they didn't have a good solid rock to stand on. So whoever pushes the hardest, that's the direction they end up going in.

[long digression on the political process]

A: I find it hard to deal with this ambivalence there seems to be among the scientists. Because they did know damn well that the numbers that they were coming up with could be way out. Probably were way out a good deal of the time. And yet they were quite free in saying, we're doing a pretty good job here folks. I suppose they did feel, correctly that they were doing just about as good a job as could be done. And what's caught up to them is that no one was willing to go out and try to make it clear to the fishing industry and the public how much uncertainty lay in all the science. And the way in which it finally became clear was the worst way possible for them. Instead of that, instead of communicating something about the uncertainty, every confidence was expressed.

[further discussion of the political mechanisms that select the highest end of a suggested range for the TAC]

A: Look what's happening right now on the East coast, particularly on the Eastern Avalon. Jesus Christ! They're buried in fish! Listened to a guy from the Battery the other day. He hasn't seen the like of it in the 20-odd years he's been fishing. Had over a million pounds in July. On the other hand, I've been watching the fish coming ashore from the 65 foot otter trawlers, the guys that are going off the Virgin Rocks, and if there's one fish in a hundred that's longer than 18-22 inches, that's about it. In other words, they're getting a lot of small fish out in deeper water and that's not a good sign. Now, I'm talking like a fisherman, the kind of stuff that the scientists absolutely disparage.

Nevertheless, in all of this our inshore fishermen have been proved to be right. Unless our scientists are going to turn around a couple years from now and say, we were right after all, the stock did grow five-fold--which would destroy any shred of credibility that they have left. We were saying, "The stock has grown five-fold," and the fishermen were saying, "You're out of you mind." They were right. But I still don't see any evidence among scientists that they're any more prepared than they ever were to go out and listen to fishermen.

And it's apparently a matter of the difficulty of dealing with the kind of information and evidence that fishermen have, the so-called anecdotal stuff which you can't quantify very well and analyze very well. Certainly can't computerize very well. So you just don't want to deal with that kind of messy information. They won't even call it data, as a matter of fact.

Q: There's simply no cultural support or established mechanisms within science for incorporating traditional knowledge.

A: The department's trying to force it to a certain degree but I don't know how much of that's public relations work as opposed to a real effort.

Q: But even if there were a genuine interest in incorporating traditional forms of knowledge, it's difficult to see how they could be translated into the language of science, mathematics, or conceive of science learning to speak another language.

A: Yes. But that's only part of the problem. The other part is attitude. If the scientists really feel, as a lot of the do, that the fishermen have bugger all to offer....

[long discussion of the log book program to assess inshore effort. Brown notes that this is being conducted by the Statistics Branch, not the Science Branch and feels that this is a missed opportunity to get scientists and fishermen actually talking to each other.]

I think it's a real problem that the fishermen and the scientists operate in solation from each other [NOTE: when they do have personal contact, it is almost invariably in the context of conflict and antagonism] How the hell can some guy become credible to you if he's just some asshole out in a boat believing what his grandfather believed? If you're a scientist and you know the truth?

TAPE #2

A: You can go back to time immemorial. There have always been fishery failures. Sometimes localized to one bay, sometimes the entire East Coast, the South Coast, wherever The fish failed for a year or two or even three or four. It made for tough times. When it was bad enough government would step in with some little bit of assistance to help people stay alive. Not on today's scale. But it really didn't mean too much because people lived off the land and off the sea anyway.

But the important thing is that the people understood that it was a natural thing. The fish failed. They didn't understand why. They just understood that they did. But they knew that the failure would only last for so long. The fish would come back. That was as certain as God. The fish will come back. So there was never despair among the people and never a reason to blame anyone for it, government or anyone else. It was a natural thing.

And of course, there was only an inshore fishery. They always knew that they could not fish out the sea. They couldn't destroy the resource. And I doubt that anyone even had a concept of destroying the resource. It wasn't even imaginable.

But come the 'fifties, the offshore fishery started. And it was a European fishery. The northern cod landings peaked at something over 800,000 tonnes in the early '70s. But over that period, people began to realize--and I think it took until the early '80s before most people in the inshore knew--that an irrevocable change had taken place.

That now you could have a fishery failure that was not a natural thing but caused by the fishermen themselves. Now they could have a failure and, maybe, the fish would not come back. And that gives you a totally different inshore community. They have a new understanding of fishery failure.

Instead of saying, "Never mind, the fish will come back," what stands between them and permanent failure, is a few politicians in Ottawa.

Q: Would you say this new understanding was the beginning of the serious criticism of DFO science?

A: I wouldn't say it was the beginning but that's when it became mass criticism. Almost like a revolution. I would say that the mass criticism from the inshore that hit DFO three or four years ago was qualitatively different than anything that had gone before. Almost the whole inshore rose up and said, "DFO, you're blowing it." And it was different in that they concentrated on the science.

Now a few mistakes and a few bad decisions could cause a failure that was not natural but man-made. Now there could be a failure and the fish wouldn't come back. Now there was someone to blame. And this was utterly different than anything they had know before.

Q: I see this same qualitative difference happening on a global scale. For the first time in history, significant numbers of people are coming to realize that we can damage our environment so badly that it cannot recover. That we can quite easily make this planet uninhabitable.

A: [Brown argues that this awareness by the inshore developed independently from other instances of eco-awareness/activism]

Q: [The interviewer argues that it is linked to a pervasive zeitgeist]

A: I suppose that the thing is that the impact of science and technology is happening all over the place at the same time. So it's giving rise to similar reactions all over the place.

But this change in the fishermen has fundamental importance to government in how they relate to fishermen and develop policy. And I don't think that the politicians have realized that this change has happened and that it's fundamental. They realize that the inshore fishermen are more active and cantankerous and political than they ever have been before. All they know is that they have a harder crowd to handle.

Q: So they see them as a political nuisance rather than a bellwether?

A: Exactly so. I tried to talk to our people about this when the massive criticism first hit, before the Alverson Commission was appointed. It went right over their heads. It had nothing to do with science, so who cared?

I tried to tell them that it isn't really DFO science that's being criticised. It's the fishermen realizing now that all that stands between them and disaster are political decision-makers so the fishermen decided that they have got to get into the political process and start hammering the government.

And either through a lucky shot or some very shrewd thinking, the pressure point they picked to hit was the science. DFO is so proud of its science. And we have done a lot of good science. So to come and hammer our strongest point, our little area of purity--it was devastating to our scientific people. It was puzzling to the senior managers and politicians. "Why are they attacking our science? That's the one thing we do right! We could take criticism of our management decisions because we are used to that but to come and condemn our science!"

It's hard to exaggerate the first reactions of our scientific people. The were puzzled, upset angry. For a while they were just like children. The shock was horrible.

Here we were being attacked in the one area in which we thought we were unquestionable. We were used to criticism from all over on our management decisions because you can never please all the competing interests. But we always thought that the science was the one pure area, free from political interference.

I look at this from the point of view of my job, which is a PR hack. And when the whole racket started, when the Alverson Commission was appointed, my advice to our managers was...and everybody was in a quandary. How are we going to stop all this criticism? My advice was, and I exaggerate to make a point, go out on our hands and knees and say, "Please forgive us. We've done the best we can but we realize we have to do a lot better. Work with us and help us." Instead we took the Alverson report, which quibbled with our science but didn't condemn it, we took that and ran all over saying, "Look, aren't we great!"

Q: So you counselled humility and they responded with arrogance.

A: Precisely so. And from a public relations point of view that was a fundamental mistake and we're still making it. The fishermen were basically understanding of the fact that we were doing our best. All they were telling us was that our best, because of the difficult nature of the science, was not good enough. And they didn't expect us to become good enough overnight.

They wanted us to admit that our science wasn't good enough and to make fisheries management decisions with that understanding in mind. Not to keep gambling on the optimistic side that we were right in our science. That's what they were telling us. "Quit gambling. Quit pretending that you know more than you know." They basically had the same understanding--that it was an extremely complex business and that all of our calculations had huge levels of uncertainty.

APPENDIX C

Interview with Cabot Martin
Conducted in St. John's, Newfoundland
March 15, 1990

[response to suggestion that NIFA is posing a challenge to science's traditional autonomy]

A: I don't know if it's science as opposed to the management process. I think there's a fairly big distinction to be made there. You're right in a sense. On the other hand, I suppose there are not many economic and social activities where the role of science is so prominent as it is in the area of fisheries management. So if you want...if science wants to have that direct role in ordering peoples lives, then they naturally have to take the commensurate responsibility to account for itself, right? The interesting thing that I find about fisheries science, its a relatively imperfect and relatively young science, in certain ways, but it has a very direct...it makes judgement calls on the way people live.

So it's an interesting process. The environmental assessment process is probably not the ideal process but it's the only formalized one that we have. And the scientific community ...and they'd probably have...and I noticed in the paper that some scientists, through their professional organization, are criticizing someone...I'm not sure who they're criticising...whether they're saying that their professionalism is being questioned by groups like ours or whether they are complaining about the minister's attitude or the...say Crosbie's attitude towards the use of science. Right now we're into a science gap as far as management is concerned.

Q: [observation about distinction between science and management at DFO and the stated interest of the scientists in preserving their traditional epistemological privilege and autonomy]

A: I'd agree with that, but wouldn't you say that someone like Mac Mercer [at the time, Director of the Science Branch] is in the management side. I've always heard him described as a manager rather than a scientist. So within the Science Branch itself, there is a solid break internal to that. I don't know exactly where that is but I suspect that it's certainly below Mac Mercer.

But one of the things that we think would be very helpful is if the scientists were given quite a bit of independence. You don't know these things until you actually go to talk to them and all the rest of it but, from what we can understand, the Icelandic system of having this marine research separate from the management process is very helpful in preserving the integrity and independence of the scientific process. And that takes organizational independence and job security independence. They can't be under budget pressure either.

They can't be totally independent obviously but there are models, for instance in the offshore oil...or we can go back to Alberta and the board that managed the oil for Alberta was always quite independent. And even went so far, in their case, to have 50 per cent of their revenues come from industry. I don't think the industry here is rich enough for that kind of thing but the model here, with the joint off-shore [oil] board, I think is part of the solution because we also have this problem of split jurisdiction. And a joint management board...you could make a case that the joint management board for the off-shore oil is a proper way of doing things...given oil. Ostensibly, anyway, the province had..not a lot of direct interest in it. It certainly had an analogous interest in controlling on-shore activity...much as it does in the fisheries situation,.. And there's obvious benefits from a joint board on merging the two...federal/provincial jurisdictions on the oil. So I suspect that the joint board ...nominees from both governments...with a staff that had independence would be a big part of the answer. I think that Dr. Harris says something about that in his report.

Q: So your criticism is not of science and scientists so much as management and the way in which...

A: That's right. The way in which the scientific process is used in the decision-making process. There's obviously got to be a two-way flow because social and economic objectives obviously have to be projected down at the scientific level. Saying "These are the kinds of things we're worried about...we need a scientific analysis. Give us your ideas." Whether that's the number of fish that should be caught in any one year or the relative appropriateness of any technology...these are social questions that need to be framed.

In addition to that, you need this ongoing, unstructured scientific process, just the process of learning about the ocean which is very, very critical.

A prime example of that would be the whole issue of trawling on the spawning grounds. In Iceland and Norway they don't allow trawling on the spawning grounds. Partly to restrict competition between the gears and partly to protect the stock from fishing too many of the stock biomass. Also because intuitively in the minds of the fishermen...because it interferes with the reproductive process.

The same goes for haddock on George's or Brown's bank, where as recently as the Hache report, the government, the DFO was saying, "Well, since 1970 we've had closed season on the spawning grounds. The fishermen believe that there's some direct effect on the reproductive process. We don't have any scientific evidence of that but we'll go along with it because we see some other benefits--in protecting stocks when they're at very low biomasses." It prevents efficiency of directed fishery. But there was this underlying idea that there wasn't any scientific evidence to support the notion. And that was the pervasive view in DFO and it was the thing which really sort of reflected the disciplines that were traditionally looking at stock assessment.

Meanwhile, down in Logy Bay for instance, there was an ongoing process of research which dealt with endocrinology dealing with the whole hormonal aspect of fish growth and life. We found that there was more work like that being done in Europe. This parallel work...because people happened to be interested in this aspect of the thing...that body of knowledge was never incorporated in the management process...but that's the evidence, or that's the scientific analysis part that is the clue to why trawling on the spawning grounds is not a good practice. So there's got to be support for basic science.

One the one hand you've got this important aspect of basic science going ahead that you never know when it's going to be useful. But on the other hand, the process of the fishery science can get dogmatic in itself, like all aspects of science. "The Structure of Scientific Revolutions" could be named *"The Structure of Scientific Inertias"*, right? There are scientific inertias. Because people get focused in on their own aspect of knowledge.

The common criticism that I hear of this unit down here at DFO and perhaps other units is that there is not enough cross-disciplinary interaction between scientists. Not enough...what are the full range of gaps that wo have here? I don't mean that to be an unfair criticism but the fact is that, for instance in Iceland, they don't do this endocrinology approach. They ban the trawling on the spawning grounds and then they don't worry about it any more.

But you'd hope that in a situation where they say that we're not prepared to ban trawling on the spawning grounds, then you look at all aspects of it. And you don't find, for whatever reason, down here you don't find them saying], "Let's write down all the things that we need to know. Let's get all the disciplines in. Let's have a totally open discussion and here's what we need to know."

So I don't know if it's because of the organizational structure or the type of people or the type of disciplines that are involved or whatever, but there doesn't seem to be this broad open type of inquiry. I think that's partly due to the pressure of the political process telling the scientists what's important. They're [the managers and politicians] saying, "I want numbers. I need numbers. I want you to count fish and I'm going to cut off your money in other areas...or I'm not going to give you much money in other areas." And that's partly true although down here they were given more money last spring and didn't bother to extend the scope of their inquiry to take in these other things. I suspect there's a significant amount of inertia.

It could be that they're just shell-shocked down there. It could be that they feel criticised and under seige.

Many of these people have not been trained...or nowhere in their training are social responsibilities. The scientists just took it upon themselves...I shouldn't say took it upon themselves...found themselves in this position where they had tremendous power over peoples lives. But I don't think that anywhere in their training or anywhere in the internal culture of DFO would you find a discussion about the social responsibility of scientists to explain and account and I think that that's a very fundamental problem.

And there's a whole range of issues that come out of that. The perception of the scientists and how he feels he fits into the whole range of different knowledge, of other questions. They seem to believe that they have this superior form of knowledge which is not additional to common sense or additional to the experience of people working in the industry. It's on a higher plane, somehow closer to the so-called truth. There's this self-image problem of the scientists.

And the unfortunate thing is you get this tension and you get many fishermen saying, "Well, scientists are full of shit." And by having that attitude they tend to undermine the legitimacy of the science in the process. And that's not the answer. The answer is better science and more accountable science and more scientists. Whether you can devise a system where that's actually realized is a big question.

Maybe it's just the nature of our social organization that people who go to university and get degrees and put shirts and ties on and work in nice offices and circulate in a social milieu that's different than most fishermen...maybe they inevitably grow apart from the people whose interests they are supposedly looking after or benefitting. If that's so, then we'll always have this tension. I think that's a big part of the problem...this remoteness...the unreality where you can actually sit in your office and have an opinion and have an explanation which is actually totally out of step with reality. Where people can be totally unrealistic. It's perfectly possible to do internally acceptable...from a competence point of view...an acceptable type of job as a scientist and yet be totally out of context, out of step with reality. You can do that. The fact that you can do that is quite an amazing concept. I don't think there are that many types of activities where you would get away with that. Right?

There is a big challenge. There are some scientists, fisheries scientists for that matter, who are attempting to incorporate...mostly in the context of third world countries...who are attempting to incorporate so-called indigenous knowledge...mostly world funding agencies.

There would be resistance to that here. Most times when I talk to fisheries scientists, they enjoy close contact with the fishermen. You don't hear people ...a lot of them actually physically enjoy the work. It's a strange problem there. I don't know exactly what it is.

It's almost like the inshore fishery is too complex for them to understand. The problems in the inshore sector are because there are so many different types of people and different types of gear and different types of fishery and so many different places. It's almost like it's too complex for people to understand. And the collection of data from fifty or sixty trawlers and a couple of cruises a year...that data base is a lot easier to manipulate and easier to handle. I think that's an apparent....I keep hoping that if the scientists got involved in the in-shore more they would find the collection of data to be easy and quite pleasant. That would help ...

There's another aspect to it...which is changing the subject completely...and that is the whole notion of the responsibility of the scientists to speak out. In the case of the letter you see in the paper...the impression I get...you don't know the full contents of the letter so you don't know why they wrote it...it seems to be a kind of notion that as professionals, our reputation has been sullied and we want our name cleared. There doesn't seem to be any hint of the public interest as opposed to their professional or collective interest. In other words, what duties do they see themselves having to speak out when they know that there are cases of mismanagement or cases of information being mishandled or suppressed.

Q: [question about internal debates in DFO]

A: We were told that there was an internal debate back in 1986 and we know that there were papers published at that time pointing out that they knew that they were overestimating the size of the stock by nearly 100 per cent. Those thoughts got submerged in September of 1986 and then didn't reappear until February of 1989. There's ...this would be...a fair indication as far as I know.

And I know that one of the parties to that debate stated in '89, last spring, that he kind of lost the debate in '86...a couple of papers were published but the general view of the department, the public stance of the department was very much the opposite. That there was nothing wrong. They said afterwards that they felt vindicated among the peer group. But that's not enough.

That's the internal structure of the scientific community. What's the duty of a person in that sort of situation to come forward and to say, look, this is not right?

Q: [question about policy preventing public employees from speaking out independently]

A: It's a general problem in government. There's no doubt about it. But the kinds of sanctions against speaking out on a moral issue, I would consider this to be a moral issue, they turn out to be more apparent than real. You do get the union protecting them.. These kinds of large...I would consider that to be an anti-democratic rule. The power of that rule is more in the unwillingness of people to test it than in its reality.

But I think the more important thing there is that the scientist is in a culture in which...the person didn't say, "I've been vindicated by my peer group but I wish I could find some way of discharging my larger responsibility to the community but I'm afraid for my job." The fact is that the scientific community ought to define the bounds of its own morality. "I'm vindicated," and that's the end of the question. It's not that I have a duty which I am somehow being forced not to execute or live up to.

It's just that scientists would say, "I have no duty. I don't want any duty. I don't see the purpose of any duty." Right? "I'm vindicated." Not frustrated. "I'm vindicated because my peer group now recognizes that I had the better analysis of the situation." It's a game. "I can hit the ball harder and farther and I won the game." The fact that tens of thousands of people suffered because he didn't speak out back in '86 is irrelevant.

I think that the kinds of questions...this is obviously on a far lesser extent... that the association of atomic scientists have always grappled with...the morality of science...what is your duty to the public when you're engaged in a government activity which you find to be morally offensive?

Q: [question about the environmental assessment process being the way to force scientists to face the social realities of their work]

A: I think that's an interim step. The first step is to shake up the system. That's what the environmental process would do. But what you do want to also create are structures, organizational structures, such that open debate is relatively free and open. If someone feels that there is a problem, they wouldn't feel constrained by the organizational structure. It might be an ideal world.

I think the independence of the scientific group is the ultimate objective. But right now you just can't get to first base as far as accountability is concerned. There's no notion of accountability. There's a whole problem there. There's a very rigid bureaucracy in a country where bureaucracies have tremendous power. This particular branch of the bureaucracy has more power than any others I can think of.

An example would be something like atomic materials where there is fairly tough control. But most aspects of life like forestry and mining there is more of an open...your government isn't crawling all over you without much accountability.

The first step is to bring in the notion of accountability. Bring in the notion of the relative contribution...and it is a relative contribution. It's one of a number of ways of looking at the problem...that science can make...and then setting up a context in which scientists will admit...will willingly and enthusiastically participate in a management system in which the fishermen are much more involved and then I think the structures will follow.

I think there will still be a need for annual public hearings on the important questions. I can't see in our society...we say we're a democracy...I can't see why anybody would have any problems with public hearings on critical quota questions. And I can't see why we couldn't have independence for the scientists organizationally. I think that those things are not unattainable but there's a tremendous amount of inertia in the system.

The worst thing that has happened is that they have been shown to be incorrect. Science can play...the fisheries can't afford...I've heard scientists say, "We can't afford to know how little we know because if we admitted that then no one would listen to us." That's twisted. You get leadership...you're recognized as a leader...one of the qualities that people like in a leader is an openness and determination to find the truth. You get respect for leadership for that and you ...the fact that you don't know everything and that your techniques are not the be all and end all...and you need input from fishermen and you need someone to haul you back from the brink of being totally out of touch with reality. All those things are quite acceptable.

So when science is tending to put the all or nothing question to fishermen and other groups themselves...unfortunately if you put the all or nothing..."I'm either totally in charge or I'm not going to be in charge at all,"...most fishermen looking at their track record would say, "Well you're not going to be involved. If you're not prepared to be reasonable, then I can't handle it." And I think that's a great tragedy. I think that would be worse.

Q: [question about the Stein group]

Ken Stein is ...his group...they have office there in Atlantic place....and basically what their mandate is to try to see what programs can be brought to bear on the social and economic outfall of quota cuts. Whatever quota cuts are deemed necessary as a result of the scientific reanalysis.

So you've got ...they've been working now since last May some time...and I think by now that he has a pretty healthy attitude towards the complexity of the problem. The depth of the problem. But his group really has been carrying the ball as far a coordinating the social and economic response and working with the province to develop a response program or response package. I suppose there really is a subtle interplay between the work of his group and the whole stock management process. Because once you accept the physical analysis of the stock one way or the other, there will be a whole range of options...once you move away from stock extinction, commercial stock extinction...once you feel safe about that question, then you have to say, "Well, what rate do I want to rebuild the stock and how high up do I want to rebuild the stock? What are my social and economic objectives for 1990?" And our position, for instance, would be we want to put it back to '65. We want '65 again. We want the sea...the sea was such before the trawlers came and destroyed the stocks. We got the 200 mile limit.

The mandate of the government in '77 was to rebuild the stocks back to the level of 1965. And that was between four and four fifty hundred thousand tonnes for northern cod. And commensurate increases for the other stocks. So that's the objective. That we're taking longer to achieve it, that we're having this dip in the meantime...so the scientific analysis that we want is to tell us how to get there. How do we get to that objective?

Unfortunately, to get to that objective, you've got to cut deep now. To allow the escapement of a lot of fish. To allow the breeding to go on uninterrupted on the spawning grounds. All of this comes at a tremendous economic cost to the federal treasury.

So one way the Stein group put forward was, we won't bring it back to '65. We'll only bring it back to '81 or '82 which is only half of where it could be. So we're missing a couple of hundred thousand tonnes of northern cod which is worth a couple of hundred million dollars to the local economy on an annual basis. So his group is posing the questions that ...to the scientists... his group is saying, "We want a slow growth and we want to cap it at say 220,000 tonnes." A ten percent increase over ten years. So I go back to the scientists and say, "What's my annual TACs to do that? There's my rate, there's my cap and you tell me what my TACs should be."

And that is the big social and economic debate that is not yet joined. That's the big debate. If we can save the stock, then where do we want to go? What are our objectives? The scientists are increasingly being asked questions that are depending on how you view the world.

In Iceland, they say we've analyzed the stock,...and it's something like what Harris did in his interim report and hopefully in his final report... and we're giving you three levels of exploitation and we're projecting them out a bit and this is what we think would happen if you did this and this and this. And these three levels of exploitation have certain social and economic costs associated with them. But we don't want to have anything to do with that...you go do that...you don't want just one number, we understand that...we're giving you three numbers, three cases but it's for you to make the choice.

When you think about it, the Stein group is really critical to what the scientists are being asked to do. At least on the stock assessment. The Harris report with its emphasis on caplin and seals and shrimp even, will force a broader, multi-species view on the management process. Its much more difficult to do. And there are these social and economic questions being put to scientists now. So with the crisis like it is, that's understandable.

In the rebuilt ideal world, maybe there would be less of these type of questions, I don't know. But in the process of rebuilding, the amount of social and economic input to the management process is just gong to be incredible. And I think scientists are going to have to get used to that.

[while walking out, not recorded]

We're in the curious position of beating up on them [scientists] on the one hand and calling for increased support for them on the other.

[re science's position that forcing them to submit to an Environmental Impact Assessment-like process would compromise their autonomy]

That's not autonomy, that's lack of accountability.

APPENDIX D

Interview with Chris Lang, an electronics engineer who builds and operates survey equipment for DFO. Conducted in St. John's, Newfoundland March 4, 1990

Part One of Two Parts

A: I perceive that the scientific information or advice or gut feeling...it probably amounts to little more than that...is not weighed very heavily in management decisions in any event. Decisions are made....

Two years ago, was it two years ago? when they [scientists] set the TAC for northern cod at 125 [thousand metric tonnes] it was what? 238. This year again they said 125 and its 190-something. Harris said 125 and Valcourt [Minister of Fisheries and Oceans at the time] said, "Go fuck yourself."

So if 125 is deemed to be...when 125 was first suggested I think that the perception was that it was a conservative number and that you'd best err on the light side. But three different occurrences of that number would seem to indicate to me that 125 is more like what it really should be and if you want to be conservative maybe it's 100. But in any event, that doesn't seem to have much impact on the decisions that are made about what the fishing level is going to be from year to year.

Q: Siddon [the minister prior to Valcourt] and now Valcourt seem to be ignoring the advice completely.

A: They would probably maintain that they are not ignoring it but there's economic pressures or social pressures or whatever to have the TAC at 230 or 450 and that he's compromising.

Q: What do you do at DFO?

A: Oh, I'm very down into the bits. I'm primarily developing equipment for acoustic surveying.

Q: Is acoustic surveying a relatively new tool in the kit?

A: Well it's been used routinely here since about '73 but it is very new in the sense that there's an awful lot of potential for what can be done.

Q: But the question is, does acoustic surveying work?

A: No. (laughs) Well, there's no doubt that they work. The problem is interpreting the results. But there's an awful lot to be learned and there's a lot to be done technically.

Q: Is acoustic surveying seen as a replacement or supplement to test-trawl data?

A: Well the big appeal is that trawling is a very expensive, slow, waste of time. If there was some other way you could sample the population, then let's go do something or try to develop....acoustics seems to be the only way. I can't think of any other conceivable way of doing it. So they've turned to echo sounder technology which has been around for a long time...just where are they?

They're trying to expand that question into "Okay, here they are, how much of them are there? what kind are they?" those kind of things which we're not real good at yet.

Q: So you look at the blips on the printouts and try to determine both species and number?

A: That's the objective, yeah. It's a very noisy environment. It's a terrible place to try and deploy people and equipment and boats to do anything. Trying to survey the northern cod stock...perhaps the best time to try to survey them is during the spawning concentrations which is February which is a terrible time to be on the Grand Banks. Good luck putting out some equipment and dragging it along behind a boat and hoping that it lasts more than an hour before you beat that crap out of it. So you've got those kind of problems to overcome before you can even start approaching the scientific questions.

Q: Do you do your acoustic work on the spawning grounds?

A: On a trial basis. This year was the first time we had any equipment that would last longer than an hour. This year we had equipment developed specifically for that environment.

Most of what we've been successful at has been pelagic species. Which are mid-water...they're not near the bottom which is another problem with cod to distinguish them from the bottom echo. They tend to be right on the bottom. The pelagic species school a lot and in higher water and not as far away from a transducer in terms of trying to get some power down there and listen to what's coming back and not get confused with the noise of the environment. So actually using the acoustic data has helped with the assessment of pelagic species for a number of years now but it's not really an input to the assessment of cod because it's still too raw right now.

But this year there's been some data collected, nobody's looked at it yet but it may be useful.

Q: From what little I've read, I get the impression that the Soviets are quite advanced in the use of remote sensing for stock assessment. Do you know anything about this? Does your department use any satellite data?

A: It's used primarily for plankton and oceanographic temperature. There's some sort of sensors, light sensors, that they use for tracking caplin schools from aircraft.

Q: So since caplin are a primary food source for cod, you could use this information for determining the relationship to cod strength?

A: Yes, there's some people who look at that but, the aerial photography, it's primary goal is in the assessment of the caplin stocks. Which is treated as a separate stock right now. But obviously, if you want to understand cod you've got to understand their whole environment. For a satellite technique to be useful in measuring the cod themselves, I don't see that it's feasible.

Q: No but current shifts and changes in ocean temperatures seem to be quite important in determining the ways in which the cod herds locate themselves and distribute themselves. Temperature seems to be very critical.

[long digression on unrelated topics]

Q: Do you go out on the [research vessel] cruises?

A: Once or twice a year. Somebody has to go. Our systems are not idiot-proof enough yet that....Our job is to get this data on computer tapes of higher quality than the scientists can deal with so we can stay ahead of them. So that their problem isn't us.

Q: Is this kind of like a game?

A: Not really. It's CYA...cover your ass. That's really our job. If the limitation becomes the quality of the data or the quantity of the data, then it's obviously our problem. There's a lot of analysis techniques that have to be developed to interpret the data. It's still too raw.

Q: Who does that?

A: Someone else. I'm low in the decision-making process in the TAC. I calibrate the equipment so in that sense I can bump things up by 20,000 tonnes here and 20,000 tonnes there or bump them down by 20,000 tonnes.

Q: How?

A: I could just cheat on calibration if I wanted to. I could say that this many blips means that many fish when in fact it only means half that many fish.

Q: How do you do your calibrations?

A: Honestly.

Q: What's the technical process?

A: That's another issue that's under development. The best way to do it right now is to use what's called a standard target. Which is something whose reflective properties are well-known on whatever frequencies you're working on. Target strength is the parameter that characterises a target. How much echo it will give back for how much energy hits it... So they're able to build a tungsten carbide structure, a ball, whose target strength doesn't change much with temperature or time. You put it on a cable under the transducer and measure the response. You know what the response should be so you can...

Q: But then there's the problem of translating that knowledge into fish.

A: Which isn't my problem! The target strength of a fish will change with its aspect, will change with its depth. The swim bladder is the major thing which causes the echo from an acoustic point of view and the deeper the fish is, the more pressure that's under. And that characteristic changes with depth and the age of the fish and the size of the fish and everything else so...

Q: Temperature, salinity, current flow?

A: Yes.

Q: [long introduction of the concept of "theory-ladeness" of observation]

A: And that problems represents itself in perhaps 25 layers before some number comes out of a survey.
For instance, you're trying to measure the spatial distribution and concentration of fish but you only have one boat and one transducer...and there are survey designers who say you're going to drag it across this path and then you'll go up there and drag it and then go down there and drag it here so that we can look at it all and get something out of the whole thing. If you had the luxury of 25 ships you could go through the one area at the one time and gather the stuff in parallel and then you could structure your analysis a whole different way. But... The fish could be chasing your boat around, for example. They might like the sound of it.

Q: So research design itself is theory-laden. [long intro of concept of social creation of scientific knowledge] So there's a tremendous amount of room for social input into stock assessment.

A: Or maybe there should be and isn't!

[long digression]

In the first part of the process your trying to determine how many fish there are. Then there's another part, how much death can they withstand?

[third party Q: but surely in the first part the number is generated in as pure a way as possible?]

A: Ideally, yes. But I'm sure it gets polluted somewhat by expectations. I would hope not, but I'm sure it sneaks in here and there. They're tracking some stock and all of a sudden you get a blip in life. Something might be wrong, right? So you go back and do it again. "I don't know if I believe this or not. Well maybe I screwed this up. There's some amount of evidence to suggest that I did and some to suggest that I didn't. Or, maybe I screwed that up or,"...there's always subjective inputs all along the process.
I guess that there are social inputs in that scientists read the newspapers and they know that they need to have more fish than they are saying that they have. If he's stuck with a question with a fifty-fifty answer, he's going to take the one that gives him the answer he's looking for.

If you wanted to insure that a truly objective job be done, you should lock the scientists up somewhere, don't let them read any newspapers, don't let them talk to anybody.

Q: Are the theories and methods of stock assessment science under any internal debate at DFO?

A: Yes, I know that there are but I'm not a participant in these debates. It would be a sick place if there weren't. I would hope that it's hot and heavy all the time, but I don't know.

Q: [about the dissenting opinion in an '83 CAFSAC report about the methodology that disappeared until it reemerged in '87 as a majority opinion]

A: They've recently set up a group of statisticians I'll call them. Survey designers. I don't know really what it is that statisticians do. They are participating in the debates or the discussions about what's right and what's not right. So I guess that somebody recognized that there was a problem and now they're addressing it. Now what actually has come out of it at this point I don't know.

Q: [about the shift from single-species to multi-species modelling]

A: That's not really a new idea. It's been around for a while...up and down and up and down several times. The first time I heard of it was around 1981. Let's do away with dividing up everybody's tasks according to species...having a cod group and a herring group.

[Third party comment: "But it all comes back to the fact that the biggest problem with super computers is that they still can't predict atmospheric phenomena. No matter how many super computers you link together and make into one brain, it's impossible for them to predict a cyclonic tropical storm. Once they know the data, they can post-forecast it. That's no problem. But they can't actually forecast it. So I really think that...there are simply too many variables. And the weather business and the fish business are all the same. You can't possible understand all the variations and fluctuations and especially given the single-point sampling processes that we use. One pass over a herd o' cod tells you fuck all. Except that for 20 minutes on a certain date at a certain place there was X number of cod in that herd. But that doesn't tell you anything about the next day or a month from now or a year from now]

A: If you're not tracking one, the weather and two, the oceanographic weather, both of which are impossible to do, then you can't achieve the third one. (biosystem modelling and forecasts)]

Q: So it's futile?

A: It's futile to get an absolute answer with no error bars on it. You can forget that one. So it's only a question of how big an uncertainty you are willing to accept as a return for your dollars. If the country is spending this many dollars and we know that we should be catching 100,000 tonnes of codfish plus or minus 200,000, which is to say we should be catching none or 100,000 or up to 300,000...to me they're wasting their money.

Q:So everybody wants to believe that we can know when in fact we can't?

A: Well, that's not the fault of science...that's human nature. We want to believe that we can control the environment. We want t believe that we are divine beings.

Q: Is there even a remote possibility, given sufficient resources, to come close to the goal of "knowing?"

A: I'd have to believe that or I wouldn't be doing what I'm doing. I think you can come up with a number that's better than a spin on the old wheel. But whether it's economically feasible to fund it, because it's expensive. And if you don't want to count any scientific gains that don't have any immediate or intrinsic value, then all you're paying for is the numbers that we're putting out every year...if you put it in those terms, which for example, the Conservative government is perceived as doing, it may not be cost-effective and it's just a waste of time.

The scientists might be better at doing it in ten years from now, but what does it get us? We still don't know...our error bars are down from 100,000 tonnes to 90,000 tonnes. Big deal. It's still a shitty number and it's no good to us. If your plus or minus starts to approach your absolute value...

The infrastructure to evaluate a stock is expensive. The operation of a ship is $10,000 a day. Not counting the people you're putting on it and the equipment, the computers and the stuff they're dragging around with them. And they're totally under-sampling it. Which is obviously recognized because the inputs to determine what the total stock is...there's not enough data available.

So they take catch statistics and level of effort statistics, all of which are polluted. There are guys catching fish that are not getting reported. There's fish plants staying open 2 o'clock in the morning till 6 o'clock in the morning processing fish that some guy brings in that nobody ever knows about. There's such a paucity of data, it's so under-sampled that you'll take any information source you can get, however polluted it might be, to try and give you a feel for how much fish there really is. But all of that costs money. It's coming out of income tax money and should we be paying for it?

Maybe it doesn't matter how much fish we catch. Set the TAC at whatever you want. You may not being having any impact whatsoever. I don't see any evidence to suggest otherwise. That if we just stopped catching fish for five years or fifteen years it might not make any difference to the population. Any population goes through all kinds of ups and downs and it might be near extinction just through environmental and biological forces that may have nothing to do with us. We, maybe, have an insignificant impact. It could be cosmic rays!

Second Interview with Chris Lang
Conducted in St. John's, Newfoundland
June 27, 1990

Q: What do you here about Mac [Mercer]? What are the rumours about where he is going?

A: He apparently has had an interest in the private sector...been thinking about jumping over into something for some time. And he probably saw the golden grenade coming and said "Now's the time. See you guys. Call me when you straighten it out."

Q: Is there any indication that he might have been pushed?

A: Speculation I guess.

Q: Is there any talk about where he might end up?

A: He's got a part time...a little project going with Ottawa. I don't know what it is. Something he was working on for the Deputy Minister. He's still going to be working on that so he'll still have his foot in the fisheries door. If they parachute somebody in to take Mac's position rather than...Larry's [Coady] doing it on an acting basis now...if they parachute a person in who is well-suited for it, then maybe you could use that to support an argument that Mac got pushed. But we could be Meeched out on this yet!

Q: What do you mean, "Meeched out?"

A: That money could be...did you here Michael Harris this morning on CBC? He was talking about the Hibernia thing and the other agreements that have come up to the point of signing...federal/provincial agreements. There are at least three. And some of them may not make it now.

Q: So that money is not in the bag?

A: No I don't think so. It still could be scrapped upon regionally. There's the gang in Nova Scotia and there's several other gangs in canada who could be after chunks of that money now.

Q: But I thought it was officially earmarked for northern cod?

A: So did I but Hibernia was officially earmarked for agreement. And these other federal/provincial agreements are supposed to be in the bag too but...

Q: What are the plans for your section when and if this money does come through?

A: Nobody knows. It must have been as long as five or six weeks ago now, when Mac was still there, they had little meetings among all the scientists and they said it looks as if everything is going ahead and you can just plan like it's going to be and in about three weeks time there will be hard cash here to deal with stuff. So start planning ahead as if you were going to have money in three weeks. About two weeks after that, his prediction was in three weeks. And that's like four or five weeks ago now.

Q: So the money hasn't shown up yet?

A: No. And Larry and Jake are very busy and inaccessible so I don't know. We're trying to get a meeting with Larry and

Jake now...a bit of logistics. We're doing a bit of field work and they're back and forth to Ottawa all the time...but it looks to me like they're being kept in the dark. But I haven't spoken to either of them for a long time now.

Q: Jake told me that they got all the money they asked for but they couldn't hire any more bodies. So they can buy all the equipment they want and generate all the extra data they want but they won't have they bodies to work with the increased data flow.

A: I didn't hear that there was a freeze. There was supposed to be as many as 11 PYs tied up in this northern cod fund.

Q: PYs?

A: Governmentese. Jobs. Person years. There was supposed to be 11 jobs come out of this...or that was the estimate that I heard from Larry. But they don't have anywhere to put them. The building is blocked right now. They don't have anywhere to put them. They don't have room for what's up there now. So if there's 11 more people coming they're going to need 20 more offices and so many more labs presumably.

This money is supposed to be divided up over five years and that's approximately $8 million a year which is about what the science budget is now. So that's a doubling and I don't see that there's the capacity there to handle it.

Purchasing, for example, and supply and services. It's all they can do now to keep up with the work that they have to do. How ever necessary or unnecessary that might be. It's a pain in the ass from our point of view. But I can't see that they can handle twice the level of work all of a sudden without something giving.

It's my perception that a lot of this work is going to have to be done outside. If you're talking about doubling the science budget you're talking about twice the level of work. So either people are only working at the 50 per cent level now and they'll be able to handle it or we're going to have a serious problem. But if people are only working at the 50 per cent level now, they already have a serious problem.

But it's interesting that you mentioned that job freeze. Maybe that's what's holding things up. If they can't have people then I don't know how they're going to do it.

Q: With all these problems, it really doesn't surprise me to hear you say that Jake is considering bailing out

A: He hasn't said that to me but that's how I view it...especially after reading that article in the Express.

Q: He wasn't talking like a guy who plans to be around for a long time.

A: Not really, no. "Fattening up one's bank account for the long weekend!"

Q: It's the sort of remark you'd expect from someone who had a job offer or two in their back pocket.

A: I can just see him swinging the bat at the hornet's nest.

Q: He was expressing a good deal of frustration with the fact that his administrative duties mean that he can't do any science.

A: That's a fundamental problem that they have up there. There's no incentive for a research scientist to take on administrative duties. Because there's no points in it. It detracts from their ability o publish. And a research scientist exists to publish and if he's not publishing then he's not doing his job.

But they keep coming to these research scientists to do these administrative jobs and they end up pushing paper all the time. There's no points in that. "Instead of publishing these three papers instead of the six or eight that I wanted to, I also did this and this and this and had 75 meetings in Ottawa and 40 here and there,"...that doesn't count. That's nothing.

Q: So once you cross into adminstration it's like the land of no return. You lose your status in the scientific community and the only thing you can do is to continue on in the bureaucracy?

A: Either that or the administrative positions could be treated as a short-term thing that everyone had to share in. One scientist would do it for two years and then he'd get to go back and do real work and some other guy would get the finger for the paper shuffling. But there seems to be no presence of professional managers...people whose interests and skill are in managing people and money. You know it's the old Peter Principal.

There's obviously a problem. I don't know what management skills are because I don't have any. But there's guys who spend their whole lives developing management skills and part of those skills is being able to extract from technical people some area of expertise that you are not up on...what the important points are with respect to managing in that field.

But from the scientific point of view there's no incentive for them unless he likes getting frequent flyer points or staying in a nice hotel in Ottawa. I can't see why anyone would want to take a senior administrative position.

Q: Is there more money in it?

A: I think so but not a lot. Not so you get people lining up for it.

Q: But not enough to compensate for the loss of research time and professional status?

A: Certainly not for me. But there's two kinds of scientists. There's research scientists and scientist scientists.

Q: What's the difference?

A: Scientists...there is an incentive for scientists to become a manager...financially. Research scientists are paper publishers. That's it. That their job...to liaise internationally with people in their field and to develop research and to publish it.

Q: Not necessarily to serve DFO?

A: Right. Which is what a normal scientist or a non-research scientist does.

Q: So are the scientist scientists more technically oriented people?

A: Right. I'm a scientist. Not a research scientist. We're categorized in the union by research group...physical scientist and biological scientists and then there's the research scientist which aren't categorized. So I guess that scientists do the services and the research scientists publish.

Q: Where do the research scientists get their direction? Do they decide what projects to pursue or are they directed by the administration?

A: Some amount of each I suppose. You might talk to ten research scientists and three would say that they are free to pursue their own interests and some are given some amount of direction. The research scientist needs a Masters degree, preferably a Ph.D whereas the scientists are Bachelor level jobs.

Q: Have there been any new developments in the hydro-acoustic business?

A: There's potential for big developments. Hydro-acoustics figures prominently in the northern cod thrust. It was October or November of last year that we finished a national review that was conducted by the Deputy Ministers office that was, where are we in hydro-acoustics? Where should we go and how can we get there? And everybody had a bunch of meetings regionally and then when the northern cod money came we had some plans and directions about what we'd like to do. I showed up in about three of eight places in the Harris recommendations. If everything comes to fruition we're looking to double our survey level and develop some new technology.

Q: For instance?

A: Multibeam sounding systems.

Q: What does that mean?

A: There are some things that are not clear about the data that we get with an acoustic survey now...that we have to pin down by catching fish with a trawl whereas you could measure them directly with a multiple beam system. What you're doing is measuring the return from a cloud of fish which depends on how big and what the distribution is of the individual members so you have to catch them and take a sample of them to come up with a number. But these multiple beam systems allow you to measure that.

Q: So you'd be able to see individual fish?

A: Yes. Then you could scale the cloud on the basis of the measurements that you make on individual fish. But there are some things that are unknown. Some research that has to work out some of the details. Not so much on the implementation of the technology but in how to interpret the data and how t remove some of the biases that show up.

Q: What sort of biases?

A: Well it's easier to get good quality measurements on fish that are bigger and to reject smaller fish because the quality of the measurement is not good so you tend to bias the population that you are characterising on the high end of the size of the fish that are in the cloud.

Q: So you'd end up overestimating the older year classes and underestimating the younger?

A: Yes. If you're not careful you could come to the conclusion that there are very few little fish in the cloud and its mostly big fish because the single fish measurements that you have which are useful are the larger fish. But all those things can be worked out.

Q: So do you drag a trawl through a population at the same time that you are scanning it to calibrate the equipment?

A: No you sample a population alternately. You pull the survey transducer behind the ship all the time in a regular survey design. Within the parameters of the survey design, you periodically stop surveying and pull the survey equipment in and let the trawl out, tow it for half and hour, pull it in and let the survey equipment back out.

Q: So you're not fishing the same fish that you are surveying?

A: In a global sense only.

Q: Wouldn't it be better for calibration to be beaming down onto a population just before you dragged a trawl through it?

A: A trawl isn't a real good sampling tool for fish in that respect because you're looking down from a survey transducer at a depth of five to ten meters clear to the bottom whereas a trawl can only sample some subset of what's right in front of the net. There's something to be said for what you suggest but there's a long way to go yet before that's necessary. The calibration is not that precise yet anyway.

Fish look very different with behaviour. The aspect of a fish changes its acoustic signal, its reflectivity, tremendously. So if you are sampling a population that for some reason is more vertical in the water or making vertical depth changes as you are measuring them, or if you are measuring a population that's turned broadside to you and swimming on the level...even though it's the same population of fish, they will look very different acoustically.

