

## IS THERE HOPE FOR FISHERIES MANAGEMENT?

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### ABSTRACT

Fisheries is not the only discipline where models have been used in attempts to fine tune an aspect of the economy. Such fine tuning can prove ineffective because of the uncertainties in the scientific underpinnings of the models and because of the omission of critical elements. In fisheries, the biological goal is to set allowable catches so that the harvest is not so large that it endangers the future health of the fish stock while it is not so low as to waste food, while the economic goal is to maximize the net economic rent generated by the fishery. It has long been recognized that the science underlying the setting of the total allowable catch is often too uncertain to justify such fine tuning, and that attempts to achieve that delicate balance has helped lead to crises in fisheries. One solution is to abandon such marginalism in favor of seriously reducing current catches.

### Empirical Models

John Maynard Keynes. Remember him? Writing at the depth of the Great Depression, he developed a new theory of business cycles (Keynes 1936). National income accounts had, meanwhile, been developed during the 1920s and 1930s (Kuznets 1934). The combination of these events led to an empirical revolution in economics: macro (or business cycle) theories had to be tested with real world data. Abstract economic thinking had to be augmented by empirical verification. An early major study was by Jan Tinbergen who, in 1939, published a statistical study of business cycles in the United States (Tinbergen 1939).

Keynes was not happy with Tinbergen's work. He noted that "The coefficients ... are apparently assumed to be constant for 10 years or for a longer period. Yet, surely we know that they are not constant." "Is it assumed that the factors investigated are comprehensive and that they are not merely a partial selection out of all the factors at work? How much difference does it make to the method if they are not comprehensive? "What place is allowed for non-numerical factors, such as inventions, politics, labour troubles, wars, earthquakes, financial crises?" These few quotations are only a sample of the rich scepticism that pervades Keynes' criticism of Tinbergen's technique. (Keynes 1938, 286-87).<sup>1</sup> In his formal review of Tinbergen's book, he noted that "if we are using factors that are not wholly independent, we lay ourselves open to the extraordinarily difficult and deceptive complications of 'spurious' correlation." Given debates that occurred 35 years later, perhaps it is most interesting that Keynes also asked "what happens if the phenomenon under investigation itself reacts on the factors by which we are explaining it?" (Keynes 1939, 309). As for expectations, which play such a large role in the *General Theory*, he notes in a further comment that expectations are handled by Tinbergen through the use of time lags, so that if expectations of the future closely resemble recent reality, the system works. But, asks Keynes, "How does [Tinbergen] deal with expectations of change?" (Keynes 1940, 319).

After a wartime lull, full scale empirical macroeconomic models revived with the early studies of Lawrence Klein (Klein 1950). Carl Christ (Christ 1951) presented a revision of a sixteen equation model of Klein's at a 1949 business cycle conference. But Milton Friedman was highly critical of the whole approach, commenting that:

The fundamental premise underlying work in this field is that there is order in the process of economic change, that sooner or later we shall develop a theory of economic change that does abstract essential elements in the process and does yield valid predictions (Friedman 1951, 112).

His conclusion was essentially that building such models with our then level of theoretical knowledge was a waste of time. Nonetheless, larger and larger macro models were built over the next thirty years (Kmenta and Ramsey 1981).

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<sup>1</sup>Here, Keynes was commenting on a pre-publication proof of Tinbergen's book.

The line of thinking implicit in these early large macro models was this: by estimating the parameters of a set of “structural” equations, you would have sufficient information to fine tune policy instruments to accomplish desired economic objectives. Ultimately a critical reaction set in with the causality critique of Clive Granger (1969) and the stability issues raised by Robert Lucas (1976). The approach gradually fell out of favor. “Structural” models which supposedly captured the main relationships in the macro economy were replaced with vector autoregressive (Sims 1980) and various types of time series models (Greene 2000, Chapters 17-18), increasingly featuring high-order difference equations (Blinder and Yellin 2001, 89). The reaction against large “structural” models was so great, for example, that during the early years of this decade, the Bank of Canada used only three equations to project economic occurrences in the United States economy (Lalonde 2000). That proved too simplistic. Now they are using a 35-behavioral equation dynamic adjustment model that implicitly responds to the complaints raised by Keynes 60-odd years ago (Gosselin and Lalonde 2005). For the most part, macro models used now are qualitatively different from the earlier ones (Brayton and Tinsley 1996). I have no comment on whether or not they are more effective.

