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Viewpoint

Cod: perhaps if we all stood back a bit?

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Abstract

Yellowfin tuna may sustain an industrial fishery indefinitely if managed by conventional models, but northern cod perhaps cannot. This may be a generality for tropical and cold-water fish. In their core habitat, yellowfin spawn continuously, year-round and from an early age, while northern cod mature only at middle-age and spawn only during a brief season. The resulting natural age structure of each species is well-adapted to the characteristic recruitment variability imposed by its habitat. Industrial fisheries managed by stock assessment models seem unavoidably to modify the age structure of cod so that very few spawning year-classes remain, though this does not seem to happen in yellowfin managed in a similar way. It is axiomatic that the natural age structure is that best fitted to the long-term survival of any species, and this fact implies that cold-water species exploited by an industrial fishery may be unable to sustain a viable population across periods when environmental stress is strong and recruitment weak: population collapses of cod and other cold-water fish under industrial fishing pressure may therefore be unavoidable. © 1998 Elsevier Science B.V. All rights reserved.

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Mathematics is a millstone that grinds whatever we put underneath it ...

A.N. Krylov, 1956

If you live between the sea and the rock on Newfoundland, there is not much to do except go fishing. So the collapse of the cod stocks of the Grand Banks followed by the 1992 moratorium – prohibiting even hand-lining for the table – devastated life in the outports: houses abandoned, families moving away. The loss of six of the seven cod stocks off Atlantic Canada put 40 000 people out of work from Newfoundland to Nova Scotia and while federal emergency income-support is nearing its end, the fish have still not returned.

Inshore fishermen told the Fisheries Committee of the House of Commons in Ottawa (Baker, 1998) that "the Newfoundland fisheries had been sold out for the benefit of corporate interests... to be bartered with foreign governments in order to promote trade in other commodities" and that "federal policies sanctioned both foreign and domestic dragger fleets in the interest of foreign policy and domestic political patronage". The Committee concluded that "Errors in the scientific estimates of the sizes of the cod stocks were seen as a major factor in the collapse and not enough has changed to re-establish confidence in the ability of the Department of Fisheries and Oceans (DFO) to manage the fishery in the future".

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You may not like any of these statements, but you will agree that the "golden thread of trust" which must exist between fishermen and fishery managers (as between British bobbies and the public) has been irretrievably broken.

Cod (*Gadus morhua*) in the northwestern Atlantic was thought by FAO in 1971 to be good for a sustainable annual yield of 1.7 million tons (Gulland, 1971), and a bright future was confidently predicted by DFO after the imposition of the 200 mile zone in 1977 and the partial exclusion of European trawlers. Nobody seems to have considered the possibility that these predictions could be quite wrong, and DFO was caught off-balance when the stocks collapsed less than 20 years later. By 1992, the cod biomass of the northern Grand Banks (NAFO Division 2J3KL) was only 2% of what it was in 1962, while other stocks ranged from 5% to 15% of their previous maxima. Only St. Pierre Bank (NAFO 3Ps) was in rather better shape at 23% of its 1960 biomass.

The collapse did not occur because Canada failed to deploy modern fishery management methods, as might have been the case off the coast of a less fortunate country. On the contrary, the DFO stock assessment biologists were (and are) well-regarded by their peers, and were supported by state-of-the-art survey equipment and ships. A formal mechanism was in place for setting total allowable catches (TACs) from each unit stock seasonally. A question you should perhaps be asking yourself is "if such a thing could happen off Canada, then why not here?"

For all these reasons, 1992 will become a defining year for fishery science. Analysis of what went wrong in the Canadian fishery must be made and must be, above all, disinterested. It is hard to convince oneself that this has so far even begun, despite all the noise. Differences of opinion, mostly between interested parties, over how such an apparently competent research and management effort could so signally have failed are sufficiently deep that reconciliation is far from being achieved.

The debate (if it can be dignified by that term) has taken two lines: *first*, to what extent the political imperatives of successive federal Ministers required that the scientific advice for stock management should be ignored and, *second*, whether fallacious models and survey techniques were used in stock assessment. The first line of argument need not concern us here because fisheries management must be – in the last resort – a political rather than a scientific issue, despite the agonising of utopists. The second line remains too focused on detail: for instance, why did DFO survey-based estimates consistently depict higher recruitment levels than independent VPA-based estimates, or why were discards of small fish consistently underestimated by DFO?