There's more uncertainty there than you would remove simply by counting all the fish that you were sampling acoustically. They have sampled caged fish but the behaviour is altered enough that it's difficult to generalize to a free-swimming population. So it's not an exact science by any means. There's a lot of work to be done.

Q: Last time we were talking you mentioned error bars and it wasn't clear to me whether you were talking about errors in the TAC or in the whole population.

A: Well just from an acoustic point of view, you can have as much as one quarter as much peak response from one population. So you could survey one population and have a certain beam response and another time you could get as low a one quarter of the same population if they were all hanging around with their noses up. So there's that level of uncertainty.

Q: So you're saying that with the technology and the techniques that you are using now you could be dealing with levels of uncertainty as much as 75 per cent one way or the other simply due to variabilities of fish behaviour?

A: Yes. Just on that one alone. Just on behaviour. And it's going to be difficult to deal with that. Maybe you could run video cameras or you could deal with it statistically somehow if you had larger data sets over more time. But there's that level of uncertainty in acoustic estimates right off the bat.

Someone could have studied a population in Norway, herring say, and applied the results to a survey of herring in the Bay of Fundy. Well whose to say that the herring in the Bay of Fundy are behaving the same way as the herring in Norway?

And on top of that there's another level of uncertainty in the physical calibration of an acoustic system. Your dealing with inhomogeneous water when you are trying to make your measurements and there's a certain amount of variability all the time. So if you are trying to make one measurement, for example with a transducer and a microphone at a fixed distance in water with no targets in between, just trying to measure the power of the signal, it bounces around a bit. There's no such thing as standard sea water or the standard amount of bubbles that can be in it so there's a lot of uncertainty inherent in the acoustic signal of a fish population.

Then there's the survey design aspect of it. You're going to take a ship over a three-week period of time and you're going to plan to tow from here to there, and there to there, and there to here, and how relevant that is to the actual population I don't know. There are people at work who know about those things but how well that's done is another level of uncertainty when you're setting a TAC...saying there are this many fish and this is how many you can take without damaging the population...well probability just creeps in all over the place.

Q: If everyone contributing their specialty to population estimates is dealing with anything close to the level of uncertainty that you are dealing with...they might cancel out or they might add up to huge errors.

A: Well a statistician might have some idea about that...how they might line up or misalign but I don't know how bad that might be but at the TAC level I can feel certain that the number is very large...the error bars...but I would guess that it's been reduced somewhat over the last five years.

Q: Given the current state of your research technology and survey designs, do you think that the information that you are generating is of any practical use as a basis or guide to management?

A: It's certainly of some practical use. It's a matter of degree I suppose. It's very expensive data to collect. It's [hydroacoustic sampling] cheaper than dragging a trawl. But it still has that problem fundamentally, that it's an expensive sampling technique. Behaviourial biology comes into it all over the place. And I suspect that that's rather a raw science.

[speaking about unmanned, automated hydroacoustic sampling]

The limiting thing about it...the raw data...we have a raw data problem...the true measurement data that's collected. There's too much data. You can't transmit it. You have to process it locally and accept the assumptions that were made in the design of the processing hardware and software. So you're forced to take the processed results.

Q: If this northern cod money does eventually show up, what sort of things will your unit be doing? I assume that you've made some provisional plans.

A: Our big project, in the short term, would be directed towards physical calibration. Essentially we'd be coordinating work by outside people...to come up with a quote unquote state of the art physical calibration of our transducer systems.

Q: What does that involve?

A: What kind of a project would it be?

Q: Yes. Last time we talked you mentioned the ship board calibration of your equipment against a tungsten ball. Would this be a further refinement of that technique?

A: Yes. A physical calibration as opposed to a behaviourial calibration or....We've had inter-ship calibration attempts in the past. With one physically calibrated system on this ship and another physically calibrated system on another ship. They both survey the same population and come up with different numbers.

Q: To what extent?

A: I would say you could talk...off the top of my head...20 to 20 per cent difference.

Q: And this is the level of uncertainty due simply to the physical calibration of the system itself?

A: That's right.

Q: And this is as close as you can get with current techniques and technology?

A: They could be improved.

Q: So to what extent are you dealing with uncertainty now in your physical calibrations...a rough percentage?

A: We do it by collecting a long time series of data and then meaning it, if that's what you mean.

Q: But you have a physical target of known size, depth and reflectivity...?

A: No. We're not using that now. Right now we're using a secondary standard which is an underwater hydrophone that has a measured, stable known response. You take you unknown and calibrate it relative to a secondary standard. Whereas the tungsten ball is a primary standard. You can have a tungsten ball with a known acoustic reflectivity at whatever frequency you're working at and...it's better from the point of view of the user. It's a more direct, less noisy calibration than the one that we do now.

Q: So you're not yet using the ball?

A: No. Not properly. We play with it a little but the actual calibration number we use is based on calibration with a secondary standard.

Q: Do you have any idea what the range of uncertainty is with your current standards of calibration?

A: No. When we're able to work properly with a physical standard, then we'll have a better handle on what performance has been. But we've recently been able to get a more repeatable result with the secondary standard than we were able to do over the last year. We've taken control of that enough that we're able to repeat a measurement...narrow down the range of variability from one shot to the next.

Q: So what's the current range of variability?

A: In terms of population impact, probably ten percent. So you go calibrate your system today and calibrate it tomorrow and the effect of the difference in the calibration would be a ten percent change in the population that you're surveying. Which is much less than the uncertainty of the behaviourial aspects of the population that you're dealing with anyway so it's probably sufficient. You could spend a lot of money and bring it down more but...

Q: So the other uncertainties don't make it worthwhile to have more accurate equipment?

A: That's my personal opinion, yes. I think they're so far off on the behaviourial aspects that it's not prudent to spend a lot of money to come up with a very, very good calibration.

Q: So in your opinion the behaviourial unknowns are the biggest source of uncertainty in the data?

A: With the possible exception of survey design.

APPENDIX E

<u>Interview with Larry Coady, Acting Director of Science</u>
<u>Conducted in St. John's, Newfoundland</u>
<u>July 26, 1990</u>

[Response to general intro from me re the confluence of social interests and influences being brought to bear on DFO science.]

A: Well, I don't pretend to understand the sociology of the fishing industry. But I can certainly provide you with perceptions about how I see fisheries sciences' role in the fishing industry changing over the last ten years and where I see it going over the next little while.

This lab dates back to about 1931, set up in Bay Bulls with a very small group of scientists. They had a five-year mandate intitially. Their first order of business was basic biology. Where are the fish? And also work on seafood technology like the improved utilization of cod liver oil, stuff like that. Some pretty basic fundamental things.

At confederation in 1949 we became part of the Fisheries Research Board. The first major research movement offshore was when we got the A. T. Cameron, a ship that gave us the capability to move offshore in a big way and to go north as well. The first few decades were involved almost entirely with exploratory fishing. We advised the fishing industry about where fish were, where they were to be found. We also worked on gear technology introducing, for example, certain long-lining technologies to the near-shore fishing industry. So the direct benefits were very tangible.

In the early 'sixties, in response to the increasing international fishery, there was more pressure for information and advice on mesh-size restrictions. The first Total Allowable Catches were introduced. And then scientists got more and more involved in assessing the abundance rather than the availability of fish. And so we switched from basic biologists to quantitative biologists--assessments oriented people.

This attained greater importance in 1977 when jurisdiction was extended to 200 miles. All of a sudden, we were faced with the responsibility for providing advice on the status of 24 groundfish stocks, 19 pelagics and shellfish stocks, all of the marine phases of Atlantic salmon including maritime stocks as Canada-Denmark issues at West Greenland and as arctic char stocks in northern Labrador. No more blood and guts biologist. We were looking at people with a very strong capability in computer science and statistics.

Then we were asked to provide five-year projections of stock status. We weren't able do it. You might as well have gone and bought a crystal ball or put on a magician's hat and pull out a piece of paper. It couldn't be done but we were obliged to do it anyway.

Q: Where did that obligation originate?

A: From fisheries management and the fishing industry who had to know. We had access to the 200 mile zone and expected to increase our presence in that zone as foreigners phased out.

Q: So you feel pressure from the upper levels of management and Ottawa to do a job that you know can't be done?

A: I wouldn't put it that coarsely. There was a genuine interest at that time in knowing what the future held for the fishing industry. Companies had to go out and buy ships. Should they buy ten trawlers or should they buy twenty trawlers? Where were stocks going?

Keep in mind that we'd just gone through a period where there was massive depletion of the most significant stocks. I mean there were northern cod catches in the 800,000 tonne range about 1960. Well, here we were trying to rebuild the resource and trying to provide advice to management and to industry as to where we were headed and how fast. To give them some information.

Q: Of course their demands are for precision because they're dealing with 10, 20, 30 year amortization, capital expenses, etcetera, they want to plan how many ships to commission, how many crews to hire, how many plants to open or close and their demands are for fairly precise knowledge.

A: Yes.

Q: Now as far as making projections as to what the state of the resource and the TACs are going to be in even five years, from what I understand of your business so far, it's very speculative.

A: It is.

Q: The environmental unknowns, the behaviourial unknowns...there are so many sources of uncertainty.

A: Exactly.

Q: That this is the nub of it. The source of the conflict between the larger-order social reality and science is that they expect science to provide absolute facts.

A: The word "science" conjures up images of absolute precision, yet there are many types of science. Mathematics is probably the purest form of human thought. If you apply your formulas properly, you cannot make a mistake. Fisheries science is not as precise.

Q: Here's where it gets interesting. Mathematics has always been billed as the language of science.

A: Statistics is not mathematics but a mathematical science.

Q: Do you think that there's been....For 300 years or so science has enjoyed a position of epistemological privilege. Since Newton certainly, science has been accepted as the authoritative form of knowledge. And society at large has accepted that. But in recent days, there have come to be challenges to science's authority.

A: I really wonder if science has been that well-accepted since Newton's time. The theories these guys were professing were that the earth was not the centre of the universe. Heresy, condemnations and all that. Beyond that Darwin's theory of evolution. Look at the fuss that created: a challenge to religion. It may have been more recently, at least in my view, since the space age where man has put the species on the moon where, and medicine with the direct benefits to the human species, where science has really gained more general acceptance.

Q: It delivered the goodies?

A: Einstein brought a lot of focus to it with a theory that no one understood but he was accepted as a genius. Maybe it was Einstein, maybe it's 20th century communications. Maybe it's better education. But people view science as being extremely precise. Science is science.

But there are many forms of science. Math is math and then there's weather forecasting. Economists use statistics, psychologists use statistics and most statistics professors will warn you that you can use statistics like a drunk uses a lamp post, for support rather than illumination. So you have many types of science.

I like to think of fisheries science as somewhere in the middle and wandering towards the mathematical end as the methodology improves.

But the big problem, the five-year forecasts, for groundfish but even more so for other species, short-lived species, we were speculating on where the resource was headed for animals that weren't even born yet. Mother nature hadn't had a kick at them yet and who was to say that the average productivity and the average recruitment to the stocks as we saw them in the 'fifties and 'sixties would continue?

Q: I've looked at the resource prospect publications going back to the late 'seventies and they're quite striking. You have the bar graphs with the actual catches for the previous years on the left going up and down showing considerable variability and then the prospects on the right are these beautiful linear ascending stair steps.

A: See, that was assuming that the only change was management practice and that we had control of it. Prior to that it was more hit and miss. Preemptive sort of stuff.

Q: Do you think this has something to do with the old order of...the concept of the balance of nature. If there's some Rousseauian notions still lingering around. That nature, in the absence of man's interference nature finds a perfect balance. But when we do interfere with natural systems, we also have the power to control them. That we can then create whatever balance we want. Whereas it's now becoming possible in the last few years to think that natural systems are perhaps not balanced but in fact chaotic.

A: Fisheries science is a very new science. Canadian fisheries scientists, on the international level, are ranked among the best. If you look at the fisheries literature you'll see evidence of Canadian pre-eminence. And that should be no surprise. Canada has one of the longest shorelines and one of the largest fisheries to manage. So we've developed the expertise to support that.

What fisheries science has learned over the last eight to ten years is to be more precise with the statistical methods that are available to them. As the data time-series improve, you have a much better understanding in hind-sight as to whether your predictions were off-base.

We only had the first comprehensive offshore groundfish survey in 1982. It was the fall of 1988 that we realized that we had a a major problem in our assessments. We recognized and corrected the problem in six years which is good science.

Q: It seems like stock assessment science is at the relatively early stage of development where you are just beginning to be able to identify and quantify the error factors, the levels of uncertainty that you're dealing with. It's still a very new science and yet you happened to intersect with a phase of the economy that is characterized by a rather hard-nosed, cost-accountable approach to public expenditures. And yet, from the outside, it looks like the people affected by your work, the consumers of scientific knowledge, are getting less reliable information for their money rather than more.

A: We will continue to use statistics and the jargon of statistics reflects very well what we're dealing with. You've got terms like "bias", "precision", "confidence levels", "error factors" and "adjustments." The new assessment methodologies that have been developed by Canadian scientists (e.g ADAPT) are proving to be among the best around. It deals more objectively with what the different indices are telling you. It tries to explain inconsistencies. Rather than treating your research vessel data and your catch/effort data subjectively, we developed a way to treat the data objectively and rank and weigh these indices in a more statistically objective way.

Q: You mentioned '88. I think it was a change in the weighting of commercial and RV data.

A: We had looked at the two and they were telling us totally different things. We made a judgement call which was in favour of CPUE and learned later that we called the wrong one.

Q: But for some time more weight had been given to the commercial data than the RV data. Why was that?

A: Thinking back to the assessment documents of that period, the feeling was that if you took the average productivity of the stock the catch/effort was probably more in line with what was happening. We had information on the average recruitment to the fishery in the northern cod stock. And we used that average. But what we found was that the productivity of the stock in the 'eighties was far less than we had imagined, less than even any of the low points back in the 'sixties.

Presently, there are some preliminary indications that we have a couple of good year-classes coming through. In a year or so we'll have some firmer indications so there's some sense of optimism that productivity is coming up a little bit. The feeling is that we're going to have a downturn yet for another year or two and after that let's keep our fingers crossed and see what happens.

It was the use of the CPUE and RV indices plus the fact that the productivity through the 'eighties was atypically low that led to the assessment problem.

Now keep in mind that the year before, the fall of '88, there were problems with the inshore availability of cod. The federal government created the Alverson task force. An international group of scientists, two of which participated in Harris; Alverson and Pope. They reviewed the scientific methodology, looked at our reasons for why we felt the cod weren't coming inshore and concluded exactly the same as we had done. That the stock had indeed rebuilt five-fold since 1977. The following year we recognized that we had a problem.

Q: Certainly for their assessment they must have been relying on the same data and methods that you were.

A: They were used a number of methods; e.g. the Laurec-Shepard method. They were evaluating the methods of our scientists and they were found to be as good as any available.

Q: That might be the key to this conflict. What's "good as any available" is not good enough for commercial and political purposes.

A: The responsibility of fisheries scientists is not to determine management strategies. We are asked to supply advice with a target fishing mortality of F 0.1 which with northern cod represents 20 per cent of the exploitable biomass. Managers ask, "At F 0.1, what should the Total Allowable Catch be?" We give them that information and they incorporate social, economic and other factors before taking a decision. That's been the way it is and that's the way it continues to be. Scientific advice is just one contribution to the management process.

Q: Do you feel that in the last year or so, scientific advice has not been used rationally, that science has become something of a...

A: We revise our assessments every year. They change each year. Prior to '88, estimates were revised upwards. No one worried about it. The first time we revised our estimate downward, it became a "mistake." We do an annual assessment, we correct as our times-series develop. Stock assessment will continue to be a correction, year-to-year.

Q: Do you have any guess, off the top of your head, as to what the level of uncertainty is for northern cod?

A: The general rule of thumb is that the best we will ever achieve is plus or minus 20 per cent to 25 per cent.

Q: Are you achieving that now?

A: No. But the ADAPT framework that we're using now, increased survey coverage and other factors, will bring it down.

Q: Where do you figure you are now?

A: Harris deals with that. That instead of an assumed fishing mortality of 20 per cent it was up around 40 per cent or higher and so that's where the difference was. And that was due to a lot of extraneous factors like by-catch and discards and predation, seals and everything else that comes into the picture. We're accommodating those variables the best we can. In some cases we're relying on foreign data which is not as good as ours. We are dealing with these things and incorporating them into the assessment models as best we can.

Q: Hydroacoustics is being billed as a way to acquire a large amount of data for less money, at least as opposed to trawling, physical sampling. But there are still rather large sources of uncertainty in the interpretation of the return signal and the biggest of these is fish behaviour. As much as 60 or 70 percent.

A: This lab has more expertise in hydroacoustics than the other DFO Regions. It's a very strong program. We've got people who are dealing with companies like Biosonics where Biosonics is actually providing us with prototype equipment to evaluate in the field.

Q: Biosonics is from where?

A: Well there are two major companies that produce high-resolution sonars. They produce ships' sonars primarily but are getting more involved with high-resolution sonars for fisheries applications. One is Biosonics which is based in the States but has an office in Canada and the other is Simrad which is Scandinavian.

Our people have also developed their own systems in-house which reflects the fact that there was nothing off-the-shelf just a few years ago. We've got electronics engineers who have developed a hydroacoustic system which was first used on caplin quite successfully. It has since been modified to a dual-beam system with in situ target strength so you get the target strength as the data is being collected.

We've also developed very fancy data-editing systems for all these billions of blips of information that you get on a transect. So we are on the leading edge of fisheries hydro-acoustics. And there's a long way to go. We've used it on caplin very well and we've used it on herring very well. We're using it on redfish because even though they're groundfish, they don't sit on the bottom but move up and down so it's a better way to enumerate stock status.

Right now there's a push to use it for cod. We can use it for tracking cod schools and enumerating school dimensions, particularly when you're dealing with massive amounts of fish. We can also go out in February when they're spawning or just before spawning on the continental edge and try to get another independent estimate of abundance which is what we've been working on.

If you look at the European experience with hydroacoustics in evaluating the abundance of demersal species, groundfish species, there are mixed signals. Most people would say we've got a long way to go and that's true. Species like flat fish you don't get any target strength at all. They don't have an air bladder. They're tight to the bottom and you get a lot of backscatter; geophysics becomes a factor. The nature of the sediments behind the fish. So there are all these factors.

But what we're saying is that the technology holds tremendous promise. To develop independent measures of abundance. And if you look at the northern cod science program being mounted over the next five years, there's money in there for technology advancement in hydroacoustics.

Q: From what I understand this money just about doubles the science budget.

A: It doubles the science budget for northern cod. It doesn't double the science budget. Two years ago we spent about 20 per cent of our budget on northern cod. Last year we doubled the amount of money spent on northern cod, this was in '89 after the interim Harris report, which effectively increased it to 30 per cent of our operating budget. With the northern cod money coming in that will go to 40 per cent. So right now and for the next five years 40 per cent of our resources will be focused on northern cod.

Q: So roughly, before this 42.8 million dollars [over five years], what was the normal operating budget for the science branch?

A: About 3.6 million dollars a year for northern cod, last year we doubled that and this year we're adding...that 42 was over five years don't forget.

Q: So that's about 8 million a year extra.

A: Right. This year we're getting about seven so we've gone from...

Q: That's an astonishing increase in northern cod resources.

A: At long last!

Q: How are you going to spend all this money?

A: Last year we received about 3 million dollars in June and we spent it extremely effectively. We purchased workstations for our CODE group, resource assessment methodologists and our oceanographers. We modified the Gadus, which is our primary hydro-acoustics research vessel, to deploy and retrieve towed bodies in ice. And that worked extremely well last February. We cleared up the backlog of observer data and purchased some trawl monitoring sensors. So that money last year was spent very effectively.

Right now we've got scientists involved in planning sessions for about 20 projects. We'll nail down work plans for that money within the next week or so. So I've no doubt that the money will be spent effectively. We've got the benefit of last year's experience and some of these projects are extensions of what we did last year. For a number of the new initiatives we've got them arranged so that they won't start till the second year so we have more time to plan. We want to make sure that we get the best bang for the buck.

But we are talking about an entirely new dimension in cod research here. We're looking at the role of cod in the ecosystem. If you're a marine ecologist, you can do research for the next 150 years and still have twice as much to do. So it will have to be well-focused and relevant.

Q: Do you have any concern that Ottawa and the general public are going to expect more fish for this money?

A: I question how they can because spending money on research doesn't increase the size of the fish stock. It will simply allow you to determine more precisely, more correctly what the size of that stock is and provide a better understanding of cod's role in the ecosystem in support of assessments.

Q: I would suggest that it's clear from the tone of the television and newspaper coverage, comments and criticisms from the corporate and political sectors, that they not only don't understand the role and nature of science; they are not interested in knowing.

A: The new research will allow us to provide better advice to managers. Better advice which means we can manage the stock to allow rebuilding at a faster rate. There's already evidence of change in the advisory process where we've started providing a range of options. This year, for the first time, we're saying we'll give you a range of options. Let's assume that this year the recruitment is high or low. Under each of those scenarios if you have a fishing mortality of 20 per cent, this is what's going to happen to the stock in the long-term. It's going to increase. If you have a 30 per cent mortality it's going to stay the same. If you have a 40 per cent mortality it's going to decrease.

Q: So for the first time you're providing your advice in a way that makes it clear that the choices made are management's choice and not science's choice.

A: There will always be some uncertainty attached to the advice we provide. It aint easy counting fish. It aint easy and it never will be. And yet weather forecasters would probably find our track record enviable. The Economic Council of Canada would find our track record enviable.

APPENDIX F

Interview with Jean Hache,
Assistant Deputy Minister for Atlantic Fisheries
Conducted in Ottawa, Ontario
November 2, 1990

[standard intro re request to tape record, confidentiality of anything designated as off the record, explanation of archiving and review of raw transcripts]

Q: We're on the air.

A: This is for your thesis or part of your studies or what is it?

Q: [short explanation of my academic standing and future plans] Since my time is limited I'll be quite direct.

A: Sure.

Q: As a manager of a large and valuable public resource, what do you expect from science? What do you need from science?

A: Essentially what we need is good, solid, reliable advice on the state of the stocks. How good they are or how bad they are depending on the situation. And how much fish can be harvested from those stocks. That's the essence of what we need.

We also need science, generally speaking, to be able to explain, clearly, what is the state of those stocks. And what a certain degree of harvesting, a certain level of harvesting will do to the stocks. And science must be able to communicate, not only to us as managers but specifically to the industry. To the participants in the industry. If you have a fish stock, whether it be northern cod or redfish in the Gulf or whichever, if you harvest 50,000 tonnes, this is what it's going to do. If you harvest 100 or 150 or whatever level. And also move on to the implications of the method of harvesting.

And this gets us in to another dimension which I think is very important. The method of harvesting, in that if you use draggers you may be catching a certain percentage of small fish that, theoretically at least and practically also, you would want to protect. Let them grow and spawn, etcetera, etcetera. So what are the implications of that versus longlining? Gill netting versus cod traps, and so forth. And be able to communicate the manner in which the stock generally, the biomass, is affected by these different methods. And steer the industry, generally speaking, in the right direction. The buzzword these days is "sustainable development" where you harvest a good level of the fish but you leave enough for growth or sustained development of the stock.

Q: That's certainly the ideal. The theoretical idea of how science should perform. The information that it should provide. Do you feel that it's doing it's job, currently?

A: I think it's doing a good job at most of those issues. There's room for improvement, however, in many of those areas. Keeping in mind, of course, the resources that they have available. It's obvious that if you have X number of scientists with two research vessels you can do so many things. If you double all of that you can do so many more things. And have that much more accurate information. That being said, I think that the quality of the information that we get, the quality of the scientific advice that we get is improving. It's getting a lot better.

And, I think, only recently, perhaps in the last few years and perhaps in the past year, we are placing more emphasis, not quite enough yet but more, on the communications aspect. On explaining to everybody, to the managers, to the industry generally, to the public what this is all about. How it works.

Q: It's no secret that science, particularly in Newfoundland and the Atlantic Region, has a severe credibility problem.

A: Yes.

Q: That, in fact, it has come under rather substantial criticism, especially in reaction to the large reassessment of the northern cod stock. And this must create very specific and serious problems for you as a manager.

A: It does in the sense that you are then dealing with an industry, as we do in the advisory process, in the advisory committee process, you're then dealing with an industry which is a bit sceptical about the scientific advice. And does not understand how a stock can staty for a number of years at a given level and even the projections are that it will sustain or grow and all of a sudden it falls. There's a reaction I suppose. You don't understand and you have a very serious credibility problem. And I think it is based as much or more on the communications aspect as it is on the quality of the scientific work itself.

Not being a scientist I hesitate to make very firm statements on science but human beings are human beings and if you establish a rapport, a degree of credibility, with somebody, with a group, with a client group, a degree of credibility whereby you explain how the stocks grow and why they grow and what will make them grow. You're talking about the protection of small fish, the environment and the habitat, etc. etc. If that is well explained and communicated then I think it will become easier to explain, at the same time, the downfall. In other words, if the credibility is established in the good times I think there is a better chance for the credibility to be maintained in the not so good times. But perhaps that has not been done.

When times are good we tend to take things for granted. The stock's going up. We don't need to. And again, human beings are human beings. If I'm a fishing industry person and you say northern cod is going to go from 175 to 210 you say great, thank-you. And you're not so much interested in knowing why. You just take it. But if you are told that it's going to go from 210 to 170 you say what happened? Why? How?

Q: And who's to blame?

A: Yes.

Q: Now, what seems to be happening from the scientific perspective is that, whether science was responsible for this itself or whether there were other outside assumptions, it seems that from '77 until very recently there were unrealistic assumptions about the degree of precision with which science could estimate the size of the stock. The current size and the projections it could make. The degree of precision. What seems to be happening recently is a reevaluation of the probabilistic nature of the work that they are doing. In fact, they are dealing with much higher levels of uncertainty than were previously thought. Again, this has very serious implications for both management, the public sector and the private sector, fishing interests. Is this an issue that you're aware of?

A: That angle might be getting a little technical or scientific. From my perspective, what has been happening, which I think is good, over the last few years is that the industry and, again, the managers generally, are presented basically with two things. With facts. This is where we are. This is the result of our survey and the result of the information that we collect from the industry, from the fishermen etcetera. These are the facts. Now, based on the facts, these are the options that you have. You as managers or stakeholders or decision makers. These are the options that you have.

If your objective, your objective I stress again is the industry's objective generally and that includes the Department, if your objective is to rebuild a stock rather quickly then obviously you will want to reduce the level of the catch as much as possible. So if your objective is to go from 50,000 tonnes and, within the next three years or five years you want to build that stock to 100,000, theoretically speaking, then you will reduce your catch quite drastically for a few years. If your objective is to go up to 75,000 instead of 100 then you can increase your level of catch. And you will still get there. It may take you a few more years. If your objective is simply to maintain the current level, if the socio-economic environment is such that you say 50,000 tonnes is what we think is the optimum, the you can increase the catch level.

So that the ultimate responsibility, the ultimate advice to the minister, and in the final analysis he decides, but the ultimate responsibility for the advice is with those who will be most directly affected.

Q: You began your answer by saying that the scientists present you with facts about the current state and then options that are predicated upon these facts. What if the facts are wrong? What if there aren't hard facts but large degrees of uncertainty? What if, as seems likely now, you are dealing with levels of uncertainty of plus or minus 25 to 30 percent? As far as estimations of stock size go. And this is a commonly discussed range of uncertainty as to the state of the art.

A: Yes. It's not an exact science by any means.

Q: So, if you don't have hard facts to work with, reasonably hard facts. Then how are decisions made?

A: Well, then we have a problem I guess. We have a problem in that we are still presented with scientific advice. And that gets us into the quality of the scientific advice. Because we still receive through the scientific process advice on stock X. Whether or not that advice is excellent or highly accurate or not so good or not accurate at all is something that, I guess, no one will know for sure. The results show down the road some years later. And that's where, as I said earlier, if you have twice as many scientists or better scientists or more equipment and so forth, sure, you can do a better job.

But the quality of the advice is something that I'm not sure to what extent it is possible there and then to ascertain that that advice is very good or not good. You have to wait until the results show up a few years down the road. And perhaps that's the down side of an inexact science of that nature.

Q: So the reassessment of the northern cod stocks that came in light of the Alverson commission and so on, of course there was the later Harris commission, must have created some problems in the management structure. Because it seems then that you can't trust science's advice to you because they got it terribly wrong and that caused a great deal of trouble on the federal level. Some embarrassment. Some confusion perhaps.

A: I'm not sure that I would say that you can't trust it. You know the weather man in the morning tells you that there is a probability that it will rain at, I don't know, 90 per cent or 10 per cent or whatever. Most of the time he's right. Some of the time he's wrong. Whether or not he's wrong once in a while, whether or not you choose to disregard him because he has been wrong a few times, becomes a question of, in a sense, what degree of perfection you expect, you anticipate.

The same can be said, I suppose, of other activities in every-day life. If scientists are wrong, as they have been and as they will be, I'm much more interested in knowing A) are they prepared to admit that they were wrong? B) can they find out why they were wrong? And C) can they do something about it? To try and prevent that same mistake from occurring again. My view, I guess going in hindsight, is that they have been right much more often than they have been wrong.

Q: What if, in this case, it wasn't a mistake but that the nature of the system that you are dealing with, the ocean climate, the inorganic and organic system and the interactions between the two, is so complicated, so complex, the dynamics are so non-linear, that this 25 to 30 per cent plus or minus is the best degree of precision available no matter how many resources you throw at the problem? What then are the implications?

A: I think you've put your finger there on one of the key elements. You can have the best scientists in the world and the best equipment. The fact is that Mother Nature is probably the most important factor in all of this. To what extent one can control the fact that in a given year you have an excellent year class and the following you don't. It's a disaster. You say, "Why?" And all those other environmental factors, "environmental" in its broadest sense, the ocean climate and whatever other factors one may be talking about whether it be acid rain or so forth. How long it will take to know what impact those other factors have, how much they influence recruitment, the pattern of stocks et cetera.

Perhaps, again as earlier you mentioned, it is, and the scientists are, I think, the first ones to admit, it is an inexact science. It is not a two-plus-two type of equation that we're talking about here. Given the fact that we're dealing with a population that is miles down in the ocean and you are still trying to count them without seeing any of them.

I think it's rather remarkable that we have achieved the degree of precision that we have given all of those obstacles. And I don't know if one can say that it is 20 or 25 per cent, you know, within that range of exactitude, or inexactitude. But the fact is that it may be possible to become more precise, I'm not sure.

As on the one hand science and scientists, the equipment are getting better. There's no question about that. At the same time, the other factors, the external factors, the environmental and so forth, are becoming more important and I don't know how long it will take to have a more precise and solid handle on the facts controlling, or the facts that we know control the stocks.

Q: Whats' becoming clear to the scientists, I think, is that it's tough enough to tell you what's out there right now. As far as projections or predictions, it's becoming clear that that task is not only extra-ordinarily difficult, it may be impossible to make useful predictions for more than a season in the future. Because recruitment is so highly variable. Because there are so many factors affecting the system. For instance a strong example of this problem is the Kirby Report. It was projecting TACs for '88, '89 of somewhere around 400 to 450,000 tonnes of northern cod. It's 197 [thousand metric tonnes] this year and it's headed lower most likely. Given this unpredictability in the system, this conflicts with some of the assumptions of management. Because management is largely about the future. Creating a desired future. Working toward desired future conditions in light of stated human social goals. Or economic goals. What if the system is unpredictable to any useful degree over any significant period of time?

A: I'm not sure that I would say that it is unpredictable. I think what I would rather say is that it is unpredictable, perhaps with a 100 per cent degree of certainty. And it is predictable, perhaps, not in the long term. It is, I think, predictable in the immediate future. I am relatively confident that whatever the scientists tell us the stocks will be in 1991 and 1992 is fairly accurate. Again, within the percentages or within the degrees that we referred to earlier.

If anybody says, "Well, what about 1995?," then I would start getting sceptical. Because it's too far away, I think, to have a very high degree of confidence. Of the projections that far down the road and perhaps that's what happened in the example that you referred to with regards to Kirby.

In my mind, what I think we have to work with now, the tools that we have to work with now, the data, the advice, the analyses done by the scientists are generally reliable within reason. It's a bit like a Gallop poll. It is reliable within reason. Sometimes it's wrong. Generally the industry and, I think, we accept that that's the best advice we have. It's not 100 per cent accurate.

But if, as I mentioned earlier, we go through the necessary effort of explaining that advice and communicating the hows and whys. How we arrive at that conclusion. What is it that we know for sure. What is it that we do not know. And not be able to admit or to explain why we cannot give a specific answer. The industry, collectively I think, are generally very wise and will see through flimsy explanations. They would rather be told the truth, no matter how bad it is, rather than be sold something that will not stand the test of time. And I think if we go through the effort of doing that, the degrees, the probabilities that sometimes science and scientific advice will not be exact are acceptable.

Q: Of course for the industry's point of view...I understand that the industry is quite sensitive to variability. To uncertainty. Because large corporate industries are based on five and ten year financial plans, amortization and depreciation schedules of trawlers and plants and so on. So to some extent the overcapacity in the industry right now is due to the optimistic errors of the Kirby Report. The crisis in the fishery then was not a stock or biological crisis. It was a fiscal crisis. The Kirby Report was really about the unprofitability of the industry. Not a crisis in the stocks. And the projections for only five years later being inaccurate by approximately a factor of two. To some extent I believe that the predictions from DFO, from the science and management side and the economic branches are responsible for the unprofitable position that the industry is in right now. Because they built more ships and so forth with the expectation that they would be catching a whole lot more fish instead of a whole lot less. So industry is not awful pleased with science and with DFO right now. Some people have been quite clear about that.

A: Oh, I have no doubt. No doubt that the degree of credibility can be improved. There's absolutely no doubt about that on the scientific advice and I think it can be improved. That being said, I would not hasten to ascribe blame here or there or any where else. There's plenty of blame for everybody to share in. There's no question about that.

There are other factors that I think must also be taken into account. The stocks...There is overcapacity, both in the harvesting and the processing sector on the Atlantic coast. A tremendous overcapacity problem. We have far too many trawlers and draggers of all kinds chasing the quantities of fish that are available. We have far too many processing plants to process the fish that are available. There's no question about that. When the conditions are good, when all conditions are good, all of those can survive and even do quite well. But when you have external factors that come in to play, and it can be any one or a combination of, market price, if there is a down-turn in the market price for cod in the United States for what ever reason. They prefer Alaska pollock because it's much cheaper. If the Canadian dollar goes from about 70 cents two years ago to about 86 now. That, for the Canadian industry, is a loss of, I don't know, around 20 per cent margin that you don't have any more. That's a hell of a blow. When you have, as we have right now, an increase in the price of fuel, it's going to go up. I don't know what percentage. It depends on events on which we have very little control. All of those factors.....And it also depends on how the fishing industry is doing in Europe. If they are doing well obviously our markets in Europe will be affected. If the herring fishery in the North Sea is very poor then obvious our herring industry will do better because the markets will be open. If the Emperor in Japan dies, and they stop importing high quality stuff because they don't eat high-quality product in periods of mourning, that will affect our market tremendously and it did.

So all of those factors, coming sometimes one at a time, sometimes two, sometimes all of them at the same time. And if you have an industry that is operating with too much capacity and it's hit by a number of those factors, it's more likely to be affected than if you did not have those other problems. So I guess you are dealing with those uncertainties that you have in the management as well as those uncertainties that you have in the scientific analysis.

Q: Is it fair to say that DFO has a great deal of influence, potentially, in determining the size and the structure of the industry? You can create incentives or disincentives that will help shape and size the industry.

A: We have some influence, I suppose. We have some influence, for instance, on the number of fishing vessels. Because there is a limited entry policy so that you and I cannot get a license tomorrow unless we are qualified fishermen and we buy a boat already in existence. So there's a ceiling there. That being said, there are other ways in which the industry can and does increase its capacity.

For instance a 25 or 30-year-old trawler is replaced by a new trawler of the same size, as it can. Chance are, that with the new technology that you can put on board now, the new gadgets that they have on board vessels, the fish finders and all this incredible technology, you increase the effort. Engine power. You increase the effort. But you increase the cost tremendously. And of course if you have a vessel that costs three times more than the other one you will need more fish to pay for that vessel.

In the same manner on the processing side, DFO has no control over that. That's provincial. So if a province....So if John Smith decides to build a plant somewhere, unless the province says, sorry, we are not licensing new processing plants. You cannot build a new plant. Then you have an increase in the processing sector. And as the processing sector increases, you have more processors putting pressure on fishermen to bring in more fish.

So in a way, I suppose, DFO does have some control but by far it cannot control, by itself, any limit on capacity.

Q: Given that there is an overcapacity in the harvesting sector now, the offshore fleet and to some extent the 65 footers. And it looks like the TACs are headed lower and lower for conservation reasons for the foreseeable future. What is going to be DFO's response to....Well, first of all, do you think it is necessary to down-size the harvesting sector to match the down-sized quotas? And, if so, how do you plan to go about it?

A: Well, I think if we're going to have a stable industry that will have a certain degree of comfort and not be constantly on ups and downs, yes, I think you do have to do something about the harvesting capacity. There are two ways you can do it.

If you have a harvesting capacity that is at level X and you have a resource that is at level X minus 10, you can do one of two things. You can increase your resource to level X by rebuilding stocks. And that is not necessarily possible. Or you can decrease your harvesting capacity to X minus 10. How you can decrease your harvesting capacity, I think there are two ways.

One is that which has been done in the past in certain fisheries is you have a buy-back program. Buy out licenses. I'm thinking about salmon licenses. Particularly in the Maritimes over a number of years there have been a number of buy-=back programs. The other is that you let the industry do it itself. Through buying each other out.

And that leads us to the individual transferrable quotas. Which are in place now in a number of places on the Atlantic coast. A number of fisheries. A number of regions. If you and I are fishermen and I decide for some reason that I'm retiring or I don't want to fish any more, and you simply buy my quota. I sell you my share of the fish that have been divvied up before based on our historical participation in the fishery. And in that way the number of participants can be reduced to a level that will find its own...that will define itself over the long run.

For instance in the Bay of Fundy herring seiner fishery. I think about six years ago there were something like 51 herring seiners. And through this individual transferrable quota the number of seiners has gone down, I believe, to something like 39 or 40. And now the stocks are at a fairly healthy level and the number of seiners has remained constant for the past few years.

Q: So by privatizing the resource, or transferring a public resource into quasi-private property through transferrable quotas, you then allow market mechanisms to...

A: Determine the level of harvesting effort. And then it's up to the industry to decide. If I feel that I have enough fish to make a decent living of it I will stay in the fishery and I will continue to fish. But if I feel that I don't have enough I can do one of two things. I can sell you my share or I can buy yours or somebody else who's willing to sell. And if I combine two quotas....And there are variations on this theme. Both you and I can decide to stay in the fishery but rather than use two boats to harvest your quota and my quota, we use one boat. We join forces and we harvest our quotas in a much more economical manner.

Q: Do you think this principal works well enough to be applied more broadly in the fisheries to other stocks? I'm thinking about cod stocks.

A: I think so. I think so. It is, in fact, being applied right now to certain cod stocks in the Gulf of St. Lawrence. For the last two years. We will be implementing such a program on the Scotian Shelf starting in 1991. It has been in place for the offshore trawlers, the large offshore fleets since 198?...not transferrable though. I should specify. Not transferrable. Just individual quotas. They are transferrable only on a temporary basis. But the individual quotas have been in place for seven or eight years. But the transferrable part of it will be in place in the southern Gulf of St. Lawrence, for instance, in 1991 for cod stocks. And it's in place in a number of other places. I mentioned the herring fishery earlier and I think that's the direction in which we are heading.

I say we, I mean governments and industry. That the semi-privatization or quasi-privatization, I'm not quite sure what the right word is, of a public resource will lead to the participants themselves basically deciding what level of harvesting capacity they need. And to turn the industry into the optimum economic benefits for them.

Q: You've been very generous with your time. Do you have any brief, concluding remarks you would care to make? Where we are headed?

A: I think where we are headed, and I think, generally, the direction we are heading is a higher and higher level of co-management. Between governments with an "S". The federal government has the responsibility for fisheries management but the provinces are involved to a large extent. Especially in the processing sector. And industry. More and more the industry are involved in the decision-making process. And are aware of certain policy decisions that are made and the directions that are taken.

Changes in the fishery, as one finds out, do not come quickly. It's an industry that is very much, largely, based on tradition as much as anything else. And it takes time and patience to change things. I don't think one can think of fisheries management in terms of revolution. It's a very slow evolution. One year you might get an enterprise allocation but people say that transferring quotas is totally out of the question. We are not prepared for that. We are not ready for that. And it may take two or three years for the industry, the fishermen themselves, to come to the conclusion by themselves that, yes, it would make sense if I was allowed to sell my quota to you. And it just takes time for those changes to occur.

If you just look back 12 years to 1977. The 200 mile limit. And you consider (I'm thinking groundfish here) we had no such thing as a fishing plan 13 years ago. The fishing plan, the quotas, the sub-division of quotas between fixed gear and mobile gear, offshore and inshore and all that. That is only 13 years old. And if you think of all of the progress that has been made since, in various management measures, and all of the progress that has been made in the scientific information. God knows we still have a long way to go.

But I think one must not lose sight of where we are coming from. And it is a relatively new industry in terms of management. In terms of the way it is managed. The idea of enterprise allocations, of individual quotas, is from the 'eighties. And the idea of transferrable allocations, transferrable quotas, the selling of that quasi-property from one fisherman to another, is even more recent. So that gradually, I think, we're getting in to a direction that seems to be acceptable by the industry.

I think we must not kid ourselves either that that will be the solution to all of our problems. The fact is that we are dealing with a limited resource. And it can not sustain an infinite number of people. It is limited and it has a limited economic potential. It has a limited socio-economic impact on the Atlantic coast, for instance. And, in that sense, whether the existing resource is divided into quasi-property or whether it is public property, common property, the fact is that it is still a limited resource and, from the socio-economic view we will always be faced with the dilemma, if there's nothing else in community X or Y, should more licenses be issued? In other words, should the fishery be the employer of last resort if there is no other possibility?

And I think what seems to be happening now, what has been happening for the last number of years, is that, no, it should not be the employer of last resort. It should be managed as a limited resource but the objective should be as much to increase the income of those who are in the fishery than to increase the number of people dependent on that resource.

APPENDIX G

Interview with Dr. Leslie Harris
St. John's, Newfoundland
August 29, 1990

Q: Could you give me your understanding of how this crisis came to be. What were the contributing factors?

A: I think the principal contributing factor to the creation of our panel was, in the first instance, the successive years of decline in the catches of inshore fish and in the size of the fish being caught, which was noticeable to fishermen. And the tendency of the inshore fishermen to rule out the natural phenomenon theories that might explain this and the counter tendency to blame the offshore exploitation and, therefore, to cast doubt upon the accuracy of the forecasts that were being issued by DFO.

On the other side of the problem was the statistical abberation that emerged in respect of the '86 survey data and the subsequent drop in the numbers indicating that something might be wrong with the models heretofore used to make the forecasts.

I am referring to the '86 fall survey which was subsequently shown to be aberrational, but which was fed into the system and used because at the time it tended to fit the growth projections that had emerged from earlier applications of the model. The subsequent realization that the figures were aberrational led to attempts to justify them, though soon there was a realization, which was a quite obvious conclusion I suppose, that the data series and data sets that they were using were of such short duration that they hadn't had time to converge toward accuracy. The nature of retrospective analysis is such that it promotes backward convergence. The data set simply wasn't long enough to accommodate this.

Also, the models that DFO were using were too reliant on one or two data sets, neither of which was virgin pure. Each of which, in fact, was subject to a fairly wide range of possible error.

The culmination of these things and the DFO scientists being required to say "mea culpa" led the government, because of the pressures from the fishing community, to find a way of addressing the questions. Not to bring in a bunch of high-powered scientists who would probably create new models but a bunch of people whose credibility in terms of their capacity to assess the situation and to state their findings without political bias of any kind, would not be questioned.

I think it was in those terms that I was approached. Not that I was a fisheries scientist or a mathematician or statistician. But that I had a common-sense approach to matters and appeared to be able to isolate critical issues and identify what they were. To get to the real issues through a lot of other stuff. And also to help the process of educating the fishing community and the scientific community about what each was thinking about the other and why.

So when I took on the mission, I was quite clear in my mind that a large part of my function was educational. And when you say that the level of debate was raised and more focused on the critical issues, I take that as a great compliment because that is what we set out to do. I think it worked. I think our discussions with the fishing interests and with the fishing community did focus attention, for the first time, on what were the critical issues in the situation. And also I think, for the first time, elucidated for the ordinary people in the industry and in the fishing community generally some of the more simple concepts that were being employed by the modellers and that related to the manner in which you actually go about counting fish in the ocean.