### **Fisheries, Economics and Uncertainty**

But this is not a talk on macroeconometric models. It’s about fisheries. So why did I start with an apparent digression? A major point of my talk is that the problems inherent in the uncertainty and instability of fisheries models have very strong implications for the policy applications of those models. What’s the relevance of the macro models? The early large structural models largely failed because: (1) the world is too complicated – too much is omitted; (2) human behavior is complex and difficult to predict; and (3) the Lucas critique had some validity – people respond to policy decisions, but not always as the policy maker has anticipated. Fishery models have similar problems: (1) too much that is relevant is omitted, a subject to which I shall return; (2) both fishermen’s behavior and fish dynamics are complex and difficult to predict; and (3) fishermen and fishing firms respond to policy decisions, sometimes in strange and undesirable ways.

I am not sanguine about the state of fisheries in general. Perhaps it’s a personality thing. I have often been accused by those who know me best as being a “half empty” kind of guy, always seeing the gloomy side of things. But I do not think such pessimism comes from personality; many fisheries *are* in trouble, some will continue to be in trouble, and others, with changing circumstances, either human or nature induced, or both, sooner or later will be in trouble. With the state of our knowledge there is little we can do but exercise the most conservative, or conservationist, fishery policies, i.e., if our goal is to conserve what we have or to regain what we have had.

Fishery management has been a series of false starts, going back as far as one would care to go. Tim Smith opens his book on the history of fisheries biology (Smith 1994) with the story of Georg Sars, who in 1864 was asked by the Norwegian government to investigate the reasons for the wide variations in annual cod catches off the Lofoten Islands. It was the economic implications of the fluctuations, primarily for banks, that led to this investigation. There always has been a symbiotic relationship between the economic and biological aspects of fisheries and fisheries management. This, in itself, is hardly surprising.

But the symbiotic relationship has not always been recognized. In his famous keynote address of thirty years ago, Peter Larkin (Larkin 1977) reviewed the history of the concept of maximum sustainable yield (MSY). The goal of fisheries managers was to permit the harvest of the maximum sustainable yield, not more (lest the stock be endangered) and not less (resulting in a waste of food). Economics was not a factor. As Larkin put it: “The various laws of supply and demand, marginal revenue, alternative options, and psychological dissatisfaction, were mostly misty mumblings of the social sciences.” He went on to say “they knew the main idea was correct and it was only necessary to do a bit more research, to get a bit more experience, and then the basic theme could be appropriately fine tuned to perfection.” Biologists and economists are not that different after all. Isn’t that what the builders of structural macroeconometric models thought?

The main idea was not correct. MSY is a purely biological concept. Fishing, however, is an *economic* activity, and economics did not figure into the calculations. Larkin’s point, of course, was that MSY was also dangerous from a biological perspective, leading to a narrowing of the range of year classes that could reproduce. Larkin puts it more dramatically: “with the reduction in the number of spawning age classes, a failure in egg or larval survival for any reason is potentially far more catastrophic in its effect on long-term abundance.” This limiting of the range of the ages of reproducing fish is suspected of playing a role in the destruction of the northern cod stock of Newfoundland

(Hutchings and Myers 1994, 2137). Larkin's conclusion was that MSY must be abandoned.