I do not wish, nor am I qualified, to analyse the precise course of events that caused the cod stocks to collapse, but a fisheries management crisis of this magnitude must make us rethink the general problem of managing cold-water fisheries. So, in this essay I propose that we stand back a bit, think afresh about some basic biology, and ask if we were realistic in managing an industrial fishery for cold-water northern cod as if it could be indefinitely sustainable?

I shall illuminate this unthinkable proposition by comparing some basic biological facts about cod with similar information about a large fish of tropical seas, the yellowfin tuna *(Thunnus albacares)*. In the eastern tropical Pacific, the yellowfin fishery is regulated by the Inter-American Tropical Tuna Commission (IATTC) and here it really does appear capable of sustaining an industrial fishery managed with modern stock assessment techniques. Both cod and yellowfin have maximum lengths of about 1.5 m and both are active predators which feed on smaller species of fish or cephalopods, so my comparison is not unreasonable.

It is surely axiomatic that the natural population age structure of any species – whether of cods or copepods – must have evolved (within the physiological capacity of each species) to optimise long-term persistence, and maximise occupation of habitat in competition with other species. This axiom is consistent with the different age structures typical of different parts of a species habitat, or under unusual external stresses, or when invading new habitat. It is also axiomatic that such differences can have only a limited range for each species, and tend to return to the undisturbed state when stress is removed. Lotka discussed this limitation in 1925: "Now, age distribution is indeed variable, but only within restricted limits. Certain age distributions will practically never occur... There is, in fact, a certain stable age distribution about which the actual age distribution varies, and towards which it returns if through any agency disturbed therefrom". If you can't agree with that simple axiom (which requires no mathematics to state), then don't bother to read any further because it is the thread on which the rest of my argument hangs.

Fishing theory depends on a simple concept. Fishing mortality will modify the population age structure by selectively removing old, slow-growing fish, hence releasing food resources for younger fish, enhancing their growth rate, and causing them to mature younger. Alternatively, it may be held that fishing mortality produces the same effect by reducing the level of cannibalism by older fish. However, it occurs, it is presumed to be a Good Thing and as Benny (Schaefer, 1954) wrote in one of the earliest bulletins of the IATTC, surplus production models suggest that a maximum fisheries yield may be obtained by reducing a natural population to about 50% of its original biomass. However, as far as I am aware, no such models explicitly incorporate the limitation assumed by Lotka: that the variability of the age structure of natural populations is viable only within limits and if pushed beyond these limits, environmental stress may prevent recovery. It is my viewpoint that this may be an inevitable result of managing cold-water fish with standard stock assessment models, though perhaps we can get away with it indefinitely in some tropical fisheries.

But before we can start in on that, let's review the reproductive ecology of yellowfin and the response of this species to several decades of sustained fishing in the eastern tropical Pacific Ocean. This area yields about 260 000 tons annually, or about one-fifth of the 1.3 million tons of yellowfin taken from all tropical oceans.

The biological oceanography of this region, as of other tropical seas, is crucial to the sustainability of yellowfin fisheries. Because of the great east–west dimension of the Pacific Ocean, seasonal changes in westerly wind-stress are not reflected in changes in the mixed layer depth (Philander, 1979), which deepens significantly only in El Niño years. Consequently, between year changes in plankton biomass and productivity may be greater than seasonal changes which are as small as anywhere in the ocean (Blackburn et al., 1970; Longhurst, 1995). Primary production and zooplankton biomass each vary by only about 25% seasonally, so larval tuna must enjoy a remarkably predictable food supply. Female yellowfin seed this reliable brew almost continuously with ova; at intervals of less than two days, batches of ova (mean batch number is 2.5×10^6 ova) are released by each female whenever and wherever water temperature is >25°C, which it is year-round from 0–20°N. This represents a daily metabolic cost of about 2% of body weight and is achieved by feeding at a rate four times faster than northern cod. Females which cannot sustain this metabolic demand simply resorb the contents of their ovaries. The cost of sperm production for males is only about 0.3% of body weight daily, so that the growth rates of the two sexes diverge after maturity at about 90 cm, near the end of the second year of a potential life span of 7–8 years. Females wear out quicker, so males dominate the population of larger, older fish (Anon., 1992, 1994; Schaefer, 1998).