People were, I discovered, using words and phrases like "F 0.1" , "retrospective analysis" and things like that though, as we soon discovered, other than the scientists, very few people knew what they really meant. I certainly didn't and I don't think any of my colleagues, with the exception of Dr. Alverson, did either. Maybe Max Short, the union representative who had been around the situation for so long had a good idea. Certainly, I found in the broader community and even in places like the AGAC council [Atlantic Groundfish Advisory Council], people who had been working with these organizations for many years, saying "thank-you for making that so clear. I've never understood it before"

So that was part of it. It was an attempt to educate. Not only to make fishermen understand what scientists were trying to do and how they were trying to do it and what the constraints upon their capacity to do it were, but also to make the scientists understand where the fishermen were coming from. And what value, if any, might be attached to their knowledge and wisdom derived from their experience. And how you integrate the knowledge derived from official scientific surveys, on the one hand, and the knowledge, or presumed knowledge, that the fishermen have gained from observation over centuries.

It's a very interesting problem. There is a tendency on the one hand to discount totally. as being irrelevant, the anecdotal information that fishermen posessed and were eager to transmit and which they believed in and the "scientific" information that DFO scientists gathered in the appropriate ways and on which they were inclined to place much greater reliance. So that too became part of our mandate. To see if there were ways in which both kinds of knowledge could be put to use--to the benefit, at least the psychological if not the scientific benefit, but I think both the psychological and scientific benefit, of the organized systems.

I may have strayed a little bit from your question because you question was the background to getting it started. But I think that the real reason why we were called into being was a perceived crisis in credibility deriving from errors that had been made and which were now acknowledged. And the juxtaposed positions on how these errors had come to be: the scientific version that it had been a modelling error based on insufficient data and an inadequate time-series and one aberrational set of results that loomed very large in a very short time-series. Or the fishermen's view that it was a set of errors deriving solely from scientists being unwilling to listen to what the inshore fishermen knew and to put their trust in the information provided by the "rapists", that is, the offshore as opposed to the inshore fishermen. There was a crisis of credibility of the scientific community.

I don't think the scientists themselves felt any particular sense of crisis in terms of their own functions. I think they were fairly confident that what they were doing was fine. It was just that for reasons that were quite explainable they had gone awry and they were prepared to fix it up.

I think on the political level, the credibility issue was one approaching crisis dimensions. So we were called into being as an impartial third party who could look at both sides and reach some conclusions.

Q: My observation is that one of the results of the wide publicity that your report received and the fact that you were able to state the case in something very close to plain English is that, for the first time, the consumers of scientific knowledge, those people with interests in the fishery and the public at large, had a good idea of how really uncertain this business of counting fish is. To the point where there are some interests that are questioning the value of the information at all. If we are looking at levels of uncertainty of 30, 40, 50 percent in the assessment, what good is that? From the commercial point of view, this is unacceptable for long-range fiscal planning. From a management point of view, it doesn't provide the kind of unassailable scientific authority which the politicians need to justify their decisions. For the inshore fishermen it simply confirms what they had been saying. That DFO doesn't know what it's doing.

A: That's true. We, of course, took the position in the end that there need not be such high levels of uncertainty as that. There certainly would continue to be uncertainties; perhaps not forever but certainly into the future until such time as we became much more scientifically competent in a great number of areas that we now are and have accumulated a great deal more data than we now have done

But we felt that, even given current constraints within the system, its enormous complexity and our incapacity to really come to terms with it because of lack of resources, in many instances, on the oceanographic side particularly, that there were technological possibilities that could be applied but that were not being applied. That there were, in fact, possibilities, checks on the data, assessment of data quality, that could improve the situation vastly.

That if you have a reliance on types of data that tend to be error-ridden, that to rely on one such source or two such sources when ten are available is to invite disaster. That by spreading your net over a wider range of options and incorporating increasing numbers of data sets based on different techniques and usages within the fishery, you can, in fact, eliminate the possibility of the kind of gross error that affected the 1986 survey results.

We believed, and still do, that the kind of serious problem that occurred with the survey data in 1986, would not have occurred if DFO had been using, at the same time, and giving some credence to, for example, an inshore index of catch per unit of effort. It could have been mobilized. It would have been difficult but it could have been done. Or if e.g., they had had better acoustical data to supplement their survey data. If they had egg-mass surveys, larval surveys. Even juvenile animal surveys. If they had, perhaps, broken down their CPUE research or data gathering by gear-type, by area, by region. If they had known a little bit more about the differing population groups that made up the stock or stocks. If they knew a little bit more about the migration patterns; whether they are constant or variable. There are all sorts of ways. If they were more conscious of the discard problems, of the unreported catch problems, of the under-reporting and misreporting. If there were better surveillance. There are all sorts of ways that we thought the data could be improved. If they were using the model which subsequently they did adopt that subjected the data to better testing for credibility.

Q: Why, in your opinion, hadn't they been doing all of these things which seem, in retrospect, pretty obvious?

A: I think they hadn't been doing them for two reasons. Two principal reasons. There may be a host of lesser ones. But the two principal reasons were, one, their absolute conviction, their mind set, which showed them that the stock was growing at the projected rates.

They had set out in 1977 with a very optimistic world view. That if you do thus and so, the stock will grow at this particular rate. And the evidence they'd been getting in that first decade, 1977 to 1986, showed, in fact, that it fitted in to the growth curve that had been projected, even the aberrational data from the '86 survey, fitted in to the growth curve, showed that there had been a slight abberation in the previous year in the other direction but that things were now back on track.

The great excitement that came with the 200 mile economic zone and the possibilities that that opened up; finally we've got it under our control, finally we can manage it, finally we know what we're doing, finally we have the power to do what we want to do. And certain goals were set based on the best data they had at the time and the figures coming in on the growth of the inshore fishery during the early 'eighties showing them that everything was on track and everything was fine.

So they really believed that things were going as they wanted them to. As they believed they should. Why bother with other unnecessary labours when what you had was giving you the results that you required? I think that is the first reason.

Q: By "they" do you mean there was a monolithic body of opinion or were there dissenting voices?

A: I think this was the attitude within DFO and within the commercial industry there was another phenomenon. I'm speaking now of the offshore fishery. They tended to accept this world view, that the stocks were, in fact, growing quite satisfactorily thank you, in accordance with all the projections that had been made. Because they were not able to see what was the impact of their improving technology. And of their improving competence to manage the technology.

I think they estimated, both the commercial industry and DFO, underestimated in a very serious way, the effects of improving technology and of improving competence and of improving knowledge and skills among the fishing community. And tended to downplay the effort side of the equation. And they tended to keep in their minds effort as a constant. As if they were dealing with the same kind of trawlers and the same kind of circumstances without realizing that over those ten years there had been a very large growth both in the gear configuration, in gear quality, in materials quality that was used in gear, in the capacity of ships, in better and more powerful engines, in winches and so on, in the electronic gear that allowed them to identify and locate fish populations, in the skill of the crews, in the knowledge and experience of the fishing skippers.

We, believe, our panel believes, that all of these growths in technological competence and capacity overshadowed the downturn in the fish stocks and were far more important than anyone realized at the time. So I think both groups suffered from the problems that arise from having a peculiar mind set which simply blinds you to what is happening around you.

And you must remember that the catch rates of the commercial fleet have not substantially declined. In fact, if they were permitted to fish ad lib, the quantities of fish landed would go up and up and up. Whether the rate, if calculated properly, would have gone up is another question. But certainly the quantities would have gone up.

And this is not a unique experience. This process has happened elsewhere in the past. Repeatedly, in fact. It just shows the perennial thick-headedness of humans who are very slow to learn from other's experiences.

Q: In the course of my research, I have been told by some people that there was and, to a lesser extent, still is a problem with factionalism within DFO over the quality of the work that they are doing. And it seems to have broken down along lines of age and academic credentials. In broad terms, a case of the old guard versus the young turks.

A: Well, I think there is some merit to that. I'm not sure the breakdown was totally old versus young but there's some element of that, I guess, there. There is a tendency for the young to place a...largely, not because they were young but because of the kind of training that they had...a tendency to over reliance on techniques that were new and, therefore, ipso facto better. I think the biggest split arose between those who, as it were, were sold on the model and those who weren't.

Q: Because the model was giving them the answers they wanted?

A: Because the model was giving them the answers. But not only because the model was giving them the answers. And this is where a subtlety creeps in that I'm a little bit diffident about.

The danger in all modelling, in my view, is that you become trapped by it to some extent. It's self-fulfilling. You're dealing with data which are manipulable and variable and uncertain. You have a variety of ways that you can interpret the data. If you've got a model that you believe in you will interpret the data in a way that makes the model work. I don't think there's any dishonesty in this, as such. A completely and perfectly honest scientist or, at least, a person who believes that he's honest and scientific...he may be honest but he may not be scientific...that's another issue too. But he will tend to see the data in the way that will make the model work. That's what happened.

And I think that there are still people in the organization, call them the old guard, who still don't believe in models. They want to see real fish. Real animals in their hands. Real tags and real data that you can count and not have numbers spewed out by a computer.

But I think the real trouble is of another kind. When I was talking to fishermen and fishing groups, I used two or three analogies to try and explain this phenomenon which I think is a universal one and has occurred throughout the whole of the history of science and technology.

A simple example is, perhaps, the Copernican revolution. You have a couple of thousand years of people looking at the earth as the centre of the universe. And the mind set is there, firmly fixed, that the earth is the centre of the universe. There's no question about that whatsoever. So you see all this other data, the orbits of planets, and it doesn't fit. But what you do instead of saying "our premise must be wrong because these orbits are impossible," you say "we have to find a fancy way of modelling to prove or to show that these sorts of orbits can be created with the earth still at the centre of the universe." So you have brilliant minds devising weird mathematics to show why planetary orbits are the way they are. Defying all logic but very seriously presented until Copernicus comes around and says "Look. You've got it all wrong. Let's suppose that the sun is the centre of the universe. All these orbits suddenly work." Well it's the same with this fish model or any other any other model.

Take William Harvey and the circulation of the blood.

People had been cutting open cadavers for years and years and years and looking at the circulation system. Looking at the veins and the arteries. Looking at the whole system. But they couldn't admit what their eyes saw because they had a conception of the heart which indicated that it was more than a pump.

Q: So theory and expectations can overpower data?

A: Exactly. And I think that's what happened in this case. Or, at least, in part what happened and I think, in part, explains why honest scientists trying to find ways to make the data fit the model....Perhaps I shouldn't have said that as bluntly as that because that's perhaps not what they did. Why honest scientists saw the data in accordance with rules of interpretation which would make it fit the model. And I think that's the danger of all modelling and it's a danger when you have a particularly unsophisticated model.

And I think the model that was being used, the bulk-biomass method, is essentially an unsophisticated, primitive model. The one that's being used now is another generation. It's better. It's still not totally sophisticated. But it's better and it does submit the data that are being used to certain tests that attempt to eliminate this phenomenon to which I refer. But that was and is the danger.

And I think without the older scientists in the establishment even being aware or thinking along those lines, because I don't think they did, concretely, at any rate. Nevertheless, being suspicious of the model and the modelling technique because it was so alien to the way in which they had done their science and were trained to do it. It was fairly difficult for them to come to terms with.

The other problem, I think, was the failure of DFO to open itself up to examination and testing by outside and totally dispassionate interests. An in-house operation, even though it is allegedly a peer-review process in which their science is tested, doesn't work very well if all of the peers are working from the same set of assumptions and the same set of objectives.

Another problem is that the various units or divisions, groupings into which the scientific establishment were broken, tended to function in compartments that were too nearly watertight. There was not enough exchange or cross-fertilization or integration of data or of ideas or of systems. The caplin group, the crab group, the cod group and so on were working independently. Whereas the ecosystem with which they were all concerned is not so broken down. It is a unitary system that functions in an integrated mode.

They attempted to deal with the component parts and not with the system as a system.

I know that this is all very easy for me to say but it's very difficult to do. It constitutes one of the really difficult problems, particulary when you expand beyond the biological system into the physical system which integrates with the biological system and interacts with it in a way that's critically important. So you do open up a whole new range of problems. And, of course, you get into modelling exercises which are enormously complex if you try to produce an integrated model.

Q: This brings us to a point where I'd like to shift to a discussion of the future. For the purposes of social planning, corporate and economic planning, certain assumptions have to be made about the future. What the state of the stocks is going to be and how many fish we are likely to catch or will be allowed to catch in five years and ten years and so on. It seems, this is my analysis, that with the modifications that have been made to the assessment process, they can do a pretty good job of retrospective analysis. They can know pretty well how many fish there were. And, this is my rough estimate, within 20 to 30 percent plus or minus what there is out there now. But the system is so complex and the variables affecting fertility, larval survival and recruitment are so huge and so variable that there are some, including Jake Rice for example, who feel that it is practically impossible to do any sort of useful forecasting. Here's where things get really sticky. Because there are demands place on DFO to do forecasting which they know is impossible but they do it anyway because the pressures are so heavy. Knowing that by pleasing their political masters and other clients today, they are setting themselves up for somebody to be shown as terribly wrong tomorrow.

A: That is true and that is a problem. I think Jake recognizes that very clearly and I think that most good scientists recognize this. Not only in this lab here but across the world. It's a critical problem.

From our point of view, the point of view of our study, the best evidence you have to go on, or the best predictive model you can use, is one based on historical experience. And the historic experience of this particular cod stock, and this is setting aside any major environmental shifts which may occur with global warming. These are likely to be so totally disruptive that anything you say makes no sense.

But setting aside that and assuming a continuance of the present physical environmental regime over time, the historical experience would seem to be the best one we've got at the moment. And that indicates that over a period of some 400 years the northern cod stock sustained an annual TAC of around 300,000 tonnes. We don't know if that was the maximum sustainable yield.

In my view, maximum sustainable yield is a concept that should be discarded anyway. I don't think it works. It hasn't worked in Europe. It hasn't worked in the North Sea, the Barrent Sea, in Iceland. It hasn't worked anywhere. So you have to play a game of caution. You have to err on the side of caution all the time. Both in your projections and in your practice.

But we do know that when the catch went up to something greater than 600,000 tonnes a decline set in immediately. We know that at 300,000 tonnes it was sustained over approximately 400 years. We know that between those two numbers must be the magic number that we're looking for. Provided the stock can be rebuilt to its pristine levels which, of course is the other proviso. And that was the foundation of Canadian management strategy from 1977 up until the crisis came in '86/'87.

Q: But that historical average was achieved with very different technology. It was largely a hook and line fishery which tends to catch larger, older fish. Whereas now the technology is catching a lot of pre-reproductive fish. They recruit to the stock two or three years before they are sexually mature.

A: If you look at our report we suggest...of course we don't suggest that you go back to hook and line technology, although that might not be a bad thing in some cases. What we do suggest is that we modify our technology so as to eliminate the catching of juvenile fish. We think that a critical part of the problem if you're going to rebuild the stock to its earlier levels...I don't know if we can ever rebuild it to its virgin levels. But if you're going to build it back to a level with which with some confidence you can project a sustainable yield of 250 to 300,000 tonnes, then you've got to find the means of eliminating the heavy plundering of the juvenile animals in the population.

Q: I'm told that the inshore fishery as it's currently practised with traps and bottom gill nets is primarily a juvenile fish fishery.

A: Yes. Not the gill nets so much. It depends on mesh size. The current law, which is reasonably well-enforced and reasonably well-observed, precludes the catching of really small fish in gill nets.

Cod traps are a different story. They don't necessarily....It is a bit of a problem. Fixed gear is a bit of a problem because of the phenomenon...But even if the population were in its virgin state, the juvenile animals are the ones that come closest to the rocks. That's a behaviourial matter which you are not going to change. The larger animals tend to loiter off in deeper water. The younger animals pursue their food supply right to the shore and in doing so become accessible to the fixed gear. So that is a problem.

But you can, I think, even there, do some weeding out of the smaller fish. If you really want to, there's no reason, for example, why you should use mesh sizes smaller than 4 inches at the back of the trap. If you go much above that you would wipe out the trap fishery because the very young animals that come to land are the ones that make themselves accessible to that kind of gear. So you have to pay a price, certainly, in terms of the inshore fishery.

Q: There is considerable debate about whether there are separate inshore and offshore stocks and, if so, to what extent. But lets assume that there are reasonably discrete inshore stocks. Or were. Given that the cod trap is a relatively recent piece of technology, is there the possibility that the inshore has been the author of its own demise?

A; Yes, that's partly true. I think, insofar as there were discrete inshore stocks that made up a substantial part of the catch. The evidence for that is very thin. There is some evidence to suggest, strongly suggest, that there were inshore stocks in certain of the bays. I don't think there's been any suggestion made yet, at least not based on substantial evidence, that those inshore stocks constituted a major part of the total of the inshore catch. I don't know what part it did constitute. But I think there certainly were inshore stocks. I think that they succumbed, not so much to the cod trap fishery as, to the gill net fishery when it was first introduced and when all of the inshore gill netters had access to the near-shore deeper trenches where the inshore population lived and really cleaned out the breeding stock pretty well completely. Whether these would regrow if they were protected if you stopped, for example, the inshore catch of all juveniles, whether they'd reestablish the bay stocks, I don't know and I don't think anyone knows. It's an important area for research.

I think the possibility for genetic tagging studies, that now appear to be possible, may be the way to go. It may be that we have to do much more tagging studies. We may have to get in to "smart" tags that record fish movements and so on.

But certainly it's possible that the inshore has been a significant contributor to its own demise. But, nevertheless, if you really try to look at what's happened as subjectively as you can, there is an undoubted relationship between the level of offshore catch and the subsequent level of inshore catch. The two seem to be tied [inversely]. So even though there were inshore stocks at one time that still exist, much reduced but still existing, the bulk of the fish that came to coastal waters on a feeding migration originated on the offshore banks which have always been the major spawning areas. The inshore spawning areas, if the existed, were relatively small, confined to a few deep trenches in several of the bays. Placentia Bay, Bonavista Bay, to some extent, perhaps, Notre Dame Bay, Trinity Bay maybe. The bulk of the fish still came in from the major spawning banks and I think that's where the salvation lies.

You'd have to change your models and the numbers, the data that goes into the models, if you discovered that there were discrete inshore stocks. But in terms of future forecasting, it's not an easy matter.

I sympathize with the scientists who are forced by political and economic considerations to venture in to that domain. Because they don't have the capacity to make accurate forecasts, really. And they certainly don't have the information to make accurate forecasts for the inshore fishery. Because the variability of the fish inshore will depend year to year on minor environmental shifts and changes that are thoroughly unpredictable given our current knowledge.

APPENDIX I

Interview with Jake Rice, Head of the Groundfish Division
Conducted in St. John's, Newfoundland
July 6, 1990

Part One of Two Parts

Q: Has the first instalment of the $42.8 million of northern cod money arrived yet?

A: Yes. My understanding is that the first instalment showed up here June 22nd. I was in travel for the two weeks bracketing that but that is what I understood to be the arrival date. We're spending it. that's for sure.

Q: You didn't blow it all on the long weekend? [reference to remarks quoted in the June 30, Sunday Express]

A: No, we didn't blow it all on the long weekend after all.

Q: When I read that comment I just about fell out of my chair. This is so refreshing, a sense of humour. Then it occurred to me, this isn't the kind of comment that somebody who's expecting to be around in the senior levels of bureaucracy is going to make. It's more like what you'd expect from someone who has a couple of solid job offers in their back pocket.

A: [chuckles] Well, that comment, as far as I can tell, was met with uniform good humour in Ottawa. [Later amplification: "I did get called about it, naturally--but not scolded. I gather people at the very top weren't happy but the senior civil servants needed a laugh worse than anyone, I think."

Q: Something else from the news. I think it was three days ago in the Telegram there was a little box down at the bottom that said that [cod] landings in Nfld. for the first three months of 1990 were up over 110 per cent compared with the three months of the previous year. Is that accurate?

A: We don't collect the statistics in Science. By that I mean catch statistics. We collect lots of data, but landings are monitored by Stats. Branch. We get the same circulars from the Statistics Branch that the press does. They, the reporters, read the numbers the same way we did.

Q: Doesn't that [the increase] seem curious to you?

A: The offshore has been arguing, more and more vocally in recent years as they see their quota being cut, that their experience has been that, not only is there no shortage of fish, but they really have never had it so good.

And our research vessel was out in February doing an annual hydro-acoustic survey and we were trying to do a lot of tagging work. We added almost two full weeks on to the time we were at sea. And there's a lot of fish out there. We got, I think it was 80,000 pounds in one fifteen minute tow when we were trying to get just a few fish to put tags on. By a few I mean several hundred to a thousand is about all you can tag and keep them in good shape. You use a fish-finder to delineate a school and then you go out and fish right on the edge of it. And what we had identified as the edge of a concentration had that much fish in it.

One of the difficulties we have, communicating to any individual in a plausible way, is that each person's unique experience can't be taken as the average condition. The fact that the cod do aggregate very densely in the pre-spawning period, aggravated by the much more extensive than usual ice coverage that we had, so the fish...we don't know what goes on under the ice. But anecdotal....It seems that all things being equal, these fish prefer not to be under the ice. We can't document it but it's folk-wisdom that fishermen as well as fisheries scientist share in. Given the extent of the ice coverage...if the fish do build up right on the edge of the ice, that's another aggregating measure an off-shore trawler can find incredibly dense concentrations of fish. Sustainable for several weeks. That doesn't mean that, integrated over the whole 2J3KL area, there's huge amounts of fish.

Q: So environmental and other variables can combine to create the appearance of abundance when, in fact, that might not be the case at all?

A: Yes. Just as they can combine to create the appearance of scarcity, particularly for the in-shore. Whereas averaged over St. Mary's Bay at least to Makovik and out two or three hundred kilometres, there can be a lot of fish but individual communities for a whole season can see none of it. That happened in Notre Dame Bay last year where most of Notre Dame Bay experienced an abysmal fishery. While the Labrador coast and a lot of the Southern Shore of the Avalon did extremely well and plant capacity rather than product was the limiting factor.

Q: The more I read in the technical literature of stock assessment...I just finished a book by Gulland, an FAO publication, which seems to be a pretty comprehensive...

A: Very good overview! Incidently, John passed away a week ago Friday. The word is just spreading through the fisheries community.

Q: He seemed to do a very good job of assessing the strengths and weakness of the various techniques and almost ran a counter argument to the fashion of whole-systems modelling now. That until you have your basics down, you're simply dissipating resources and scattering data points. That, in fact, environmental variables might be so great an unknown, uncontrollable, that there's a possibility that management in any sort of precise sense on anything other than a single-species basis might not be very reasonable. Which I thought was an interesting counter to...

A: Yes, that's an issue that gets discussed at length in several fisheries science bodies. CAFSAC has a whole sub-committee on whole-systems and environment. ICES has sub-committees on multi-species management, a suite of sub-committees on environment, another sub-committee on hydrography. And the fisheries science community is continually seeking a balance between not being totally myopic and not being so dissipative of their resources that you don't know enough about any one thing to provide advice.

Q: There was another point, or underlying assumption, that there are natural equilibriums. That in a theoretical global ecosystem without man's interference, there is a "balance of nature." And given stable fishing pressure, stocks will reach some sort of equilibrium. But what I've read of Robert May's recent writings suggests that natural systems might behave more like quantum states, which is real interesting and then you can tie this in with the stuff coming out of chaos theory and the picture gets real interesting.

A: I was last week...well, the two weeks I was away, bracketing that June 22nd date, was all at meetings about how to relate fisheries management, fisheries science, to what we're learning about global climate change. Because we can't wait till the final data are collected that convinces every one that, yes, the climate is changing and this is the direction and this is the speed. We need to begin to think, what is it we want to know about the marine ecosystem, particulary the resources in it that provide wealth. In light of the fact that the environment that they are living in might be changing out from underneath them. And a lot of these things about, is it going to be a chaotic response, that you keep monitoring and monitoring and monitoring and you don't see much change and suddenly you get a massive switch to a different position.

Many of us think, based on what we know of ecosystem theory, and just what we know from personal experience with details of our little piece of it, that's very likely what's going to happen. Certainly a lot of the problem with northern cod, in terms of the industry's expectations of what would be available by now, may reflect the fact that, for long periods from the late 'fifties through the 'sixties, were producing about two and a half times what we're pretty confident was produced from about 1970 through to the present with annual growth rate and recruitment fluctuating but it's been fluctuating around a pretty stable average for twenty years. And its an average of less than half what it seemed to be fluctuating around before.

But the data start to get really shaky. By the time you're back into the 'sixties you're relying very largely on foreign fisheries where you don't know anything about the sampling and the quality control on the aging and stuff. So we're uncertain of the quality of the data. It could be pretty shaky and still suggest that there was a long period of time when, not just the northern cod stocks, but all the cod and haddock stocks, both on the North American side of the Atlantic and the European side of the Atlantic, all seemed to be producing annually many more young fish with quite reasonable growth rates, than have been produced in the last two decades.

We can't go back and do the sort of ecosystem-level research back then to figure out what it is that changed, but there's certainly reasons that any cautious scientists should pay close attention. It goes through, certainly, gradual changes. You fish a little harder, you're going to get certain things and if you ease off you get responses. You can show that the short-term behaviour is pretty predictable on a year-to-year basis. But the long-term behaviour is going to be influenced by stuff we don't have pinned down yet.

Q: One of May's point seemed to be that, as far as the ability of man to mange an ecosystem or even a part of it, you probably can't control these phase changes but you can effect the time periods between them. Compress them or extend them. And he was looking at the history of catches, from the North Sea particularly, and they showed random variation around some equilibrium point and then a phase change, either up or down, to a new equilibrium point and so on....I've lost my train of thought....

A: Management can effect the intervals between them. All other things being equal, it's easy to defend a decision that we should concentrate on things that are under our control. And that's why, I think, that a lot of money is spent on looking at the fishery. Because that is something we can control, in theory, to the extent that a government can control the behaviour of its citizens.

We can control the amount they're catching, the age composition and stuff like that. We can tune that as finely as the participants feel is in their interests. Which gets back to the importance of communicating...they realize what they gain by playing by the rules.

As opposed to some of these environmental influences where, even if we could quantify in detail how it affected catch, there's nothing you can do. You're the relatively passive recipient of what it chooses to do to you. We need to know something of these environmental influences because...if for no other reason than to make the data we collect meaningful.

We're seeing a lot of variation. It's nice to be able to know that you can attribute it to some known influence, even if you can't control that influence, so that what's left you can make better decisions about. And as you said, if the system does have non-linear responses to some of these kicks, it tells you something about the margin of error you do or don't need to leave.

[Later amplification: " The point is to have confidence that if a lot of the variance can be shown to be due to the environment, you can factor it out. The remaining variance might be more tractable as fisheries management impacts."]

Q: You go through the DFO forecasts from the 'seventies and early 'eighties, the three or four or five bars on the right are all forecasting a linear increase...

A: Go talk to the economists. Those resource prospects come from the economists. They're the bane of our existence. I understand very well why they're needed, Industry...we could argue to industry that we can't predict that far into the future but until they figure out a way to build a ship in six months and decommission it in two, and still run a viable operation, they're going to need five-year and ten-year planning horizons.

[Later amplification: "My answer is not entirely true. Science in Ottawa contributes to the resource prospects as well. There is a decade-long struggle between regional scientists and headquarters. In all the regions, Science argues that we *cannot* guess future recruitments and *cannot* project five-plus years into the future. Headquarters deals daily with politicians, other departments, and industry. All those clients have legitimate reasons to want to know what the future holds. The right position isn't all that obvious to a third party is it?"]

Q: So there's a tremendous amount of demand, whether it's realistic or not, from the corporate fishing sector and probably finance and industry...they need to deal in certainty.

A: And not just...it is most acute for the big industrial fishery, where I'm using the word "industrial" not in the European sense, but in the North American sense, big capital investment. The small in-shore fisherman is less dependent on it, but certainly the ramifications for the individual for allocating a whole lot less capital assets incorrectly costs the individual inshore fisherman at least as much as it costs the president of some big fishing company...or the shareholders. Most of the shareholders of these companies, often...this is just one piece of a big portfolio.

Q: I suppose that the last thing that Vic Young [president of Fisheries Products International] would want to hear is that he's operating in a non-linear, chaotic environment.

A: The thing is, that in a way, they know it. I'm in a position now that I deal with these guys...maybe not at the level of Vic Young...on a day-to-day basis, but people like Herb Carter and people like that I see at meetings all the time. And I had my preconceptions of bloody capitalists....These are people who have good brains, have a certain amount of humanity, as much as you'd find in any individual picked off the street as far as I'm concerned, and they know the difficulties that they're dealing with.

And yet, to use the analogy that I used at the ISOF [International Symposium of Operational Fisheries] meeting last fall, you can't put the genie back in the bottle, in the sense that the technology exists to increase the efficiency and other people are going to opt for it even if you could argue that, in the long-term, you could argue avoiding the technological advance because eventually the systems's going to switch back to the old state...or at least to a different state incompatible with your big capital investment.

You cannot maintain viability on a short-term if you're not keeping up with those who've taken what may be a short-sighted option because the system's been in this phase, if it is a phase, for twenty years.

So who knows? It's a very difficult syndrome not to get into. If the rest of the fishery is committing itself to an avenue that is profitable under the existing conditions, you're not going to be in the game long enough to benefit from ignoring the short-term conditions because the long ones could be different.

Q: If it is a non-linear, chaotic system then there's absolutely no point to strategic planning or thinking because you can never know which way it's going to jump or when.

A: We hear that from many components of the industry, not just big off-shore companies. Why keep such a restrictive target harvest level when you can't be sure that there's going to be any benefits from it? You can be sure that there's going to be a cost from keeping the harvest levels down. The degree to which we can convince people that there's benefits has been really weakened in the last half-decade.

Q: Because you know you're giving it [the system] a nudge, you just don't know in what direction. This could be very disconcerting for some of the more traditional reductionist scientists.

A: Yes.

Q: I'm reading a little monograph written by Werner Heisenberg, written in the late 'fifties towards the end of his career called "A Scientist Looks at Nature" [actually "The Physicist's Conception of Nature"] discussing the evolution of the scientific conception of nature and observation and the scientific process through the quantum revolution which he more or less precipitated. So these sorts of debates have been going on in physics for some time, at the most rarefied level of science but now they are spilling over into resource management, corporate reality, daily reality.

A: Yeah. I, personally, am very uncomfortable with their implications although I can't counter them as being inaccurate. The implications are, generally, that the rewards of being a cautious steward are probably going to be lower than you used to think. And how do you defend being really cautious in your management of any resource if it's likely to change dramatically despite your best efforts to the contrary. We can fall back on Bob May's argument that it appears to be possible to extend the period that it stays in favourable phases and hopefully shorten the periods that it stays in unfavourable ones.

Q: Have you read the book "Chaos: the Making of a New Science"?

A: No.

Q: It's real interesting because a non-linear equation with only three or four variables and an energy input...you have a damping component and an energy component in this equation. You start to run it on a computer and it will run along with random variations around a certain point. Well, they start to feed more energy into the system and it will go into a thoroughly chaotic state. And then they feed even more energy in and it will settle down into another sort of equilibrium state with small perturbations. And so...this actually is hopeful. You start to theorize about how you can do your work by modifying...adding or subtracting energy from the system or increasing or decreasing the damping on some part of the system. You can actually kick it up or down in phase shifts. Now this is in very simple, theoretical mathematical models, but one would assume that if this has some useful correspondence with reality, not an actual description of reality but a useful correspondence, there's some very interesting work to be done.

A: There's some very interesting work being done...in Canada. The leaders I know of are Louis Legendre and his brother Pierre who are at, I believe, Laval and UQAM but they're both biological mathematicians, they're mathematical biologists. I don't know what the relative emphasises are. Very capable, competent people. I've been impressed with a lot of the work that they've done. Louis in particular has got into arguing that the things that we should be studying about ecosystems are not a lot of the traditional stability and connectivity properties but the total energy in the system and those mechanisms in the ecosystem which retain energy and those that dissipate energy. Once you've identified what those mechanisms are, studying the balance of the dissipative versus the retentive mechanisms is the thing to study about ecosystems.

And a lot of the talk at the meetings, particularly the one in Halifax, on the northwest Atlantic, what should we be doing? We've had people all the way from Chesapeake Bay up through Norway, Denmark on behalf of Greenland and Iceland, talking about what we should be doing to prepare the fisheries' response to climate change. And the focus is on these dissipative and retentive energy mechanisms on the fishery supporting [continental] shelves was something we felt was...a way of....

A lot of work we do comes into that framework. We just haven't couched it that way. And it might be the way of viewing our work that begins to help us to cast our results in ways that are relevant to the climate change question.

Q: There's one other item I'd like to talk about if you have the time.

A: Yes.

Q: This money that's coming, is it specifically earmarked for northern cod research?

A: The $42.8 [million] is specifically earmarked for northern cod research. That doesn't mean that it all going to be spent on cod. There's things like a fair bit of money for studying physical oceanography and biological oceanography. But that is specifically in the 2J3KL area and of all the things you can ask about biological oceanography and physical oceanography the things...there's a direct one-to-one correspondence between each initiative and at least one recommendation in the Harris report.

So there's a proposal to...the jargon is "quantify the biomass spectrum." It's been proposed that the amount of biomass at each size category, from microscopic up to whales is...it's log-linear. If you put everything on a log scale, you get a line that's a straight slope. And the steepness of the slope is a function of how productive the ecosystem is. And how efficiently...well, the intercept is how productive the ecosystem is, the slope of the line is how efficiently it passes energy to the phytoplankton on up.

And it is a...it's been a theoretical idea that's been around for a long time with some support. It's a way of looking at things that stimulates interesting questions and uses of data even if it turns out not to be true in detail. But that's a case where it's clear from the beginning of the ontogeny of this project on quantifying biomass spectrum, our research surveys do a real good job on things from about twenty centimetres up. That leaves a lot.

And rather than try to do the whole thing, we're going to focus on that slice of it that is about the size of things that cod eat. So rather than doing a poor job of pushing it all the way up from nanometres to two centimetres, we'll focus in on the two centimetre to twenty centimetre category. From little bugs that you can actually see up to capelin. And see if we can get that part of it quantified well.

The decision to focus there is based on the fact that this is the suite of things cod eat. And once we've quantified the relationships between everything we know about cod and everything we know about their food, we're going to want to know how the food is balanced with its food.

Q: One of Gulland's main points was that the thing that affects stock strength most is recruitment and we don't know much about what affects recruitment. The early....Any fish is most vulnerable from the egg stage 'till recruitment. And that where there are a host of environmental unknowns including food supply, temperature, drift, predation and this is what you're going to be...

A: We've got projects on drift, we've got projects on simply delineating exactly where the 0 group, the age one, the age two are before we can get into process studies, what causes the variation. We know places where we can go and reliably get them every year but that's different than knowing how typical those places are of the areas that they occur and don't occur.

So, among the first things we're going to try to do down there is do a defensible survey of the candidate nursery areas. Nursery areas being the jargon for where fish younger than those caught in the nets hang out. Once we've delineated the areas which they prefer and which they clearly avoid, the you're faced with all the work of saying, what are the differences. That's a descriptive task. You've got the descriptive task of trying to differentiate the area.

And then you get into the experimental task of, now, can we characterize the mechanisms, out of all the differences between these areas, what mechanisms is it that lead to this differential survivorship or mortality.

Q: So the money is spread over five years. That's roughly eight million a year for science. How does that compare with your existing annual budget?

A: For science, we have about seven million a year.

Q: So this effectively more than doubles your budget.

A: The budget that we in science have to deal with this region.

Q: I should think that this will put rather a strain on your infrastructure.

A: Yes.

Q: How are you going to deal with that?

A: With the money comes some new positions. We're going to get about a dozen new positions. We have commitments of using what's called "py slippage." That's, at any given moment, not all the positions that exist at a station are occupied. And every day that an existing position is unoccupied, you get what they call "py" or "person-year slippage" that you can allocate.

So the scientists, the hands-on scientists, we get more bodies. The paper work we handle out of py slippage. And the paper work out of northern cod has been flagged as...well, not just northern cod but the whole fishery adjustment package. Because there are also more people in surveillance and stuff like that. So that means more paper is going to be generated. And where before, the needs that we got money and people to fill, have been the priority needs for this PY slippage, we now have the resources off the top.

For instance, the data from the observers [on the offshore trawlers]. Collecting it on the operations side, processing it on the science side. For a long time, a lot of the slippage that we had available to us had gone to get more bodies handling those data, because we think they're important. And we've getting more of them than the people allocated several years ago. Now we've got enough bodies to handle the data. And we have enough resources in the package to handle the data when it comes back to the lab. And we don't need to keep devoting several integrated years of slippage to processing the data and getting the observers out there.

That filters down a level now. We've met our needs in science so now the people down in purchasing can get new people to come in and help them buy the stuff and handle the requisitions and stuff.

Q: It's probably still going to put something of a strain on the system.

A: Yes. And what I've been doing is putting together...I've got to have done, hopefully by the middle of next week, detailed bench marks for this year for every single project in the 28. Some of them are just not...there's no field work we can do thais year. But we can do a lot of the planning and buying hardware this year. Do the shake-down work on it when it doesn't matter. So that next year's field season...because it's a five-year project, not a one-year project, we can schedule things to be really demanding at different times through the five year period. So we can spread the impact we're having on the infrastructure over that five year period.

Q: You mentioned 28 projects. I assume that they respond to recommendations in the Harris report.

A: Some of them respond to more than one. Harris had, I think, 31, 32 recommendations. And we've got 28 components in the science package. About half of the Harris recommendations deal with the management, not the science. And there are some of the more open-ended recommendations that we've got two or three projects addressing it.

Q: Have you set this response, these projects down in printed form?

A: This is part of what I've got to have done. What I've got...what I was playing with when you came in is something mailed through the computer in Ottawa. Basically it's a document from the Treasury Board itemizing funding. And what I want to do by the end of the long weekend we've got is to have gone through this, pull out the stuff the Treasury Board wants in terms of accountabilities and deliverables, which are irrelevant to any audience that isn't an accountant.

And that will be going out to all the scientists in the lab and to our public mailing list, everybody who gets our pre-documentation, will get a copy of...."Here's what we're up to,"...an invitation to come talk to us about ideas they have within that. So that, I hope, to have in the hands of people by the middle of next week.

[Later amplification: "There were two things--the financial stuff for accountants, and the paperwork on the projects with levels of funding. It was the latter which was to go out to the public. There was nothing secret about the other documents--just boring."]

Interview Jake Rice
Conducted in St. John's, Newfoundland
August 14, 1990

Part Two of Two

Q: In the course of talking to DFO scientists, my thinking has changed considerably. I'm moving more and more away from the theory of the sociology and science and toward the practical realities. The lives of tens of thousands of people and hundreds of millions of dollars are at stake here. It's no longer a theoretical exercise for me. I find myself in the midst of something that has extraordinary consequences. My sense of responsibility is growing. It's not the sort of situation that academics, especially young ones, find themselves in the midst of. A typical MA thesis is not something of such moment.

A: It's interesting to hear you say that because it's an evolution that I can really remember going through. When I joined the department, I was hired out of the university with a...twelve years as a graduate student and a faculty member. I can remember pontificating to my students about "Well, if you grasp the theory really well, the special cases will fall out just fine." I carried a very theoretical, arm's-length attitude into my first-year fisheries meetings.

I had the good fortune, about a month after I joined the department, to go to a big international meeting that happened to be held in Halifax. One of the days was devoted to looking at multi-species management models. It was an area that I was hired to work in because my background was in community analysis. So I went there and listened to the papers for the day. I went there feeling really intimidated because I had been reading the fishery literature and it seemed really sophisticated, equations and stuff. I listened to these talks the whole day, got invited out for a beer afterwards, and made the comment to someone, who was, fortunately, quite tolerant, "Well, you know, after listening today to a whole lot of talks, my feelings about working on multi-species management aren't quite so intimidating. It's clear that there are a lot of people working on it but they haven't got very far." And this person looked at me and smiled benignly and said "Yes. And they were very bright people too."

And that has always stayed with me. A lot of the people that I have met, a surprisingly high proportion of them, are very bright people and they work really hard. But they don't get very far in terms of solving the problems so that we have recipe that works and you get the right answer, or something very close to the right answer, every time.

And unlike some kinds of fields where maybe you do have some uncertainty, in the fisheries thousands of people's jobs, just as you say, depend on the answer. And it's just really hard to get it in a way that anybody is sure it's right. And it turns out to be right in retrospect.

Q: My thumbnail analysis of the situation at this point is that fisheries stock assessment science had the extremely bad luck to run into a set of shaky socio-economic and biological conditions at a time when it is at a very young stage of its development as a science. In the beginning you were using very simple production models and now you are trying to make the transition to much more complex and sophisticated models. But at this point in the development and the transition, you are putting most of your effort into finding out how much you don't know...discovering the sources of error and uncertainty, So in fact what appears to your critics as incompetence, the downward revisions of stock strength estimates, are in fact signs that you are beginning to get a grip on sources of error. But it doesn't look like that to the political types or the corporate fishing interests or the larger society. It looks like incompetence.

A: It's interesting, again, the history of fisheries is a little different than any other natural resource that I know of. People talk about the golden age of fisheries in the 'forties and 'fifties when a theoretical foundation with really good population dynamics, mathematical foundation was laid and it was vastly beyond any data that were available. And it was vastly beyond the theoretical foundation of any other resource that I'm aware of, renewable or non-renewable.

All the other resource that I'm aware of have built there management on the gut feelings of the old guys and the new changes have been totally data-based. You just collect enough data and then you just run it through these big multi-variate analyses to beat it into shape. When geology went from the old prospectors to a modern science, they did it with kreiging and it was totally data-driven.

Fisheries is the only natural resource, either renewable or non-renewable, where there is a really big theoretical framework and it took us about 25 years to collect enough data to catch up to the theory. And the theory, at the time it was developed, was vastly better than that available to any other resource. And the problem came, not because the theory was wrong...any theoretician will tell you that any theory is better than before the theory but the people who come after me are going to make it much better.

But what happened was that economist and sociologists and people like that could read these good theoretical papers, recognize the mathematics of it...they weren't dealing with some old forester's or prospector's intuitive knowledge of what's out there. "Give me an equation. I can use the equation," says the economist.

So the whole economics and sociology of the fishery, to the extent that it was built on anything besides expediency, was built on the theoretical framework that the fisheries people were using. And now that the data have caught up and the fisheries scientists.....

It's only been since about the mid-seventies that we've had enough data to say that this part of this theory should not be the basis of management. It's too weak. And we should replace it with something nearer to the data. Not back to something intuitive that there's no way to defend or explain after the fact. Like any science, the goal is to collect so much data that the description of the data is the description of the stock.

Q: My reading of the early theory is that it reflected the dominant ideology of western liberal market capitalism. Very Adam Smithian. The myth of the balance of nature. Equilibrium states. In a free market all forces are balanced. In nature all forces are balanced. And that goes back further to Rousseau. And what seems more likely now is that the concept of the balance of nature is a myth that's tied to a specific political and economic ideology.

A: I see the same parallel. I'd add the caveat, though, that it's not just fisheries but all of mathematical ecology evolved in an equilibrium framework. Fisheries wasn't being left behind by the theory of ecology at all. I can remember the last population dynamics course, a graduate course, that I taught at Memorial; 1980. I brought in a couple of papers that for the first time were talking about multiple stable points in populations

[Later amplification: "And this is important. Until the early 1980's, every widely-acknowledged theorist dealt primarily with equilibrium models in ecology. Only since the mid-1980's onward has this been debated: Schoener, Cody, Diamond, U.S. Strong, Simberloff etc."]

Q: Would this have been [Robert] May?

A: May was one of the very first. [digression on May's lecture technique] But from the time that somebody gets an idea in print, to the time when all the applications of theory use these new ideas,...a decade's not a bad timetable. In fact, fisheries population dynamics was the focus of ecological theory in the 'fifties and 'sixties along with MacArthur and Hutchinson and a bunch of ornithologists.

They took the fisheries population dynamics models, generalized them to vertebrate population dynamics where things are a little more measurable, because birds only lay a few eggs a year, but it was all very much, as you say, equilibrium theory. The idea that change and...change was fine. Because it's a variance around a mean. But the idea that the mean itself is meaningless was really hard for a lot of people to accept.

Q: What, in your opinion, is the reason that fisheries is taking such a slagging right now?

A: Right now....I had to spend the morning at a meeting with Saga Communications, and I'm not going to put this off the record, as far as I'm concerned it ought to be talked about freely. Two weeks ago we had a woman who's holding science responsibility in the department's communications branch come down [from Ottawa] and talk about what the communications plan should be for the Atlantic Fisheries Adjustment Package, not just the science. She was interested primarily in the science part of it but it was the whole package.

Saga communications is a private communications consultant company. They've been hired, I don't know whether it's by the minister or by the cabinet, to put together a communications plan for this area as well. And it was very interesting to listen to the two...even in communicating science you've got this adversarial relationship or the potential for it. There's certainly jealousy and distrust between the two communications groups. Each one seeing the other as a threat. The in-house and the out-of-house.

Q: Do you know who hired them [Saga]?

A: No. I'm not high enough to know. Certainly Saga reports at the mInisterial level or higher. So I'd say minister, cabinet or someone like that.

Q: Federal?

A: Federal. But particularly with the out-of-house communications group, and to some extent with the in-house communications group, it's getting clearer and clearer talking to them that people have known for quite a while that some kind of restructuring of the fishery, and not just what they did back in the big restructuring in the early 'eighties when they took a whole bunch of small bankrupt companies and made a couple of big bankrupt companies, but the capacity to use resources....