However, in general, MSY was not abandoned at that time nor has it been abandoned since. A 2004 publication of the FAO continues a recent tradition of monitoring the state of specific fisheries throughout the world. The FAO's criterion for determining whether a stock is overexploited or not, is whether the stock is being caught at or beyond the maximum sustainable yield (FAO 2004, 33). The United States maintains a similar monitoring program, basing its evaluation on a comparison of the actual catch with the long term potential yield, which is essentially the MSY (NMFS 1999, 7). MSY is a concept enshrined in the Magnuson-Stevens Act in the definition of overfishing (NMFS 1996). One might argue that total allowable catches (TACs) are based upon stock assessments and not MSY, and that therefore my emphasis is misplaced. I don't think so. MSY establishes a target, a goal to be met when possible. Take the following from a paper on Namibian fisheries: "The TAC for hake is set at 200,000 metric tons...The maximum sustainable yield for hake is estimated between 250,000-300,000 metric tons, *so there is still room for improvement.*" (Wium and Uulenga 2003).<sup>2</sup> A recent analysis of United States rebuilding plans stated that "The goal is to restore fish populations to levels that can produce maximum sustainable yields" (Sumaila and Suatoni 2005). MSY is seen as an objective; it should not be.

At a resource rent workshop, held at the World Bank earlier this year, there was a call to shift away from the MSY concept (World Bank 2006). It's 2006 and still there is much progress to be made in this area. But simply setting a slightly more conservationist target will not necessarily help the fish stocks. Canada, in the mid-1970s, adopted a harvesting target for northern cod of  $F_{0.1}$ , a target that by definition is more conservationist than MSY (Lear and Parsons 1993, 67; Gulland and Boerema 1973). Adopting this target did not in itself protect the northern cod, which collapsed less than 20 years later. Of course, whether the  $F_{0.1}$  target was ever really implemented remains an open question.

### Environmental Factors

With MSY as a target, the objective of the fishery manager is to set TACs "at the margin", where, as precisely as possible, an "efficient" level of catch is determined, permitted, and hopefully achieved, such that there is no waste of fish that could safely be, but are not, caught. Such an objective is probably unattainable. In an ideal world, perhaps the natural cycles of fish dynamics would be purely systematic. In the real world, there are stochastic elements, making the life of fishery biologists difficult. But of even greater importance, the parameters of the natural cycles are in a constant state of flux because of environmental changes.

In its massive 2005 scientific volume, the Arctic Climate Impact Assessment (ACIA), discussing the effects of arctic climate change on fisheries, emphasized the effects of changes in sea surface temperatures because these are the data most readily available and most often studied (Symon 2005). However, the ACIA emphasized that critical elements were missing from their studies, elements such as: upwelling; water mass mixing; temperatures throughout the water column, primary production (phytoplankton); and secondary production (zooplankton) (Schrank 2006). The importance of these factors has been known for years (Cushing 1975). Even more fundamental, and mysterious, is the role of large scale atmospheric pressure anomalies. It seems clear that the El Niño/Southern Oscillation phenomenon has an important effect on South American stocks of anchoveta (Fagan 2000). But very little is known, for instance, about the role of the North Atlantic Oscillation on world wide climate change in general, and on fish populations in particular.

Predator/prey relationships also have been discussed for years (Smith 1994). The collapse of Newfoundland's northern cod stock has been studied to death. Yet little if anything has been decided about the cause, or causes, of the failure and its lack of recovery. To what extent was the absence of capelin (from the right place at the right time) a major cause of the collapse (Lilly 1994)? And, of course, what were the relative roles of fishing versus natural mortality (Lilly and Murphy 2004)? I join those who believe that the most important cause was overfishing, but negative climate shifts occurring at the wrong time and affecting a weakened stock, played an important role as well (Drinkwater 2002). The issue is not, however, decided (Fu, Mohn & Fanning 2001).

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<sup>2</sup>Emphasis mine.

When Canada's northern cod moratorium was announced on July 2, 1992 (DFO 1992), it was also announced that the moratorium would last for two years, after which it was assumed that the cod would recover. Some thought that two years was unrealistically short, that ten years or more would be required (deYoung and Rose 1993). It is now 14 years later and there is still no sign of recovery. Why? The strongest official statement I have seen is that "cod assessments have concluded ... that predation by seals is a factor contributing to the high total mortality of cod in the offshore and the high mortality of adult cod in the inshore (DFO 2005a)." This is far from a definitive statement. All it means is that it is believed that seal predation has played a role, which no doubt it has, but how much of a role and what other factors are involved remain unstated. Another factor is continued excessive fishing, both legal and illegal. The relative effects and interplays of fishing activity by humans, predator/prey relations and climate change on the fish population remain largely unknown.