This reproductive strategy results in remarkably constant recruitment to the adult population: cohort analysis suggests that recruitment of fish at 30 cm varied only from half to double the average figure of 77×10^{-6} individuals in the IATTC area annually during the 28 year period prior to 1994. Even El Niño years do not disrupt recruitment, as we shouldn't expect them to do. After all, in such years, the thermal structure of the eastern tropical Pacific comes to resemble that of the western Pacific, which is prime yellowfin spawning territory.

As a consequence of all this, both recruitment and availability (catch per days fishing of Class six purse-seiners) have remained remarkably constant in the IATTC-regulated area since 1962, although annual catches have more than doubled. Yield was down, and stock biomass somewhat reduced in the early 1980s, but both subsequently recovered rapidly and the stock remains strong to the present day (J. Joseph, personal communication).

More germane to my argument, however, is the fact that the age structure of the population has been modified by the fishery only moderately. The two oldest, male-heavy year-classes (VIs and VIIs) were much reduced quite early in the fishery so that the adult population came to comprise only four year-classes (IIs–Vs), with the largest biomass composed of IIIs or IVs. It is very significant that this age structure shows no progressive evolution from the 1960s right up to 1996 (the most recent data I have before me), as is the fact that these four year-classes (II–V) are all mature, reproducing adults. We may well conclude that this population has not been modified beyond what Lotka might have thought of as a point of no return.

How different has been the fate of the poor cod! The age composition of the remnant population of northern cod prior to the collapse of the early 1990s had become a travesty of its natural, pre-fishery structure, which included fish up to at least 20 years old. Eventually reduced to less than 10 year-classes, it came to have an age composition more appropriate to a warm-water fish, with one important difference: that only a few of the oldest year-classes were mature.

Of course, the age composition of cod stocks of different seas had different natural age compositions prior to modern fisheries, but to make my point better I will restrict my discussion to the Labrador–Newfoundland fish. The annual catches taken from this stock dwindled (with some recovery after the Canadian 200 mile limit was implemented in 1977) from about 620 000 tons in 1962–1966 to 225 000 tons in 1987–1991.

Unlike yellowfin, significant changes in age composition, average size, and age at maturity occurred during this period, at least partly imposed by the fishery. The biomass of fish aged 10–20 years declined from 48% in 1962 to 8% in 1990: after 1992, these fish were totally absent (Shelton et al., 1996). Consequently, the mean weight of individuals was reduced from >2.0 to about 0.5 kg (Haedrich and Barnes, 1997).

We can derive an estimate of the age at maturity prior to the modern fishery from the median age at maturity in the NE Atlantic at comparable latitudes, which was about 10 years (range 6–14), so it is a reasonable assumption that half of the 1962 stock biomass in 2J3KL comprised 10 year-classes of adult, spawning fish. Since the 1962 population was already modified by a decade of industrial fishing, we may also assume that the unfished stock would have contained an even higher proportion of large and reproductively active fishes. In both NE and NW Atlantic, fishing strongly reduced the median maturity age: by 1975, in the NE Atlantic this was down to 8.5 years (Nakken, 1994) and in 2J3KL in 1993 was only 5.5 years (Morgan et al., 1992). If I am correct that an approximation to the natural proportion of big fish is critical to the long-term survival of high-latitude fish stocks then something, it seems to me, was seriously amiss (see Fig. 1).

But a brief excursion into the oceanography of the Newfoundland region will be useful before we get to that. From the point of view of a larval fish, it would be hard to find a region more different from the benign and predictable eastern tropical Pacific: both seasonal and between-year variability in the NW Atlantic are as great as anywhere in the oceans. The thermal gradient at the sea-surface is unique, for coastal pack-ice along the south coast of Newfoundland and the 18°C surface water of the Gulf Stream are separated by only a few hundred kilometres during the winter and early spring. Long-term climatic trends, such as the post-1920s warming which continued until 1950, or the ‘great salinity anomaly’ of the 1960s and 1970s, leave strong signatures in the oceanography of this marginal region which lies between sub-tropical and sub-polar seas. The 1980s were a period of cooling in the sea off northern Newfoundland.