The scientists knew in the mid-'eighties, when all the predictions were golden, that the capacity to consume resources was growing faster than the resource was. And now that the resource isn't growing, for what we hope will be a short period of time, the crunch is in there. And everybody is looking for someone to step forward and take responsibility and be the one who says "The buck stops here."

We have to do something about too much capacity chasing too little resource. Which doesn't have to be the same thing as too many fishermen chasing too few fish. There are ways to run a fishery which involve ways of minimizing the capacity of fishermen and keep an awful lot of fishermen in the game. But you can't maximize the players and maximize the capacity of each player whatever the resource is doing.

We in science played into the hands of this political hotbed by being incorrect, by being wrong in our projections of what the stock was going to be doing in the late-'eighties. So we were an easy person, in the short-term, to put the blame on. But all kinds of people that I hear from now, not just fisheries scientists who have reason to feel burned by the whole issue, but people in communications, critics inside and outside the department, and in other branches.

When you get individuals sitting down like you and I are in somebody's kitchen talking frankly, they knew all along that something was going to have to be done. And what I hear is the last minister who was willing to stand up, take the heat and give a clear message to everyone who reported to him, "This is the type of fishery that I see and this is the type of fishery that is consistent with all the information that I get about the resource," was Romeo LeBlanc.

He made a lot of people angry but he made very few enemies. Because he could justify everything he did and everything he did was consistent with an image of the fishery that could be defended. And there hasn't been a minister since him. So its's not saying that the Liberals were right and the PCs [Progressive Conservatives] are wrong. There were Liberal ministers following LeBlanc as well.

But the complaint that keeps coming back to other branches of DFO, not just science, is that you cannot get a single, consistent message of what this fishery should look like. Managers, even more than scientists, need clear-cut objectives in order to achieve anything. And the more conflicting and diffuse and unspecified the objectives are, the harder it is to implement the policies of the will of the people as reflected by the government in power.

For science, it's the same thing. We can provide advice on the consequences of this activity, that activity. Some activities are just not compatible with good conservation of the stock. Of the wide range of activities that may be compatible with the conservation of the stock, you can support this, you can support that, but the choices among them are going to make some people angry.

If we'd been right in '86 and '87 and the stock had continued to grow...it will never grow forever...and if it hadn't been '88, '89 was basically the big explosion of science credibility, it would have been '91, '92. It had to happen.

Q: Could you elaborate on that?

A: To the extent that the stock could not continue to build forever, but the capacity of industry, even if you cap the number of participants and allow them the ability to increase their ability per capita to catch fish, there was going to come a time when the capacity of the fleet exceeded what the resource could supply.

And at that time you've got the choice of do you have many fishermen living in marginal poverty or fewer fishermen living in something resembling a comfortable middle-class existence, maybe even better.

Southwest Nova Scotia is in that problem right now with that huge, capital-intensive middle-distance fishery. I can't recall exactly the figures. It used to be that the boats were capable of catching, let's say, 300,000 pounds a year. But they could break even at 200,000 pounds. If they caught 300,000 pounds, they made a hefty profit. Now they've got better boats but instead of being able to catch 300,000 pounds a year, they can catch a million pounds a year. But they need 900,000 pounds to break even. And that's not big factory freezer trawlers versus little inshore fishermen. These are single owner boats, not a big, corporate fleet. This is just unrestrained technology.

It's sort of a cop-out to make technology the villain. But the analogy I used at a talk that I gave back at ISFO

[International Symposium on Operational Fisheries] on a panel is that technology is like any other genie. Once you let it out of the bottle, you can't get it back in. And all the stories about genies getting out of bottles that have tragic endings happen because people want the genie to make them rich. If you would just use the genie to keep yourself from getting poor you might not need to put the genie back in the bottle.

And that's what I meant when I said that it had to happen. As long as the interests in the fishery, on every scale from smallest to largest, have an interest in using improved technology to get richer and richer, there had to come a point where, whatever the euphemism that's in vogue at a particular time, restructuring, down-sizing...restricting the number of participants to a number that's smaller and putting a limit on how much the individuals can take out of it.

It had to happen and people are going to be unhappy about it. Communities are going to be hurt badly by it. It takes a very strong politician to stand up and say "It was inevitable and it's my job to make some hard choices and take the heat for it." I wasn't with the department when LeBlanc was minister, but an amazing spectrum of people from card-carrying PCs to card-carrying NDP [New Democratic Party] and the Liberals in the middle of course, seem to speak of the LeBLanc days with a great deal of nostalgia.

It was a department that had a clear idea of where it was going and a department that could count on a good deal of support all the way up the line as long as all the decisions and actions were consistent with that vision.

Q: Let's go back a bit to where you said that science had got it wrong. Can you give me a bit of background on how and why you got it wrong?

A: My analysis of that is that the tools we were using...and this gets back to the fact that we are only just beginning to get the data to find the weaknesses in the theory that we are using...the tools we were using were flawed and we knew they were flawed but they were the best on the market. You can go back to the Alverson report and see that stated quite clearly. And what Harris makes a big deal about, to use the jargon, the age-disaggregated versus the bulk-biomass tunings, within two years of the first age-disaggregated tuning method being put on the table in a meeting, it was being used by CAFSAC. And that gives you about a year to pick up the technology and become familiar enough with it to use it. Because we experiment very cautiously with something as monumental as the fishery.

So it took us a year of using it to be confident that we understood what it was doing. And the next year we were using it in the assessment. And it gave a better focus, a better fix on what the stock was doing, than the previous tools we were using. And what it meant was that where we had estimated that the stock had been growing about fifteen percent a year all the way back to the mid-'seventies, it had in fact been growing about 10 to 11 per cent a year.

When you compound that over the better part of a decade, the end-points are pretty far apart. A message that still hasn't gotten through to the public. They really believe that the stock went from an end-point of a stock growth rate of 15 per cent compounded over a decade to the end-point of an 11 per cent growth rate compounded over a decade. And they say that we were wrong on the whole trajectory. It's not an easy thing to explain to people who are not familiar with compound interest rates. Not just looking it up in a table but the mathematics behind compound interest.

[Later amplification re management under conditions of uncertainty: "We are working VERY hard on 'management under uncertainty.' Unfortunately, the greater the uncertainty, the more conservative the management must be to avoid eventual crisis (where 'eventual' is one to three decades). Governments (and managers) need VERY strong will to keep things restrictive when short-term signs are good. But all models of management under uncertainty (and unreliable data, or incomplete data, are an excellent source of uncertainty) require such strategies."]

Q: They're not interested in that stuff for the simple reason that the effects are as real to them as if you had in fact removed a third of the fish from the ocean.

A: Yes.

Q: Quotas disappear, jobs disappear. You've just wiped out the stock as far as they're concerned.

A: And to say that it was never there is irrelevant. I agree with you. The other thing that has been the bane of us, and I'm speaking ill of people who are no longer around to defend themselves...But at the time of the extension of jurisdiction, science was asked for a bunch of projections. The economists need ten or fifteen year projections to look at investment patterns and rebuilding things.

Any scientist would have said "We can't look that far ahead into the future. Four years from now and we can talk about right now. Four years from now about eighty percent of the fish being taken by the fishery will be from year-classes we have not yet seen today." You're just gazing in a crystal ball. The scientists were told...they refused the first two times they were asked for fifteen year projections.

Q: What years are we talking about?

A: We're talking about '76, '77, '78, in there as we, Canada, was getting ready to extend jurisdiction. The first couple of times they said we absolutely can't do it. But the word came back down, "We must have these projections. If you don't give them to us, we'll give the job to the economists and they'll do it."

Q; This would have been from Ottawa, DFO central?

A: I don't know if it was DFO or one of the economic portfolios. This greatly precedes me. But there was very strong pressure put on the scientists to produce long-term projections. I've had hauled out for me copies of the documents that went up, loaded with qualifiers. "This is assuming that the recruitment stays at the historic average..." We're talking about 1976. The last year-class they saw was 1972. So they're taking the average from...basically '62 was the first one they had a fix on, to '72.

And that was at the time when they had the huge removals from the stock. And we have lots of reasons now to believe that the numbers that were being reported and the age composition of the catch being reported in the 'sixties was really inflated compared to what was actually being taken. The age composition particularly. Likely they were mining a much older stock than was being reported as being harvested.

Because if they keep the age composition young, we say this is the rate at which the fish are replenishing themselves and they can continue. But if they were reporting a really old age composition, it would have become evident much earlier that they were mining a stock of old fish and the age composition of the harvest could have been seen to be shrinking over the decade they were harvesting. It would have been recognized then, before the collapse, as a warning sign.

So we have reason to believe now that the numbers being reported in the 'sixties were systematically manipulated in some way. The fact that the scientists were really suspicious of them at the time made them reluctant to use them. But being put in position where they were being told "You must produce these figures," they took them.

The average number of fish entering the stock through the 'sixties was up around 800 million. We haven't had a year-class that size since the late 1960s.

[Later amplification: "This doesn't make clear that the suspected (we have no evidence) misreported catch by European countries was due to their interest in keeping ICNAF assessments optimistic, and each country's share of the catch as high as possible, because they all knew that when ICNAF did go to quotas, each country's share would reflect their historic reported share. Hence, report as much as possible. Canadians were reporting truthfully, as best we can tell now."]

Q: Do you think that was actually the case or is this what was deduced from the suspect catch data?

A: There's no way to go back and determine whether it was misreporting of the catch data or the fact that...these high recruitment figures were reported for cod stocks everywhere in the world. It could have been that the environmental conditions...certainly in the late 'fifties and early 'sixties were the warmest period the ocean's seen since the 1920s. You've got this long-term climatic signal. And it could have been that the ocean really was that productive then and it simply hasn't been since. For most of the decade its been the coldest period since 1900 to the 'teens [1913, 1914].

People who like to model these things will tell you that there's a 37 year cycle.

Q: So you could use a very simple energy model of the ocean. Heat is energy and in the form of solar energy it's the primary source of energy for the system. You pump more energy in and you get more biotic productivity.

A: That's another case where the first reliable data collected anywhere in the world, started coming in in the 1950s. And if we're looking for signals on decadal cycles...we're looking at the 'fifties, 'sixties and 'seventies. We've had three decades. But meteorologists looking at sun spot cycles and continental cycles of temperature and moisture and stuff...I said 37 years because that's where a lot of people say you've got these long-term cycles, a third of a century long. And if it is about a third of a century long, the 'sixties would be a peak and the eighties would be a trough.

So we were projecting what would happen through the late 'seventies and 'eighties, if it is climatic...and this is strictly conjecture although there is all sorts of analysis being done on the problem. There's lots of plausible theories to tie it all together. The problem is that we can't test them till we get another 30 years of data.

So whatever caused those really high recruitment, either lying or climatic influences, the recruitment levels in the 'eighties were less than half. So industry was expecting gearing up for an annual influx of resources that were twice the size of the influx that actually materialized.

If I were an investor I would feel rightly outraged. An investor in my own boat or a million dollar investor in a major fish company. Probably a smaller fisherman more so than a big one because what he has left to live on is a smaller pool than a major corporate investor. But those investors were banking on projections based on the best data available but the data simply didn't apply to the 1980s.

If the political system had evolved ten years later, if they'd been looking to extend jurisdiction in 1986, 1987, if this cycle continues, and we've got a couple of good recruiting year-classes coming in right now, and the inshore fishery is going quite well this year. The prognosis looks good in the short-term. We could have been blamed for being pessimistic in five years...1993-1994. The fishermen would be complaining every year of a cod glut. They wouldn't have the processing capacity to process it or the harvesting capacity to harvest it.

Because the data was giving a really pessimistic signal. And I don't say that because I feel persecuted, but all the time i've spent talking with biologists, they're all quite comfortable now talking about the variance in the system that we're dealing with. If you look at northern cod recruitment since 1972 it's been down in the neighbourhood of 175 million fish and as high as 400 million three times each. Three full cycles of nearly three-fold change in size.

Q: You're speaking of numbers with a fair degree of precision. Is that warranted?

A: Talking about the history from 1972, which was a big turning point in the way that the old ICNAF countries...

Q: Let me interrupt for a moment. What I'm trying to get at here is whether science has brought criticism on itself by pretending to or seeming to be capable of delivering knowledge with a degree of precision that was not warranted.

A: In this case, the recruitment from 1972 to I'd say about 1983-'84 now, we're quite confident of. And we're quite confident of them because we can count them. The sampling starting in 1972, international standards were adopted, exchanges of the otoliths so you could check that the Spanish and the Portuguese were aging fish the same way we were. The data became much more standardized in how it was being collected and handled.

You take the year-class produced in 1972. There are almost none of them around now. All you have to do is count up the number of four year old fish in '76 and five year old fish in '77, that were caught, and so on. You total them up and you've got a bottom line. It had to be at least that big because we've seen that many fish. The natural mortality is a wild card that you through in but it's a scaling factor and there's any number of ways to go about showing it. Unless there's a systematic change in natural mortality over time, and that's not out of the question if you're dealing with a heavily polluted body of water like the Baltic sea,...

Q: Or in our case possible changes in the ocean climate like a sudden influx of cold water.

A: Yes. But that's a point event and would show up as a marked...let's say that you're tracking a year-class and the bottom fell out of it. I certainly wouldn't suggest that that has never happened. But when it happens it really stands out from your relatively smooth decay of a year-class over time.

Q: So what you're saying is that you're getting pretty good at virtual population analysis but prediction is still anybody's guess.

A: Still anybody's guess. The totalling up of what was out there is now getting t the point where I would say ten percent error is generous.

Q: But what's out there right now is still a big question much less what will be?

A: Yes. What's out there right now I would say 30 (i.e. 30 per cent confidence interval around the estimate)...for a stock where you can trust the data that you're getting from industry.

Q: And is that a given now?

A: In northern cod we believe it is with the small proviso that we don't know about discarding in the inshore. But the observer coverage on the offshore really is working. They've had all kinds of undercover experiments and the observers really are doing the job of keeping the industry honest about what goes in the log books. You occasionally hear accusations that there's collusion, the observer's being paid of by the captain and stuff like that. I've heard, they'd never consult science about it because it would blow the whole thing, but we've heard of at least three investigations all of which came out with a completely clean bill of health for the program. So the offshore is being reported accurately. The inshore, because it is on an allowance rather than an allocation, is free to overrun the allowance any time it wants to.

Q: What about the middle distance crowd?

A: The middle distance crowd is a new kettle of fish and it's one that many of us, and I don't mean just scientists, are really wary of. I don't know what's going on on the middle distance trawlers.

Q: It sounds like no one does. There are continuing reports of massive discards and unreported night-time landings at fish plants.

A: That would not have been a problem until the last few years. For the core period of late 'seventies and first half of the 'eighties they don't represent a source of substantial error. But they certainly could now.

The scientists are....When J.J. Maguire had his big presentation back in May of the northern cod assessment, it wasn't just the northern cod advice, it was, here's the assessment. And the message he kept stressing was, we're not going to say whether fishing mortality is .47 or .52 or .57. Relatively small nuances of a number of things can influence that bottom line. What we will say is that we are damn sure that fishing mortality is way above our target and we need to lower it. And how much we need to lower it and how we go about lowering it are decisions that we are consulting with you people on.

The stock is not going to collapse overnight if you keep fishing it at the same level it is. It'll collapse...you can't fish it at this level forever. But in the short-term like 1990, 1991, the stock will survive and stay healthy and continue to reproduce itself, especially because there's some evidence of some good recruitment coming up. But if you keep fishing at this level, the more you lower it, the more it's going to rebuild. That's a very different message than scientists used to give. They used to...I agree. They used to be guilty of saying "The number is this."

Q: Is this a symptom of the long-standing position of privilege and authority that science has been granted and enjoyed since Newtonian times?

A: When I came into the CAFSAC steering committee....I wouldn't put it that strongly. Again, very pragmatic people having been burned a few times. For a couple of years they said, "Here's the confidence interval that the answer lies within." And you give that to people who aren't used to dealing with confidence intervals, try to explain to them what a confidence interval is and they say "Oh! That means that I can take the number at the top!"

After two or three years of getting burned that way, the elder statesmen of the discipline developed the principle that, if you give them a choice, they're always going to take the most optimistic interpretation they can. So unless we believe that the upper number really is as good as the mid-point, you better give them the mid-point. And that was the reasoning behind it. It wasn't...certainly going back to 1982 when I joined the department.

At no time, then or later, did I feel that the scientists were deluding themselves about how accurate their results were. It's just that they didn't trust anybody further on in the process to take a range of options as anything other than an invitation to take the most optimistic one. And I think that fear on their part was reasonably well-founded. But in doing so, they really set themselves up.

Q: Because that created the illusion and assumption of precision on the part of the consumers of scientific knowledge.

A: One of the really frustrating things, to the working scientist, about both the Alverson and the Harris report and many of the people interviewed frequently from the university, biology people, ocean sciences and stuff. They will never stick their neck out and say, "The answer that they gave of X was wrong and it should have been Y." They are quite happy to say, "X is wrong," but they will never allow themselves to be pinned down to what the substitute is.

Q: That's not quite true. I seem to recall the Harris report, equivocating a bit but saying things like, "On the whole, we are inclined to think that fishing mortality has been considerably higher than estimated and is probably in the range of .35 to .50 instead of F^{0}.1."

A: Yes. We have learned from their equivocation, and I think there's a little bit of bitterness that doesn't have to be there. That if they can do it and be recognized as the leading experts on the cod stocks, we're going to start doing it too.

It would of been very easy, when we did the really controversial assessment that lead to the Harris report, to have said, the answer must lie somewhere between this figure and this figure. Rather than spending another day and a half in a room with twenty people saying, "If we give 100 per cent of the weight to the commercial catch rate we get a .3. If we give 100 per cent of the weight to the research survey we get a .55. How do we reconcile them?" If we'd been willing to stop there and say, "This range bounds the answer and we tend to think it's on the upper end because we have reasons to believe that the research vessel is likely to be more accurate."

That's about what Harris did. And there were a couple of people who didn't want to go any further than that. But the advisory system, up until the northern cod problems, really wanted the scientists to resolve it down to a point, with the message coming back that, "If you don't do it, who's going to? Who's in a better position than you are to reconcile the conflicting information?" And that's the kind of stroking that any professional, not just scientists, but any professional within his field...."Who's in a better position than you to reconcile divergent information in your field of specialization?"

Q: And you never heard the bomb ticking?

A: We kept doing it. I'm sure there's a part of me that's responding defensively but there's a part of me that's also responding very honestly.

I sat through a lot of discussions with those people. And I think the conviction was that if we don't do it, the people who will....Because at the end of the day it has to be done. You can't come out and say, "The TAC is going to be somewhere between 150 and 250 thousand tonnes and we're going to watch and see how it goes and tell you half way through the season where we want to end." You just can't manage the resource that way. They need an answer.

Q: So through various pressures, some of them externally imposed and some of them internally imposed, you came to deliver your advice in a form that gave an illusion of precision that was not warranted and that you knew was not warranted. But you felt that both for the good of the resource and for your own personal and professional reasons that this was the best of several choices.

A: Yes. I think it's really important that at some point in your work you go back and look at the NAFO reports because up until 1987 the assessment was done in NAFO. And in the NAFO Redbook is where the annual assessments are reported. And in the Redbook you'll see, going back as far as I went to check in preparing for the court case, the qualifiers are all there in the text. There's this class of reasons to worry that it could be higher and this class of reasons to worry that it could be lower.

CAFSAC has been active since 1978 but, for a long time, because there were both foreign allocations of cod within the 200 mile limit and a fishery outside the 200 mile limit, it was treated as a stock that was a trans-boundary stock. And trans-boundary stocks are assessed in an international forum. The southern Grand Banks stocks are still done in NAFO. The decision to do the northern cod assessment as a strictly Canadian stock was made in 1987, I believe. It was resisted by a lot of European countries which fish outside the 200 mile limit.

They argued very strongly that it should not be treated as a solely Canadian stock because there is a trans-boundary component to it and Canada is signatory to agreements which acknowledge the legitimacy of international review of trans-boundary stocks.

Q: Let's shift to current stock assessment practices. You've said that you think that commercial catch data from inside the 200 mile limit is now reasonably reliable. Let's talk a bit about RV data, survey design, physical sampling and some of the hydroacoustic work. What's your assessment of the state of the art?

A: The research vessel survey...It's time to review the stratification program where we use a random stratified design. You know about stratification in things. I think a lot of the stratification theory came from the social sciences rather than the physical sciences. It's time to review the stratification design to see if it's the most efficient one possible. You can't do such a review until you've had the better part of a decade of surveys. The whole point of stratification design is minimizing the within strata to among strata variance. And you have to have several replicants to get a handle on those variance estimators.

There's no question that stratification is a gain over completely randomized design. Whether we have the optimal stratification design is one of the jobs that has been on the plate for about two years and we're into the third year. We just can't get to it because we're busy servicing crises instead of doing something like that. We're looking at whether there might be a gain in going to fixed stations rather than a new random sample each year. You run a risk of increasing your bias in exchange for getting a much lower variance in your estimate. But with ten years of data we can get a handle on how big the bias is.

It would have to do with whether there are systematic changes in the distribution of fish over time. It's quite likely that there are. Cold years are systematically different than warm years. That's an empirical question that we now have the data to look at. And we plan to have that work done, not in time for this year's survey but in time to design next year's survey.

So the design itself is pretty well grounded in statistical theory and I feel pretty comfortable with it. Whether we've got an adequate sampling intensity is hard to know. More data are always desirable. Certainly being able to increase the number of sets by only 20 per cent with the extra money we got last year enabled us to take the variance in the estimate of numbers down 42 per cent.

And if we can increase the sampling effort a little bit more we might be able to get it down a little bit more. It looks like there is an asymptote. That's part of the simulation study we'll be doing. What happens if we only increase the effort 10 percent, fifteen percent, twenty percent? So you can begin to plot the shape of the decline in variance. Is it still decreasing quickly so that there will be more gain or is it at the point where it's beginning to flatten out? For a further meaningful decrease in variance you might have to triple the sampling effort and that's just impossible.

Q: But you're not sure whether you're at or approaching that point yet?

A: We'll know by this time next year.

Q: To what extent are you using hydroacoustic data and is it seen as a replacement or supplement to physical survey data?

A: Supplement to. One of the things we've discussed, because there's a lot of money in the northern cod package for hydroacoustics....It was suggested several places, including the Harris report, that we get a hydroacoustic index from the inshore.

Everyone that we've talked to who's knowledgable about hydroacoustics has said emphatically that the technology does not exist to give you a reliable index during the inshore mid-June to mid-September period. The spatial heterogeneity of distribution is so great that you'd have to have thirty boats....

I mean, if you could have thirty or forty boats all working full time with a full complement of scientists on them you could cover the area from Cape Race up to Makovik. But you'd have to do it on a scale of half a kilometre at most to do a decent job of getting something reliable. That's just impossible right now.

Offshore, we've tried for three years to get an offshore biomass estimate when they're in their pre-spawning aggregation. For a couple of years we've had technology problems because all the hydroacoustic gear is still reasonably delicate instrumentation. We used to wreck it on the ice and do hundreds of thousands of dollars worth of damage each cruise. We have those problems solved and we're off to a good start in mapping the total size of the offshore concentration during the pre-spawning period.

So we're pretty optimistic that by this winter...I'd say that the winterization program we had last year was about eighty per cent successful. And the twenty per cent that kept us from completing the job, we expect by next year to have it really well worked out. The kicker there is the winter ice conditions. Now that the hydroacoustic gear works during the winter we can deploy it essentially anywhere the boat can go.

Unfortunately there will always be ice so thick that no boat that tows fishing gear can get through it. But what we will be able to do is say that the absolute biomass estimate in the area that a boat can operate in is this much. And if the number is big we'll be happy. If the number is small and there's a lot of ice, there's always the possibility that the fish are simply under the ice. It is folk wisdom that fish don't like to be under the ice.

Q: But that may simply be an anthropomorphic projection.

A: Yes. You ask people why and they say that it's so dark down there. Well they're down 350 metres or more and there's no light getting down there whether there's ice or not. It's dark wherever they are.

Q: I've talked to some people doing hydroacoustic work and they've told me that there are still a lot of sources of error that they can't control yet. For instance, they took two identical machines, calibrated them identically, put them on two similar ships steaming side by side over the same aggregation of fish and the variance in the return signal was something on the order of twenty to twenty five per cent.

A: That does not surprise me.

Q: And the technical people I was talking to said that that isn't the big problem. That isn't the real source of error. They're not working too hard on that yet because the behaviourial variables are even greater. There's no point in making the machinery more accurate when the signal strength varies so widely depending upon the orientation of the fish in the water column. I was told that in the most extreme cases, the variance can be as much a seventy per cent.

A: And that also doesn't surprise me. For a long time the Norwegians were being held up as the great example of the application of hydroacoustics. They were actually doing the hydroacoustic surveys and using it to tune their cohorts. When they were over here last November for the cod/caplin working group meeting, they said they had abandoned the hydroacoustic index for assessment purposes.

They still did the survey. You learn a lot about the biology of the species because you can map local patterns of distribution, do some oceanography, differentiate cod and caplin and stuff. We would never cut back on our hydroacoustic efforts because we are learning so much about cod from them.

But people who think that in the short-term hydroacoustics are going to give us direct biomass estimates, replace traditional indexes, aren't really familiar with what people who are trying to apply it to a fisheries context will say. There's a great deal of private sector interest in hydroacoustics and you can find promotional literature that promises everything. But if you look at people who are applying it...Everyone says it's extremely valuable, extremely enlightening to do, but to take a number and say this represents the fish that are out there is a step that I don't know anyone who's in fisheries is willing to take.

Q: It seems to me that at present it is the most theory-laden of all the sampling techniques and the most subject to influence from other unknowns.

A: That fish orientation problem you pointed out. I know of three different groups of people who have worked on the pure mathematics of what happens when a signal is bounced off randomly oriented objects. How can you reconstruct the total number of targets? It's a problem that's of interest because it has Star Wars applications. You've got a radar signal showing a bunch of objects coming over the horizon and you want to know how many of them and what they are. Star Wars money has bought a lot of bright people working on the same problem but we still don't have that nuclear umbrella up there yet. Radar and missiles or hydroacoustics and fish. Mathematically it's the same problem.

We were talking about indexes, the research vessel index, hydroacoustic index, the catch per unit effort from the offshore. The catch data, as I've said, we're quite happy with. The effort data is shaky. Not because they lie but because there's no reason that they've ever had to be systematic in what they do and how the skippers keep their log books. What's effort? From the time the net hits the water until it's back on the deck or from the time it hits the bottom until the time you start to haul back? And there's been an awful lot of technological progress.

Industry, whether it was vested self-interest or not, and I say vested interest because industry was quite concerned with what a low influence the CPUE data had on this year's assessment, they have been incredibly cooperative in making available to us really detailed records of their best skippers. The skippers personal log books. Not the required information that goes to statistics branch but what every skipper keeps.

They have come to us saying, "Tell us exactly what you want and we'll provide it." They will try to match vessels, because both FPI and NatSea have vessels that are the same in everything but name but they may differ in the time that certain pieces of technology were introduced. "We'll try to match skipper expertise, we'll give you two identical vessels, and we'll give you the skippers' histories and the time at which certain pieces of technology were introduced." It was this trip that they first used the SCANMAR sensor to say where the trawl doors were.

They're providing all this information to us and they've come through with what we've asked. "Tell us what piece of technology you're interested in and we'll give you the data to refine what effect that technological change had on your CPUE index." We haven't solved the problem of getting effort really reliably down, but boy, has industry shown an incredible willingness to make available to us the information that may help us do that. It's a non-trivial analytical job to go through all the data but just to have it offered that freely shows a real act of good faith on the part of industry.

The final index we hope to have very soon is the inshore. We have the logbook program which like any big program has had a rocky start but each year it looks better. And one of the things that we're getting with the northern cod resources is a dedicated biologist to spend the whole summer going from community to community, whether it's the logbook program or some mutational form of the logbook program.

But this will be a person devoted to spending the whole summer dealing day-to-day on the docks with the inshore fishermen and spending the rest of the year converting what he collects into some sort of an index which will start off with equal weighting in the assessment process, i.e., it will have as much chance of influencing CAFSAC's view of the stock as any other index does. We hope to have that person staffed by October so they can spend the winter getting to know the fishermen and the associations. Send them around to the winter meetings and stuff.

Q: One of the most common criticism I hear fro the public about science is that you just hide away up in the White Hills and we never see you except when it's to tell us bad news. When we first met in that meeting with Mac and Peter Shelton, Barbara asked a question about the place of traditional knowledge in the process of resource assessments whereupon Mac launched into a long story about a scientist who had spent too much time with fishermen and come to a bad end. The way he told it, the story clearly had a moral and that moral was that it was not only a waste of time for scientists to spend time with fishermen but that it was potentially dangerous.

A: I can't recall that story exactly.

Q: The point was that this person had mis-placed sympathies which were very human and perhaps understandable but not only had he neglected his duties as a research scientist but in the end the conflict between the two cultures had in some sense destroyed him. The moral of the story was very clear.

Don't fuck around with fishermen and if you do, look out! I was very surprised at the edge buried in that story.

A: Mac, at the time that you talked to him, was not a completely objective person. He had, as it afterwards turned out, a quite legitimate fear for his own neck.

But certainly in the time I've been with the department, there's been a long history, well I can't say long history because it hasn't been that long, but going hand in hand with spending a lot of time dealing with the inshore fishermen, is a really severe case of burnout. And a great deal of frustration.. Not with the system for discouraging you from doing that. I certainly have...If I went back and went through my book I probably went to 15 inshore fishermen's meetings in the two years I was head of division of groundfish. That's not a great record but it's not a bad one either. As Division Head I wasn't always the preferred...If it was about particular species, they'd want the specie biologist responsible for it. And that's where the burnout came.

Henry Lear is a classic case. A really excellent cod biologist and son of a fisherman who is 77 and still out more days than not. The Port de Grave Lears. He became the person the department would send to every hostile meeting of inshore fishermen.

It's a really difficult position to be in. They're often angry about advice you never gave. Decisions that aren't based on the advice you did give. Or you can only tell half the answer because the other half is still being debated in Ottawa for its political sensitivities. I, and no other scientist in the department that I know of, has ever been asked to lie. But we certainly have, at various times, been discouraged from revealing the whole truth.

Every government has to do that to its civil servants. You can't have everything that's going on in the halls of government ending up in the newspaper the next day. You have to allow the people whose job it is to make policy talk about what the advice is, what it means, come to the conclusions and make the policy.

When it gets awkward is when you have a northern cod assessment done in January and revealed in the middle of May. That's a very long hiatus. Not to lie but simply say, "Yes, I know what the results of the assessment are but I'm not at liberty to discuss them." Dealing with fishermen's groups a lot you can't avoid finding yourself in situations like that. That context of things is really a recipe to burn somebody out.

I don't know who Mac was talking about but certainly Henry is the example I've seen and it wasn't that anything bad happened to him. He left Newfoundland, he's still with the department. He has a very good job he's happy with. But he was a real loss to Newfoundland because he was a good biologist and so deeply rooted in the inshore fishery that he could go down to any dock in Newfoundland, be accepted as someone who would und ,rstand them, and come away having understood what they ...d to say.

Q: So his burnout was due to the conflict between his native culture and his adopted culture as a federal scientist?

A: To the extent that any case like that has a one sentence explanation, yes. Roughly that.

It wasn't just that. At the time he was being sent out, '85, '86 when they had their big trough in catches, it wasn't a stock decline because the stock was stable for the period when the inshore catches dropped for a couple of years really seriously. No question about it. They went from over 100,000 tonnes down to about 70,000 tonnes for a couple of years. Then they jumped back up to 100,000 tonnes.

It wasn't just that he had a party line that he had to toe. It was that he was really at a loss. He believed as much as any of us that the stock was in good shape but the inshore fishermen were not catching fish.

Now people are saying in hindsight that the inshore fishermen's low catches were the first sign that the scientists were wrong. The fishermen's inshore catches were completely incompatible with what we now view as the trajectory. The stock built until around '84, stayed stable to '87 and then dropped probably 15-20 percent with the really poor recruiting year-classes we've had coming in. So it went up, went flat and no it's down. The inshore went up, dropped a lot, was down for two years, went up and has been climbing slowly ever since. This year the projections are that it's probably going to be the best year in 20 years for the inshore. So the inshore catches are not tracking what we calculate as the total stock trajectory.

A lot of his burnout was that he could relate to these people, he could share the pain they were going through and as a scientist he didn't have any answers! At that time we believed that the stock was still increasing and we weren't right. It wasn't. But the stock wasn't collapsing. At that time, when the inshore catch was going to hell, the stock was maintaining a stable state. The years it has declined are the years that the inshore fishery has gone up.

Q: Does this provide some credibility to the proposition that there may be discrete inshore and offshore stocks?

A: Everybody believes that once upon a time there were big inshore stocks. There are certainly remnants of them but the remnants aren't large enough to support much of a fishery. And the evidence for that is how hard we work, not just in DFO but in Memorial, to get 30 or 40 fish to tag in the winter. It's a hell of an effort. If there were enough fish in the inshore stock to account for these fluctuations, we would be able to find enough fish to apply a 1,000 tags.

Aside from fluctuations in the stock, the contribution of the total stock to the inshore fishery has to be influenced by some kind of environmental factors. One of the excellent correlations, and these are only correlations, is the one between winter ice and inshore catch. The years of really heavy ice have been years of good inshore fisheries.

Now it's being said, "Well, heavy ice means the offshore trawlers can't operate." So it's the absence of the trawlers that gives you the good fishery. The thing is that this correlation goes back to the 'thirties. Since the sinking of the Titanic, there've been reasonably good ice records. And the correlation holds up back to the thirties and before we had the fleet of offshore trawlers.

Q: What's your personal opinion about the controversy surrounding dragging on the spawning grounds?

A: I had to read every paper and everything that's ever been written related to that topic for the court case. So my personal opinion is very much tied up with the science background. And I think it's a total crock of shit. Every component of it is being misrepresented.

The territoriality component of it is really characteristic of large numbers of adult fish being held in a small container than should be there. Anytime that happens, what you get is two or three of the largest fish, and it could be one or two depending on how crowded they are relative to normal conditions. They'll simply become dominant and control the core of the tank and drive everybody else off into the corners. Anybody who studies animal behaviour will tell you that's not evidence of territoriality, that's evidence of interspecific intolerance. You don't want other individuals close to you. And if you're in a limited space what that means is that the most dominant individual controls a lot of the space and the next dominant controls the next dominant and eventually you run out of space and all the sub-dominants are crowded together.

Even the people who've done the basic work which was being presented by NIFA as evidence of territoriality, say right in their papers that when you get the fish down 350 meters, when they're up off the bottom, which they are during the breeding season, there are no physical cues to begin with. The classic concept of territoriality is completely inapplicable because there is no space they're going to defend.

What they are going to do is try to keep other individuals, except sexually receptive members of the opposite sex, from approaching very closely. A net going through a group of individuals who are not tolerating close approach by others simply means that they move relative to the net. Maintain the intolerance. And it doesn't break down any social structure at all. If it were traditionally territoriality and you drive them off their space, the way you talk about birds, that could be more of a problem.. But there's just nothing to suggest, from cod or any other schooling deep-water fish.

The thing about screwing them up so that they don't produce any fertile eggs that year...certainly if the net goes by a pair that is just ready to mate and drives them apart and they each release their eggs and their milt, then you've lost one spawning. But it's well-established in any study that's been done, lab or field, that cod are batch spawners and they don't release all their reproductive products in one push.

For an adult cod it will be about a dozen time over a three-week period, usually about three days apart, they will release a pulse of eggs or sperm. So if you do screw up one mating, you've got 11 more chances.

Do you have permanent residual effects? We can catch fish in February, put them in a holding pen that's the size of this table, keep them there till the boat docks, throw them in a carrying case which fits in the back of a big station wagon, take them to the lab, throw them in another holding tank, and three weeks later they're courting normally and producing fertile eggs. Ken Waywood, who's working on cod aquaculture down in St. Andrews, has to make sure that he keeps the males and the females separate because he's doing this under controlled conditions. And he say you can hand strip them throw them back into the tank, and if he puts a male in with the females, within a day they are courting. And these are females who've been stripped as well as males, they're courting and producing fertile eggs.

And it's really hard to believe that a trawl going by but not catching you is more stressful than being dipped up with a net, being hand stripped of everything that can be milked out of you and then being thrown back in the tank.

Having said all that, industry is capable of catching fish in large numbers in January and February and early March. And they seem to be capable of catching fish in the late fall. To put the public at ease, the government may decide as policy, no, we're going to close the offshore spawning banks for three weeks at the end of March and early April when they're at the peak of spawning. To leave them alone.

But I've told other people and I'll tell you, I'm wiling to bet a dinner for four at The Stone House [a very expensive restaurant] that there won't be a shred of benefit accrue to the stock from the closure other than if the closure results in catching a total of fewer fish.

I think the stock would be much better off if people who were concerned about recruitment postponed the fishery in January and February into March and April so that you get some of the eggs and sperm released. If you drive the fishery into January and February you're catching a lot of reproductively capable fish a month before they're going to spawn.

And that's a likely thing to happen. If you close March and April, you're going to increase the catch in January and February. All those fish you're taking the last two weeks of April have done most of their breeding before you've caught them. If you drive that fishery into February, you're going to lose all of them.

My guess, my professional opinion, it's not a guess. It's based on more hours of reading than I've ever chosen to do about anything. The issue of disturbance is really anthropomorphic red herring. Particularly because cod....

A mature female cod will shed two million eggs in a year. And the difference between a really poor year-class and a really good year-class is the difference between two out of two million and eight out of two million surviving. I have a lot of trouble believing that anything other than the environmental conditions the eggs encounter are the really dominant feature. Two out of two million or eight out of two million is the difference between riches or poverty.

Q: So it seems that except perhaps in the heaviest fishing years of the 'sixties, that environmental factors affecting the natural mortality of the pre-recruitment stock are more important than fishing mortality in determining the size of the fishable stock?

A: Yes. But we focus on fishing mortality because we can control it. And we learned from the 'sixties that if you don't control it, even under what appear now to have been fairly benign and favourable fishing conditions, you can still drive the stock to hell.

APPENDIX J

Interview with Jean Jacques Maguire, Chair of CAFSAC
Conducted in St. John's, Newfoundland
October 28, 1990

Q: I'll start right in the middle of it. I think I'm beginning to understand why fisheries stock assessment science is having such a difficult time meeting the expectations of its client groups. It's an interesting form of science because it does exist to serve the interests of essentially two client groups; one, the industry and two, the needs of the political and management structure. So it is very different from academic science in that respect. There is some conflict resident in that distinction because the evaluative structure and advancement criteria for research scientists in DFO is based on academic traditions; on research and publishing. Not, particularly, their effectiveness in meeting the needs of the client groups. So there's potential....

A: It's a very serious problem as a matter of fact.

Q: There's a second, more serious problem in the surprising inability of stock assessment science to acquire any practical or useful knowledge, knowledge of the requisite precision, to fulfil the needs and expectations of the management structure and of the planning needs of the commercial industry. It's not simply, I don't think, that the systems involved...the macro-system and the sub-systems...are so complex. They are. But that we've tended to think in linear terms of management. Of man's management of natural systems. Rather in classical liberal market terms. If you take less fish now, if you kill less fish, there will be more fish later...all other things being equal. But I've begun to read some chaos theory...the theory that natural systems are essentially non-linear. Existing in quantum states, if you will. Bob May started to do some work in this area, his work in population dynamics. So these are some of the sort of things that I'm beginning to think about. I guess now I'd like to backtrack and review your background and your current work.

A: Okay. I started working with DFO in 1977 in BIO, Bedford Institute of Oceanography. I've essentially worked in assessment. I've worked on north west atlantic mackerel assessment. Several cod stocks. 4VSW--the eastern Scotia

Shelf, and 4TVM--the southern Gulf of St. Lawrence. Pollock, redfish, various stocks.

I got involved in the Canadian Atlantic Scientific Advisory system, which is the only one for Canadian scientists, since 1978. The way the system is structured, you may know, there are five species sub-committees; groundfish, pelagic fish, invertebrate/marine plants, marine mammals, and anadromous/catadromous fish. And the way these work is that they are peer review groups that meet and review individual scientists' work. And there's a second-level peer review. So I have chaired two of these sub-committees; ground fish and pelagic fish...in reverse order. Pelagic first and groundfish second.

Groundfish is the most complicated. Not really the most complicated. That's where the analyses appear to be the most complicated. In fact, the most complicated are the anadromous/catadromous and the invertebrate/marine plants. Because invertebrate/marine plants is so diverse. There's not a standard, agreed-to methodology that you're going to use. And I've been chairperson of the committee of CAFSAC since 1989.

Q: Of the groundfish committee?

A: Of CAFSAC.

Q: There's been a history of crises in the fishery. Particularly the groundfish industry which is, of course, the most economically significant, culturally resonant and politically sensitive. But it seems to me that this current crisis is the first one where science has been challenged and criticised. The previous crises, especially the one the Kirby task force responded to, were essentially socio-economic crises. At that point the projections were for TACs of 450,000 tonnes by now. What is your analysis of what went wrong, if anything went "wrong", in stock assessment science?

A: You've mentioned something, it's the closeness to the clients. I think that's what went wrong. We distanced ourselves from the clients...from what we were supposed to do. And we came to be seen as an impediment for the industry.

It was before my time, but I understand in the ICNAF days when all the foreign countries were fishing off the east coast here, the Canadian scientists were very, very close to the Canadian industry. Because what they had to do then was to work for a common objective. The common objective was to build the resource and kick the foreigners out.

So that was achieved in 1977. Kicking the foreigners out was achieved in 1977. And rebuilding of the stocks, most groundfish stocks, happened very, very rapidly, by 1980, 1983, they were rebuilt. That's the system I've known.

The entire system I've known is that the industry is going to....Well, let me rephrase that. Don't be too close to the industry because the industry, their natural tendency will be to over-exploit the resource. You're not at their service. I think what I'm trying to say is that there was some kind of a confrontation. That we're not on the same team any more. We had been on the same team for eight to ten years. Working for a common goal. And now you were on separate teams. And you more or less had separate objectives.

The objective of the fisheries biologist was to conserve and protect the resource. And the perceived objective of the industry, mobile gear, offshore, capital-intensive, was to over-exploit the resource. That's what was being expected, I think. So that's one thing that went wrong. We distanced ourselves from our clients.

The other thing is that, because of the particular management system that we choose, which is based on Total Allowable Catch, because of that system, there was more precision required of us, fisheries biologists, than we can, than we could offer. I think we thought, at least me, naively, ten years ago I thought that our assessments were much more precise. I think that the experienced people at that time knew that they were not that precise.

But you have a management system that reacts, let me say, dramatically,...or there's a strong reaction to a change of five per cent. If you change the TAC, any TAC, of northern cod or any other one, by a very small margin, it's going to create big problems all the way down the pyramid. And when you realize that the precision of the stock assessment is, at best, on the order of plus or minus 25 per cent, then you realize that there's a discrepancy. And what we're doing is that each year we were adjusting the TACs in relationship with the variability in the data.

And there was total discrepancy between what the assessments that we were doing were saying and what the clients were seeing. Because we had two groups of clients, as well, with opposing views. The inshore seeing one thing and the offshore seeing something else. And often times we thought we were somewhere in the middle but being somewhere in the middle, you've got no one agreeing with you.

So I think those are two of the main reasons. One, we distanced ourselves from our clients so they didn't see us as being helpful to them. And second, the system was expecting more precision out of us than we could offer.

Q: Do you think that science itself had any responsibility in creating that expectation of precision?

A: We created it ourselves, to a point. With the help of fisheries managers. That, of course, is my biased perception. You probably haven't been to an advisory meeting where the TACs and management measures are being discussed.

Q: No, but I'd very much like to if it's possible.

A: It's possible in the Scotia/Fundy region where these meetings are open. Here, it used to be that clients were consulted each on their own. You go and consult with the inshore and then you go and consult with the offshore. But on the Atlantic-wide it's combined. So fisheries managers have a very difficult task when it comes to discussing TACs and management issues.

I wasn't there so I don't know if it was an explicit demand, if it was implicit, if we obliged, but my guess is that we were being offered a very gratifying and important role. "Here's your role. What we have to do is very, very complicated. So please don't make it more complicated by saying that the TAC that you're proposing is not precise." It could be anywhere from 150 to 300 instead of being 200. "Don't say that, please. Help. Help. Say just one number." And I think we obliged. And, as I said earlier, maybe we....I know that when I was doing the assessments way back, I thought that our precision was maybe plus or minus ten percent. Maybe a little bit better. So we obliged.

And we did not come out and say, "This is not very precise. This is between this and there." There were other reasons for that, one of them being...again, our role, our perceived role, was protector of the resource. That if we gave a range we knew that the upper end of the range would be chosen. So we didn't know, at the time, how to present it and still have people go with the mean. In stead of going with one extreme of the range. I think we did, yes, play a role in those greater expectations. But there was always caveats that were either not read or not remembered.

And the number that you've quoted for the Kirby report for 2J3KL cod, those numbers, they were worked up especially for the Kirby Report. But they were also, at the same time, in the five-year report which are called "Resource Prospects for Atlantic Canadian Fisheries."

Q: I've read them.

A: And you've seen the caveat in the introduction which says these should only be taken as guides to likely events, or something like that.