Newfoundland fishermen, eager to fish and anxious earn a living, apply pressure to be permitted to fish whenever there is a concentration of cod, regardless of whether or not that is the only spawning concentration, and therefore the only hope of recovery. In April of this year (2006), DFO protected the northern bottlenose whale under the *Species at Risk Act* while explicitly denying such protection to the northern cod. The Committee on the Status of Endangered Wildlife in Canada had recommended that the northern cod be declared endangered. The Minister of the Environment rejected the recommendation because of the economic effects of such a designation and the "fact" that "the cod fishery is at the core of the cultural roots" of coastal communities. The Minister's announcement also noted that previous closures caused a political backlash from the industry and the provinces (CanGaz 2006). Meanwhile, DFO claimed it would "continue to pursue strong conservation measures" with respect to northern cod (DFO 2006a). What such "conservation measures" amount to in the face of political, economic and social pressure remained to be seen.

It is precisely this concern with the economic and social implications of sharp reductions in TACs, a concern that is not unreasonable in itself, that helped sink Newfoundland's northern cod industry more than a dozen years ago. It's the kind of thinking that has kept the European Union fishery managers from accepting repeated pleas from fishery scientists to close the North Sea cod fishery (ICES 2003; EurActiv 2003; ICES 2004, Fisheries Research Service 2005). And it was precisely the opposite view that led Norway and the Soviet Union, in the face of an apparently severely reduced Barents Sea cod stock, to cut the cod TAC drastically for 1989 and 1990, easing the way to a quick recovery (OECD 1991, 149; OECD 1993, 149; Oistad 1994, 17).

Let me be clear: there are virtually no offshore northern cod, the total spawning stock biomass is estimated to be 2% or less of the 1980s average, which was already less than half the biomass of the 1960s. There are some inshore aggregations, the most important of which is located in Smith Sound, Trinity Bay. Its fish population is estimated to have declined by one-quarter from 2001 to 2004, after which the time series was abandoned, apparently as a DFO economy move.

The northern cod fishery was closed to commercial activity in 1992. In 1998, a small directed northern cod fishery was permitted. In April 2003, with no recovery of the northern cod in sight, the directed commercial and recreational fisheries were closed (DFO 2005a, 2006c). Now, in June 2006, ostensibly in the interest of narrowing "the gap between industry and DFO about the status and management of the [northern] cod stock," a small commercial inshore fishery is to be open to inshore and some nearshore vessels. Yielding to pressure from the industry, DFO is assigning 3,000 pound quotas to each of 2,300 groundfish license holders (DFO 2006b). The total catch would not be very great, but probably it would still be around 10% of the total biomass. Usually, this would be considered a very conservative TAC. But here we are referring to a fish stock that might very well be close to extinction.

In its 2005 assessment, the DFO scientists, with typical understatement, warned that "there is a risk that fishing in the inshore will impede recovery in the offshore." If there is low recruitment, the fish population is expected to decline further with TACs of the magnitude that are now being allowed (DFO 2005a; Baird, Bishop and Murphy 1991). With better recruitment, the small allowed catches would probably do little harm. But, you cannot count on strong recruitment, and I would suggest that you cannot adequately police the fishery to ensure that the quotas will not be exceeded. Is this applying the FAO's "precautionary approach"? Is this what the Minister means by continuing "to pursue strong conservation measures"? Whatever the justification offered, this is just an example of fishery managers acceding to pressure from fishermen. Regional CBC broadcasts (both radio and television - June 8-10, 2006) have shown fishermen ecstatic over the opening of the fishery, claiming that it is "about time" and that "there are plenty of fish." The broadcasts have also quoted fisheries biologists who are no longer under the discipline of DFO