Moreover, the topography of the outer Grand Banks and the Flemish Cap is such that the Gulf Stream sweeps in a great cyclonic arc into the southern Labrador Sea. Mesoscale eddies are generated at the conjunction of the flows of the Labrador Current and Gulf Stream, the cause of interleaving of cold and warm water masses along the shelf edge: fish stocks of regions of conjunction between opposing current systems exhibit anomalous recruitment patterns (Power, 1996). Mesoscale eddies may entrain bodies of shelf water out over the deep ocean potentially capable of carrying sufficient numbers of eggs or larvae to reduce groundfish recruitment significantly (Myers and Drinkwater, 1989). Finally, flow of melt-water from Hudson Straits and the Gulf of St. Lawrence is highly variable, and modifies the shelf environment accordingly. It will be no news to you that others have already commented extensively on the consequences of such a strong environmental variability for cod recruitment in this region (e.g. Koslow, 1985).

Biological processes also exhibit strong temporal variability, dominated by the early summer pulse of the spring phytoplankton bloom when growth rates increase by a factor of 30–40, leading rapidly to an order of magnitude increase in plant biomass; the availability of the micro- and meso-zooplankton for larval fish is equally strongly seasonal. Whatever cues

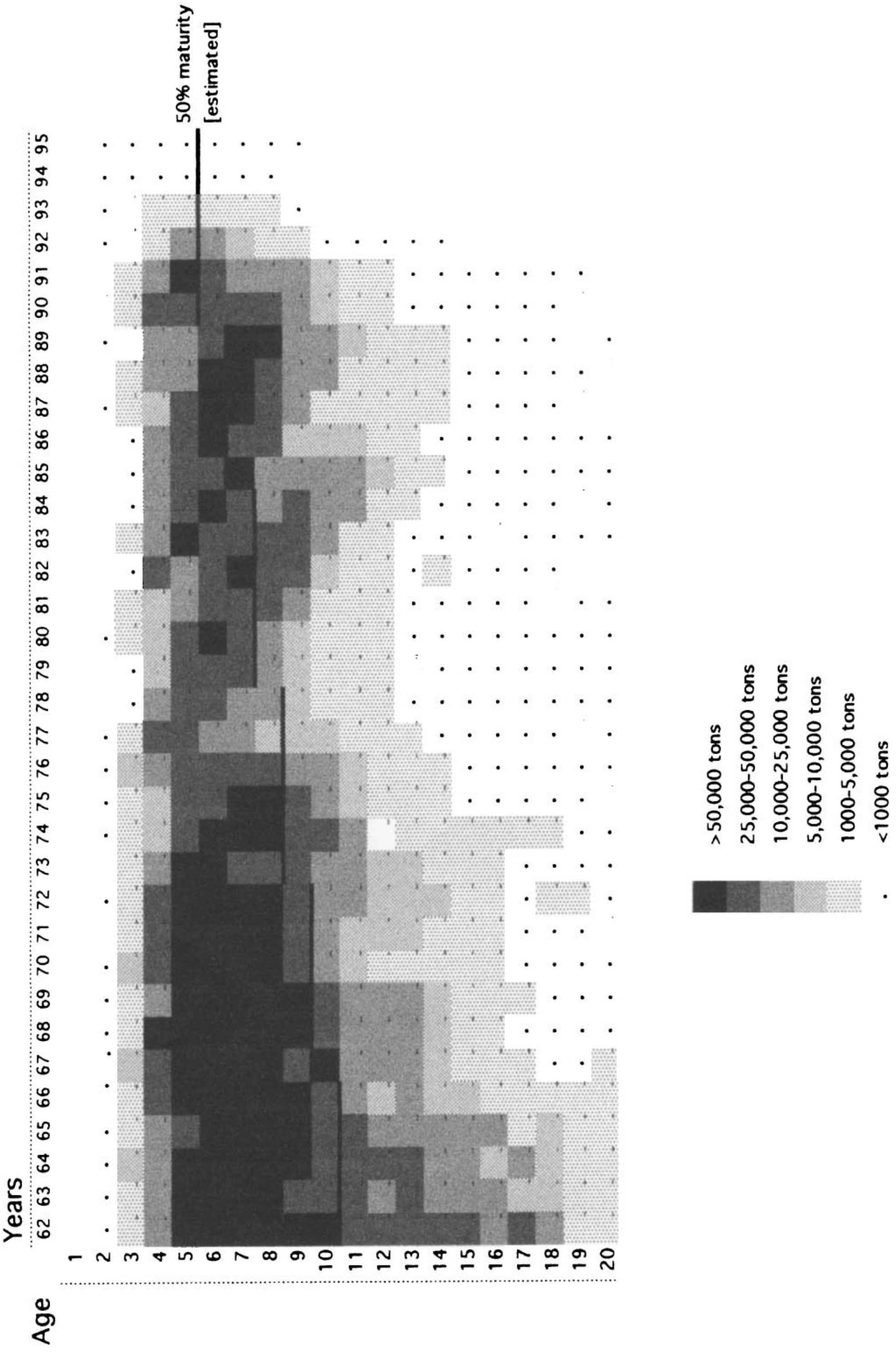

Fig. 1. Biomass of cod taken from each age class in NAFO Division 2J3KL, 1962–1995; heavy horizontal line represents estimated evolution of age at 50% maturity.

determine the spawning date of cod, the probability of an encounter between their larvae and a suitable concentration of suitable food must be less assured than for tropical tunas. As a result of all this, an order of magnitude difference between small and large year-classes was not unusual even prior to the modern fisheries, and intervals of around five years between strong year-classes seem to have been usual even in relatively unfished populations. Intervals between super year-classes may have been even greater.

At this point, it will also be well to bear in mind the ability of cod to establish populations distant from the source stock when conditions moderate so as to make colonisation possible – the classical case is, of course, the 1920s invasion of the coast of Greenland by Icelandic cod, with regular exchange of adult individuals between the two areas occurring after the mid-1930s. This colonisation eventually failed because the Greenland stock was unable to produce satisfactory year-classes after about 1960, during a succession of cold years.