Q: There's one thing that's remarkable about those, that's striking. Every....You'll have a zero date, the present. And on the left hand side is a bar graph with the actual TACs and catches and there's quite a bit of...

A: Variability.

Q: ...variability. And on the right hand side are the projections which ascend with a beautiful, linear stair-step precision.

A: Yes.

Q: Always going up. Always linear.

A: Not for all of them.

Q: For northern cod for the last ten years that I've looked at.

A: Probably, except for the most....Yeah, well, that's right because I don't know that there has been a resource prospect produced since 1989 which was the new perception of the stock. But for northern cod, there was no way to calculate it differently.

The way this works, the way the assessment works, this type of catch projection works, is you look back and you say this is what history tells us. The history of northern cod was that average recruitment was about 600 million fish a year. So when you do your catch projection, you say average recruitment has been 600 million, our best guess is that average recruitment in the next five years is going to be about 600 million.

But what did happen is that those high...strong recruitments, are based on data from the 1960s. Several hypotheses. Maybe northern cod was more productive in the 1960s. Maybe there was some over-reporting of catches by foreign countries because the TAC management system was going to be put in place and your share of the pie would be based on what you did catch. There was some incentive for that.

But for the last 20 years, since 1970, it's remarkable that the recruitment goes up and down and up and down and up and down with an average of about 300 million. Half of what the long-term average is. Quite a striking difference. So there was no way in 1980 or 1983 that that could be seen. Being, at the same time, in a psychological climate where the inshore was doing very well in the early 'eighties and the offshore was doing extremely well. So, yes sir! The stock is rebuilt. We're going back there.

Q: What lead to the re-evaluation of the degree of precision that you are working with? You said that when you first became involved with the assessment you assumed that you, and I presume your colleagues, were working with....

A: I'm a bit more naive than my other colleagues.

Q: But there certainly has, in the last two or three years, been a...

A: Recognition.

Q: ...a recognition that you are dealing with levels of uncertainty far higher than was previously thought. Are you familiar with the events and, perhaps, the internal debates that lead to this new recognition?

A: Yes. I think, for most people, it looks as if the 1989 assessment of northern cod was where it started. But it didn't start there. In my view, northern cod was the last one to be revised and not the first one. The first one was 4VSW cod. About 1985 or 1986. I'm not totally sure which year.

One of the characteristics of the technique which we use, which is sequential population analysis on which you've probably read, is the further back you go, the more confidence you have in your assessment. It's called a "convergence." When we extended jurisdiction in 1977, we said there was all those big foreign trawlers out there. Fishing mortality must have been very high. We've kicked them out and replaced them with large trawlers but much smaller and much fewer of them. Fishing mortality must have gone down. If fishing morality is down, stock size is higher. We held that belief for five, six, seven years.

But as time passes, you do the assessment and you estimate the fishing mortality is .2. So that was in 1980. You do the same assessment in 1981 and you estimate that the fishing mortality was again .2 but when you look back, you see that it was .25 for 1980. Whoops! What happened there? After a few years, you look back and you do the assessment in 1985 and you see that for the first part of the 'eighties the fishing mortality was about .4. So you say, why would it be .2 today? There's no reason for it to be .2. The boats are fishing as hard. They're out there as long. Their efficiency has probably increased. Which we didn't take into account. And there's nor reason for fishing mortality to have decreased. So it must be .4. And of the alternative explanations, .4 was totally acceptable.

The first one was 4VSW cod and then all the others, more or less, came that route. And 4VSW cod went, at that time, from probably 54, 55,000 tonnes TAC to 38 or 40. Which was a big drop. But it's only 14 or 16,000 metric tonnes compared with 266 to 197 which is a perception of a much larger change. That was the first one. The others followed.

Q: Are you familiar with the history of the Alverson Commission and the Alverson Report?

A: Yes.

Q: Until some recent discussions, I was under the impression, as perhaps most outside observers were, that first critique of the way science was doing its job came with the Harris Report. Because the version of the Alverson report that was made public, to the extent that it was critical, it was very mild and, in public, it was called a vindication by the scientists.

A: Yes.

Q: I've been told by several sources that the original, the first draft of the Alverson report was considerably more critical and the lessons made public in the Harris report were originally....

A: That I don't know. That I don't know. But the Keats Report was the first one. That was commissioned by NIFA. And that was not taken very seriously at the time.

Q: By science?

A: By DFO science. Yes. Because the analysis was somewhat naive, I think and because it was easy for us to discount it. The TGNIF report, in my eye, the way I read it...I don't know if I've seen the same version, but most likely...was critical. It was critical. Maybe it's the broadcasting of it that was more positive. But I think it was critical.

It must be remembered, as well, that there were not that many conclusions that could be reached. The TGNIF report suggested, when you look at it, that the difference between the TGNIF report and the CAFSAC assessment was much greater than between the Harris report and the CAFSAC assessment. The Harris report and the CAFSAC assessment are essentially the same. They're bang on. They're saying exactly the same thing. While the TGNIF report was saying that CAFSAC has over-estimated stock size.

Q: But, as I recall reading, the difference was something like five per cent.

A: It was more than five per cent because our assessment at that time was for a fishing mortality of about .2. And theirs, their range, was from .2 to .4. And they picked in the middle, .3. So there's a much broader range. And the difference was quite large.

Q: What I'm thinking of is not the original but the DFO report called "The Science of Cod" and the first page was about how DFO had estimated that the stock had grown 5.5-fold and this independent review had concluded that it had only grown five-fold but really, that's pretty close and really we're doing a terrific job.

A: That was the interpretation that we wanted to give it. If you look at it from a different perspective, the assessment that TGNIF did showed that there was about a third less cod than we said. And the Harris report says exactly the same amount. So for TGNIF, that's the interpretation. For broadcasting, for publicity, the way we decided to use it.

Q: Where was that decision made. That certainly would have been made at a level higher than the Science Branch.

A: Probably. I don't know. I don't know.

Q: And yet science is taking the public heat for that.

A: I wouldn't say that it was higher than science branch.

Q: No?

A: I think if....Again, I don't see ourselves having done much different with the Harris report than we did with the Alverson report. We used the Harris report almost the same way saying, "Look! Harris reaches almost the same conclusion as we did." That's the way we used it. Except that the noise that was generated has been much higher because the stock status that was estimated was much smaller.

Q: Perhaps the trigger was that the Alverson report did not result in a direct reduction in quotas whereas the Harris report did.

A: From the handling of it, as well, there's another slight difference. Which is that the Alverson report was presented, more or less, by DFO and the Harris report was presented by Harris.

Q: So there was more of an opportunity to manage the presentation of the Alverson report.

A: I think so.

Q: I recall reading comments by John Crosbie and others to the effect that, if this is the best that DFO science can do, it's not good enough. And why are we...why do we continue to support their massive budgets? This would have been back in...right around the release of the Harris report. Do you recall that incident?

A: Yes.

Q: What would be your response to that?

A: The response is a question. What do you need to manage fisheries? You need some kind of scientific information to follow up. The second question is, how do you want to do it? And from my perspective now, and this is really my own perspective and not DFO-wide or CAFSAC-wide, is that doing it by managing catches, by regulating catches, may not be the best way to go. At least from our perspective. Because our precision on catches may be only plus or minus 25 per cent. I say "minus" but in most cases it's plus 25 per cent. We rarely underestimate the stock. We more often overestimate it.

But if you look at some other things, like fishing mortality, the reason that we're that imprecise, well, plus or minus 25 is not bad, is that incoming recruitment...we don't know very well what recruitment is going to be and it has a very large influence. But on fishing mortality we're a bit better. And I'd say that there are things we can say with more precision, almost absolute precision. I can tell you right now that fishing mortality for northern cod is most likely above F-max. That we should decrease it. So that's relatively clear.

Q: Even at 196 [thousand metric tonnes]?

A: Yes. The Canadian TAC. There will be foreign fishing outside on the Nose in 3L so we expect that the catch is going to be something like 215 or 220 right? Total catch. So the fishing mortality needs to be decreased. We know that. If we were managing sea-days or some fishing mortality units instead of catch units then we would know that we need to decrease that amount. And then once we have decreased, we can assess again. Have we decreased enough? Are we below? Have we reached our target? This is potentially a more stable way.

I wouldn't want to give the impression that it's totally simple to do it because it's not that simple. If you decrease the number of sea-days, then you will make sure, as a fisherman, that the number of sea-days that you're left with are better used. So you're more efficient during these days. So it's not straightforward.

Q: And that would then create....

A: Our conclusion....What I wanted to say is that our precision may be a little bit better. And still, weather forecasts is probably something like that. Plus or minus 25 per cent, right? Depending on what you do. If you say, "It's going to rain," you probably have a better chance of being right than if you say, "It's going to rain and the rainfall is going to be this much." So if you ask me if the fishing mortality is above F-max, I tell you "Yes. It is. Most likely." "How much?" "Probably this much." I have less confidence in that amount. So really, it boils down to what is the question.

Q: So personally, your assessment is that the stocks are still in decline?

A: No. The stock is stable.

Q: So what is F-max?

A: There are two things that are easily mixed. F-max and F-MSY. And they come from two entirely different models. F-MSY is from a general production model that has a shape about like this [draws a curve]. And once you're above that then your stock is going to decline. F-MAX comes from a yield-per-recruit model which is something like that [draws another curve]. And F-MAX is the fishing mortality. But the behaviour of the stock, this is only catch. This is yield. The behaviour of the stock depends on recruitment. So if you're above F-MAX and got good recruitment, the stock is going to increase. But you're not going to get as much weight out of that recruitment as you would if you were fishing at F-MAX.

Q: But surely managing at F-MAX is much more sensitive to variations in recruitment which is essentially unpredictable.

A: What fishing above F-MAX means is that you're catching the fish a little bit too rapidly. You're not leaving them enough time to grow. So if you were fishing them a little bit less, for the same number of fish, you'd get more weight. And we're not getting that now.

Q: Another....

A: But that's a wrong perception. That the stock has decreased. The stock has decreased from about 1983-'84 to about 1987-'88. Since that time, it's been quite stable. And at best....The stock is not in danger. The stock is not in jeopardy. Cod stocks are extremely resilient. Unbelievably resilient. And there's good year-classes coming in. If anything, if we fish the same in 1991 as we did fish in 1990, catches are likely to increase a little bit and stock size to increase a little bit.

Q: There are at least two good year-classes in the pipeline.

A: Yes.

Q: What would you say to a critic who says, "Give me the reason why I should believe what you say now when two years ago...

A: You were wrong.

Q: ...you were so horribly wrong." Why?

A: Well, you do what you want, but this is the best advice we can give you now. We're going to qualify it. We're giving a range of options now. This is the best assessment we have. But in the past, what we were telling you...I'd say five years in the past...we were telling you that F0.1 is the only way to go.

So if I were doing today what we were doing five years ago I would be telling you you've got to cut down the catches to 100,000 metric tonnes. But I think in recognizing where we were in terms of exploitation rate, in terms of stock status, recognizing where we were, we recognized as well that the stocks, some of the stocks, were more resilient than we thought. And it may not be required, it may not be necessary to go immediately to F0.1. Or even go to F0.1 at all.

It depends on what you want to achieve. And for northern cod today, depending on what you want to achieve, if you want to achieve growth, if you want the cod stock to grow, then you should decrease the catches. The more you decrease them, the more the stock is going to increase. If you're happy with the size of the cod stock the way it is now, then you can keep the catches about where they are. If you want to got to F0.1, you've got to go to about 100. If you want to go to F MAX, you've got to go to 160, 170. If you want to go half way from where you are today to where you want to go, which is F0.1, the you should go about 145, 150.

So I think, relating this back to what you were saying at the beginning, is, by providing those numbers, we're trying to be more useful to the fishing industry, to the fisheries managers and to the over-all system. At the same time, we're achieving what we thought was our job all along which was protecting the resource. But by protecting the resource, you don't need to create so much disturbance in the socio-economic fabric of the system. You may want, by recognizing ourselves that there is some variability around our estimates, we're saying don't make rushed, hasty decisions. The cod's not going to disappear. We've got time. So let's look again. [pause]

I'll say something while you're thinking. You've got a system, a very structured system, for the provision of biological advice. Now if you want to manage the fisheries properly, because really you're not managing fish, you're managing people, and communities and plants and stuff like that, it would seem to me that it would be useful to have some sociological input, some economical input. These inputs may exist. I know that from the economical side they do exist. But whether they are peer-reviewed, like the biology is, I don't think so. I know it's not a structured peer review and the information is not public. So I'm raising a red flag there. You're basing your decision on very structured and peer-reviewed biological advice but you must take in to account economic and social information as well. How do you get it? Where do you get it from?

Q: There's been a lot of agitation for the need to include so-called indigenous knowledge, fishermen's knowledge, in the assessment process. This has a lot of political and cultural currency at the moment. It seems that there's some resistance within science to this idea. That's understandable because the language of science is mathematics. Even if there were a willingness on the part of science to incorporate this knowledge, it would be very difficult. It's like speaking Mandarin Chinese and English. They're two different systems of knowing. Different evaluative traditions that seem almost mutually exclusive.

Q: It depends how you perceive yourself...we perceive ourselves. I think for a very long time we perceived ourselves as holding the true picture. You, the inshore fisherman, have got your perception. You, the offshore fisherman, have got your perception. We see the big picture. You see only part of the picture. And, because we thought we saw the big picture, we thought we didn't have to explain too much to you what you were seeing. Or to reconcile what we were seeing and what you were saying. We thought it was good enough to be somewhere in the middle of you two. And that we didn't have to explain.

But if you change your position, or your point of view or your perceived role, and if your role now is one of counsellor, of advisor, a useful counsellor and advisor, if you're an inshore fisherman and you tell me that you observe this, the cod not coming inshore, whatever, my first reaction is going to be well, I'm going to dream up an explanation. "Dream up" not having a negative connotation, but I'm going to try to think what the reasons are and to offer you that explanation. And I'm going to hope that you're going to be satisfied with that.

But we've got to do more than that. And the difference of language shouldn't be that much. The onus is on us to be understood. It's more difficult for us to be understood. Because it's easier to talk about "RV" instead of research vessel surveys. It's easier to talk about "CPUEs" instead of catch-per-unit-of-effort. "Non-linear least square minimization" and stuff like that. Instead of verbalizing and explaining what they are.

I think the question is not so much introducing anecdotal and local knowledge and stuff like that. The objective is to understand what's going on and to try to explain what's going on. Is to relate. Is to go out there and say, "What do you see? What's your explanation of what's going on? We'll go and check it out." And we must go and check it out.

Q: But so many of the people that I've talked to, younger scientists as well as older, are either implicitly or explicitly dismissive of this knowledge. I've had people say "How much can you learn from a bunch of stupid, illiterate fishermen? That the dogberries are heavy this year? What good is that!?" And I suppose now that Mac [Mercer] is no longer there I can say this. When I first met Mac and was talking with him he went in to a long, seeming digression of this business of scientists falling in to the trap of spending too much time with fishermen. They lose their perspective. They lose their edge. They lose their...

A: Objectivity.

Q: They lose their objectivity and inevitably come to a bad end. And I think he was probably speaking of Henry Lear, among others. At least that's what I've been told. But the message seemed to be directed, not simply at me but at the other people who were in the room that day who, in this case, were Jake and Peter Shelton. And, in my limited experience, this attitude is more common than not. Particularly because of the evaluative and reward...

A: ...structural appraisal system. You're totally right. To me, that's not easily solvable. Very difficult to solve. But I agree with the perception that if you're too close to the fishermen you start to see things their own way. And you lose....

What it boils down to right now is that we've got clients and we're producing stuff that's totally useless to them. We've got no links to the clients. If we want to continue to do that, that's fine. But we're going to be out of business. If we want to stay in business we better get closer to the clients. It's straight free-market, or whatever, economical forces.

If I'm close to the inshore fishermen and you're close to the offshore fishermen, we're going to argue and we're going to reconcile our perceptions some way. And what's going to come out of it is going to reflect a little bit of both. So I don't see a big problem with that.

Q: That's in principle. But in practice there's not a fishing wharf in Newfoundland where a DFO scientist could go and not be laughed off.

A: Yes. Because what we see, what we're describing, the status of the stock, does not jibe with what people are seeing. So we must go....I want to come back, as well, to the appraisal.

But, from marine biology perspective, I have, and I think I'm not the only one, looked at the system as being too big to have structure. Chaos. Things happen but you can't predict them. They don't have structure. It happens this way this year and next year it's going to happen differently.

And this may not be the case. There may be more structure than we think. And we may benefit from looking at this bay or this small wharf where things are happening differently from the others. We must look a little more at the parts instead of only the big picture. Again. that's not only between offshore and inshore but also geographically. So that's one thing. I think what we've got to do....

Until now most of the things we've done is say, "Look! This is the assessment and we know it. Okay? This is it. You may not like it but this is it." And now I think we've got to change that. We've got to go and say "This is our best estimate of what's out there. What do you think?" And we're trying to do that now. Formally.

You may have heard or seen that we're making an advertisement to go and be invited by groups to go and discuss the assessments. All the assessments. Groundfish. All the groundfish assessments. And we're going to go and say, "This is our perception. This is our best estimate. What do you think?" They're going to tell us "Well, that may be so but we've observed that seals have increased. We think that your perception of the inshore is wrong. Because more of the gillnetters and longliners are now fishing further offshore, 50 or 75 miles offshore on the Virgin Rocks. You're still including them in the inshore so your perception of the inshore is wrong. How much of the inshore is that?"

We're going to get these questions. And what we must do is go back next year, or in the meantime, and say, "We presented you with what we thought the stock was doing and you had questions. These are our response to your questions. Those that we could answer. The others we can't but we're working on them." Or we're not working on them. But there must be a clear, continuous exchange.

Q: But even that, although that would be a tremendous....

A: We're doing that. We are doing that with people who are directly involved with the assessments. I've done it for several groups on northern cod. The main players on northern cod, we've met with them. Individually. We haven't met with, FPI, National Sea, NIFA, inshore, and Fishermen, Food and Allied Workers, all of them in the same group. Because then they can't talk with total honesty with us because there might be something else at stake. When we meet with them individually, they have been very frank, informative and useful meetings.

Q: Isn't there still a kind of residual assumption of epistemological superiority here? That they have the questions but you have the answers?

A: No. Well, they have observations that they want us to verify, I think. But it's not done in a spirit of superiority. You can't feel that superior to these people who make their living out of it and know more about it than you do. You may think that you've got the big picture but there's all kinds of information that they have that we don't. They have information about misreporting, about discarding, about all kinds of practices that we don't take into account. They don't exist because we haven't quantified them. So they're going to raise those points.

Q: How are you going to deal with the resistance of research scientists or other people to spending their time in ways that there are no points for within the internal structure of DFO?

A: You need to find points. Simple. It's as simple as that. I don't know how to do it and it's very difficult. Extremely difficult to do it and I have no idea. We want to reward who communicate and exchange and do stuff like that.

We say that out of one side of the mouth and then when it comes time to look at promotions, we say, "This one's got 15 primary publications this year. You've got one. Forget it boy. You've met with fishermen. You've met with broadcast people, radio people, university people. You've met with all of these people. But what do you have to show for it? Nada! You're out."

We've got to change that and I don't know how to do it. I don't have a clue. But it must be recognized. I'll give you an example. George Rose, when he came back from his cruise, had an interview on the fishermen's broadcast talking about the large aggregation of cod that he's seen out there. And that interview was a beautiful interview. Very nice description. George told a very, very nice story of what he had seen and that would give credibility on what we were doing.

Now his doing that...and George is also relatively close to the inshore fishermen...his doing that, which is one thing we want him to do, resulted in a lot of problems for us. Because then you've got to react. The perception that there's more cod than we thought there was. That the assessment is wrong because he's seen that many cod the assessment must be wrong and so on. The reaction could have been, was not but could have been, "Why the hell didn't you shut your mouth?" Which is not the objective. The objective is that he's going to go out there. He's going to talk about his research and his results. And it's going to create flak. And we'll need to react to it. It's not going to be an easy job. Gone are the days when you stay in your office and tommorrow's going to be like today and yesterday. We need to react a little bit more.

Q: To move to a slightly different topic, it seems to me that in the last two or three years, the atmosphere of criticism and turmoil, both internally and externally,... a lot of the research scientists and managers, field managers or line managers, who have had opportunities to go elsewhere have taken them. Some people I've spoken to say that they don't feel that they're going to get the support from Ottawa that they need to do their job. They feel that if it's politically expedient they're going to be hung out to dry. That they can't trust their superiors to stand up for them. If they're doing their best job, they still feel insecure about the amount of support they can expect from the upper levels of the structure. And consequently, are leaving for less sensitive positions in DFO. For instance, I gather that in the Science Branch [in the Atlantic region] all the directors' positions except one are now acting. There's been a lot of turnover. And as one person who is leaving told me, "There's a lot of openings for research scientists." And I asked, knowing what the situation is now, what competent, qualified scientist with other options would jump into this precarious situation.

A: A masochistic one.

Q: And this person's answer was, "Well, there are always new Ph.Ds and then there are the Russians." It was tossed off as a joke but those are people with no other options. So, if this is the case, the criticisms, the accusations of incompetence, there's the possibility of them becoming a self-fulfilling prophecy if all the best people don't feel they're getting the support they deserve for doing their best possible work and they're bailing out for less sensitive areas, the vacancies are going to be filled with less than first-rate people. Or there's certainly that likelihood. From your position is this anything to worry about? Is this, in fact, happening?

A: The assessments themselves, we've got a peer review system. Which has one main advantage and one main disadvantage. Let's talk about the main disadvantage now.

The main disadvantage is that it's going to take you longer to get your good idea through because you need to convince your peers first that you're right. So that's going to be longer. The system is going to be cumbersome, somewhat conservative. Inertia.

The main advantage is that your assessment, if it's wrong, is going to be seen to be wrong. It's going to be corrected. It may take a while but it's going to be detected. The assessment side is not bad.

But your question is not really related to that. Your questions is really about the people that are here. And I think it's unfair to....Well, there are several things. For those people involved with northern cod for the last two or three years has not been an easy job. It's not been a rewarding job either. So, of course, you don't expect that people will be attracted to it. But there are research positions....For the research scientist....There are no questions that there will be good research scientists.

There are extremely good recruits coming in. John Hoenig who is with the CODE. All the people in the CODE, they are very, very good. So the recruitment, if you take the time to recruit, you're going to find good people. And there's enough good, qualified, English-speaking Ph.Ds in North America that we're not going to have a shortage. Whether they're going to stay for long or not depends on how well we chose them, we select them.

That's a different story. They may be very good but you shouldn't select them only for good. You should select them to make sure that they're going to stay for a while. It takes more of the personal suitability side than the knowledge or abilities.

But also, to describe the management now, well, Mac has retired so he's being replaced on an acting basis by Larry. Larry's been involved with northern cod all along and has been Mac's right hand for as long as I can remember. Which is probably close to ten years. Jake has gone to the west coast and, as a research scientist and as a father of two girls, you don't have a choice if you want to keep your personal family life and your productive scientific career, which in my opinion, Jake was best at.

And Jake had two hats as you know. One for the codfather and one for the program head. The division chief.

So the section head is being filled with Claude Bishop who is very competent and has lots of experience and knowledge as a scientist. And the division chief by Bruce, Bruce Atkinson who has a lot of experience in the assessment and is a very organized person. A good manager.

Q: Which of these two is going to be administering the special northern cod research effort?

A: Jim Carscadden but Jim is really on an acting basis. He doesn't want to do it on the long-term. The selection process is under way to find someone on a permanent basis.

Q: On this special $42.8 million over five years...

A: I don't have a clue how much money it is.

Q: It's a lot of money and it essentially doubles the science branch budget for five years. Some of the people I've talked to are wondering how it can be spent. The physical plant, the facility of the White Hills is already strained to the limit. There's little or no room for more bodies or equipment there. Certainly you can buy more sea time and the hydroacoustic development effort can use a lot of that money. But, what I'm interested in is the risk that Ottawa and industry are going to want something for their nearly $50 million dollars. they're going to want more fish and bigger quotas. Now, that's not rational. It's impossible to know what the state of the stock is going to be five years from now. But surely you must....

A: I never perceived it that way.

Q: No?

A: No. I never perceived it as buying fish. I thought we were trying to buy knowledge. But you may be right. You may be right. There's something....

Q: They're going to have to answer to the public. Ottawa is going to have to answer to the industry and to the general public for the way they spent....

A: But science is not the only thing in there. You're talking overall close to $600 million dollars. Five years. The management, the CEIC, the employment, all that stuff. So science is only a small part of it. But the way I would do it is....

The basis of it is the Harris report. There were several recommendations in the Harris report. So I would make a link between what we're doing and the Harris report. Say, "Harris said, improve your knowledge on this. And this is what we've done. This is the result." And that's the link that I would make. I think that's accountability that we have to show.

Q: In the same way that you already have the annual program reviews?

A: They're two things. The theory, the face of it, on paper this region had the best program review that you can dream of. It was regular. It was structured. It was organized. But it didn't yield the results that were expected. And I think that's because the meat was taken out. The process was there but the intention may not have been. The process was there but the intention was not to get the clients' input and to act on the clients' input. That's a hypothesis.

Q: As we say in the United States "All show and no go?"

[discussion of J.J.'s schedule and the time remaining for the interview]

Q: On the basis of my questions and the general tone of the conversation you might have some observations or comments that you wish to add. About the current state of stock assessment science and where you see it going in the future.

A: Where I see it going is increased communication of the uncertainties in the assessment. Part of the reason for the shit we got was that people thought we were precise 100 per cent. So when they realized that there was a plus or minus 25, at best, they think you're full of it. Really, you're not being very useful. So communicate the uncertainties. Be useful.

Instead of being theological about what should be done, provide advice on what's feasible. You still have your reference but provide useful advice on what's feasible, what's achievable in the coming one, two, three years. Recognize and make it known that some of the variability in the assessments and the catch forecasts is essentially based on variability in the system. They're not real reflections of changes in stock size. They're essentially reflections of variability in the data.

The difference between last year's assessment and this years' assessment is a very good example of that. Stock status is exactly the same between 1989 and 1990. Exactly the same. Except that we've done the assessment slightly differently. Which resulted in a difference of about 25 per cent. So instead of using the catch rates on their own and the survey on their own and combining, we combined at the beginning of the program. So instead of doing two assessments and taking the average of the two, we did one assessment and that resulted in a difference of 25 per cent.

Q: This is curious. You say that the stock status is the same but the assessment is different. But isn't the assessment how you determine stock status? And how can the two pieces of knowledge exist independently?

A: Okay. Last year when we did the assessment we said that the stock has been doing this, peaking at about like this [draws a curve]. So we said, this is 1988.

Q: Okay. The line is lower but it's the same curve.

A: We do it this year and we find that it's like this [draws another curve]. So stock status is the same between 1988 and '89 but it's different between the two assessments.

Q: So the status is stable but it's stable at a lower level.

A: At a lower level than we estimated last year.

The other thing we've got to work on is the appraisal system. We've got to make it possible, we've got to find rewards for people who are working on things that are relevant to the management questions. We've got to find a system...I'm not just talking about this region. This applies to all the regions of DFO in Atlantic Canada.

Science was several years ago, many years ago, maybe responding a little bit more. But we didn't have good management system. We didn't have transparent management where you have the work planning process and reporting on what you had done and stuff like that. Not very structured.

Now we have that very structured. But one of the pitfalls of that or the negative side is that you do your work plans between about December and January. Over those two months. And my problem....If I don't perceive and communicate my question to you during that time period, then I've missed a slot and it's going to be the following year. So we've got to change that. It's not possible. All the questions and problems do not surface between December 1 and January 31.

Q: Certainly, the issue of reevaluating contributions to the DFO process....Under the current climate, the people who are doing the most valuable work for DFO are those who are working to restore the credibility of science with the client groups.

A: Yes.

Q: Yet there still is...you gave the example of George Rose...there are certainly people who felt that he shouldn't have....Internally, his work caused more trouble for them.

A: Some people did have that perception. But we've got to recognize that we've got to live with this. Live with these perturbations. The other thing that we've got to work on is getting a lower profile for biology. Biology taking its appropriate place in the system. Which is when information and advice....One part. One element. The factors that must be taken in to account when you make a decision. The others being social impact and economical impact. And probably for political impact as well. When you are making decisions you must take into account political impact.

Q: Is it possible that you might be considering staff positions within DFO for economists and social scientists?

A: There are. There are economists now but most of them are in Ottawa. The only time....DFO is a very decentralized department. And most of the action is taking place in the regions. Except for the economists who are mostly in Ottawa. The larger contingent is in Ottawa. But it is by no means comparable to the biology side of it. Sociology I think we would hire. Hired guns. To start with. I'm not sure but I think that their contribution would be to say, "Don't even think of this regulation. It's not going to work."

Q: The regulation makes technical sense but it's socially impossible to implement.

A: Right.

Q: Or they might work in the other direction. Sociologists familiar with both realities, the scientific reality and the social reality of fishermen and fishing communities might even become proactive and introduce suggestions that could then be evaluated scientifically for their technical feasibility.

A: Yes. But it's just that for the practicality of it you've got about a couple of hundred biologists thinking of possible ways of management regulations and maybe three or four sociologists. So they would be swamped and then would have to react. But in the long-term it's totally possible. Maybe there will be a job for you.

APPENDIX K

Interview with Henry Lear
Conducted in Ottawa, Ontario
January, 1991

Note: This interview took place in Henry's office in Ottawa the day after the Gulf War started. I was quite distraught over this ominous turn of events and, as a result, this interview is not as wide-ranging or incisive as I might have wished.

Q: I'd like to begin with a little bit about your background--I know you're from Port de Grave--your family's background and how you came to be a fisheries scientist.

A: I grew up in a fishing community. My family's been fishing in Newfoundland, we've traced it back almost 300 years. We have a long history in the fishery.

I got my grade 11 and went teaching for a year. I saw a notice in the Post Office for a job at the Fisheries Research Board so I applied for it and got it--as a technician. I stayed there a couple of years and then went to university. At that time there was a scholarship on the go--the Fisheries Research Board had a scholarship. They offered me that one the first year. I wouldn't take it the first year. Newfoundland independence and all that stuff. I went through and got a BSc. At the time they had a fairly strong program in marine fisheries, population dynamics, oceanography--there were four courses--post-graduate courses, so I went on and did a Masters on Greenland halibut and I went to work in '67 as a biologist working on Greenland halibut. After a couple of years we had a shift in staff and that was phased out so I went to work on salmon for eight years. In '78 I came back to work on groundfish again--on northern cod.

Q: When you were working in Newfoundland, what was the actual work you did?

A: I was involved in the basic biology at first with the Greenland halibut--the turbot. We surveyed the bays to get the stock composition and age distributions before the fishery--the really intense fishery in Trinity Bay, Notre Dame Bay and Bonavista Bay and White Bay.

With salmon I had mostly to do with the Greenland problem. We were trying to sort out the American and European components which we did with the use of scales, electrophoresis, blood types. We did a fair amount of tagging. I was also responsible for the marine fishery in Newfoundland; migrations and following trends in the stocks.

With northern cod, one of the questions at that time with the offshore fishery was, "To which areas did these stocks contribute?" At that time we weren't sure if there were actually different components of the stock or whether they were all intermingled, pretty well continuous along the continental shelf. So I suppose I was the first one really to tag successfully on the offshore concentrations. We started in '78 on the Belle Isle Bank. And after tagging for two or three years and watching the results it seemed to be pretty obvious that these components were contributing to various inshore fisheries.

They'd be over a wide geographic area and there's a fair degree of overlap but by and large you could say that the Belle Isle Bank was contributing mainly to Labrador and the Great Northern Peninsula. When you get down to the Funk Island Bank it's mainly the northeast coast of Newfoundland, some Labrador and down to the Avalon Peninsula. When you get to the northern Grand Banks, it's mainly a southern component moving down over the top of the Bank and into the Avalon Peninsula.

We also tagged in inshore areas and, again, we pretty well bore out what Templeman found in some of his tagging. If you tag in an inshore area like La Scie the fish come back to the same general area in successive years. So there was a certain degree of homing. Not to the same degree it is in salmon but there seemed to be a trend.

One of the other things that came out....I started tagging in bays, with the juvenile cod. I grew up with the idea, from fishermen, that some of these bays had local stocks. Even though Conception Bay was a shallow bay, we knew that fish had over-wintered there because my grandfather and his people had fished up in March some years. And fish showed up again in May and June so these fish didn't just go out and come in to the bay that quickly. And I tagged. Again, where we could find a few fish, the fish did stay around in that same bay that summer.

One of the most striking examples--one of the biggest surprises I got was the summer I tagged at Cape St. Marys. In September of '86 I think it was. And the fish, instead of moving offshore, actually moved up into the bottom of the bay where they were subjected to a winter fishery and then they moved out to Cape St. Marys in the summertime.

So we had a local inshore stock that was exploited almost year-'round. And it was a fairly substantial exploitation rate. We're talking up to fifty per cent. We're not talking F0.1, F0.2, twenty per cent. So this was mainly exploitation by the local inshore fishermen.

And back in the 'sixties, before I had gone to university or during the process in the summertime, we had worked on projects where we did gillnetting in the Bays. Bonavista Bay, Trinity Bay and Placentia Bay. Back in those days we were getting fish ten years old, ten pounds, in April and May. Right when they were spawning. These weren't offshore stocks. These were local inshore stocks. And these were essentially fished out by gillnets. I suppose the draggers sometimes came in off Cape Bonavista too and put the nets to them but mainly it was the inshore gillnet fishery that knocked them down. In concert with the turbot fishing at the time. These were local stocks and that was part of the equation at that time.

We've always had variations in inshore catch in Newfoundland. That's the story of the fishery. It's why my grandfather and great-grandfather spent almost half their lives up off the Labrador. Because there was no fish in Conception Bay in certain years. And there were no offshore draggers back in those days. These are the things that you have to place in the balance when you are looking at all these things.

The point I want to make is that back in those years, you had a certain cushion. When you had these local stocks. If the fish didn't come up into the traps, you could always get your line trawls and set them out in the deep water and get something. It wasn't a complete washout. But once we knocked down these stocks, then you were totally at the mercy of migrants from offshore which had a high degree of variability.

And this really was what exacerbated the problem when the stocks declined. And probably changing climate conditions, why the cod didn't come in. At the time when the stock was greatest, we had one of the worst years in '81, when the stock was at a peak coming up from '74. So even when the stock was increasing we had these dips.

There were a couple of years when you could explain it by temperature. Temperatures were -1 to -1.7 in the cold intermediate layer off Cape Bonavista. The cod just weren't going to make a break through that. There might have been eddy systems or breaks that they could get in through. Back in '85, '86 we did acoustic surveys off the east coast, Cape Bonavista mainly, and from the sounder records, you could see the cod coming in towards the coast from the bottom up to sometimes 200 metres above the bottom. They're semi-pelagic once they stop spawning so they're either pelagic or semi-pelagic. And you could see them bunching up against the cold intermediate layer. [draws a schematic]

Here was the bottom, and here was the cold intermediate layer. You could actually see large concentrations just bunching up under this cold intermediate layer and you wouldn't see anything up in this once you got colder than -0.5. They'd make a few incursions up into it. And all the time caplin were migrating back and forth even down among these codfish and back up again in the night. The cod just wouldn't follow these caplin up into that cold water.

But that doesn't explain away the decreases in catches and doesn't explain why there were over-estimates made on biomass and underestimates made on mortality.

But the fact remains that it was unfortunate that the stock declined at this time because it clouded this issue. It discounted that type of information. Because the fishing mortality was underestimated and the stock wasn't as great as it should be, it masked what I considered to be a very important piece of information and that was the importance of the environment in the migration of the codfish. The baby got thrown away with the bath water.

Because the stock did increase something like 350,000 tonnes from about '75 to, you know, it was up to a million and a quarter tonnes in '84-'85 when we ran into the problems with the inshore fishery. There were several good years of recruitment. One of the problems was that it just levelled off. Not so much the older fish but the recruitment. We just had a couple of bad year-classes.

It's ironic, looking at the northeast Atlantic. Here in Canada there's been panic because we're fishing at 0.45. They're fishing at 0.8. Even Iceland is up at 0.8. That's probably 55-60 per cent of the stock. 0.45 is probably 35-40 per cent of the stock. So they're fishing about double. They're trying to get back to F-max. We're trying to get back to F0.1.

Q: The assumption is that there's a linear relationship between spawning stock size (and then there's all the variables that affect the mortality of pre-recruits) but there's a clear assumption that there's a near-linear relation between spawning stock size and fecundity. That you get more fish and you get more eggs.

A: Well, that's probably as far as you can take it I would say.

Q: But wasn't it about 1966 that Art May did some work that showed a non-linear relationship between spawning stock size and fecundity?

A: You're looking at two things. If you're looking at spawning biomass and total number of eggs, [draws a curve] you probably have something like that. The more cod you've got the more eggs you're going to have. I think that what Art May was doing was the length of a cod versus the fecundity. And that was a log relationship. It was exponential. The larger the cod, the more eggs.

Q: But some of the things I've read seem to suggest that there's also a density-dependence in this. The more fish there are, the fewer eggs each individual fish produces.

A: Oh yes! That's providing that the food supply is limited. You have a certain amount of energy available. If food supply is the limiting factor then your growth rate declines. The amount of food going into egg production declines so therefore you have fewer eggs and probably smaller eggs. And probably fewer viable eggs.

Because, even in the Pacific for salmon, they've shown, for example, the larger eggs have the greatest chance of success. Of course growth rate comes in there too. You could have twice as many fish but only half the weight.

This is a lot of what happened in the northern cod. It all gets sort of fuzzy because back in the 'seventies you had a fantastic growth rate in the northern cod when the population was down, naturally. As the population increased it seems that the growth rate declined very steeply. So that even though your population was increasing, your biomass wasn't increasing at the same rate that it should have. Which is what threw our projections off. Now, granted, our projections were out because we were using an average recruitment that had occurred before. What else can you use?

Q: But also you were using weight and length at age data from the 'seventies when the stock was really depressed. When growth was not food-limited. I understand that this was the key to the reassessment...

A: That was it, yes. One of the things.

Q: That using new data, current weight and length at age data, to do the calculations. But what I've been told from several sources is that...well the interesting question is why you were using weight and length at age data that was ten years or more old for so long?

A: I can't answer that one because I wasn't in on the assessment. The samples were taken every year so I thought that they were using current...When the projections were made was back in the 'seventies and early 'eighties.

Q: Projections, yes, but the yearly assessments up until '86, '87 were, as it turns out, overly optimistic. And it wasn't until the '88 assessment was it?

A: The one in '89 was the one that dropped the bombshell. But I was under the impression...if you go look at the matrices, they were using the average weights, the current ones. According to last year anyway. But I'm not familiar with it because I was sort of on the outside. I was doing migrations and that kind of thing. Jake would know about that and Claude Bishop. And Dick Wells was in on all of the assessments but of course Dick is dead now.

Q: I've heard some interesting stories about conflicts between Dick and some of the younger scientists about access to data.

A: [long pause] Well, all scientists have conflicts over data.

Q: I've got both sides of the story. I talked to, on Dick Wells' side, to Sandy Sandeman and on the other side Ram Myers, who was one of the people who complained bitterly that Dick Wells wouldn't give him access to the data that he needed to do the reassessment and it wasn't until Alverson came in and blew the doors off Dick Wells' safe that he got...

A: I'm not sure that it was that way. I think it was a two-way street. It's easy to place all the blame on Dick Wells now that he's gone. He's a convenient scapegoat. But, put it another way, I suppose, if you spend 20 years collecting data and, all of a sudden, somebody stands on your doorstep and demands it all, what do you do? Do you give it away? There are such things as negotiation and compromise. Balancing it out. And the proper balance just wasn't struck, I guess.

Q: That's exactly what Sandy pointed out. That because the promotional and reward structure at DFO is so heavily weighted in favour of publishing, that you see data as your investment and your life's work.

A: That's all you have. It's not a level playing field. This is the problem. I suffered from the same thing. Because you're so tied up in doing your job that you just don't have time to publish. You just can't concentrate and focus on getting the publications. You're the one, if there's a brush fire, you get called out. You're the one who's got the experience and you've always been there and it's so easy, right?

And you hire someone, it isn't just Ram, it could be anybody. They're brilliant and they come in and you've got this wealth of data you haven't published and they say, "Well, this is not right. This demands publication." So you hand it over and they get half a dozen papers and next thing you know they're two levels ahead of you. And you say, "What am I doing? I'm only a slave!"

And this is where the problem lies. People who are working very, very hard, working overtime without getting any pay or anything else. Were not getting any type of reward. Not even promotion. Whereas somebody'd come in who was quite free to come in and take the data, you were giving them a free ride. It was sort of creaming off in a way from someone else's life. This is the crunch. That is the problem.

Q: So the structure does not encourage cooperation.

A: You can call it the structure. But I think the reward system for research scientists doesn't allow that. You get penalized.

I spent a lot of time talking to fishermen for example. It was interesting. I came from fishermen and I could easily talk to them and I enjoyed that. Carrying them information and discussing things with them but in the end it didn't do anything for me. People were just passing me by.

So that's just one example. Now we've come to a crunch where we've got to have people talking to fishermen, interacting and liaising and all that stuff. When I was doing it, it was nothing!

Q: J.J. Maguire is quite concerned about this. Although there's a lot of talk about increasing the communication with the clients, there are still no points for it, no institutional rewards. When I talk to other people about this problem, your name often comes up as an example of someone who has suffered because of this.

A: Well, I have no one to blame, only myself. I knew what the rules were and because I cared, I suppose, I suffered. I'm not blaming the system. I'm not blaming anything. I knew the rules and because I felt a certain way, a certain dedication, whatever, that's what I did. Dick Wells did the same thing.

Q: What happened to you?

A: Well, I just never got the publications to get upgraded, pure and simple. No one caused me to suffer. The rules were there on the page in the book. You had to have a certain number of publications, which I didn't have and there was no way I was going to get them in doing the type of job I was doing. So it was a vicious cycle and I was party to the cycle because I enjoyed what I was doing. This was the difficult part.

I thought at the time, and I still believe, that I was doing a good job. But because I did a good job, and enjoyed doing it, and kept doing it, which only ground me a little farther down, it was counterproductive to my own career interests.

Q: There's a lot of talk now about trying to incorporate fishermen's knowledge in to the scientific assessment process. The inshore logbook program is one example of the attempt to do this. But when I speak to scientists privately, there is a wide range of opinion about whether this is A. possible and B. a good thing. What are your opinions?

A: Back in '86 I think it was, we looked at the situation in a little technical report we did for the Director General. And one of the first recommendations, we said it was of paramount importance to include catch and effort data from inshore fisheries into the assessment process. And really that's what counts. You have to have some measure of your catch rates in the inshore fishery to know what you're dealing with. Just looking at pure catch is not enough.

And you don't have to give every fisherman a logbook. You take half a dozen in La Scie and half a dozen in St. Anthony and a sample from other major fishery centres. That'll pretty well give you a fix. That'll tell you what's going on.

The one about the local knowledge, the anecdotal information and the historical, I don't know what you call it. The folk memory if you like. I think this is valuable, extremely valuable. But the problem, and I've thought about it a lot, is how in the name of God do you quantify it? Because of our training, our Western thought I suppose if you like, everything has to be analytical, structured, logical, clear. We don't have the scope for intuition that the Eastern philosophies would allow.

This is the problem I've seen with this type of information. And there's a gold mine there! Or there was at one time. A lot of it has been lost. I remember making an observation, it was a good 20 years ago when people were leaving, going to Toronto and then coming back. For three or four hundred years, we learned from one another. It was an oral tradition that was passed down. And all fishing methods were orally transmitted. It was a continuous chain.

But I think the chain was broken in the 'fifties and 'sixties. People went away. And then some of them came back but the information flow was sort of truncated. And they went and set gillnets in a place where you wouldn't set gillnets. Or they'd set gear in a place where only one fisherman could fish, or only four or half a dozen fishermen could fish because there were certain unwritten rules that said you set your gear parallel on the slope. And another guy coming behind you sets in a certain way.

It was the sociological side of fishing I guess. It allowed for the maximization of a piece of ground. Rather than just one person going in just dog in the manger and keeping his gillnets on that piece of ground and probably not hauling them for three or four days.

Because you can take the best piece of ground in Conception Bay, take five gillnets, and you can ruin it for everybody and you won't catch fish enough for brewis for yourself. For the simple reason that they're not set right. You look at you catch/effort and you say I've got five gillnets out there and I only caught ten fish. His grandfather would have taken those same five gillnets and set them and probably have got twice as many fish. Because he knew the way that the fish moved around that piece of ground in response to the way the wind was the day before.

I grew up setting line trawls around Bell Island and Kelly's Island and you didn't always set the trawls the same way every day because you knew that the fish were deeper or shallower depending on the way the wind was the day before. So intrinsically we were using temperature.

We couldn't detect the temperature but we knew that the water moved back and forth and around the ground. We knew we had to go deeper if there was a northeast wind. Because you had an influx of water coming in that forced the warm water down and your cod went down another five, six, ten fathoms probably. And when the wind went southwest you'd go shallow again because your warm water on the surface got swept back out again and your bottom water up-welled and the fish came up the slope. And how do you work that in to a catch-per-unit-effort?