complaining that the fishery should not have been opened. DFO scientists earlier had warned that the productivity of the northern cod stock is so low that even were all fishing activity to stop (including the bycatches that have never ceased) it is possible that the stock will not recover (Shelton *et al.* 2006). After a decade of discussion, the term “precautionary approach”, in Canada at least, is still used very loosely (Rivard 2006). But surely, whatever it means, it should not mean ignoring the scientists’ warnings and opening a nearly defunct fishery for cultural or economic reasons. The fish are endangered; don’t make it worse.

The future of the fishery in Newfoundland looks even bleaker than this (and I am not even considering current problems with the crab fishery or with the fish processing sector). Ostensibly in the interest of moving from single species stock assessment to an ecosystem based approach, DFO claims that the “ecosystem-based approach to science cannot be done by DFO alone” and therefore DFO will withdraw from doing stock assessments, leaving it to the industry to make the necessary determination. In the interest of saving \$6,000,000 per year, the government is giving its assessment function to the party with the greatest short term incentive to inflate the numbers (DFO 2005b). Given that DFO management expenses in Newfoundland alone are close to \$40,000,000 per year, this seems a relatively minor savings at a great loss of science (Schrank and Skoda 2003). Nothing is wrong with broadening single species assessments, but to leave them in the hands of an industry that often seems shortsighted is not the way to go. And weakening stock assessments before suitable ecosystem analysis has developed is also not the way to go. This was pointed out by Niels Daan, who, after he edited the papers from a 2005 ICES symposium on quantitative ecosystem indicators, raised serious questions about the whole exercise (Daan 2005).

### **Fishery Management Success?**

Having resided in Newfoundland for a third of a century, I am more familiar with its fisheries problems than with others. Its fishery is also a dramatic, if perverse, case.

In the decade after the extension of fisheries jurisdiction in 1977, the Canadian fisheries service, the Department of Fisheries and Oceans (DFO), was cocky. They had a sophisticated science program matched by an equally sophisticated fisheries management system. Both facets were probably among the best in the world. A senior DFO manager, a biologist, termed the years 1985-88 “The Glory Years”, stating that “Federal intervention put the industry on the road to recovery” (Parsons 1993, 375). Perhaps so, but this statement ignored the fact that the inshore fishermen had done poorly throughout the 1980s, blaming their situation on the offshore trawler fleets, a view that received some support from the Alverson committee that reviewed the state of Newfoundland’s inshore fishery (Alverson 1987). This statement also ignored the fact that the fishery was very heavily subsidized, especially during the 1977-1981 period (Schrank *et al.* 1987; Schrank 1995), which, in turn, led to continuing pressure on the government to allow excessive fishing. This statement also ignored the fact that some DFO scientists were becoming concerned, one even writing a suppressed (or at least unpublished) paper that claimed that the science was not worth an “*anus rodentum*” (Winters 1986), leading to a political furor a decade later (Hutchings *et al.* 1997a, 1997b; Doubleday *et al.* 1997; Healey 1997). This statement also ignored the fact that the optimistic 1986 assessment results led the scientists to doubt the weaker 1984 and 1985 results (Baird, Bishop and Murphy 1991), instead of doubting the more cheerful results of 1986 that are now seen as anomalous (Bishop and Shelton 1997). Finally, and most importantly, this statement also ignored the fact that by the time of writing it was understood that ever since the extension of fishery jurisdiction in 1977 the northern cod population had been overestimated because undue weight had been given to commercial as opposed to research vessel data. As a result, fishing mortality was being underestimated and fishery managers were setting excessive TACs (Bishop and Shelton 1997, 25; Baird, Bishop and Murphy 1991). So much for the “Glory Years”.

But I do not want to leave the impression that the problem lies with fishery science in Newfoundland alone. Quite the contrary. Fisheries biology has a long way to go before it can fully incorporate environmental factors.

I would not go so far as to say that overestimation of fish populations is endemic, but it certainly is not limited to Newfoundland. In the early 2000s, for instance, at least two reports on the quality of Icelandic cod stock assessments were prepared. They concluded that the assessments were overly optimistic, thus setting the stage for excessive TACs (Pope 2000, Rosenberg 2002). The second of these reports stated that, given the information available at the time, the errors were not predictable, but to avoid similar problems in the future, greater attention should be paid to the uncertainty in the estimates, and in addition, managers should avoid making decisions at the margin. Iceland’s goal,

beginning in 1995, was to harvest approximately 25% of the fishable biomass. Despite this goal, during the late 1990s, catches were up to 39% because of overestimation of the size of the stock (Government of Iceland 2002). Perhaps because of the ITQ management regime in place, the problem was noticed and corrected before serious damage was done. But if the problem of exceeded targets can occur in Iceland, with its ITQ management regime, it might arise anywhere.

### Severely Limited Catches

Nearly a quarter century ago, Michael Sissenwine, later the chief scientist with the United States National Marine Fisheries Service, warned of the uncertain environment confronting fishery scientists and managers (Sissenwine 1984). He credited most of the uncertainty to the failure to predict recruitment (which may be impossible to predict because of inherent uncertainty in the natural processes) and noted that when good results are achieved, they “may in fact be an artifact of the fitting procedure.” He accepted that *growth parameters* are relatively constant for marine fish, although we now know that, probably as a result of genetic responses to size selective fishing pressure, such growth characteristics as weight-at-age and age at maturity are variable and, at least for northern cod, fell in the decade leading to the stock collapse (Lilly *et al.* 2003, 17-18, 78; Shelton *et al.* 2006). However, Sissenwine understood that *natural mortality* varies over time, manifestations of the effects of environmental factors. Yet, in most calculations, natural mortality is taken as a constant. Although recognition of the problems introduced by uncertainty is more general today than it was when Sissenwine wrote his paper, it is not clear how much practical difference this recognition has made.

Considering the importance of changes in environmental conditions and in predator/prey relationships, I found it surprising to discover in one of the few recent studies of changes in natural mortality, that natural mortality was calculated merely as a residual from sequential population analysis (Grégoire and Fréchet 2005).

Only occasionally is environmental information incorporated directly into the stock assessment process. Prior to a capelin collapse in the Barents Sea in the late 1980s, for instance, a single species catch target criterion, essentially MSY, was used. With the weakening of the stock, new models for stock assessment were adopted that took explicit account of cod predation on capelin. Extensions to include harp seal predation are apparently underway (Gjøsæter, Bogstad and Tjelmeland 2002). Environmental information, however, is most often only used for retrospective explanations of why fisheries fail, rarely as an intrinsic part of the stock assessment process itself.

But the high degree of uncertainty in the scientists’ work is still largely ignored in their applications to management. Sissenwine concludes with the statement “that recognizing uncertainty is a necessary step to learning to live with it” and that, to live with it, fishing mortality rates have to be reduced. Scientists are well aware of the stochastic nature of their empirical results but appear to be hesitant to focus on it. As an example, during the stock assessment meeting for northern cod in 2003, it was agreed “that adding stochastic variation around the central trend would be unhelpful, and possibly misleading” (Lilly and Murphy 2004, 27). Some stock assessments now explicitly take cognizance of the stochastic nature of the results, but the confidence intervals are often extremely wide and frequently are buried in appendices (Lilly *et al.* 2003).<sup>3</sup> At other times the uncertainty is simply ignored, as in the latest stock assessment report for St. Pierre Bank cod, which explicitly makes “deterministic” three year projections (DFO 2004).