We talk about the technology change with the offshore draggers, that they became so efficient that we couldn't account for it any more. The catch rates were going up and up and up and yet the stock was staying level. But then you come to the inshore and you have to look at the sociology as a technology.

Q: So you're suggesting that the opposite has taken place in the inshore? That knowledge has been eroded?

A: It could be in some cases. No, I think it's balanced out. I have to qualify that one. I think it's balanced out now. Where most people have sounders so they can actually go along the slope and see where the fish are. Or they can look at a trap before they haul it. And the fish are just not there any more.

But I have heard a lot of fishermen complaining. When I talk to fishermen. I've heard them complaining that you get some fishermen going out who don't know what they're doing and putting gear on the ground and ruining it for anybody else. Because you just can't just set your line trawls, your gillnets, across the ground.

There's one other thing I think we've missed. When I was growing up in the 'fifties, everybody had a trap boat and was their own boss. But gradually, in Port de Grave, they got away from that and got into longliners and instead of waiting for the fish to come in, they went out after the fish. So that's a whole new development there.

But it's really not new. Except that they go to the Virgin Rocks now. My father and grandfather and great-great-grandfather went up to the Labrador. Or they went to Cape St. Marys. It's not really different. They're returning to a cycle that was there for many decades.

Q: Do you have any idea why the inshore fishery was so good this year, on the east side of the Avalon anyway?

A: It was pretty well south of Cape Bonavista. It seems to me that what happened, I don't really know a lot about it, the cod seemed to be distributed farther south in the winter and the spring concentration. And these fish moved en masse into the southern area. And if something changes, that concentration might spread farther north and you get a more even distribution along the coast in another year. Or you could get in some years, there was a bigger concentration in '82, for example, up north. And that was the year we had the record catch when we had good distribution along the coast.

It depends to a certain degree where the fish end up after spawning. If we have a really cold year and the cod are forced south like '85 and '86 when they were on the Nose of the Grand BAnk and the Germans caught them. Then you're into a situation where you're probably going to get most of your inshore fishery in the southern area.

Q: About this issue of dragging on the spawning grounds. I've talked to both Jake, who was the official department spokesman, and Cabot so I've has both sides of the argument. What's your opinion?

A: Well, it depends on what you mean by "spawning grounds." The cod aren't spawning on the bottom for one thing. they're up semi-pelagic, above the bottom. So I can't see that it's going to affect eggs, for example. Because the eggs float, they don't go back on the bottom. If you're talking herring, where the eggs attach themselves to the kelp and the rock and the substrate or whatever, then you're probably talking damage. But with the cod up in the water column, swimming, I can't see it. Again, anything is possible, but I can't really see it.

Q: Jake's point was interesting. That if there is a ban or a moratorium on dragging during the spawning season, what's probably going to happen is to effectively push the fishing back onto the pre-spawning concentrations where you'll be catching them before any of them have had a chance to spawn. And probably the net results would be deleterious to the stock.

A: I'm not sure that it would have that much affect. Because whether you catch them in January or the next December, the quota allows for a certain volume of fish to be taken out of that stock. Whether you take them before or after spawning might not make that much difference. It's a circle you're looking at. If you're looking at just one year then maybe you could say yes, but you're looking at a cycle.

Q: In the same conversation Jake said something else that I thought was quite striking. The average cod produces around 2 million eggs and the difference between a good year-class and a bad year-class is the difference between six or eight of those 2 million eggs or only two of those 2 million eggs surviving to recruit to the stock. You're talking about astonishingly small statistical variations, between two in two million and eight in two million. That's a seemingly insignificant difference and yet it makes all the difference in the world. When you start to think about these numbers, it's astonishing that there's any stability in the stock at all.

A: It's remarkable, really, that it does hold. If you look at the number of survivors who make it to four years old it's probably point one percent or something. I don't know what it is. [between .0001 per cent and .0004 per cent] But it's very very small.

Q: And there's so many potentially lethal situations that per-recruits can encounter. Sudden temperature shifts, predation...

A: Winds and currents. They could be just swept out over the top of the Bank you know. It's endless. Food for example. If they don;t get the right food when the yolk sac is absorbed they're gone.

Q: The idea of managing the stock on a yearly basis, counting the fish each year, much less making projections seems almost impossible.

A: It's very, very difficult.

Q: Even Gulland said that the very best he thought assessments could do was plus or minus 25 per cent and he was doing his work in the North Sea which may well be a more stable ecosystem than the northwest Atlantic. So this means that if the stock is really a million tones, the assessment could vary from 750,000 tonnes to 1,250,000 tonnes. And if you're trying to manage through quotas at the F0.1 level, then the quota could vary from 150,000 tonnes to 250,000 tonnes just through unavoidable variance in the assessment. Ram told me that he ran some of the zonal survey data through normal measures of variation and, at the 95 per cent confidence level, the lower end of the answer included zero.

A: It's very uncertain, yes. So I think you're starting to appreciate the enormity of the task we were faced with back in '77, '78.

Q: But it seems that back then a lot of people thought it could be done. Once you kicked the foreigners out and competent, dedicated Canadian scientists got in there and took control,...

A: I don't think the scientists ever thought that. I didn't. I think we realized that the problems were still the same. We thought we'd get a better handle on catch/effort through surveillance and enforcement but the natural variance was still there.

Q: It seems that in this atmosphere of large areas of uncertainty and variability, that given that an answer had to be given to the minister, that the answer invariably came out at the high end, the most optimistic possible interpretation of the data. And until the '89 assessment the numbers never went down. They always went up. Which means that they thought that the real answer was at the high end of the uncertainty. And this is what I'm trying to figure out.

A: The answer probably lies in the catch rates. The offshore catch rates were going up and that was one of the things that they were using to tune it.

Q: But the catch rates were gong up because of more efficient technology and increased skipper competence.

A: And the research vessel survey results were just yo-yoing back and forth and were very difficult to tune your assessment from.

Q: And most scientists prefer order and stability in their data so they perhaps gave undue weight to the commercial CPUE data because its was orderly and showed stability and growth.

A: It was good, hard scientific data and we're conditioned to think that if something is good, hard and concrete, it's true.

Q: And of course the increased commercial catch rates corresponded to expectations and projections. They expected that once Canada got control of the stock in '77, then they could rebuild it to its historical levels. And there were some fairly firm and public commitments made to this goal.

A: But when the scientists made these projections, they also attached several pages of caveats. Which got torn off and put into the waste basket and all that got looked at was the one page of projections. A lot of people tend to forget that one. Everything was there, that it was based on average recruitment and certain growth rates. If the recruitment doesn't come through, this was wrong. And the recruitment didn't come through.

Q: But, the reader was left to believe that all this would hold true over time. You might be off by a year or two. The caveats were there, yes, but the tone was that, "we are being good, cautious scientists, but what we really think's going to happen is this. We'll be off a little but not by very much." And a s it turns out, it appears now that they were off by a whole lot. And even Al Pinhorn did some projections which he probably finds real embarrassing now. And I was told that there were projections run up especially for the Kirby Report...

A: Well, projections were done every year. Five-year projections, whatever. But when we took over jurisdiction in '77-'78, we never had a survey of northern cod, not really, not on in divisions 2J and 3K. Not until the fall of '77. That's the first one we had. So that was the data base we were working with. It was impossible. But you had to do the best you could with what you had. And I suppose, in retrospect, we didn't do that bad. You'll get a lot of opinions that we did a terrible job. But when you consider what we were working with, we were extremely successful. It depends on which yardstick you use. If the Europeans had a mortality rate of 0.45 on cod in the North Sea they'd be dancing. Or the Norwegians with their mortalities from 0.8 up to 1.

Q: I don't know about the Norwegians but the North Sea seems to be a very different ecosystem than the Grand Banks. It seems to have a higher rate of energy cycling because it is relatively shallow, relatively warm. So perhaps if the biological turnover is higher, if the biological energy rate is higher, you can hit the stocks harder than here.

A: You can only take so much fish...

Q: But if the growth rates are higher...

A: But their biomass is declining and that's the telling thing. If you had a stable biomass in spite of those fishing mortalities, then you could say, well, the bottom line is they can do it. If you look at any of ICES reports, their stock is going down and their spawning biomass is going down.

Q: I was talking to a couple of visiting Norwegians last year and they said that things are so bad over there that the government is simply taking a third of the fleet out of the water and closing a third of the processing sector. Or maybe it was two-thirds. It was an astonishing number. Maybe it was two-thirds. It was just brutal. Hang it up. Get out of the fishery. Apparently they're in big trouble.

A: They are in big trouble. You're looking at a stock that produced a million tons in some years. 800,000 tonnes. Now they're down to something like a hundred thousand tonnes.

Q: Was that primarily because of fishing on the stock itself or also on their food? Are they a caplin-eating stock?

A: Yes. And then they decimated the caplin. But they had bad recruitment too.

Q: And that can happen for any number of reasons that no one has a clue about. I was reading a paper by an economist down at the university of Maine named Jim Wilson who's gotten in to modelling fisheries dynamics using economic information in the models. But he's also finding that if you run multi-species, multi-variate computer simulations with a cap on total biomass or total energy in the system, you get strongly non-liner responses. And unpredictable, chaotic responses to very small changes in initial conditions when you start to run the model. I've been reading about Chaos Theory lately. Have you read anything about this?

A: Not much. A bit. Sissenwine dealt with some of this with the uncertainties in scientific advice.

Q: It seems to me that up 'till now, scientists have assumed that the fundamental dynamic in the marine bio-system is essentially linear. A linear system tending towards equilibrium states. And that it will respond in a fairly linear way to inputs and extractions of energy. In a predictable way. And this assumption seems to underlie the current concept of management.

A: The whole concept of it was stability. We could take out some of the peaks and valleys and stabilize...everything could be stabilized and that may not be so. It's subject to natural fluctuations which, at times, defy our explanation. Certainly our projections.

Q: So if, in fact, the system is strongly nonlinear, or components of it are, not only do projections go out the window but it may not be that our fishing pressure is the most important variable.

A: There are a number of people who have made that observation. Fishing, sure, is a factor in everything. But it may not have the effect that we like to believe that it has.

If you look at the herring population in the Gulf of St. Lawrence, the herring population was almost wiped out by a disease, ichthosporidean and the two biggest year-classes that they ever produced came from that spawning stock, the

'59-'60 year-classes. One of the better year-classes that ever came out of northern cod was the '73 year-class when the stock was very, very low.

I suppose you have to have a good spawning stock there. It's a basic building block. You have to have a minimum spawning stock. If you keep a huge spawning stock, there's no guarantee that you're going to get good year-classes. But sooner or later you're going to get some. Unless things change drastically from what they were in the past. Unless it's truly and utterly chaotic. But if there is such a thing as orderly chaos, randomized chaos, if you like, sooner or later you're going to pull a good year-class out of that spawning stock.

Q: Chaos theory is sort of mis-named because one of its main points is that there is a strange kind of order in apparently random events. A snowflake is a perfect example. It's a product of random forces and events and no two are exactly alike, their exact shape is unpredictable and yet they are orderly. They are all basically hexagonal [check]. You can be successful depending on your level of prediction. You can never predict the detailed shape of the next snowflake but you know it's going to be hexagonal. People like Wilson are saying that the only level on which there seems to be equilibrium in the system is on a total energy level. So he's running his models with five different species. He uses the known biological parameters for cod, haddock, redfish, flounder. And then he introduces a pretend species, something he calls "bloom." This is short-lived but tremendously fecund and opportunistic. He said it corresponds in the system to species like sand lance. So when he runs his models, there are often weird, chaotic responses for each individual species but the total biomass, the total energy level, remains relatively constant. And what I find interesting is, what are the implications for management? The way it's been done up 'till now hasn't been terribly successful. It hasn't made anybody happy. And I suspect it's because the management is based on a flawed assumption. Now if in fact it's more realistic to think of the ocean in terms of nonlinear dynamics, how would that change the scientists' relationship with it? How would you construct a management strategy? Have you given this any thought?

A: Well, I've thought about it but I don't have an answer. If you look at an assessment, for example, you're really only hindcasting, in a sense. Your only estimate is your recruitment that you plug in. Which you get from your research vessel surveys. You're making the assumption that your growth rate is going to remain the same and that your recruitment will be at a certain level. And on that basis you do the assessment.

But in the long-term, you're still basically looking at the concept of stability. Equilibrium is always there in the back of your mind as the basis. If you're looking at the uncertain, the chaotic situation, you could still do your assessments but how they would relate to the type of management that takes that into account, I don't know. You certainly couldn't plan your fishery in the way you plan it now.

You'd have to have a multi-year plan or whatever. You're pretty well left to take what you can get out of it which leaves the door wide open, dangerously. Because you then revert back to fishing as hard as you can. You're right back to the old free-for-all system again. A race for the fish. So I guess the whole basis for the management was to try to put a cap on the amount of effort that was there so that the people who were there could make a living.

Q: How about letting the effort vary among available species? Let the fishermen make up their own mind what species they were going after. So that as one species increased in abundance or market value, that would get fished until something else showed up.

A: Then you're back to pulse fishing again. The problem there is...essentially this is what the Europeans practised in the 'sixties. For example, they moved to cod, haddock, pollack, silver hake, herring, you name it. By the time they were finished there was very little of anything left. This is the danger you run into on that one.

Q: But there was no cap on the effort in that situation.

A: Well, if you can cap your effort you might be able to get it to work. But you would take the stability out of the industry.

Q: If you capped effort at a relatively low level, and then let the effort distribute itself in whatever way it saw as rational,...?

A: Again, I think you could get caught. Say, for example, that the price of cod keeps going up and up and up. The fishermen keep fishing and fishing and fishing until your stock is pretty well decimated. There's your cod stock gone. If the price of cod remained the same, then that might work. They'd switch to something else because the catch rate on cod was going down. But if, for example like in '87, the price of your cod went up and up and up, they'd fish them to extinction. Probably not biological extinction but certainly commercial extinction. So you always have to have these caps on the individual species too.

Q: But if we go back to the idea that the system is stable only at the total energy level, then the only level at which you can manage is in the amount of energy that you take out, which is the same thing as effort. Effort is an energy extraction equation.

A: What you're saying would work very very well with the trawler companies. So they wipe out the stock one place, they move and fish somewhere else. But if you're an inshore fisherman on the coast of Labrador, what do you do when you've fished you're pulse of cod stock? There is no other species.

Q: This brings up another touchy issue which I think is something that's going to have to be talked about pretty soon. And that's, at what cost is the inshore fishery maintained? As people. I think it's clear that the Mulroney government, at least, would just as soon do away with the inshore fishery. I think that's clearly the intent behind the individual transferrable quotas, ITCs, that are moving eastward. That went in to the Gulf this year. And what's going to happen when the public property, the fish, gets converted to private property is....You look at most private property situations, you look at agriculture. It becomes largely concentrated in the hands of capital-intensive organizations. The dragger companies will but up, within a fairly short period of time, all of the inshore fishermen's ITCs, and they'll be gone out of there.

A: I don't know if that's happened anywhere. Tell me somewhere it's happened.

Q: How long have there been ITCs? They first came in on what, herring?

A: Herring, yes.

Q: How long have they been in effect.

A: I can't answer that question.

Q: But they haven't been there very long.

A: No. They haven't been there very long. Several years. But to me the idea of ITQs brings in a certain measure of stability for the fisherman too. They can take the quota whenever they like. At their leisure if you like. They're not out there in a race for the fish. Jeopardising their own safety in a lot of cases.

So you know that on January the first, the fisherman down in wherever he is, Bonavista, he's got a thousand tonnes of cod, he can tailor his fishing season to catch it when it's most convenient and economical. I think that was the basic premise behind a lot of this. Because it seems to me that most of the fishermen liked them. Granted there are some people who wouldn't like them. If you were a top dog fisherman. But if you were the average fisherman....That way you're not penalized if your engine breaks down and your out of it for a month. You've lost your fishing season. But if you have your ITQ you get your engine fixed and you go fishing. So you have a safety net if you like. Granted, there's always the danger that you're talking about but there's always a danger in everything.

Q: But couple that with what the federal government has been trying to do, changing the unemployment rules and regulations. And all this talk about "rationalizing" and "professionalising" the fishery. Particularly the inshore fishery. And it seems to me that these are code words for shutting it down. All this talk about too many fishermen and too few fish.

A: That's coming from the fishermen themselves in a lot of cases. You talk to the fishermen and they're the first ones to say that there are just too many fishermen. Now whether that's true or not is certainly a matter of perception.

Q: How did that come to be? There was a period there where the number of fishermen fell quite dramatically.

A: Yes it did. Back in the 'seventies. The fish stocks were down I guess. And as the fish stocks started to rebuild and things looked promising, they came back in again. And in Newfoundland you have the problem of what else are you going to do? There are no alternative forms of employment. One time you could go to Toronto or Alberta but you can't do that any more. It's just too expensive in Toronto and there are no jobs in Alberta.

[short digression--the talk turns to global warming]

Q: You throw the possibilities of what might happen if global warming is real and it's enough to drive you crazy.

A: Well, there's no one knows what's going to happen. If the temperatures fluctuate and the currents change, you could get anything from a fantastic year-class to nothing.

Q: And on the Grand Banks it would likely increase the chaos rather than decrease it because you've got huge amounts of cold water coming down off the melting ice caps, I can't see it doing anything but increasing the volatility of the weather patterns.

A: Well, it's already volatile on the Grand Bank anyway, on the southern edge. For example, the haddock. It just zoomed up in the 'fifties. You had a couple of big year-classes and then it just dropped off again. There was no recruitment. They're at the northern end of the range. We got cooling.

But it's interesting that the niche was taken over by the yellow tail flounder. I remember in the 'sixties, going out and we'd get a yellow tail flounder, we'd bring it in. It was a rare specimen. And when the haddock went they took off because they were eating basically the same bottom fauna that the haddock were eating. But they were also at the northern end of their range. So you had the two of these species at the northern end of their range interacting with each other.

Now the yellow tail recruitment is declining, it will be interesting to see what happens to the haddock. There are indications that there are a couple of fairly good year-classes ahead. It's on the periphery that you see these types of changes that give you indications of what's probably happening in the whole system. And if it's happening there, you go to northern Labrador where the cod are on the northern end of their range and you get a few cold years, that has to affect it.

Q: That's another point that I wonder about. The 2J3KL stock is assessed as a unit. But if, as you said from your tagging studies, that they tend to be regional....

A: At spawning time they seem to congregate. But the problem is that there's such an intermixture, in the summertime, that sorting it all out is a nightmare. Because it's not always the same from year to year.

Q: But wouldn't the growth rates be different in different parts of the range. And is this regional variability in length and weight at age incorporated in the model? I don't think it is.

A: It's fed in by division. How it's treated afterwards....I'm not sure. I've been away from it for a while.

Q: What is it that you're doing now.

A: I'm a research assistant for Scott [Parsons] for a book he's writing.

APPENDIX L

Interview with Brian Morrissey,
Assistant Deputy Minister, Science
Conducted in Ottawa, Ontario
November 2, 1990

Q: What I find most striking about the current fisheries crisis is....It may or may not be a biological crisis. No one really knows at this point. The crisis seems to be about what is and is not an acceptable level of uncertainty. What counts as valid knowledge and what doesn't. Within the scientific frame of reference, the traditional academic evaluative traditions, probability and uncertainty are simply interesting problems. From a management perspective, I suspect that when DFO stock assessment science was set up, the expectation was for a much greater degree of certainty from science's input to management than has turned out to be possible. Similarly from the corporate sector's perspective. So the first question would be, how the revealed uncertainties of stock assessment, even on the current state of the stock, much less predictions, affect your ability to manage the stocks?

A: I heard three questions in there, Chris. Is the crisis real in biological terms? Secondly, are expectations of certainty unfulfillable? And thirdly, how does uncertainty affect the day-to-day management business?

Speaking of northern cod, the only predictor of the future that I know of that's reasonably useful is the past. Empirically, within those circumstances at least, you know that it did happen and consequently, it could happen again. If you look at the data given in the Harris report, Harris shows us that northern cod has supported a catch of somewhere between 200 million [sic] tonnes and 300 million [sic] tonnes. There have been fluctuations outside of those bands but they're rare.

If you accept that as a reasonable expectation of what the future could give us then, biologically between 200 thousand tonnes and 300 thousand tonnes, to me, seems a reasonable expectation. Assuming that the world doesn't change from what it was [inaudible word or two]. That in my mind is biological reality.

If we ask ourselves, do we have a crisis of expectation, as distinct from a crisis in a biological sense, then we may have had unrealistic expectations. What seems to have happened in all of the North Atlantic from about the second world war on, is that catching capacity of fish exceeded nature's ability to provide it. Bigger vessels, echo sounders, better gear, more experience at catching fish. And the catches went up very significantly on both sides of the Atlantic after the war. Here they went from the historic 200 to 200 thousand tonnes to about 800 thousand tonnes.

Again, looking at history, what had happened, speculating on the whys, obviously 800 thousand tonnes wasn't sustainable. The stock crashed at that time. We know that two to three hundred thousand tonnes is sustainable. We know that 800 thousand tonnes isn't sustainable. What are the reasons? We can only speculate.

One possibility is that something has changed in the environment in the North Atlantic. Productive capacity seems to have gone down about 30 percent on both sides of the Atlantic. So you've got environmental change. It could be temperature, salinity, I really don't know. Is it simply a reflection that, with greater catching capacity after the war, we simply went in to a stock that hadn't been subjected to that fishing pressure, took out a lot of the capital, if you wish, instead of just harvesting the interest? And once that capital was gone it was gone. We had a depleted stock and have had to work since '77 to build it up.

So if I could sum all that up, what history tells me is 200 to 300 thousand tonnes is realistic. Eight hundred thousand tonnes we've seen is not sustainable. And for me, looking toward the future, what I would have said is two to three hundred thousand tonnes is what I would hope to get out of that fishery. If I got less I would be a little disappointed and if I got more I would be pleased. I think we have had expectations that exceeded nature's proven historic track record to provide.

The second question is expectations of certainty for management. I would say, "Yes." There are expectations that science produce scientific predictions for the future of stock assessments. I see this not just in this department but in other departments where you deal with numbers. If, for example, we were to say that the total allowable catch for, let's say, 250 thousand tonnes is possible in a given year, 250 thousand tonnes is a hard number. It's quite different from 251,000 tonnes or 249. Because you say a number it has a precision that covers the uncertainty on which it's based.

I really haven't found a good way in this department or in another department where I was involved in residues in food to say seven parts per million. Well, there you're drawing a firm line with a very unsteady hand. It could easily be point six or point eight. Point seven implies that it's that number and no other. It couldn't vary.

My other comment on uncertainty, particularly in this department, is that people's lives are affected by the number that's given for the TAC. And because people's payments on their gear and their boats are fixed, and are dealt with with great certainty by the bank, they are under pressure to have a catch and a cash-flow that has equal certainty. In consequence, for them looking into the future, they are frustrated by not having consistent predictions of catch. In other words, 250,000 tonnes every year into the future. And because they are frustrated they become angry and when they become angry they direct their anger at whoever seems to be frustrating them.

In the fisheries, we have not privatized the fishery in the sense that we have privatized the Western land in this country. Everybody got a hundred acres when the west was opened up. We've only had the offshore fishery for 13 years. So really it is the new west. The new frontier. And it has largely been held as a common resource property. Rather like some countries have held all the land as the property of the State and allowed you to farm a little piece. In that context we have positioned ourselves between nature and the fishermen.

I grew up in a fishing family. We had a trawler. We had a lobster boat. And there was a department of fisheries but this was thirty years ago. Like God, it was something you heard about but never saw. It really had very little impact on our lives. In some years we got a good catch and in some years we got a very bad catch. It was an act of God. And we didn't go about blaming God. It simply happened.

What's happened in recent years is that fisheries has stepped in between the act of God and the receipt of that act by man. And have become the focus of anger for the bad years. We went out and fished without the department of fisheries and in the bad years there was no one to blame. It just happened. So I would say, "Yes." There is an expectation for certainty. And we have become the focus for frustration and unhappiness when we can't provide certainty.

The big question was how does uncertainty affect the day-to-day management? I'll take the lead-off from the last question. Because there's a perfectly understandable desire on the part of the fishing community and the fish processing community to have some certainty in the stock they can get, those they can take, that translates into pressure on this department to produce A) certain numbers and B) consistent numbers over a period of time to avoid fluctuations and C) to provide increasing numbers because decreasing numbers are punishing. Punishment to them translates to punishment for us. If we don't produce those ever-increasing numbers. So yes, I would say that uncertainty has made it more difficult. And the fact that the State owns the property has made it more difficult.

Q: I'm going to roughly quote one of my previous sources who said that, in hindsight, it seems that DFO has been setting the TACs in response to variability in the data rather than, necessarily, in relation to any change in reality to the northern cod stock. That in fact, the widely-shared guestimate of the levels of uncertainty in the current state of stock assessment is somewhere on the order of 25 to 30 per cent. This seems to be generally accepted among the scientific community as the best that they can do. And, given the uncertainty in other variables, predictions--useful resource projections of more than a year are coming to be understood as perhaps impossible. So that's again two questions. One, the opinion expressed to me that TACs have been set in response to...reflected variability in the assessment data rather than reflections of biological reality. And the question about the possibility of useful predictions.

A: I'll make two comments on that Chris. One is that both those statements are absolute statements. One says that TACs have been set based on variability in the data rather than variability in the stock. That's an absolute statement and, as such, I wouldn't accept it. The one thing we know in the fisheries is that it's based on probability and variation year to year. Certainty and absolutes tend to be untenable.

What I would have said is that the comment about the confidence interval for a given prediction around a given stock being plus or minus 25 per cent, that basic figure is in Gulland's text book. One of the basic text books on the fishery...speaking of European fisheries. So if I accept Gulland's text book as being correct, that's the standard uncertainty in the European fishery. That they haven't been able to improve on.

Let's take it for a moment that it's a standard in this business and about as good as you can get. To go from that conclusion to say that TACs are set in response to variations in the data rather than variations in the stock, presently I think is quite untrue. What that number tells you is, let's assume that we said the TAC for next year is 250,000 tonnes. And that was based on confidence intervals on each side of it of 25 per cent. We have a very high degree of confidence that the number, the correct number, is

250,000 tonnes plus or minus 25 per cent. Plus or minus 50,000.

So the truth in a given year could lie between 200 and 300 thousand tonnes. That's a standard behaviour in any statistical sampling whether it's an opinion poll or whether it's a market survey or whether it's a survey of the number of fish. What's quite important is that the confidence that the correct number is 250,000 tonnes is very high. The confidence that it is 200,000 or 300,000, while those are possible, it tends to be one year out of 19 or one year out of 20 that it will be one of those extreme cases. So you're getting closer and closer to the truth. Closer and closer to a high degree of confidence as you move closer and closer to the 250,000 tonnes.

The other comment on it is, because you're not taking a stock assessment in one given year as one given picture of the stock, you are not dependent on one sampling. You, in fact, have a series of years. So that if your series of years gave you a trend around 250,000 you have a higher degree of confidence that the truth is close to that range. That gives you a second check.

A third level of check is the suggestion in the Harris Report that because any sampling plan, for instance an opinion poll to see if George Bush will be elected president where they use a high level of sampling, say 1,200 samples, you always see as a codicil to the prediction that they survey said that Mr. Bush would get 45 per cent of the vote plus or minus 5 per cent and this is right 19 times out of 20. You're seeing the same sort of conclusion in doing a fish stock. That's because of the 19 times out of 20. One time you can get a completely wrong number. Not even within the plus or minus five. And the best way that I know of to protect yourself against that is to have two or three separate indicators. So if one of them gave you quite a wrong number you have two or three others to put you back on the right track.

And that's one of the things that Harris recommended in cod. He said if, for example, you are using the research vessel survey as one indicator, that's fine. But it could be wrong, quite wrong, in certain years. There's nothing wrong with the people involved. It's simply a matter of not counting every fish that you're sampling. He said try to have a separate indicator. The commercial vessel sampling is a separate indicator. So that if one is off the other might correct you. And he said two indicators are good but that three indicators are better than two so try to have hydro-acoustics as a third, completely separate indicator.

And in some stocks we do, for example, a winter and summer survey. So they give you two samples. In effect two checks. To say that because there is variation in the data that you cannot use the data is to throw out all statistical methods and all sampling plans. I think that would be unwise.

Q: Just to go back to the indicators that have been used. It appears that up until the critical reevaluation that the final assessment was tuned with heavier weightings given to the commercial catch data than to the RV data. And it....From an outside perspective one can construct a hypothesis that the reason for this is that the commercial catch data showed a reassuring stability and, in fact, optimistic figures whereas the RV data, gathered randomly, showed disturbing variability. And, therefore, when one is dealing with assumptions of a linear dynamical system, the unexplained or unexplainable variation is undesirable. One tends to look for stability and consistency in data if you're assuming that the system's natural tendency is to seek equilibrium states rather than reacting nonlinearly and chaotically. Now, of course, since the reevaluation there are different weightings and different tunings and different basic assumptions in the model. But, in your opinion, is there any basis to this hypothesis of mine?

A: Let me try to answer, Chris, and if I haven't quite understood the question stop me and put me back on track.

In trying to assess any fishery in terms of what stock is out there, in terms of your capital invested, what harvest can reasonably be taken out in terms of you interest payments, you're looking for a way of sampling the stock that is out there in a way that is as unbiased and consistent as possible.

The research vessel is the least biased way that I know of. The basis for a statistical experiment is that you hold all of the variables constant except one. In this case the one that fluctuates is the stock going up and down. And to hold all the other variables constant it means consistency in the vessel that you use, the gear you use, the manner in which you select the trawl sites, the stratification of the areas that you're going to sample in and depths that you're going to work in. So that, insofar as possible, the only thing that changes is the amount of stock. Then if you get a different number for the amount of stock, you can reasonably conclude that the stock caused this change and not something else. That's the advantage of the research vessel.

The disadvantage is that it's very costly. You can only do a relatively small amount of sampling. So it has a pro and it has a con.

The commercial fleet, on the other hand, has very large amounts of sampling. They fish a lot and every time they fish they are taking a sample of the fish population. The other big advantage of the commercial data is that it's low cost. Somebody else is paying the fixed costs of taking the sample. The con side is that the commercial fleet is in the business of changing the way in which it does business constantly to improve its profit position. In other words, to get maximum outputs for minimum inputs. That leaves you with the difficulty in interpreting their data of, is the stock assessment they give you, the variation they give you caused by the number of fish in the sea or does it reflect a variation in the ability of the vessel to catch them? That's the conundrum.

What usually happens is that you try insofar as is possible with the commercial vessel is to make as much use of it as you can because it's a huge sample and it's cheap. But if you can find, to use your term, a linear relationship in the trend of their improved ability to fish, then you can calculate in a correction for their improved ability to fish.

If, on the other hand, their ability to catch fish is changed by one-off type changes where a new piece of gear has suddenly come in that we haven't got experience with in the past, or a new rule has been imposed that changes fishing patterns, you don't have a trend to base it on. It means you may end up debasing your numbers.

That doesn't say that one research vessel is better than the commercial vessels. It is if you had unlimited money to do lots of them. But you don't so you're in to a trade-off. The big advantage of using the two indicators is that if you get a bad number, it could be one year in 20 you get a bad number or one year in 10 depending on the kind of sampling you're doing. If you have a trend that's saying your stock is growing and suddenly you get a number that's way off the scale the other indicator, whether it's commercial, research vessel or hydro-acoustics, can put you back on track.

What seems to have happened with the research vessel in, I believe it was the '86 figure, is that it gave a figure that was out of the previous trend. Quite a ways out. And the judgement call was, was the stock a lot better than we had thought or was this an artifact? Without being able to look into the future and get future data it really was quite difficult to make the call. The commercial indicator at the same time was showing, as you've said, larger numbers of fish out there than the research vessel was. And this is leaving the one odd year out. In consequence, it would have been nice at that stage to have had a third indicator that might have told you which one of these two might be indicating the true trend.

Harris's comment was, in cases of uncertainty, try and pick the least uncertain. And Harris's suggestion was that because the research vessel is so consistent year over year, give increased weighting to the research vessel. At least you know what its strengths and weaknesses are. You can get an off year but given that, you should be fairly consistent over time. In commercial vessels, because of the imponderables, it really is hard to be sure how to interpret that data that you're getting. So Harris said, err on the side of safety. Give weighting to the research vessel.

What has happened subsequent to the Harris recommendation, for example, this year we first of all looked at the research vessel and the commercial vessels as two separate indicators. To see, are they giving us consistent signals which support each other or are they giving us different signals. The second thing we've done is to examine the data given by both of them to see, particularly in the commercial vessel area, where have we got consistent data? In other words, where things haven't changed and we're relatively comfortable that we're being given consistency over time. The interpretation there was that, for the middle ages groups, the data was quite good.

For the second step, first of all we compared them independently. We combined the data to give us larger sample size and shorter confidence intervals. And what that did for us is that it reduced the gap between the purely research vessel estimate of how big the stock was and the commercial vessel indicator to about, speaking off the top of my head but about, half of what that gap had been before. So that, in my mind, was a useful step. Anything we can do to get the best possible information out of those two data sets really is useful.

If we could get hydro-acoustics to work, that would give us a third indicator. The more information you have the shorter you make the gap between the decision you've got to make and the data you've got to make it on.

Q: The time is about up but I'd like to ask one last question about the possibility of useful predictions. Of resource projections. When one looks at the resource projections for past years it's quite striking. You have year zero here. And on the left there' a lot of variation, actual catches and TACs. And on the right there's this gorgeous stair-step projection. Always increasing. Always nearly linear. And this tells me something. Why should the future be any less variable, and always in a rising trend, than the past? I think that the current situation in the fisheries can be traced, especially the overcapacity problem, can be traced to the Kirby Report. And the wildly optimistic resource projections that were contained there. They were projecting TACs in 1989 and 1990 on the order of 450,000 to 500,000 metric tonnes. That's more than one order of magnitude off from reality. And yet there's tremendous demand for projections. From the commercial sector particularly. The financial sector of the fishery needs to make five and ten year business plans. Will it ever be possible to make useful resource projections? More than one year class away?

A: I think the answer depends on what degree of certainty you're willing to accept as useful. I find it helpful to go to extremes. Because extreme cases by their nature make certain things obvious.

If, for example, we were to say that since resource projections, or predictions of anything for that matter, what the weather's going to be tomorrow, are by their nature not absolute, and if we were to take the extreme case and say that because by their nature predictions are not perfect perhaps we shouldn't do them at all. If you follow that line of logic we make no predictions. We put no controls on a common resource fishery.

What history tells us, going back to the middle ages, is the tragedy of the commons. Where the kings, with the best intentions, set aside some common land for their subjects to use. You had a race to use that land because if I didn't get my share this year you took it and there was nothing left for me. Fishing capacity is so great now that if something were not done to predict what can be harvested with reasonable confidence in what should be left there, we likely would damage the fishery in a very short period of time. That's an assumption but it's my assumption at this point in time.

If, on the other hand, we were to say, to take another extreme....People like to have certainty because they have to make ten year business plans and guarantee payments consistently over ten years. Then let's make ten year plans of what you can harvest from the stock. That really is the debate which took place in the 'sixties and 'seventies around Maximum Sustainable Yield.

What seems to have been concluded at that time is that, in a naturally varying stock, in order to have maximum sustainable, in other words, every year the same amount, you would have to take a very low and conservative amount. So you're holding yield and allowing the stock to fluctuate but you're not taking the peaks or valleys.

I don't think either of those extremes would be helpful. If we took a sustainable yield in a way that we were quite certain would be sustainable, it would likely be a very small yield and in certain years we would leave a lot of fish in the ocean. My instinct tells me that the most useful solution, if we could become reasonable people and accept that there is uncertainty in this business and nobody is able to predict the future perfectly, I think that for those stocks that lend themselves to it a multi-year TAC would be useful.

I'll tell you why given the uncertainties. There are some stocks which grow and decline over a period of years. And if, for example, we are reasonably sure that the spawning biomass, the mother stock, is relatively low it should be given some time to build. Let's assume for a moment that it's going to take a few years to build that stock. And in every area equivalent which is cyclical, that I'm aware of, ups and downs in the economy or ups and downs in the [unclear word] cycle, it is very difficult for people in a business sense or in a human sense to accept right angle turns. In other words large TAC this year, small TAC next year.

If we know that we should rebuild a stock, let's say that there's a stock which we have been harvesting historically at 50,000 tonnes a year and we feel that it should go to 40,000 tonnes a year to allow that stock to rebuild. We could go to 40,000 tonnes immediately and rebuild it quickly or we could go to 40,000 tonnes in three or four lock steps. Say 50,000 this year. 47 next year. 45 and 40. Let's assume that allows the stock to rebuild in a safe biological way. The price you're paying for a smoother change over a longer period is that the recovery will be over a longer period also.

If you look at human behaviour, people seem to accept difficult decisions if they're not surprises and if they're phased in a little in the future with a little forewarning. For example, if you look at the unions negotiating salary increases, if the union's offered 15 per cent over three years it often gets broken down as package of, let's say, 7 per cent, 5 per cent and 3 per cent. The 7 per cent is quite acceptable now. The decision to take a 5 per cent is a year away. A decision, psychologists tell us, only becomes frightening as the decision point is approached. You have a year to accommodate yourself to it and the 3 per cent you have a further year.

So I would say on those stocks where we know the stock needs rebuilding and where there's no biological reason to prevent us from doing it over time, assuming that the community we serve were willing to live with that time frame, I think that multi-year plans in some stocks could serve a useful purpose. It would avoid the abrupt right angle changes that are so hard to live with.

Q: And I assume that northern cod would be one of these stocks. That multi-year plans would be most appropriate to long-lived, slow recruiting stocks.

A: Yes.

Q: That they're less sensitive to variations. Large numbers, high density, long-lived stocks.

A: Yes. Take again, for example, and extreme case. Assume that you had a species of fish that recruited to the fishery this year and died at the end of this year.

Q: Shrimp for instance? Aren't there some shrimp fisheries like that?

A: Let's take a hypothetical one. It's safer. Let's say it recruited and died in the same year. That means that you could not have a multi-year plan because you do not have a multi-year stock. You're forced to a one-year plan. But for those that are a little more spread out and which lend themselves to it, I think it could have value. The codicil that you would have to put in there is that if for any reason the data on which the assessment were made was superseded by better data or different data a new decision would have to be made. But that's the uncertainty that you live with day to day in the fishery in any event.

APPENDIX M

Interview with Ram Myers, Resource Assessment Modeller
Conducted in St. John's, Newfoundland
August 28, 1990

[Discussing the process by which things went wrong]

A: There was a group of people who did not want others to have a close look at the data. It was very subjective. Virtually nothing was published.

Q: Who were these people?

A: You want names? Dick Wells. He's dead now. They were convinced that the stock was going up. Honestly, completely convinced. There were other people...I was outside of the assessment process. People who were not within a small group were very much discouraged from examining the data. There's a long history of that.

Q: So stock assessment was run as an exclusive club?

A: No. It was through CAFSAC. But if I wanted to model the distribution of fish in relationship to temperature, this was fought very hard.

Q: Why?

A: Paranoia.

Q: But if they were convinced they were right, who were they scared of?

A: I don't know. But what went wrong with the process, why the mistakes were made, was this exclusive attitude to examining the data. That and some sociological reasons. The group dynamics of the process.

It's very unscientific. Not in terms of the mathematics. Well, it's unscientific from my point of view. There's a group of people that gets together and they meet continuously. And in order to make progress at these meetings, you have to accept certain things as common. Otherwise you'd be arguing about every point. This is simply the way the process worked. It almost has to because these are human beings. It's one thing to talk about perfect people but they aren't.

And there are certain things that are inherent in the process of having a group of people examining things, like a small society. And within that group there are people who are very much opposed to something in the stock assessments. But since it is consensus, anyone in the group who....within CAFSAC. There's a local group none of whom have Ph.Ds.

Q: These were people who had been hired under Sandy's directorship?

A: Yeah. Some even before.

Q: Under Wilf Templeman. Going back that far?

A: Oh yeah. One of the fundamental things to realize is that the Canadian system works by putting a group of scientists at different levels, some active researchers but mostly people who are trained on the job. And traditionally they've come from a standard biology background. You put them in. Your try to shelter them from outside interest groups. And they try to come up with an independent decision.

This process probably works better than any other process I can think of. Not that mistakes aren't made. The only interest is in people who've said something and they want what they've said to be true. As opposed to inshore fishermen saying the quotas should be lowered or the offshore fishermen saying the quotas should be raised. It's a decision-making process without advocates, in the traditional sense.

Q: But certainly it generates advocates internally?

A: Yes. But when the quotas were generated pre-CAFSAC, when it was the old ICNAF system, there'd be different national groups arguing for different things, advocacy groups. As opposed to that, you've got a group as much as possible shielded from the outside forces.

Q: I seem to recall a note of warning in the '82 or '83 CAFSAC report...

A: George Winters?

Q: And then that voice disappeared until the '87 assessment.

A: No. That's not true. It didn't disappear. It simply...Remember, it's a consensus process. Unless you're willing to go to meetings and just slug it out...The meetings aren't over until they've come to a consensus. A decision has to be made. There's no such thing as saying, I don't know. This is a process where saying, there isn't enough information, is not acceptable. Decisions always have to be made. And consistently abundance was overestimated and fishing mortality was underestimated for years and years and years.

Q: Why? What were the contributing factors?

A: I can't speak for the period before '83 or so because before then I knew nothing about the process. But even then it was beginning to become clear. I think that after that, a group of Canadian scientists were forced to make a prediction about how many fish there were going to be and they made a prediction.

Q: Was this for the Kirby Report?

A: Yes. They were asked what would happen if you put in a 200 mile limit and cut back on fishing. [NOTE: this must have been before '77 then, not for the Kirby Report which was published in Dec. '82] They weren't really keen, as I understand it, I wasn't there, and they produced something with confidence limits. Okay, so they said that the stock was going to go booming ahead. And therefore their reputations...and they wanted to force reality to be what they'd predicted. I think that was the key thing. There were technical problems. But there were sources of information that made it abundantly clear that that wasn't true. There was data that wasn't consistent with that. That was consistently ignored.

Q: By the core group within DFO?

A: By the core group and the whole CAFSAC process allowed it to be ignored. This data consisted of, for example, by calculating mortalities from the research surveys. Research surveys are very variable. Nevertheless, you can calculate average mortalities. And these were much higher than what was claimed by the assessment. It was completely inconsistent.

Q: Natural mortality or fishing mortality?

A: Fishing mortality. Actually, you compute total mortality and then subtract out...but this evidence was ignored.

Q: Was this virtual population...?

A: No. This was much simpler. Just how many age five do we have this year versus how many age six next year. And there is a lot of error in this but you can calculate roughly.

Q: And yet this data was discounted in favour of commercial catch data?

A: There are several issues that are confusing you. What you're referring to is that up until the '86 research survey, that is, the '87 assessment, the VPAs had been tuned against the commercial catch per unit effort. The reason that the commercial CPUE was going up is that they were learning how to fish. Introducing new gear. Commercial CPUE data is not very reliable.

Q: But until very recently it was weighted more heavily than the RV data.

A: That was up until...Now the research survey data is more variable.

Q: So it's scarier?

A: It's scarier. It's a very big ocean out there to survey with limited resources and they spend a lot of money on it. If the surveys had been sufficiently accurate then there wouldn't have been this problem but that would have cost a lot more money than was available. It's an inherently very expensive thing to do.

Q: And commercial data is free, its more stable and there's a lot more of it.

A: But it contains trends and bias.

Q: Yes, but I'm trying to understand why it was consistently weighted more heavily than...

A: The key thing to understand is that it conformed to what people, some people wanted to believe. It's a little more complicated than that but I don't feel it's a lot more complicated than that. Then in '86 there was a huge increase in the abundance in the research surveys. So they quit tuning against the commercial surveys and tuned against that and got a tremendous number.

At that point was when the inshore fishermen yelled and screamed. And the Alverson Commission was called. I had nothing to do with this until that time because I was completely outside. So this was '87 and I was asked by someone on the Alverson Commission to examine the data because I had developed new mechanisms for evaluating research survey data. It had to be done quickly. I concluded, in about four days, given access to the data, that their claim that there was an increase, from the research surveys, was simply false. For various reasons.

Q: Could you be more specific?

A: There had been changes in the timing of the surveys, over time, which had effected the estimates of abundance. Also they were just very variable. The symmetric confidence limits for the 3K population, they're not symmetric but you assume they're symmetric, the 95 per cent confidence limits included zero for that year. So simply, there were a few big catches. It was a little more complicated than that but 3L did not show an increase in '86. 3K did but there was almost no information. And 2J did. But it was very variable and the timing had changed. Once you start including these factors, their conclusions were not very robust. I concluded based on...not even reading the assessment document...I did an independent analysis of the data, that the stock simply was not increasing at that time?

Q: So at that point you were not working for DFO?

A: I was working for DFO. But simply because you work for DFO doesn't mean you're allowed to examine data.

Q; So it took Alverson coming...?

A: This was Mac Mercer's major fault is that he allowed the power blocs...I was asked by someone on Alverson's commission, the one person who knew what he was doing, John Pope...

Q; So it took Alverson coming in from outside to force them to let you see the data. To crack the safe for you?