A decade after Sissenwine’s seminal paper, William Doubleday, a biologist and fisheries manager with Canada’s DFO, discussed the role of scientific advice in fisheries management (Doubleday 1993). When fishery managers substantially reduce the TAC for a certain stock, industry is well aware that the implications are reduced catches, reduced short term profits, reduced employment and reduced plant capacity utilization. Concerned with the political reaction by the fishing industry to management actions based upon fishery science, something I referred to earlier, Doubleday was interested in how the reactions could be muted, concluding that fishermen would not be satisfied

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<sup>3</sup>For instance, in Lilly *et al.* 2003, p. 88, in a table entitled “Estimates of cod abundance from surveys in Division 3K...”, the point estimate for December 2002 is 41,853mt with a standard error of 10,255. A 95% confidence interval ranges from 21,753 to 61,953. See also the comment on large standard errors that made the statistical distinguishing of “large and less abundant yearclasses” of capelin in the 1980s and 1990s impossible (DFO 1998, 4).

unless the fluctuations in catch quotas were relatively small. He felt that the criterion of small fluctuations in catch quotas was unlikely to be satisfied in the foreseeable future for most stocks.

According to Doubleday, fishery management should not require high levels of precision in scientific performance, since high levels of precision are unlikely to be achieved. Given that management is most sensitive to scientific error under TAC systems, Doubleday concluded that “the most promising way to achieve acceptable reliability is to aim for a low exploitation rate” (Doubleday 1993, 380). Catch less fish. He suggests that for many stocks a harvest rate of 10% of the fishable biomass would permit long term sustainable and consistent catches. While rarely applied, this ultraconservative criterion ostensibly has been used in managing Newfoundland capelin since 1979, (Shelton, Carscadden and Hoenig 1993). It is difficult, however, to tell how successful the approach has been since there has not been a formal Newfoundland capelin assessment since 2000 and there were no biomass estimates throughout the 1990s that could form the basis of an evaluation (Carscadden 2006). How does science progress when the government is constantly stripping it of resources?

While the idea of severely limiting catches has been in the air, the idea still has not caught on. Ten years after Doubleday’s intervention, Andrew Rosenberg (2003) argued against managing at the margins; it’s too risky. Rosenberg quotes John Gulland: “Fishery management is an endless argument about how many fish there are in the sea until all doubt has been removed – but so have all the fish.” Substantial uncertainty in fish stock assessment is a fact of life, and subtle approaches to the problem are unlikely to be effective. The idea of finding methods of fishery management that are relatively insensitive to scientific error is very appealing. To the extent that Marine Protected Areas are effective, among management regimes they probably rely least upon scientific stock estimation (Doubleday 1993).

I have already referred to DFO’s self-satisfaction with the results of its cod recovery program after the extension of jurisdiction in 1977, a sense of self-satisfaction that did not last very long. How does one judge whether fisheries management is a success? To what extent does success depend on the relative constancy of environmental factors? One does not know. Fisheries managers do their best. A proper management regime helps. But there is always doubt. Jeffrey Hutchings and Ransom Myers, writing shortly after the northern cod collapse, had it right, in my opinion, when they said: “It is only when a population declines in abundance to a level at which it is not economically viable for harvest (commercial extinction) and when its very existence is threatened that the reliability of a given strategy can potentially be evaluated (Hutchings and Myers 1994, 2127).” And that evaluation, by definition, is gloomy.

### **More on Fisheries Management Success**

Some grow impatient with such gloom and doom. Objecting to the emphasis on failure in fisheries management, a couple of our colleagues recently edited a book entitled *Successful Fisheries Management* (Cunningham and Bostock 2005). In seeking a measure of management success, one of the authors states that the “conservation objective...is to ensure that the potential productivity of the fish stocks are used to their full advantage without endangering the underlying health of the stock” (Bennett 2005, 23). On its face, this is a perfectly reasonable statement. But it makes me nervous. What is “full advantage” and how do you know that you are not endangering the health of the stock?

Science, for the most part, cannot tell us with much confidence when the stock is endangered until that stock is decimated. The best that science can say is that unless fishing mortality is lowered to such and such a level, the stock *may* be endangered, while the fishermen see that the cutbacks *must* directly reduce their short-term income, and not too many fishermen are in a strong enough financial position to worry too much about the benefits of the current cutback for the indefinite future. Between a biological *may* and an economic *must*, the economy will “win”.

The accuracy of most stock assessments is implicitly accepted in the book. The introductory essay mentions biology, interestingly enough, combined with “luck”. The author states that “the real difficulty with the biological dimension to success is that fisheries managers should neither take the blame nor credit for changes due to environmental factors” (Bennett 2005, 37). Luck should have nothing to do with it. Environmental factors change. These affect fish populations, and only occasionally can fishery managers do anything directly to mitigate the effects of the changes. However, what managers can do is recognize that environmental factors do change and adopt strategies that permit the fish stock to withstand strongly negative environmental change. This gets us back to our main theme, that fishery managers must avoid operating at the margin. TACs should be set sufficiently low so that the fish population can

withstand a strong negative shock.

### **A Small Core Fishery**

Doubleday suggested a catch rate at 10% of the fishable biomass. A number of years ago, Giulio Pontecorvo and I suggested a “small core fishery” in which the industrial base of the fishery would be kept sufficiently small that modest TACs would be adequate to support the industry (Pontecorvo and Schrank 2001). The TACs should be small enough that, with even substantial changes in the fish population, sustainable and more or less constant catches could be taken from year to year. Only the most extraordinary environmental change could endanger the core fishery. When it seems clear from stock assessments that there are additional fish that could safely be caught, risk takers could be invited in, possibly by auction, to catch the surplus, but with no commitment whatever that they would be allowed into the fishery in the future. The primary goals of our approach are the protection of the fish stock and the maintenance of a sustainable and economically viable core fish harvesting industry. A perfectly reasonable alternative goal is that a steady supply of fish be supplied to the market. The small core fishery without the auction would satisfy all of these criteria. But if risk takers are permitted to fish the “surplus”, the amount supplied to the market could vary considerably. We were aware of this conundrum, but were afraid that leaving too much fish to die natural deaths, thereby denying this protein to humans, would probably be contrary to the most basic of goals -- human welfare. We therefore opted to permit what we considered to be “safe” variability.

We received some polite commentary on the suggestion, usually accompanied by the comment that it could not be done. One author went so far as to respond in *Marine Policy* (Polacheck 2002), arguing that our approach would accomplish nothing: it would set up two groups of fishermen, risk takers and risk avoiders, and that the former group would create the same pressures for excessive TACs that we are trying to prevent. This is a legitimate concern, and the system would have to be designed to prevent this situation from occurring.

The author also questions our determination of the permitted excess catch. If we have so little faith in stock assessments, how would we know that any catch in excess of the core TAC could be taken? This could also present a problem, but we never said, nor do we believe, that fishery science is useless, far from it, just that it cannot be relied on at the margin. To make up an arbitrary number, perhaps 75% or 80% of the excess would be permitted to be caught. Whatever the merits of these objections, they do not obviate the need to build into fishery management a safety factor that is largely missing today.

### **Conclusion**

I started with some comments on the rise and fall of Keynesian structural macro models. When I was a graduate student, it was widely accepted that structural models would allow us to fine tune the economy to achieve desired results and mitigate business cycles. Once again, Milton Friedman intervened with a series of papers and debates (e.g., Friedman and Heller 1969), continuing his arguments begun twenty years before to the effect that the models were not good enough to accomplish the purpose for which they were being applied. To me, the analogy is clear. The present state of fisheries models is such that they are not sufficiently well developed (and may never be) to permit fishery management to be fine tuned using those models.

Is there hope for fishery management? Of course. It's absolutely necessary. But we must use the tools available to us, taking full cognizance of the critical problems resulting from the uncertainty inherent in fisheries science. Large, very large, safety factors must be built into the management system. The current state of the world's fisheries suggests that fishery management to date has not been highly successful, and that much more conservative management is necessary.



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#### ACKNOWLEDGEMENTS

I wish to thank J. E. Carscadden, J. Gates, R. Hannesson, G. Pontecorvo and C. H. Pye for helpful comments on earlier drafts. I remain responsible for any errors.