A: That's right. And this was just to do my job. And it has created an enormous number of problems for me. There are people who just hate me for doing that. In retrospect, with a lot more data now, it's abundantly clear that it was true. After the Alverson report things were restructured and they realized that fishing mortality was basically twice what they thought.

Q: So instead of catching 20 per cent of the fishable stock they were catching about 40 per cent?

A: Roughly. So the Harris group was largely unnecessary.

Q: That's an interesting statement because I've been told by everyone else that the Alverson report basically confirmed the DFO science was doing a good job, confirmed your results that said the stock had grown by five-fold.

A: The basic problems were corrected by the Alverson Commission. At least as far as the mortality rate.

Q: As far as the assessment process itself went.

A: Yes. That's right.

Q: So the word went out internally. The problem was recognized internally but there wasn't a public acknowledgement that the problem had existed and that it had been corrected?

A: The earlier reports of the Alverson commission including an analysis by John Gulland and John Pope. You know who these people are?

Q: I've read Gulland's FAO text on stock assessment.

A: He died just recently. John Pope and I are friends. I see him around. I think they went easier than they should have. But the report was modified.

Q: So it had a greater affect internally than it did externally?

A: It did change things. Even though the people internally were saying the Alverson report vindicated us, it was clear at that point that large mistakes had been made.

Q: But it took the Harris report to articulate the problems, errors and solutions to the larger community, to the consumers of scientific knowledge.

A: From Alverson it was clear that fishing mortality had been grossly underestimated. When that was rectified...It was an interesting process...which meant that fishing mortalities had to come down and quotas had to be cut. There was a huge hue and cry and another commission was called. This was a process. The Alverson commission shook things up. There was recognition that fishing mortalities had been underestimated. When that took effect and new quotas were recommended, there was enough uncertainty created by the Alverson commission that those weren't believed.

The Harris commission had nothing new to say other than a lot more money could be spent. The person who did the reanalysis, John Pope, said the assessment is basically OK. John Pope actually recommended some fairly drastic changes which Harris wasn't willing to take. For example, eliminating the trap fishery.

Q: Eliminating it entirely?

A: Yes. They catch baby fish! They do! They catch really small fish! It's a stupid fishery! Just in terms of yield from a fish in the ocean, it's a very stupid fishery. And I was in the room...I was asked to do some more analysis informally for the Harris commission, I was in [?placename?] with Harris and John Pope and Alverson, Jake Rice came over as well. I was there independently. And I was there when John Pope suggested the trap fishery be eliminated completely. It's not something where you can change things gradually. You have too...

Q: I take it he's not a Newfoundlander?

A: No. But in a sense the best-regulated fishery may be something like the Falkland Islands' squid fishery which is operated entirely by foreigners and it's operated as a business as opposed to a welfare system.

Harris was not going to say anything that would in any way be unacceptable to his perceived...Whether you agree with the point or not, whether you believe we should eliminate the trap fishery or not, you don't not eliminate it because people will dislike you personally!

[general discussion about the new money for northern cod research and the difficulty of replacing the recent departures from science branch]

A: Scott [Aikenhead] would have left anyway. And Mac Mercer...I don't know why he left. One reason was probably because of the criticism. And the only part of the criticism that I think was deserved was that he allowed the power blocs....He didn't allow the data to be accessed freely. And that was a very serious mistake.

Let me explain one simple consequence of that. When the Kirby Commission made their report, they projected an increase in cod based on their remaining at the same weight per cod as when the 200 mile limit was imposed. It turns out that cod growth is strongly related to density. The more cod there are, the smaller they are. This was, in fact, noticed several years before that. But the person who worked it up, the access to the data was denied him even after it was done. It was never reported. So this is one of the effects of not allowing....And this was when Sandy Sandeman was....That was allowed to happen. It was forbidden for that person to publish anything. That person was Scot Aikenhead.

Q: So the knowledge was there but it was suppressed?

A: Yes. And for no good reason.

Q: By who?

A: In that case, Dick Wells. And Sandy Sandeman was director and allowed it to happen. Mac Mercer, to his credit, tried to change things but didn't try hard enough. That happened, number one, because he probably wanted to publish the data himself later but never got around to it. Or maybe there was some deeper psychological reason. But that wasn't the only case. Derek Ross was here and he was forbidden access to data. Jake Rice was, for years, forbidden access to the data he was hired to work with. This was before he became management level. There were all kinds of examples of that.

Q: Would you characterize it as a case of the old guard versus the young turks?

A: Yeah. And Jake Rice became an old turk [sic] just like that [snaps fingers]. It was an amazing transition.

Q: After he became management?

A: Yeah. We're basically a tribal society and once you become a member of a tribe, the tribe is all-important. In this case the cod assessment biologists were the tribe and they were certainly protected which was pretty foreign to me. Through all of this I remained an outsider.

Q: Tell me a little more specifically what it is that you do in DFO. Resource assessment modelling?

A: Yes.

Q: So you work for John Hoenig's CODE group?

A: Yes. John Hoenig is not willing to stick his neck out. Not willing to go in there and slug it out. Do you know what I mean? Some times you have to go in there, and this is an important issue. People's livelihoods are at stake. And you have to be willing to go in there and slug it out.

During the Alverson Commission I sat around a table when I was giving my reanalysis. And there was the director of the lab, directors from Ottawa. Everyone involved in the process. And I was presenting this report to John Pope and John Poole. And basically I said the cod population hadn't changed in the last six years and that the fishing mortality was at least double of what they were claiming.

All my co-workers were there and everyone of the, without exception, violently disagreed with my analysis. Without exception. It began with Dick Wells saying, "Well, you really can't expect us to say anything different. We've gone through the process and this is the CAFSAC document and this is what we've concluded. Therefore, you can't expect us to say anything different." Which is an incredibly anti-scientific approach to the topic.

A lot of the things that you're talking about are not science in any traditional sense. The process is not science. You're talking about something that has more to do with tribal societies....But all science might be like that. But with this in particular, the process is very different than scientific research.

Q: But don't most other scientific debates get resolved in a consensual way? They are debated in the journals and at meetings and the eventual resolution is a matter of consensus.

A: Not necessarily. You can have issues where a consensus has not been reached for fifty years!

Q: Ah yes. There doesn't have to be an answer tomorrow.

A: Yes. That's the big difference. For instance, interpretations of quantum mechanics. No one doubts the basic formulations but there is not really a consensus in terms of the interpretation. [At CAFSAC] A decision has to be made. A number has to be put forward. "I don't know," isn't an answer. And the person who waits longest. The person who believes strongest and is willing to stay out of town in a hotel the longest is the one....So it's not even a consensus, it's....

Q: A war of attrition?

A: Almost. And most people are not willing to stand up and have a lot of people telling them that they are wrong. They won't do that. I don't usually go to these assessment meetings because I don't like them. I don't like the process because I get incredibly aggressive.

Q: They are probably just as happy if you don't go.

A: That's true!

Q: [statement that the key to understanding this "crisis" is as a conflict of evidential contexts; scientific, political, social and corporate-capitalist]

A: It may not be knowledge. If you are a corporate entity and you are borrowing money at 14 per cent interest rate and you have a fish stock that is growing at 10 per cent, your optimal thing to do is to take it all, sell the boats and do something else. That has nothing to do with knowledge.

Q: Let's try it this way. Within the internal evaluative traditions of science, you can be doing a pretty good job but that might not be good enough to be useful to the corporate or political interests. And from what I've heard so far you are doing a good job but you're in an early stage of development of a new science....

A: No. I don't think this is a new science. I think they were fucked up. There was enormous...There was a lot of information that was "true" because people believed that it was. So there was something wrong with the process at that point. But I think that was largely corrected before the Harris Commission.

Q: But one of the things that the Harris Report did was to publicly communicate the large degrees of uncertainty that you are dealing with. So I think that in a way, the consumers of scientific knowledge, the corporate and political sectors, were more comfortable when you were wrong with apparent precision rather than when you are correct but with large confidence intervals.

A: No confidence intervals were ever communicated. The problem was that there was a persistent bias. Fishing mortalities were underestimated tremendously. That was the problem.

Q: Let me ask you a specific question. What sort of confidence intervals are you dealing with now when you make an assessment with the new, revised procedures?

A: The key thing to get right is the fishing mortality. And I would imagine that our fishing mortalities for a given quota would be within .05 of what we expect them to be. If we think that mortality is .4 the confidence bands are .35 to .45.

Q: So what percentage of overall error are we talking about?

A: It depends on what quantities you're talking about. If you're talking about what are the errors in a certain survey in any one year, that's going to be larger than the error in the estimate of the biomass in any one year because information from a number of them are taken into account. The problems that we've had have been structural problems that have allowed large biases in.

Q: Aren't there still some large gaps in your knowledge about fishing mortality. For instance the middle distance fleet is very poorly monitored and has tremendous incentives to under-report and discard. One hears stories about midnight landings at small plants and huge slicks of discarded, undersized cod.

A: There are those problems. There are also the problems of, if there are inshore stocks, their fishing mortality would be much greater. So there are those problems. The fishing mortality of the inshore is really large on ceratin portions of the stock. That's true.

Q: And that's mostly pre-reproductive stock.

A: Yes. Small fish.

Q: So that would have a greater effect on the future population. Greater than catching the same numbers or weight of mature fish.

A: Yes. The trap fishery is not a great way to catch fish. They are getting quite small fish.

Q: So before people started dragging offshore in the 'fifties and 'sixties, the trap fishery was viable because there were so many...

A: Now wait a minute. There were long lines. There were other ways of catching...other kinds of gear. Whole communities migrated up to the Labrador. They don't do that now because there's a subsidy that allows them to fill Conception Bay with traps and bottom gill nets. So I think that the structure of the fishing gear has changed drastically as has where the fishing takes place.

Q: The trap is a fairly recent development?

A: Yes. As is the bottom gill net. The fishing mortality now is much higher than it has been in the past.

Q: I'd like to get back to the point that I think is the source of the frustration and criticism from the groups outside of science and that's that you haven't been able to supply reliable, precise knowledge for the commercial sector to base their five and ten year plans on and for politicians to maintain nice, stable quotas and build nice, stable processing sectors. The volatility is at best an embarrassment and at worst a disaster for a lot of the consumers of scientific knowledge.

A: What you're saying is that a lot of these sectors would trade off the mean yield for a lower variance. And in order to have that, you have to allow the stock to rebuild. Which means allowing...reducing fishing mortality. And you want to change the quotas gradually.

Q: But they had to be revised drastically downward.

A: But in a sense, you can make five and ten year projections. But you have to make assumptions about the behaviour of the fishery. And you have to make assumptions about the variation in recruitment. You can't do anything about the variation in recruitment. You can have a sequence of poor year-classes. That's simply true.

Q: So it's conceivable that even with zero fishing mortality you could have the stock drop just from a couple of disastrous year-classes.

A: That's right. That being said, the variation in recruitment for northern cod is not very high as fish go. [shows me a publication on the historical variations in recruitment for 100 North Atlantic fish stocks] The errors made in the past were largely preventable. The process broke down. I think it was largely repaired by the Alverson Report. Not completely. The flow of information is still not completely open. Some of the problems are still there. I'm not going to fight it. I'd just as soon do other things.

Q: So you just sit in the CODE group and wait for people to come to you with questions?

A: No. I generally feel that I can recognize problems before other people do. The analysis that I did for the Alverson report was stuff that I wasn't asked to work on but that I felt needed to be worked on. There was resistance in terms of getting the data but there was never resistance from Mac Mercer in terms of doing the work. Is any of this helping you?

Q: Yes. But it's not what I'd expected to be talking about. I had expected to be talking about these issues at a more removed level. Building a new science under conditions of extreme uncertainty and considerable criticism and hostility.

A: I think this notion of building a new science is...I mean a lot of the issues that we're talking about are not very...Plotting a growth rate against numbers of fish is not very complicated. It's not a new science. That wasn't done because either there were people who wanted to do it themselves and didn't get around to it or for whatever reason kept it from being done. It wasn't really hard to do. It was obvious in the data. It was not done. That has nothing to do with science.

Q: So in your opinion, a lot of this talk about the difficulties of building a new science is a cover up or a way of explaining the failures of the past?

A: Well, I think that a lot of the failures of the past were tribal in nature. That has nothing to do with science. Except scientists are human like everyone else. These people generally do not publish in the peer-reviewed journals. There was almost nothing from this group of people doing the work that was published in open literature.

Q: And yet these were the people who decided what was done by who and where?

A: Yes. More than they should have. The Director was reluctant to exercise his full authority.

Q: And their relative authority was perhaps a function of their long tenure and institutional inertia?

A: Yes.

Q: May I speculate that these people were by-and-large Newfoundlanders and younger, more academically credentialed people were by-and-large come-from-aways?

A: Yes.

Q: So there was resentment to all these college educated mainlanders who were coming in and trying to tell them how to run their fishery?

A: I think it was much more personal than that. To be fair, there were Newfoundlanders who fought long and hard. There was George Winters who wrote that paper saying northern cod assessment was not worth a rat's asshole. So I don't think it's fair...there's a bit of that but that's not the whole story.

Q; I'm trying to see as many people on different sides of this issue as possible. I'm going to be seeing Sandy [Ted Sandeman] in a week or so.

A: Ask him why data was not allowed to be analyzed when he was director. And give him the example of the growth rate/population study that Dick Wells kept the data out of.

[break: discussion about the sociology of science]

There's a big question about how science differs from the normal ways in which things are done. In some ways this stock assessment] is more like engineering. And before engineers had solid rules about building bridges, bridges fell down a lot. Bridges don't fall down much any more.

[break: discussion about the Falkland's squid fishery]

Do you know who Colin Clark [Clarke?] is? You should. He wrote a book called "Mathematical Bio-economics." He made the quite serious recommendation that you'd be better off dropping DFO, dropping all the income supplements, all the government programs and just let people do what they want to the fishery. He said, "Of course you wouldn't have any fish then." But it wouldn't be this tremendous drain on the economy. You have to ask the question, "Is the fishery as it is presently run a net benefit to the economy?" At no point in the Harris Report did they look at the cost/benefit for any of the recommendations. Nowhere was there an analysis of what would be worth knowing as opposed to what would be nice to know. I think that is a big problem with that report.

APPENDIX N

Interview with Jim Roache, Director of Communications
Conducted in St. John's, Newfoundland
July 24, 1990

Q: Let's begin with a brief recap of your professional background and experience.

A: I'm Director of Communications for the DFO Newfoundland Region. Prior to that, I had a long and broad background starting in private sector broadcasting with VOCM in St. John's and other smaller stations and then with the CBC for 16 years, most of that in television as a journalist and manager.

I've worked in the DFO Department of Communications in Ottawa for two years and when the current crisis began to develop in the fishing industry they felt that they needed someone here who had a fairly broad communications background, but who also had a fairly intimate knowledge of the region and of the fishing industry and of the Department of Fisheries and Oceans. And by definition, the kind of person required fit my resume to a "T".

So they approached me and said, "We've got a set of problems down here and we need somebody to go in and define more succinctly where the barriers to communication are, why the message isn't getting out. How can the department respond more effectively?" Go in as a consultant essentially. Devise a set of communications plans appropriate to what you discover.

Q: And you've been here how long now?

A: Since March which is five months.

Q: And what are the problems that you've found here?

A: Well the big problem...One of the first things I did was a media analysis. Basically sat in my hotel room for a week with the press clippings for the preceding six months and discovered that Fisheries and Oceans was either not mentioned or mentioned in a negative way in almost 100 per cent of the stories in the local papers including, more disturbingly I suppose, the specialed sections of the papers. Those should have had a more balanced and insightful interpretation of events.

The expression on the street when I arrived and up until two or three weeks ago was that "The arse is out of her." There's a crisis in the fishery. The bottom's fallen out. The stocks have collapsed. Which was a terrible overstatement of the case. Not because it made DFO look bad or DFO science look bad, but because the general public and people who work in or who are closely related to the fishing industry are done a disservice. A spectre is held up...the bogeyman. They are left to worry about things that aren't true.

Which is not to say that there aren't problems in the fishing industry. There are. The fishing industry has to be rationalized. It's overcapitalized. It has been used as the employer of last resort. It has been used for political purposes in the past. All of those things are true. But to move from there, those facts, to a perception that "the arse is out of her" is to sensationalize. So I saw my job as being, in some way, to bring reality and perception closer together.

Q: Why and how did the reporting on the fishery come to be so one-sided?

A: That begins, I suppose, on a philosophical level. When I left CBC after 16 years, I left because I was concerned about a growing tendency in my discipline, which was journalism, to be sensational. A tendency to try to sell papers, try to get the ratings, try to get the by-line. There was a move away from the old journalistic ethic of objectivity, balance and fairness. And the move was towards sensationalism or a willingness to use only part of the truth to make a "better story".

The emphasis in the so-called new journalism is towards the "better story" and unfortunately the "better story" is what we sometimes used to call yellow journalism. The careful shading or elimination of some of the facts so that you're left with a "better story." It serves a couple of purposes. You get that "better story" day one and you also allow the corrections, clarifications and amplifications that ensue to spread that story over a longer period of time and maintain public interest over a longer period of time. So you can sell more papers or get more ratings or get more by-lines over that longer period.

There is now, unfortunately, a journalistic preference, investment in, bias towards the sensational as opposed to the old objectivity, balance and fairness--which does nobody any good in the long-run. I'm afraid that the journalist has become part of the story, part of the issue, in all too many instances now. There are worse offenders in some situations and in some organizations and in some media than in others and I can't generalize to say this is true of all journalists, but too much of the time it's true.

And I found in my own experience with the Corporation that I was often working with young people who challenged me as being a dinosaur, naive, idealistic and who tried to encourage me to opt into their ethic and go for the gusto, go for the headline.

Q: Are you saying that the current perception of a crisis in the fishery is an artifact of the media?

A: Not entirely. What I am saying is that the media has made a less-than-ideal set of circumstances into a crisis. I'm not trying to say that there isn't a problem in the fishery. What we have is a situation where the stock hasn't increases as quickly as everybody had anticipated. We have a crisis of expectation.

When we went to a 200 mile limit people in general, and I think industry as well, felt that our ship had come in, that the time was at hand when we could catch as many fish in as many different ways, throw in as much technology and as much capital as we liked, making it as labour-intensive and as capital-intensive as we wanted, pulling out all the stops in marketing the product. It was a gold rush kind of mentality. It was going to be a boom as opposed to the historic bust.

We haven't had the boom, but we haven't had the bust either. What we've got is something in between. And the something in between is that big fish companies have good balance sheets and good stock market performance and had high levels of employment and operated at about 60 per cent capacity for a goodly number of years, say the last five years before "the bottom fell out." And we should never cease to acknowledge that fact. That's the good news. The good news is that there was a recovery. We were able to harvest that resource in a way in which, with the 12 mile limit, we weren't able to.

Q: What share of the responsibility for the creation of these false expectations belongs to DFO?

A: I think we're all guilty of creating that kind of mentality. Which is to say that DFO made a contribution as did other departments of government, as did the large fish companies, as did fishermen, as did the small and medium-sized processors. Everybody was on the bandwagon.

Right now, there's a temptation on everybody's part to try to scapegoat somebody else for how this situation developed. The fact of the matter is that the situation that has developed is not a catastrophic one. We have the healthiest cod stock in the world. It simply hasn't increased at the predicted rate.

Q: In that case the fishermen and the man on the street want to know why the quotas are going down if the stock is increasing.

A: The quotas are going down in keeping with the fact that the biomass hasn't increased to the extent that everybody had predicted. It's going down in a move to rationalize the industry. It's going down to put things in a proper balance so that a smaller effort by fully professional fishermen and processors can produce a better living for participants than historically anybody's been able to do from that resource.

Q: Are you saying that the reduction in quotas is not so much to protect the stocks but a strategy for rationalizing the fishery?

A: It's both. It's first and foremost a move to preserve the stocks. To allow the stocks to grow at some optimal level whereby they will allow a reasonable number of people and organizations to exploit that resource in a rational way. Quotas give us targets beyond which we're over-exploiting the resource, beyond which we're trying to get more out of the resource in terms of employment and return to stockholders and political benefits than that resource can sustain over time. So to that extent it's a rationalization.

Q: Do you see science being used as a tool or scapegoat of this socio-political program of rationalization?

A: That's too cynical an interpretation to put on it. What I do see science as having done is having failed, over the last ten to twenty years, to elaborate to the public in a language that they could understand what they could expect from science, and to elaborate the job that was being done by science at the time it was being done, with a clear explanation of what should be a reasonable expectation on the basis of the resources that were available to do the work. In other words, it should have been clear up front, communicated consistently throughout, that here is the job that we scientists are trying to do, here are the resources that we're allowed to do that job, here is the short-fall in those resources, here is the probability of accuracy, and therefore, every number and every option that we give the mangers is given with the caveat that within these conditions and limits, our best guess is.

It really wasn't made clear that the scientific work that was being done wasn't foolproof. It wasn't really clear that the recommendations given to the managers weren't really recommendations as you and I understand the word, but were sets of options or ranges of numbers which had certain likely outcomes attached. Now scientists understand that to be the case and take it for granted that everybody else understands, but people don't.

Had people been properly attuned to what was reasonable to expect, had they been attuned to the fact that there was a certain element of risk or uncertainty associated with the recommendations that the scientists were producing, had it been explained by the other parties, the managers and the politicians, that there were other variables--that the scientific output was only one of the inputs for the fisheries manager or the politician to weigh in determining the TAC, and those other considerations are equally important from other perspectives, there would not be a problem.

The scientist would say, the number or range with the statistical probability of certainty attached, is the important thing, whereas a sociologist might say that social considerations are more important. We have a tradition of people being able to live off the fishery.

We have a tradition of it being the industry of last resort. The economist, on the other hand, particularly if he was from the rationalist school, would say bigger is better. We've got to take care of the offshore. We've got to take care of the big plants, the big fleets. We've got to get the industry structured into a few large, manageable units. So where you stand depends on where you sit.

There wasn't enough of a caution issued by the scientists in the first instance and the managers in the second and the politicians in the third. We all rode the wave of our own expectations. And we're now in the middle of a crisis of those expectations, not a crisis in the state of the stocks.

Q: Isn't that perhaps a bit of wishful thinking given the levels of uncertainty that we're still dealing with? My understanding of fisheries stock assessment science is that it is a very new science and it is at that early stage in its development where a great deal of its activity is directed toward uncovering, documenting and attempting to mitigate sources of error and uncertainty. So it is in the peculiar position where the sources of error, the known ones, have actually increased in recent years. From the point of view of science, this is an inevitable and essential part of the process of creating knowledge. Until you know what the number and magnitude of error sources are, you can't work to minimize them. And the news that got out about science was, yes they've been seriously underestimating mortality, the models didn't work very well, data sources such as CPUE were discovered to be sources of as much uncertainty as data, research design, sampling techniques, all of these sources or raw data for the assessment models have been recently been shown to be more or less inaccurate and billed as mistakes and incompetence in the press. But from the scientist's perspective, this is normal science. However, these things have all led to a reduction in the estimate of the stock biomass of about half over the last few years.

A: You're probably more conversant with the specifics of the situation. My knowledge of the techniques and the tools is less than yours. What I understand, though, is that two major studies have more or less "vindicated" scientific effort in terms of the methodologies that were used.

Q: That's science's interpretation of the reports. That's not shared by the general public or the political management and the commercial sector.

A: No. And there's all kinds of reasons for that other than what the scientists were doing and how they were doing it. I'm not really in a position to judge the extent to which, or whether at all, the work that DFO scientists were doing is flawed. So it would be unfair of me to comment.

Q: Let me quote from a presentation given by Leslie Harris at the Grad House last January. He said "this fancy method of counting we had was wrong. Garbage in, garbage out is an age-old formula." So clearly, his personal opinion is that the current state of stock assessment science is at best useless and at worst misleading. And he was the chair of the commission that you say vindicated science.

A: Well I hope he's either wrong or he's unduly pessimistic. None of the information that I have been able to uncover in my six months of investigation would allow me to conclude that anything other than what I've told you is the case.

You hear reports offshore from the trawler captains and they're sailing over schools of fish that sometimes run twenty or thirty miles. We just had a research vessel come back that tracked an extremely large school of cod on its way from the offshore to the inshore, feeding all the way, feeding on caplin, growing and getting fatter and fatter.

In the last two or three weeks you may have noticed that there's a hush in the media about the "crisis" in the fishing industry. People are catching fish inshore. There are a few isolated communities where they're not catching anything, but in most communities, even as we speak, people are doing very, very well. The trap fishery has picked up immeasurably from a very slow start.

I've been observing the middle-distance fleet that docks on this side of the harbour and for just about the whole period I've been here, those otter trawlers are filled to the waterline every time they come in. At first--March, 1990--the cod were very small and that disturbed me, but that was only for the first two or three weeks and since then the cod are what I'd call medium. They look fat. Every boat is full and the turn around time on those boats is as fast as they can get in, off-load, get ice and get back out. It's just non-stop.

There are people catching fish in the industry, offshore, middle-distance and now inshore. Which certainly doesn't support the hypothesis that the stock is in collapse. There are people who will say that there's a bigger problem than there really is. There is some anecdotal evidence to suggest that some communities are not getting what they might, but I'm not sure that they ever were.

When you were in my office recently I showed you a paper from 1887 that detailed the same types of problems at that time that we have today. I'm not simply trying to make DFO "look good." My job, as I see it, is to get out complete, timely and accurate information, rather than to necessarily make DFO "look good." My job isn't to make the DFO scientists "look good."

But to suggest that a scientist or group of scientists can accurately, 100 per cent of the time, predict the amount of fish in the ocean or the number of northern cod that exist in the Atlantic, is an absurdity by definition. The best that you can ever reasonably accomplish is a good guesstimate, backed up by whatever good, concrete data you can collect over time.

There's no question that our scientific method has evolved over time, changes, modifications, improvements, more resources have been thrown at it. And that process is going to be continued and be escalated under the Atlantic Fisheries Adjustment Package. But to suggest that DFO never made a mistake seems to me to be equally inaccurate as suggesting that the stock is in total collapse. Neither scenario is correct.

In fact, DFO scientists and other people with environmental concerns have been monitoring those stocks with the best resources available to them and with all the good will in the world for the past 20-25 years. In fairness to them, they've done as good a job as humanly possible under almost impossible circumstances at times. You just have to look at the weather conditions under which they have to work, the physical constraints and the shear impossibility of the job to begin with. It's a miracle that they weren't totally wrong. In fact, they were only off the mark a little bit.

They can't perform the miracles of the loaves and fishes without the loaves. I think that's the real knock...that we can't have perpetual boom. But we don't have perpetual bust either, and that's the message that I feel a responsibility to try to get out. And the way we have to do that, or one of the ways, is to popularize the scientific message. To get the scientists talking to the industry and the general public as much and as well as they talk to other scientists. And that involves not only talking but that they talk in a language that can be understood.

Q: Now that you've done your assessment and found that DFO science has failed to communicate its position accurately or effectively, how do you plan to remedy the situation?

A: I think that there's the ideal approach that one would take in that kind of a situation and then there's the approach that one is constrained to take because of the nature of the system because of the resources and the manpower that's available under these circumstances. It's always harder to get the horse back into the corral rather than to keep it in the corral in the first place.

We're now in a situation where we have to reeducate people. It would have been easier to educate them in the first place rather than re-educate them out of a perception that we have allowed to form for some time. So I don't want to begin to suggest that this is an easy task at all. I'm not sure that it's immediately "doable." I think it's a long-term project.

Under the AFAP program [the Atlantic Fisheries Adjustment Package], one person-year and some thousands of dollars have been allocated for a communication/education initiative of some sort to do two things: to clarify a lot of the misperception and misinformation, the inaccurate and incomplete information that's out there already about the state of the stock. How the TAC, how the range of options, is established by the scientific community. How the managers and politicians move from there to establish the formal TAC. What the role of CAFSAC is. How to encourage greater participation by the industry, inshore fishermen as well as trawlermen, so that anecdotal information is incorporate in some way into the model to better or more fully assess the state of the stock.

There's a whole list or projects that they're about to implement. So we have to correct old information and implement the range of new projects with expanded resources. And then we have to communicate the progress and the results of those initiatives in some way that can be understood by the layman, by the fisherman, by the processor, by the person who works in the plant.

Q: Is it fair to say that, at present, the industry, the general public and the political sector, for various reasons, feel that DFO has failed to do its job?

A: No. I think who the scapegoat is depends on whom you talk to. There are some people who will blame the foreigners for overfishing on the Nose and Tail. There are some people who will blame the foreigners for encroaching within the 200 mile limit and overfishing. There are some people who will say that we're giving away too much to the foreigners where they are not fishing illegally with over-the-side sales of allowing them to come in and fish underutilized species. There are some people who accuse us of being lax in enforcement, allowing too many discards or allowing over-fishing of the TACS because we didn't have the manpower.

There's no shortage of scapegoats for the problem that's occurred. I'm not trying to suggest for a minute, I'm not trying to be an apologist for DFO science and the position in which we find ourselves. DFO has a certain amount of responsibility to bear but the fishermen knew and know what the state of the stocks were and are.

The media and the general public were the ones who didn't know and don't know. They get their information through a filter. They get it second-hand. We talked about the tendency of the media to sensationalize the down-side rather than the up-side. The general public is interested and concerned but they don't quite believe that the "arse is out of her" quite yet.

You see too many fishermen in Newfoundland living a quality lifestyle. Which is not to say that there aren't fishermen who aren't. God knows, it's not the ideal circumstance. But there are a lot of trawlermen making a good living. A lot of people who work enough time in the plants to have a nice home, a nice standard of living and a nice quality of life. A lot of inshore fishermen are in the same boat.

There was a period of time when the Rationalist School of economics had convinced us that the family farm had to go and the inshore fisherman was operating with a question mark over his head. Time has proved that if you solve one set of perceived problems you create others. I think you're seeing the same thing in the fishing industry.

We went through a period where there was a definite shift in orientation toward the rational economic and away from the social, for better or for worse. And we're all going to have to grapple with this issue of diversification. If you take capital and you take property, plants, equipment and people out of the fishing industry, whether it's the offshore sector or the inshore sector, and move them to something else, somebody has to answer the question, "What else?"

And that's also part of this program. The Atlantic Fisheries Adjustment Program, through Canada Employment and Immigration and ACOA, and I know the provincial government is looking at it through the Doug House Economic Recovery movement and so on, people like the Chamber of Commerce and the rural development associations. Any number of institutions and groups, governmental, quasi-governmental and private, run into the problem of "diversify into what?"

There's too much scapegoating going on. It's too easy for DFO Newfoundland Region to scapegoat DFO Ottawa. It's too easy for the federal bureaucracy to scapegoat the scientists. It's too easy for the scientists to scapegoat the mangers. It's too easy for the province to scapegoat the feds. Its too easy for the offshore to blame the inshore and vice versa. And everybody blames the middle-distance guys. And on and on. The foreigners are high on the list as well. It's madness!

The fact is that all of us helped create a problem that's not nearly as serious as it's perceived to be in the public mind. We have a situation with a healthy cod stock that hasn't increased as quickly as our fondest hopes and aspirations would have had it do and we have to adjust. That could be painful.

Q: Let me pose a hypothetical question that you are going to have to deal with sooner or later. Okay. You're telling me that the stock has, in fact, increased. That they're healthy. But just two years ago, one year ago, the Harris report said and CAFSAC said that there should be huge reductions in the quota.

A: You're confusing the quota and the biomass.

Q: The Harris report, which was close to the CAFSAC numbers, said that the biomass had been overestimated by about the same percentage as the quotas should be reduced.

A: That's not true. The biomass has not been overestimated proportionately to the reduction in the TAC. The increase in the biomass has been overestimated. The TAC has been drastically reduced downward towards some optimal level of exploitation.

Q: If you read the appendices in the Harris report, if we now assume or believe that we were not fishing at the F=0.2 level but at something around or in excess of F=0.4, then that would lead us to believe that the estimate of the available biomass should be drastically reduced. The report presented a range of possibilities saying if this F value is true then this is the state. If this F value is true then this is the case. All of them represented significant reductions in the biomass estimate. But the point is that we still don't know. There are still huge sources of uncertainty in these estimates. So how would you respond, when you'd just said that the stocks were healthy and growing, to someone who'd done their homework and asked you to prove it?

A: Number one, I won't respond. I don't have to respond because this question would get passed on to some scientist who has the expertise to respond. What I have seen when Larry Coady and J.J. Maguire are confronted like that, they have the answers, they have the numbers that show where the Harris logic is flawed. They can address those issue more authoritatively that I can. And that's why they're the designated spokesmen when it comes to the specifics of a situation.

What I'm trying to communicate is the more general perception. Dr. Harris is a historian. In all fairness and with all due respect, even though he did sit through those hearings, he would need the judgement of Solomon to say that the scientific data was wrong or that there was another easy way to have done it and, therefore, we're back to square one.

I don't think we're back to square one. I don't think we're in a situation where we have no knowledge whatsoever of the state of the stocks. In fact, every piece of concrete evidence seems to suggest an increase, not as great an increase as we had thought, and an over-investment, an overcapitalization in the industry which we now have to address through rationalization.

I think that we could have continued to fish at a higher level but that would have been irresponsible. Short-term gain for long-term pain. Take the cut now. Rationalize the industry. Professionalize the industry. Increase the scientific effort and increase the accuracy and quality of the output of the scientific effort. And then we can optimize our utilization of the resource. That's what this whole effort is all about. You can't do that without a certain amount of pain and you can't do that with a 100 per cent guarantee no matter how many scientists you put out there, no matter how many boats, no matter what kind of techniques they use.

Even after putting the anecdotal information that's going to come from the offshore and inshore into the mathematical models that the scientists use, there's still going to be an element of uncertainty. There are still forces at work that we cannot fully control and do not fully understand. The environmental forces. Water temperature, tidal conditions, what happens in the food chain.

Dr. Harris says we should do an ecological assessment. Fine. But the magnitude, the shear difficulty of doing an environmental assessment so that you could be 100 per cent sure of a rational exploitation of a resource of the size of northern cod in an environment the size of the North Atlantic. You can't get there from here!

And that's the message I want to get out. We're doing the best we can under very difficult circumstances. We are trying to be good stewards of the ocean, good stewards of the industry because if there's no industry there's no DFO. So there's an enlightened self-interest on the part of the DFO scientist and the DFO manager and the DFO communicator. We're trying to do what we see as best for the industry and best for the country and that means conserving the resource while allowing optimum exploitation.

And yes, I think it's fair to admit that we overshot the mark a little bit for, say, a ten year period. But "it still ain't all that bad." There are still a lot of people making a very good living from the fishing industry and there's still some grounds for optimism.

But we're going to have to address that very difficult and dangerous issue of rationalization and that means certain communities are going to suffer. Certain individuals are going to suffer. And maybe the bottom line of some of the big processing companies are going to suffer during the period of adjustment.

Meantime, while we are moving in that direction in incremental steps, we hope to God that the business experts, the economists can come up with something into which we can diversify effectively to become a modern, technological society. We have to find a place in the country other than fish.

Q: So you see the current crisis as a socio-economic crisis. Not a biological crisis?

A: Both. There's a biological crisis to the extent that the resource hasn't increased as much as we'd hoped.

Q: But you maintain that the resource is not in trouble. That in fact is quite healthy. So how can that be characterized as a biological crisis?

A: Because, while the stock has been growing, the amount of capitalization, the amount of property, plants, equipment and the number of people trying to exploit that resource and the technology to exploit that resource has been growing at a higher rate. We've been trying to get too much out of too little. That's the problem we have to address and that's a social, political and economic issue rather than a biological issue.

And that's something that's really not within the purview of the scientists. The scientist can carry on with his research and continue to expand some of the tools and techniques he uses to get a better fix on what the biomass is doing. Then it's up to the manager to determine at what level the TAC will be set to exploit that biomass so you can optimize the number of people who live off it and the growth that you'd like to project for it over time. It's a difficult balancing act.

It's a social and economic and political management job that lies before us. It's not a disaster area. It's not unsolvable. It's something to which, if we devote the right people and the right energy, we can resolve. We can find alternatives for people in the Newfoundland fishing industry. We can educate our people and find them other kinds of work to do in a modern society that will allow the people who remain in the fishing industry to do better.

I'm overcompensating. I'm being a bit of a Pollyanna, but I'm doing that to balance your honest, but very negative, perception of the current state of affairs, because your's reflects, very accurately, the perceptions I'm seeing in the media and amongst the public at large.

The more knowledgeable people in the industry don't talk the way you talk. But they're not the ones you will see interviewed on "Here and Now." You'll see the inshore fisherman saying that there's no fish because none have struck inshore at his dot on the map. Or you'll see interviewed the head of a labour union at a plant that doesn't have quite enough fish because, again, the fish haven't struck inshore at that dot on the map. Those are the realities that we have to grapple with.

What I am suggesting is that if you took every dragger off the Grand Banks and out of the North Atlantic and tied them up, there still might be no cod at that dot on the map and that fish plant still might not be feasible in a free enterprise economy.

Q: What I've been doing in this interview is being unnaturally aggressive to simulate the kinds of questions that you're going to get to the kinds of answers that you would give to hostile critics.

A: But when push comes to shove, I won't be the one who's in the hot seat. The primary spokesman is the minister. In this case Valcourt. Other than that, it is a designated spokesperson appointed by the department in their respective areas of expertise.

I tend to be a broker of information and line up the media person or the academic such as yourself with the person in the department who has the specific information to deal with the query. Anything at the policy level, of course, has to be referred to the minister's office. I am the communications officer for the bureaucracy. But I'm still an information broker. I can only connect up the dots. I can, to a certain extent, translate the technical jargon, the scientific jargon or the bureaucratic jargon into "everyday language."

My function is as a conduit in and out. I monitor the environment, feed information back into the organization, the bureaucratic side and the political side, and then I help program the output of what ever strategic decisions are made. And my role in programming that output is to articulate to the public and the people in the industry the things that are going to help them in making the decisions that are going to help them produce a better life.

And that, in the current crisis, means trying to mitigate the spectres that have been created by the media. Saying that the sky is not falling, the world is not coming to an end, that we have a viable resource. All we've got to do is do a better job of stewarding it. And we've got to do a better job of aligning our resources so as to exploit them at an optimal level.

By no means does that constitute a crisis. It constitutes a re-ordering of priorities. And that's my role. To manage the expectation so that the strategy can fall in behind that expectation so we can all more effectively exploit the resource, so that we can humanely redirect out of the industry those organizations and communities and individuals who have to be redirected out of it.

Epilogue:

Speaking to Jim on the telephone Aug. 27, 1990 we discussed how his work was going. He said that Saga Communications (as mentioned by Jake Rice) had just completed an evaluation of DFO communications/public relations for the minister's office in Ottawa. Jim is not permitted to see the report, but has the distinct impression that it is highly critical.

In further discussion of how DFO science got itself into this mess visa vis its public and industry credibility, he said, "We were wrong because we didn't have enough data, which would have been OK, if we'd admitted it at the time. But at the time we were wrapping ourselves in the 'scientific mantle' and making pronouncements."

As a result, the newly announced scientist/fisherman communications initiative is problematic. "They might just be laughed out of town in certain instances because scientific credibility has taken such a beating already."

APPENDIX O

Interview with Edward (Sandy) Sandeman, former Director of DFO Science Branch, St. John's Conducted in St. John's, Newfoundland September, 1990

Q: [Opening request to tape the interview]

A: I'm always suspicious when interviewed by the press because they always tend to take you out of context. But when you tape the whole thing, then that's a different matter. Furthermore, the press also extracts only what it wants to use, and in doing so usually selects only those part of the interview which are controversial and to which people will react. But if you have the whole record then there should be no problem.

Q: It's my impression from reading the chronology of DFO science, the history beginning with the little station at St. Andrews up 'till now, it's my impression that you were the director during a particularly crucial phase in the transition of scientific activity, the paradigms under which it was conducted.

A: That might be so. I don't see it quite like that because I think the really crucial change actually took place back in the 'fifties with advent of landmark books by Beverton and Holt [1957] and Ricker [1948].

It was during this period that the focus of fisheries science changed to a mathematical approach and the modern science of fisheries population dynamics really took off. This was really quite a difficult time for those in fisheries science because they were neither trained or even had an aptitude for this new discipline.

Fisheries scientists of that era were trained to taxonomy and the microscope, and it was a difficult challenge to change from biology to mathematics. In their university training persons who tended to be non-mathematically inclined turned toward something like biology. They chose something that didn't require a mathematical background and now found that the calculator had to displace the microscope which previously was their major tool. That was a major challenge at the time and one which has continued to influence the relationships between scientists even to this day.

When I joined the station in 1953, a major priority was on exploratory fishing, defining where the fish were, and trying to understand their basic biology. Because you can only apply mathematical techniques once you know the population characteristics...the growth rates, the mortality rates and that sort of thing. The fishery was in an expansion phase and the expansion was outstripping the science. Because there was no shortage of fish. There was no need for conservation. At least that is the way that the Canadian fishing industry saw it.

In the early days of NAFO, what they tried to do...ICNAF in those days...was to bring in mesh regulations. It was called "saving gear." You were saving the young fish. And that was really the only effort that the international community exerted on behalf of conservation. It was put on a mathematical basis, yes, but it was very simple and naive approach to the problem.

The push didn't really develop until 1970 when most of the ICNAF community started to realize that there were problems. That gross over-fishing was taking place. That there was just too much effort no matter what mesh size you used. And I guess really that's when our scientists were forced to become much more mathematically oriented, and to use the tools of population dynamics. As we ventured into the realms of population dynamics it became evident that we had to get people on staff who were trained in more than biology. Preferably a combination of biology, mathematics, physics and computer science.

Yes particularly computers. We required people who were versed in computers and who were prepared to use them rather than shy away from them as many of the older "biologists" were prone to do. Who were...well, the modern fisheries biologist as opposed to the one who was trained only to classical biology and to the microscope. A multi-disciplinary approach was the order of the times.

Q: During this transition was there any resistance from, for lack of a better word, the old guard, the old microscope biologists, to the introduction of these new techniques?

A: It wasn't the techniques. The problem was data. You had guys who had worked for 15 years on a given species; had worked hard, spending many days at sea or in the field, to assemble a data set, which they were looking forward to working up and publishing papers which would not only enhance their scientific reputations, but because of the reward system that was in place within the service, would also likely lead to promotions and financial rewards.

As well, I think it is important to realise that these people who were now in the middle management category, also had administrative responsibilities which ate into the time that they had available for their research function, and with the new emphasis on "consultation" within the department, more and more of their time was being devoted to attending meetings. Meetings with fishermen and industry as well as the continual round of departmental and international meetings such as those of ICNAF or later those of NAFO and CAFSAC.

This gave rise to a situation where many of the older scientists, the ones who had worked hard to assemble useful databases that they had all sorts of plans to use, got more and more involved in meetings and less and less time was available to do the research, analysis and writing up that they wanted to do.

At the same time, you now had the newer generation of fisheries scientists who were entering the field who were anxious to apply their newly learned techniques and indeed had been hired because of their capability in this respect. It was the task of the Director and his management team to try to encourage harmonious working relationships between the old and the new so that joint papers became the accepted norm, and the new techniques were blended with painstakingly gathered data toward the publication of joint papers.

In this there were many success stories, but also there were several failures. Clearly good cooperative ventures are more a function of the personality of the scientists concerned than institutional regulations, and personality disharmony occurred more frequently than one would wish.

My impression is that these conflicts were more frequent when the new scientist was a recent PhD. graduate who still considered that he or she knew everything, rather than the case of the more mature scientist who could appreciate that experience played an important place in really reaching an understanding of biological phenomenon.

So we reach a situation where on the one hand we have those who have worked hard for several years designing experiments and assembling extensive data bases to test their hypotheses and on the other the scientists have not exerted themselves in the tedium of data collection and the planning of field programs, but who by the nature of their training, have skills and techniques which applied with understanding will likely lead to significant advances.

They [young scientists] are starved for data. Wanting the data. And yet unprepared to see the other side of the story and not prepared to take the trouble, I guess, to accept the fact that experience usually has something to offer and that cooperation in this sort of situation is almost always superior to an antagonistic approach.

Yes there was a clash for data. There probably still is, and there probably always will be. The guy who's invested 15 years of his life knows that his advancement is dependent on publishing and he's got this data that he wants to publish. He doesn't want to release it to someone else. Okay, usually he'll do a joint paper if it's applying new techniques and they're working on the same data and they've got a nice team going. Yes. But if they can't get that team going then you've got friction.

And that friction is likely to be relatively common when you have situations when recruitment to the service occurs in spurts with relatively long pauses in between. This is not only a problem of the laboratory in St. John's but it is everywhere.

When I was acting as Director at the Lab in St.Andrews N.B. I saw the same thing there and in fact I remember one young fellow in one of the labs in the Maritimes who wrote something, I think, like 18 papers in his first one or two years. He mined the data that had been collected by others, ignoring any plans that they may have had to use it and what was achieved? A series of rather superficial papers which lacked real understanding and which applied a variety of techniques in such a manner as not to achieve any real advancement in knowledge as well as a group of disgruntled older scientists who felt betrayed in that they were denied the final fruits of planning and field work that they had slaved at for several years. superficial. If sense had prevailed and he'd spent another year or so working with the experienced scientists and had understood the data better, the joint papers that would have been produced would have left everyone happier and he could have made a much more solid contribution to science. That was the sort of problem which was there.

Q: I ask this question because I have had sources from the younger scientists' side...

A: You'll always get that.

Q: ...telling me that they had data withheld from them, that they were denied access to...

A: Oh, they will!

Q: And some of them have tended to paint it in terms of scientific irresponsibility and outright malicious withholding of...

A: It's possible that there is some malicious withholding but I think you have to see both sides of it. Our promotional system is totally dependent on two things; published papers and international recognition. If you become chairman of an international commission or chairman of a large scientific body or something like that, you get credit for that. But you get most credit for papers published.

Q: And probably papers published are among the criteria for the selection of chairpersons of these bodies.

A: Well, to some extent that's also true. In fact, that is the main criteria I guess. You've got to be well up in the field before they select you. So, you know, these guys have an investment of time in it. The young guys don't realize that, I don't think, in most cases. Number one, they don't look at their promotional problems. They aren't worried about promotion. The world is theirs! The fact that in our promotional system...and it's worth your studying it because it's a very important part of a research scientist's thinking. There are certain levels....Do you know the system?

Q: Only very roughly.

A: Well, I think you should know the system because it really gives an insight into why you get these problems. The system has some extremely good features about it. The main feature that I think is good is that you can get a scientist who's on his own. He has no empire under him or anything like that and he's earning as much money as the Assistant Deputy Minister. If he's a top-notch scientist he's working on his own at the bench. Maybe with one technician. And publishing. And publishing first-class stuff. The system allows that and is tailored to allow that. So it means that you don't have to spend your time in administration and build up a pyramid so that the more people you get under you the more promotions you get which is the standard civil service way. That is a big strength.

I think that if there is a weakness, the weakness is that there's not enough brownie points, for lack of a better word, given to contributions made to the organization. You get a fellow like Jake Rice who....You know him. You've talked to him so I can use his name as a type example. A guy who is a program head. Who is chairman of this and chairman of that. He's a super chairman. He's got a broad spectrum of interests. So he's doing all sorts of things of value to the organization and maybe not publishing as many papers as he would like.

So that, I think, is a weakness. You do get an imbalance. The guy who's giving himself to the organization and his papers are suffering.

Now the way it works is that there are basically four levels. The RES 1 level, which is the Research Scientist One level, is the recruitment level. A young Ph.D. You have to have a Ph.D or the equivalent to get into the RES scale. So the young Ph.D coming in would normally be an RES 1. And if he's publishing reasonably during the first two or three years it's almost automatic, three or four years, that he moves up into the RES 2 scale.

And the RES 2 scale....Most young scientists don't recognize this. They don't think about it. But the RES 2 scale is figured as the scale that most...the average scientist will reach the top of. And not everyone will go on to the three or the four scales. Approximately 60 per cent of the population of research scientists are in RES 1 and 2. (You should get a copy of the regulations because I cannot remember the precise figures.

In order to get up to the RES 3 scale, which is the next level up, you've got to have a very good publication record. It's only 32 per cent of the total population of research scientists in Canada can achieve that scale. So you know there's competition to get there. And the competition is extremely vigorous! It is! So that you have to have, number one, a good publication record and, number two, usually you have to have something else like chairmanship of something or you're really top of your field in something, in order to get into that scale.

And then the fourth scale, which is only five per cent of the research scientists' population in Canada, is the top scale. And that is reserved, really, for people who are the best. The Rickers and people like that become RES 4s. In Newfoundland we have, I think, one. In Nova Scotia there are possibly two or three.

Q: Who's the one here in Newfoundland?

A: Al Pinhorn. He made a tremendous contribution to the ICNAF scene. He has a big publication record but that was his big contribution. He was sort of one of the major...major players, I suppose in the ICNAF and the formation of NAFO...the final years of ICNAF and the formation of NAFO. He was chairman of the research committee and etc., etc., etc. So he's the only one. And that was after 25 years of publishing and doing things....So that's the system.

Now the average Research Scientist 1 who comes in doesn't think of the system. Doesn't think that some of these guys who are at the top of RES 2, their only way of getting further is to get some of these papers out that they've been collecting the data for for years! That they've tried to write up and they just don't get a chance!

So there is a conflict there. But my advice to any young person who's coming in that has got ideas is to do joint papers! I mean anyone....And Ram Myers is a good example. Almost every paper that he's done is a joint paper with someone! He applies his techniques and uses someone else's data and assembles a joint paper. And both people get the credit for it then. Maybe not as much as the first person who is usually RAm. But his publication record is superlative!

Q: And yet there are still, from what I understand, echoes of hostility bouncing around the walls of DFO as a result of his contributions to the ALverson Commission.

A: There may be hostility. I don't know. But I expect some hostility. Some of it's plain jealousy!

Q: Because he's the one who did the reanalysis of plotting growth rates to population density. Or did Scott Aikenhead do that with him?

A: I don't know. I've been gone for three years and I'm not right up to date.

Q: But this was back in '86.

A: Well, I left just before the Alverson group came on, I guess. And I don't know who did the work but I expect some jealousy.

Q: From what I understand, this paper was the first suggestion that the data...or the extrapolations and the conclusions reached from the data about abundance and growth rates...were seriously flawed. That, in fact, growth, in terms of total biomass, had not been as great as hoped for and predicted. And that very simple things such as the fact that cod in large numbers grow more slowly than cod in small numbers....That they had been projecting growth rates based upon growth rates observed during the depleted years of the early and mid-'seventies. And as the stock rebuilt, growth rates tailed off as the population density increased and this led to a serious revision in the estimation of the total spawning biomass. Are you familiar with this?

A: No. I'm not familiar with that. I haven't made a point of keeping up with the 2J3KL stuff which is what this was. But it doesn't surprise me that there was a little bit of enmity there or rub' ~ the wrong way.

We had a scientist who came on staff while I was director...what was his name now...who made it almost his sole work in the lab to criticise the work that was being done. And that wasn't popular. Because it was destructive criticism. It wasn't constructive. Nevertheless, it fulfilled a function. Certainly I took exception to one or two of the papers that he published. But I would never stop him publishing. There was one of them which never should have left his desk and I am sure he is now sorry that we did not stop it. But in science you've got to have both sides. If someone feels they're being overly criticised, it's up to them to use the scientific media to correct it.

Q: I think that this may be one of the reasons that DFO science has come in for such a public slagging recently, is that the general public and the consumers of DFO scientific output, the corporate sector and the political sector, don't understand how science works. It does proceed by disproof. It is a probabilistic...

A: I think you're quite right. That's one of the things. The other thing is that they don't understand the basic facts of science. They don't understand how an assessment is done. They don't understand that in doing an assessment there are all kind of assumptions which are there. They don't think about all these things. And when our scientists are asked to make a prediction, they make a prediction with all kinds of caveats and "if" statements surrounding them. Probability statements. If this happens and this happens then something can be expected. But if something or other happens...and so on. And when this comes out in anything but a scientific journal, it comes out as a bare prediction. That this will happen! Never mind if, if, if, if, if! And, unfortunately, most of the trade and most of the non-scientific people, all they read is the final shortened version which says that this will happen. And it doesn't.

Q: In all you've talked about so far, there's....You've talked about stock assessment science in relation to values and norms, evaluative traditions that are internal to science. But DFO science exists to some extent...at least in the minds of the bureaucratic and political structure and the corporate sector...as a service industry. That's the public justification for the rather large amounts of public money that are expended on it. Then the question arises, if science can't provide us with knowledge of the degree of certainty we need....From the corporate point of view, they need to make five and ten year plans to construct and amortize plants and trawlers over a considerable period of time based upon the projections of what their allowed catches are going to be for that period. The political sector has to make management decisions based...They expect to be able to use science as the legitimizing or justifying ground for their decisions. And if it's unreliable or unpredictable then they are in trouble. Is there any recognition of this within the scientific community and, if so, how do the scientists feel about it?

A: There is certainly very strong recognition of the basic fact that we're a service, amongst the administrative side of it because we are continually having to justify this, that and the other thing in order to get funding. As far as the scientists are concerned...You've talked to Larry Coady and Mac so you've gone through our review system, right?

Our review system try to bring our scientists....Well, one of the things it does, it brings scientists into contact with the fact that we are a service organization. That we've got certain things that we've got to do.

And my own guideline, as a director and as a research manager, was always that we try to spend approximately 80 per cent of our time on the service function. Research towards service. And there's a group of 20 per cent...and this figure varies....I mean there's some scientists you'd let them go much higher than 20 per cent. But on an average, 20 per cent of the time is devoted to things that are "may pay offs." Real research. They're not the things that we have to do every day to provide our assessments. To provide our projections. Nor are they things which are keyed to just improving our techniques. They are research lines which are interesting. Which may pay off or may not. We don't know. I think much of the work that Ram's doing is of this type. Not all of it. I think he spends 70 per cent of his time in straight service work. Service to others. But a good scientist should be able to spend at least 20 per cent of his time on long-term work which may or may not pay off. In addition to the service.

So I think a director has to recognize that. You won't need scientists just to do the service work. Scientists have to have more than just the service. Especially when, if you are a stock assessment scientist who has to produce his stock assessment twice a year....I mean, it's a relatively mundane job. Reading your otoliths. Getting your age distributions. Getting your weight/length curves and all the things you need for the stock assessment. You've got to have that extra 20 per cent to follow up lines that look interesting and to do the other things.

Nevertheless, I think that the service side of it....I think that everybody in St. John's recognizes the service side of it because of the review system that we have that forces that.

Q: Isn't there a potential conflict here between the political masters and the corporate consumers of scientific knowledge and the evaluative traditions that you've outlined that lead to promotion?

A: We have the evaluation which is a scientific evaluation for promotion which is a separate exercise to the annual review process. The annual review process is one which the industry is invited to. Other scientists are invited to from the other regions. The senior people in the people in the department are invited to. The consultants who are in different, allied fields are invited to. And the academic crowd are invited to who care to come. And this is an attempt to show these various people that what we are doing is, in fact, aimed at helping them.

And always a big part of that session, especially the one with industry, a big part of it is, "Why aren't you doing this which would help us?" And you can say, usually, that you are doing some of that but you're not doing maybe as much as they would like. And you then ask the question, "Well, what part of the work that we are doing should we stop doing in order to transfer our efforts to do this job?"

And that they can never answer because all they want is more and more and more and more. And we have to put priorities on it and they just realize then that we can't do everything. I think there's an attempt there to do what industry wants.

I think where we fall down, and where we've always fallen down, is in translating science to industrial terms. We'll never get that right. Partly because the way we say things is not what industry wants because we always have these conditional clauses that surround everything that we say. Industry doesn't like that. Industry, however, blames us for not providing the stuff, the predictions that they would like to have to allow them to expand their fleets or to do other things. Because our science is full of uncertainty.

Nevertheless, I think that the economic uncertainties of life are far, far greater than the science uncertainties! The uncertainties that surrounded the fishing industry when the fuel price went up, the last fuel crisis, the last round of trouble in the gulf, was a far bigger impact than anything to do with stocks. And that's still part of their problem now. The economic side of it as opposed to the actual stock size.

Q: There have been two major crisis in the 'eighties. The first, in the early 'eighties was the one that spawned the Kirby commission. That crisis was essentially an economic crisis rather than a stock crisis. Or at least it was perceived that way.

A: There were stock overtones to that as well. In actual fact, there's been more crises. Every six years there's a crisis in the fishery, is the accepted period that you hear time and time again. This is another sixth-year crisis sort of thing. In the 'seventies there was a stock crisis. Under the NAFO or ICNAF regime all the stocks were extremely depressed. There's no doubt about it. There was a stock crisis and Canada was really worried about it and that's what brought in their position at Law of the Sea and gave us control as a costal State.

Once we got control as a coastal State, I think there were two main things that happened. One is that our aspirations were far too high. I mean what people did in thinking and planning, what the industry did, was say we've had X number of large stern trawlers fishing on the Grand Banks for the past few years; we can take that over now. Of course, forgetting that the large number had whacked the stocks down to practically nothing. So they failed to put the thing in perspective inasmuch as we had to rebuild. And, you know, I think they just got too big of an expectations. So that was one thing.

And the other thing was the economic crisis that came in about '75, '76 which was oil mainly.

Q: But in the Kirby report, presumably based on scientific projections, they were projecting TACs by now of 450,000....

A: 450,000 tonnes for 2J3KL, right.

Q: And based on these projections, industry geared up for larger catches; more plants, more boats....

A: They didn't build more plants. They were about 200 per cent over-capacity then and now I think they're about the same.

Q: But certainly you understand the drift. These projections created what turned out to be wildly unrealistic expectations. Because it appears now, with the reassessment in '87 I think it was, that the 2J3KL stocks have not grown nearly as fast as predicted back then. In fact there are some, including Jake now, who feel that the growth rate over the last two or three years has been flat and possibly even slightly negative. And I think that this realization, this reevaluation, precipitated the current crisis. But you say you haven't kept up on....

A: No, I haven't kept up that much on 2J3KL I must admit. But I think it probably has precipitated the current crisis. The forecasts were not good. There's no doubt about it. They weren't good.

Q: Do you think there's any possibility of doing good forecasts given the....

A: Not in the long term. I don't think there is in the long term. To me, the key thing is recruitment. And recruitment is variable. We can't predict recruitment! Until we can predict recruitment, and I rather suspect that we will never be able to predict recruitment, our predictions are always going to be relatively crude. The predictions are probably the best we can do in terms of at least not allowing fisheries to slide down hill. But we're not going to squeeze MSYs out of them! Ever! And I think it's kidding ourselves and industry is kidding itself if they ever think that we will.

The whole management system has to be tailored to whatever recruitment there is. Biological systems are not static, change is a fundamental fact of life, and one of the necessary arts of fisheries science is to recognise such changes in a timely manner,to try to understand them and to build them into the models that we use. There are,however, changes which we cannot understand.

When I first joined the station, this is one of the sort of anecdotal things, everybody but everybody when they started reading ages was given Labrador cod--2J3KL otoliths to read. Because the were so regular, so clear. It was just a tremendous introduction. You could count the rings and know the age and it was all very clear. There were odd occasions when there was a check or something and you could see the check in every one of them because everything was so clear. And it was a great learning tool.

You look at 2J3KL otoliths now and they're totally different! They're not clear like that! There's been some sort of ecological change! I mean it's nothing to do with fishing. It's just...they're different! There's no regularity to it. Something changed. I don't know that anyone's looked at it. But that's the sort of thing that....Life is different!

Q: The bureaucratic structure of DFO was established in light of post-'77 expectations. That with Canadian control and good scientific management that the stocks could be rationally managed. That forecasts could be made.

A: Not necessarily. It depends on what you mean by forecasts. Forecasts can be made....

Q: That stability could be brought to the industry.

A: No! No one has ever, ever said that stability can be brought to the industry! I don't believe that.

Q: Not from the scientific point of view. But perhaps from....

A: Where you've got variable recruitment you can't have stability!

Q: Right! But I believe from my readings and research that this was the expectation from the corporate and political sectors....

A: I think that's probably true, yes.

Q: And a lot of the criticism that's coming from these to sectors now against science is a result of them being disabused of this notion. Having to face facts. And they're saying, we've spent millions and millions and millions of dollars on science which is of no apparent practical use for our needs. Our political needs or our corporate planning needs.

A: That, of course is the question. Because if you look at the stocks compared to '72 when this started, they're all way up! They've been built up! They're not continuing to be built up perhaps as well as we'd hoped....

Q: And yet these lads have just had their quotas slashed drastically as a result of what looks to them like scientific error! Screw ups! So they're not fishing for 450,000 metric tonnes this year, [as predicted by DFO in the 1983] they're fishing for 196,000 with the scientific advice saying that we got it so wrong that we think that you really should be fishing for only 125,000 metric tonnes this year.

This is a shock to them. And it causes them to say, "If you boys can't get it any better than that, why should we keep forking over tens and hundreds of millions of dollars to you?" This is their perspective, not mine.

A: I can see their perspective, I must admit. I can see their perspective and it's a hard one to answer because they don't appreciate the fact that it's not an exact science. That there are errors. But it's just as exact as any of their other economic forecasts.

Q: I see a danger that this external criticism of science has the potential of becoming a self-fulfilling prophecy. That good scientists like Jake who don't care to deal any more with this level of hostility, operate in this kind of environment, are bailing out.

A: No doubt about it. That's the truth.

Q: So, in fact, whereas in the late 'seventies and early to mid-'eighties you were assembling an internationally respected top-notch team of scientists there, now the ones who are any good, who have any options are bailing out and heading somewhere else that's less fractious, less unrequited.

A: Well, I know you're right and I don't know what you can do about it. The fact is that no scientist wants to be continually in the front line as far as the press is concerned. It's not what science wants. And, in a way, I think it's wrong that our scientists are being put in that position. It should be the directors and the administrators who are doing that job.

Q: Which is exactly why some of the scientists are leaving. Because they don't feel that they are being...that the bureaucratic structure above them is not willing to support and protect them.

A: I think that might well be true. I think that's one of the key things of a director's position is that you have to protect your scientists. Now many of the scientists don't like it. They want to get involved in other things. They want to get involved with fishermen's groups. But you know damn well that as soon as they do, number one, they'll do less science. Because they don't want to appear at a group without being well prepared. So, not only do you have your meeting but you have probably the week before in preparation for it. It could be minor stuff. It could be major stuff.

It could be research which is useful. I don't know. But usually there's some that is unproductive, just getting ready to meet the fishermen.

So there's that side of it. And the other side of it is the press. I don' think any scientist should have to face the press! That's just not part of their job! And, again, I know a director can get in trouble with his scientists by not allowing his scientists to face the press. Some love to do that! But you've got to recognize the people who want to do it and who can do it productively without interfering with their research. It's a key thing of a director's job. Protect your scientists from the press and other influences which are likely to have a negative impact on them and their science.

Q: But if there's no support up through the ADM and into the Minister's office for a director doing that, which it appears that there's not now, the director will fall into a defensive posture as well. There's reason to speculate that Mac either jumped or was pushed. In either case, he probably left unwillingly. Somebody had to take the fall...be seen to take the fall for the criticism in the Harris report. And a month or two latter, there goes Mac. You can draw your own conclusions.

A: Yeah. It was an unfortunate similarity of timing. I don't`know what the conclusions are because one person did take it in Ottawa and that was Bill Doubleday. I mean, he was the one who was the fall guy. It didn't need to come down any further. He was ADM Science...Acting ADM Science...and he got relieved of that job.

Q: But that didn't get the play, the press play, here in Newfoundland, and Newfoundland is the focus of the criticism from the larger population....

A: It's not, you know! The criticism in Nova Scotia is just as much, if not way higher. Because they...there are many more vocal fishermen involved. It's not just the big companies. Many more of them own their own boats.

Q: So there's a lager percentage of independent operators in Nova Scotia?

A: That's right. Not plants but boat operators. And they're very vocal. They're well-educated. And they're prepared to shout. So it's not just Newfoundland. But that's beside the point. I take your point.

It is unfortunate if that really was the case but we have to recognise that it is the way that government works, and indeed is what the public demands. The approach of Ottawa, DFO, Ottawa, to anything to do with the press always seems to have been...unless it's a ministerial statement... say nothing and it will be forgotten in a few days time. That's always been their approach. And the whole business of the controversial 2J3KL assessments....

Our scientists were right up in arms a year or two ago when the Alverson report basically exonerated them and the methodology that they used but the press and the comments of the general public focused almost entirely on the more negative aspects in the report. The silence that emanated from Ottawa was deafening.

Q: So their feelings that, in times of controversy, they are abandoned by Ottawa are justified?

A: I think it's justified, yes! In the same way, we have seen that, as soon as any question comes up which has unpleasant consequences, say down-sizing of a quota, the decision makers (who are not the scientists) usually take every opportunity to "protect" themselves using such statements as, "Well, that's what the scientists tell us." However, if the quota is to be raised, somehow the message seems to come through that it is by their (the managers) diligence that this is happening.

Quota decisions are not scientific decisions. They are socio-economic-political in nature. The scientists give their advice and this is blended through an involved consultation process with other advice from the industry and from the socio and political arena before a decision is made. And yet, when the news is bad the answer always seems to be, "Oh, that's what the scientists tell us!" It's just not true! But that's the easy way out and we've always kept quiet about it.

Q: The system, as it's structured, not only doesn't encourage but doesn't permit sub-sets of the department, in this case the scientific units, from speaking independently in the public arena. There's a heavy disincentive for Canadian civil servants....

A: That's true. I don't know how you can get over that. It's not just Canadian civil servants. Any civil servant cannot criticise the government in power at least in matters concerning their own departments. If you start criticising the policy of your own department, the whole structure of government system falls apart. You've got to have that, and not only in government, for it applies in industry as well. He who pays the piper must call the tune.

We are usually accountable to our employers and it is really only in the sheltered cloisters of academia that scientists or others have the freedom to say what they like. Of course, with this freedom there is an unwritten understanding that the academics will not abuse their position of knowledge and trust with irresponsible statements. With the increasing power of the mass media, this abuse is unfortunately more prevalent and more and more academics seem to be prone to pontificate on matters about which they know little and are not qualified to speak.

Thus, while the government scientists are to some extent muzzled relative to matters of Government policy, they are not restricted in matters of scientific fact, except that they are accountable for what they say and this may be held against them at some future time. The academic on the other hand can quite easily abuse his or her position of public trust, and utter nonsense with impunity knowing that few will criticise it and in any case it will be forgotten in no time at all.

Q: I'm reminded of an incident last year, I think it was last fall, where a group of Canadian lobster biologists were prevented from going to a meeting in the States, an academic meeting, because the conflict between Canadian and American minimum size regulations was under negotiation at the political level, and they were order not to attend.

A: I don't know the circumstances of that. That may or may not be the real reason. The fact is that, when it comes to meetings, our scientific staff, any scientific staff, have problems because of questions in the House. "Why was it necessary that 15 people went to Britain or to Denmark to the ICES meeting? Fifteen! Fifteen people!" You know, the political approach. And yet the ICES meeting is probably one of the three or four meetings which are valuable to people in fisheries. There's maybe five altogether. And you've got a staff of 200. Fifteen is nothing.

But we're always faced with that sort of question. And there are always restrictions on the number of people attending meetings just because of political questions in the House. Why was it necessary? How much money are we spending on people flying around to meetings?

The public perception is that when scientists go to meetings, it's the same as the Lions Club going to a Lions convention. That it's a big wing-ding. And it's very hard to live that perception down.

So there are reasons given. Sometimes it's because of questions in the House. Sometimes it's because of a big push to save money. Some political reason or other. All conferences are cut out unless they're absolutely essential. there are many reasons like that.

Be that as it may it is also quite possible that the real reason was that negotiations were at a particulary sensitive stage, and the last thing the government wanted was to have to take a defensive posture or even change their approach because of a statement made by one of their own government scientists.

The fact is that negotiations usually involve a certain amount of choosing and even slanting the facts to suit a particular line of argument, argument, and they are particularity sensitive to counter arguments or even an emphasis on different facts by persons who can be considered to have knowledge or opinions which can be attributed to representing one or other side in the negotiation. Now which was the true reason in this example I don't know, but I would guess it was a combination of circumstances that gave rise the decision.

Q: It seems to me that another thing that happened, at a very unfortunate period of time, was that, as the science of fisheries stock assessment advanced from a fairly young science, it entered a phase in the 'eighties where you began to understand what the sources of error were, what the levels of uncertainty were that you were dealing with. Beginning to quantify, for the first time, how much you didn't know. How big the job really was. It became apparent that this wasn't a simple job. As they made the transition from single-species models to multi-species models. From simple production models to more sophisticated models. That is when it became obvious that there were huge unknowns. Hugely variable inputs to the stock size. That this was a much,much bigger job than anyone had ever guessed. Much more difficult. You'd got pretty good at retrospective population analysis but as far as what's out there now, the best guess seems to be that you are dealing with levels of uncertainty with the final assessment number of 20 to 30 per cent one way or the other. And as far as forecasting goes, that may be, as we've discussed, nigh on impossible.

A: I think all that was recognized. I think it may not have been recognized by the bureaucrats and that's always one of the problems. They didn't realize how big and complex a problem it was. I think the scientists realized how big a problem it was because we were talking about the problems of multi-species and about the problems of single-species as well. Single species just bringing in certain interrelationships. The food and feeding relationships rather than bringing in the whole complex of multi-species management. But just thinking of cod in relation to caplin for instance. These are the sorts of things we were addressing in the late 'seventies, middle 'seventies. I think we realized how involved the subject was.

You have to realize too, that until 1982...no, until 1978...all we had was one vessel that was capable of off-shore research in the whole of the Canadian North Atlantic! The A.T Cameron. That vessel was shared between the mainland and ourselves. But until we got the Gadus Atlantica, which was in 1978, that's all we had to cover the offshore area from the Arctic to the Gulf of Maine. There were two small inshore vessels as well which were useful work on inshore problems such as work on herring and caplin and tagging of groundfish in the inshore area. But basically, the key vessel was that one vessel. And it wasn't until the extension of jurisdiction that we got that second vessel. And after that we got a replacement for the Cameron, the Wilfred Templeman. And in addition, the Alfred Needler for the mainland. Lack of research vessels and an offshore capability was a fundamental constraint on the development of fisheries research in Newfoundland.

Q: That brings up a point that Larry raised which I thought was very interesting. He says fisheries science is the only natural resource science that he knows of that was theory-driven rather than data-driven. His point being that things like forestry and mining science were based upon an accumulated fund of data. That they made a transition from old foresters and prospectors data but that fisheries science was theoretically well-developed in advance of much of the data and that the theory was largely derived from economic models.

A: I don't agree with that sentiment. I think that there is some truth in the statement in the North American context. But much of the theory was developed on the history of the European fisheries in the North Sea for instance, where concern about overfishing was being voiced in the 1920s and where a tradition for the keeping useful international statistics and maintaining coordinated fisheries research was in place in the 19th century and was put on a formal basis with the founding of ICES in 1904 or thereabouts. It was only really after the war that the Northwest Atlantic fisheries started to expand, and in recognition of the lessons learned in the prewar fisheries of europe and the

North Sea, Canada pushed very hard to establish an international forum for monitoring the expanded fisheries in the area and instituting a means avoiding the overfishing that had occurred in Europe. Thus it was that ICNAF came to be.

Q: I think Larry was talking about the northwest Atlantic...

A: In the Northwest Atlantic, the theory was there in '53...

Q: That the theory was there but that it took a long time for the theory to be modified in light of accumulated data.

A: In our area there's no doubt about it. In the 'fifties all we were doing was exploratory fishing. That's basically what we were doing. Exploratory fishing and trying to understand the basic biology and distribution. Why they were where they were. Where they spawned. Their growth rates. What they fed on. All these things. That only started in 1950, really. There was a little bit of work done in the 'thirties. There was a little done in the 'forties. Only on inshore species. And then when we got the Investigator in 1946, that's when modern fisheries research really started in Newfoundland. It was very late!

Q: Let's go back a bit to the discussion we were having about the demands form the consumers of scientific knowledge to participate. Especially from the inshore, there's a litany of criticism. That science doesn't listen to our knowledge. That they don't value our knowledge. The inshore crowd feels pretty ignored. And then let's couple this with your observation that the scientists who attempt to address these issues, these concerns and attempt to participate more fully with the fishermen, become less than optimally productive as scientists. I'd be interested in you thoughts on this general subject.

A: Well, you've got several questions there, though they are all related. There is a fundamental reason why, to a large extent, we ignored the inshore cod fishery. The reason being that it was an extremely difficult to study.

We did not ignore the more locally distributed species such as lobsters (there has been continuing research on this species since the 1930s) or even herring but in the case of cod which was a migratory species, the fishery around the coast varied according to the type of shoreline, the bathometry of the inshore area, the geographical position on the coast the local practices of the fishery on top of the numerous variables which can control the varying production in the stocks and their migratory patterns.

In effect the additional variability was such that for meaningful estimates of stock abundance you had to study the whole coast of Newfoundland. That's a very large area. Whereas if you leave it until the fall/winter period and you do the work offshore, with the vessels that we now have, you can at least get you estimates of abundance within some sort of error bars that are at least acceptable. But to do that within the inshore area is an impossible task!

So we tended to downplay the inshore area. It was just too big an area to cover with the people that we had. When the fish went offshore into concentrations, we could much better devote our time on those concentrations. So you're quite right. We did ignore that inshore area to a large extent.

Now, the other part of it is the potential knowledge to be gained from inshore fishermen. We continually get blamed, for not using this fund of knowledge. I have some very definite views on this which are not necessarily supported by my colleagues and which I am sure differ from those who pursue an anthropology or sociological bent.

I think the inshore fisherman has very little to contribute to the solution of the fundamental problems of stock assessment and science in support of fisheries management. There are a few exceptions. There are a few fishermen who think, and see beyond the bounds of their local interests, but the comments of the vast majority are self serving and extremely restricted in geographical range.

If one is studying the distribution and movements of the fish in a local area then one would be wise to use their local knowledge, but at scale of a fish stock, so much work is required to separate the hay from the chaff that it is probably better to take a more objective approach from the very start.

For the most part the majority of them have a litany of mumbo jumbo which they bring forth each time you talk to them. About where the fish are and why they're not here. they relate it to things like the berries on the trees. Sometimes observations of that sort have some value such as " when the wind is such and such a way you get catches". That's acceptable.

When I was going around trying to understand a bit more about Newfoundland and the fishery, I just got completely turned off by inshore fishermen and their views. Because they were totally unscientific! And you'd try to get them to approach it from a scientific viewpoint and they would say, yes, they'd be happy to help. But in many cases they couldn't write, in the old days, they can now, so they couldn't keep a log book for you. You'd pick the one or two better ones and they might keep a log book for so long and then they'd say, "B'ye it's just too much trouble! I just can't help you any more. Sorry." It was just banging your head on a brick wall. So I tended to downplay inshore fishermen as being useful to the scientific process.

There are some who are different. I worked on shrimps for a time which tends to be inshore fishermen in bigger boats. Most of these guys are the best inshore fishermen. Because they're the ones who have the gumption to get the boats somehow. They're not content just to go out to set the trap the same place his father set it before and if the fish don't come, complain. And I certainly got on very well with most of these guys. They were prepared to think a bit.

They still didn't read and write, many of them. And that made it difficult to communicate by writing. Writing is so important. Very little of what we do is spoken. It's all writing. But on the whole they were they best and I could get on with them and I could work with them and I found it valuable. And they helped me a lot. But the average inshore fisherman, no b'ye, I just don't think so.

And I think they're being exploited right now by people like Cabot Martin. He's only got one real reason for it. He's going into the political arena before very long. And that's his way of getting there. And he's drumming up all sorts of hoohah one way and the other. He's always been a difficult person to get along with. If he had his way, he'd have our management system the same as in the States. A thought which absolutely appals me because I think their management system stinks. Ours has got its faults but theirs stinks to high heaven.

Q: What are the differences?

A: Well, the differences are that all the decisions are made by the fishermen. And the fishermen make decisions for today, not for five year's time. So every stock in the States is right down at the very bottom that it could be.

Q: You're referring to the regional management system?

A: Yes, the regional management groups.

Q: With more or less perpetual reassessment with input from all interest groups but which tends to be dominated by the fishing industry.

A: By the fishing industry and they make the decisions. I mean look at the groundfish in the States! It's abysmal!

The Georges Bank groundfish. There are a few success stories. There are some. Where you get a particulary dominant person coming out who can lead the meeting and, by political savvy or whatever, gets his own way. And his own way might be a five year or ten year future.

But a fisherman never, never, never, in spite of what conventional wisdom might say, thinks in any more than today! I say that dogmatically because I'm convinced of it. They will always raise the point that this is their existence. "Of course we are interested in conservation. It's our future!" But you give them the choice of the big catch today or the possibility of higher catches tomorrow or the future, they'll always take today. And I've seen that time and time again. And they'll lie in order to get today rather than tomorrow.

So I'm one of these people who, though I accept that we can gain a lot by using fishermen in the right mode, think that we have to be careful and selective in the types of things that we use them for. And, again, I think you've got to look at their leaders and the motives behind their leaders.

Q: Of course, I've talked to Cabot. I'm talking to everyone from all sides of this issue because I'm trying to be reasonably scientific...or at least, objective in my work. I'm trying to talk to as many voices on as many sides of the debate as possible.

A: Well, that's the right way to do it. No doubt about it. You'll get some outlandish views like some of mine and....

Q: There's an interesting debate between Cabot and Jake Rice on the issue of trawling on the spawning grounds. And it does seem to me from my assessment of the evidence that Cabot is exploiting an anthropomorphized emotional response. That Jake's evidence is far more reasonable. That, in fact, what's going to happen if there's a legal or bureaucratic response to this pressure is simply to move the fishery up into the pre-reproductive phase of the concentrations. Which will result in less fecundity, less spawning success because you'll be fishing directly on the pre-spawning concentrations. Fishing on the spawning concentrations, some of them are going to reproduce.

A: Many of them have already reproduced.

[digression on the Jake/Cabot debate]

A: They're talking about caplin now. NIFA is thinking about going...extending it toward caplin. And I would have thought that anyone who...if they ever were on shaky ground....The whole caplin fishery is on a spawning fishery. They're catching spawning females for the eggs. That's what they're catching. Cabot Martin! I've never seen eye-to-eye with him and I've had many arguments with him through the years.

Q: He can be quite strident.

A: If he's got an audience he will be. But he can sit down and be quite sensible if he's on his own. There's no doubt about it. He has a different approach when he doesn't have the audience.

Q: Perhaps to another issue....As a scientist, you may not want to comment on this...but the issue of transferring common fish stocks from the status of common property to private property. First of all, there's....The first step was the enterprise allocation quotas which transferred it into quasi-private property. This all devolves from the theory of the tragedy of the commons.

A: Right.

Q: Now, with the sale of the fish plants at Burgeo and Canso, where everyone involved in the transaction denies that the stocks or quotas are being sold. But why would anyone pay $12 million for a $2 million plant unless the quota went with it and was valued, right? What are your opinions on this and, further, what are the implications for the future of effective stock management? You needn't comment if....

A: No, I've got opinions on it. I'm not speaking as a scientist. I'm speaking as an individual. And I spent some time in my career working for various groups in Ottawa looking at common property resources and the way to manage and all these things.

I like the idea of privatizing the resource, so to speak. Giving people quotas that are a saleable item. That can be auctioned. They can realize the value for it. And they have to pay a tax to government on it as well. It's the people who catch the fish should be paying for the research and the other things. Not the general purse. So that if a fish plant is going to enter the field, it should have licenses. It should encourage private individuals who have licenses to provide fish to this particular plant.

Work out cooperative systems so that they have a fleet, either their own fleet or a private individual fleet is going to provide the fish. So that they get their fish on an enterprise allocation or some sort of system whereby private individuals or companies own the fish...the rights to fish. And rights to certain amounts of fish, on a sliding scale, depending on the stock size, etcetera.

No, I think that's right! The one thing I'd add to it is that there's a tax on it. That they should be able to auction it freely in the open market. But the government should receive an annual tax on the quota.

Q: But isn't there a possibility....At first gloss, it sounds quite good. That fisheries science service is user-funded through taxes.

A: Right. But they'd never be able to afford it.

Q: Now...Yes, there's that question. But on an more theoretical level, if a service is user-funded, doesn't that open the possibility of bias in the service in favour of the funding agency. One knows what the user wants. The user wants to hear that you can catch more fish. Or certainly, not less. And therefore, open a bigger hole for user pressure in the political process.

A: I think there's a possibility of that. But under our system, which is a benevolent dictatorship, as I describe it, with the Minister as the dictator, I think the possibility is far less than with many of the other systems of fisheries management that are in the world.

Q: But you have a concentrated corporate fishery which can marshall rather formidable economic resources. One can clearly see the possibility of....Well, it's already happening. We've got people like Jim Roche being parachuted in from Ottawa to take over the communications end. And he talks the classical Thatcherite litany which is the need to "rationalize" and "professionalize" the fishery. Which essential means to....That's a code phrase for killing the inshore and turning it all over to....

A: No, no! Not necessarily! I don't think....

Q: And when Newfoundlanders talk about the fishery they mean the inshore fishery.

A: Yes.

Q: The cultural identity of this place is so....It may be economically "irrational" granted. And it's a very small percentage of the GNP of Newfoundland now and it's shrinking every year. But it....Newfoundlanders' strongest shared cultural identity is with the outports and the fishing stages and the flakes, the trap skiffs. And you take that away from them, what else do they have? There's no "rational" reason that there should be anybody living on this rock in the first place!

A: But isn't that where the politics comes in? That stops the power groups and the lobbying groups etcetera, is the fact that the votes are in the outports? That's the balance I think.

Q: Yes. But that's exactly what introduces the...That's exactly the source of the most "irrational" of the criticism of DFO science and of fisheries management as it now exists. The "Cabot Martin factor" if you will. What Cabot's talking about....He isn't speaking "rationally." He knows that. He's speaking emotionally. His logic is based in culture and tradition.

A: I don't know exactly what you mean when you say Cabot Martin is speaking that way. A decision has been made, right or wrong, that the outports of Newfoundland are important to those who live there. And no matter what happens in terms of licensing in the offshore, those communities are going to be allowed to continue to exist and, hopefully, to maybe do better than they are. And that's why, in our management system, we have allowances for the inshore and quotas for the offshore. So there is flexibility there. And I think part of the flexibility is that if, in fact, in any given year the allowances to the inshore aren't used because the fish never came in, which is one of the problems of the inshore fishery, the reallocations can be made to the offshore fishery or other inshore fisheries. So I think that while that is there, its' a principal of fisheries management.

It was certainly announced as one of the principals of the management system. If it's changed, then there's a political upheaval and all sorts of hoohah in the press by the inshore fishermen. And rightly so. They have problems. Sometimes because the fish don't come in. Sometimes because the stocks are low. Many other things. But they're always going to have problems in some area of Newfoundland in the inshore fishery. Historically there've always been areas where the fish don't come in. So there's always going to be one group of fishermen in trouble.

The problem in the press comes up where you have a spokesman like Martin that always takes the one place or the dozen places it's bad and blows it up out of proportion. Whereas this year, the fishermen in Torbay have never, never, never done as well as they have this year!

Q: But on the east side of the Avalon, in Bauline and Portugal Cove, it was a bad year.

A: Yes, a bad year. So Cabot Martin will be speaking, "The stocks are down! The poor fishermen of Bauline!" So, you know,....

Q: But one of my sources made an interesting point. He said that up until the growth of the offshore trawler industry in the 'sixties and 'seventies, that the inshore fishermen knew that there would be good years and bad years. There'd always been good years and bad years. They didn't know why but they knew that the fish would come back. They always had. They knew this too. It was an act of nature, an act of God, whatever. But this person's analysis was that, for the first time in the history of the inshore fishery, that in the early 'eighties it began to dawn on them that there was another, new factor at play here. And that was the offshore. That we had the potential to fish the stocks down so hard that they might never come back. That it was no longer an act of God or an act of nature whether they came in or not. That it had a lot to do with how heavy the offshore, the draggers fished. And this realization was the point at which the inshore became radicalized and activist. Before that they'd said, "Yes, they come and go. But they'll always come back." Now they realized that they could go and never come back.

A: Well, I think some of that was political leadership of course. Some of that was the Smallwood policy of get the fishermen out of the boats. A lot of them did get out of the boats and the number of fishermen dropped quite dramatically. It went down to something like 11,000 I think. I don't remember the year now but I guess it was 'round the 'seventies, sometime in the 'seventies. It's now up around 35,000 again or some figure like that. So it's a three-fold increase.

Now that three-fold increase is not the old timers. That is mostly young people who can't get a job anywhere else. Because of this they no longer have any corporate memory. There are few old timers left in this new industry and the "facts" of years gone-by and the historical record handed down through the years of the way the fishery was has been lost. You became a fishermen at ten and you listened to the old timers talking until you became an old timer yourself. The lore was passed on. And that's some of what I was talking about earlier. Some of the lore was nonsense, but years of success and failure were remembered and the fact that there were good years and bad years was one of the things that the old timers all recognized.

I think when you got this big expansion, three-fold expansion, most of which were young people coming in who had no historical reference to look at, they started to think about other things. What is the cause?

And the point I always make about recruitment....Recruitment is the one thing we can't predict in any way at all. It's vastly variable. People can understand that the dogberries are good one year but the next year they're terrible. They understand that because they can see it. But somehow we've never been able to get through to fishermen and others that recruitment of fish is the same! There are good years and there are terrible years.

Now, another thing that I think's helped that is the squid. Squid used to have their ups and downs but they never were longer than three years. Three years without the squid was a very rare event historically. Now, we've reached six years. Five or six years. I'm not sure of the exact time. And that, again, is part of the lore saying well, the squid seem to have gone. Maybe our fish will go too. And, when there's a quota, if you can blame someone else and get their section of a quota, that's good politics. And that's what Cabot is doing to some extent. So you try to push the things that will help you.

Q: But isn't it a fact now that, if you have a particularly tragic congruence of serious scientific errors, bad management judgements and factor in the fishing power of the offshore fleet, in a year of two it is possible to fish the stock down....

A: Fish the stock down but not to extinction!

Q: Not to biological extinction but to a point where, given the growth rates of cod...three to five years to recruitment, five to seven years to sexual maturity...there could be eight, ten years or more of real famine.

A: There could be. But there could be at any time even if there's no fishery! And there was in historical times! It always comes back to recruitment. You look at the historical data and you can see failures for ten years. The thing just going down, down, down. There was no new recruitment for a ten year period and of course the stocks went way down without that. But they built up again. And we've had periods in recent history where there's been failures of five years.

Q: The difference is, yes, these events happened due to fluctuations in ocean climate conditions or whatever. But that was with a very different gear technology. Up until very recently we had hook and line technology and no electronics.....

A: That may be so but you can look at other stocks like the North Sea stock. They've been dragging it and dragging it and dragging it and they take the fish that big! [holds hands about a foot apart] And yet it is still one of the most productive stocks there is! recruitment happens to have been good all the time. And the stock has never collapsed, so to speak. The North Sea one. The Arcto-Norwegian stock is way down but the North Sea one has been fished and fished and fished and it is still providing a large catch every year. Mind you, they're tiny fish. Recruitment is what it comes down to!

Q: Recruitment and....That's a relatively benign sea as I understand. It's very shallow. It gets a lot of solar energy. It's a rich environment.

A: It's a very productive sea. There's no doubt about it.

Q: But the northwest Atlantic, particularly off Labrador in the 2J3KL area is certainly much less productive.

A: No doubt about it. But it still comes down to recruitment! No recruitment, no fish. We can try and manage and we can try and do things the best we can but if we don't get recruitment, we're screwed!

Q: So that argues for very, very conservative management.

A: It doesn't necessarily.

Q: No?

A: No. Herring is being managed....

Q: I am speaking of cod, not herring which are relatively more fecund and shorter-lived. They have shorter cycles.

A: Shorter cycles but not all that shorter. Relative to 2J3KL cod I guess they are but relative to 3NO cod, south coast herring or Gulf herring....Well, it's clearly something that more work needs done on. But that's another of the big gaps is the whole business o understanding survival from the egg stage to when they settle on the bottom. We know nothing! Absolutely nothing!

Q: As Jake put it, the average female cod might expel, over the course of the breeding season in several events, two million to three million eggs. And the difference between a bust year and a boom year is two of those eggs recruiting to the fishery or eight. And you think about that in terms of odds. One in a million versus four in a million. The difference is so slight. You think about this statistically and it seems hopeless for us to be able to understand, much less predict the influence of the variables or manage them.

A: We can't do anything about it and we'll never be able to predict it I don't think. But I suppose that there are things that we can do. We can do a bit more on...and Jake has firm opinions on the fact that we're not affecting the spawning stock by fishing it.

But certainly, I think that one of the things that Cabot does have in his favour is that we should have done some work on it. We should know something about it. We shouldn't be relying entirely on other people's work. We shouldn't be relying entirely on the historical side of it. There are some direct things we could do and we should have done. We didn't do them, partly, because we didn't have vessels. I think we identified them as problems.

People were worried about this back in the 'fifties because back in the 'fifties was when the first winter fishing started. The questions came up. With one vessel what could we do? We did plan one program, I think it was back in the 'sixties, trying to do some sort of ecological survey of survival of eggs and larva and drift rates and things like that. But it was just a drop in the bucket. And that's a big, complicated subject.

Q: And yet absolutely critical. But it doesn't matter how critical; it is if it's not doable given available resources.

A: That's right. We don't have the resources. We've got to put our eggs where we can get answers. That's got to be one of the criteria of the work that's to be done is, is it reasonable to expect an answer form it? Is the work worth doing to get an answer?

Q: [making polite noises to wind up the interview and soliciting recommendations as to other people I should talk to]

A: [re: Art May] He'd be well worth listening to . He has the spectrum from the science to the Deputy Minister. He'd be one of the most valuable people you could talk to. And he was involved in the Kirby task force as well.

[a few words about Templeman]

It would have been nice if you could have talked to Dick Wells before he died. He was one of the custodians of data that everybody complained about. I think you might get a different perspective by talking to him. Tom Pitt retired about four years ago and he was another of the custodians of data. He worked with flatfish. Wrote a lot of papers but he was more the old style. And certainly he was one of the ones that people complained about holding all the data.