But now let us get back to our comparison of two species whose habitats are separated by 50° of latitude and consider the consequences of the different relative ages at which they partition metabolism between reproduction and growth. Something like this difference is an inevitable physiological consequence of very different temperature regimes for the growth of poikilotherms, yet it ensures that the reproductive strategies of the two species are well-matched to the environment to which they must entrust the survival of their larval progeny.

Yellowfin sprint to a size at which they can exploit the nektonic resources of the open tropical ocean and then maximise their ability to produce a constant succession of batches of ova for the rest of their lives: somatic growth is then relatively slow. Cod, on the other hand, maximise somatic growth to ensure the existence of large individuals in the stock; then, and only in middle-age, is any metabolic effort diverted to reproduction.

You don't, as they say, have to be a rocket scientist to model what the consequences would be for yellowfin if even two or three successive years produced no recruits. Obviously, the stock would run out of reproductive potential very quickly: there is no large reserve of juveniles to grow on to maturity during such an unlikely event in the tropical ocean. Nor, on the contrary, is it difficult to model how a cod stock having 10 mature year-classes, and 10 more coming on, might maintain an adequate reproductive potential to bridge poor-recruitment gaps extending even as long as a decade.

Perhaps this ability had already been lost at least 10 years prior to the collapse of the Labrador–Newfoundland cod, when most of its biomass was already concentrated in less than half the number of year-classes that existed in the unfished stock. Catches during the final five years appear to have depended on the arrival of individual year-classes, almost as if a simple aquaculture system was being culled. Exactly how the TAC came to be set at 190 000 tons in 1991, only one year prior to the stock collapse and the fishing moratorium, is not for me to enquire. Now, six years later and in the absence of a fishery, a further decline in stock size is not precluded and so we must begin to consider the possibility that something irreversible may have occurred.

Now, this finally gets me to where I wanted to be. To a very simple-minded re-statement of Lotkas axiom, and a suggestion that any fishery is sustainable only if the age structure of the stock is not so truncated that reproductive potential cannot be maintained across appropriate periods of recruitment failure. This should be no trick in tropical seas, perhaps, but a potential show-stopper in fisheries for long-lived, large species in sub-polar seas where highly variable recruitment is normal.

It is to emphasise that mathematics alone cannot determine where the boundary of sustainability lies that I have used a quotation from a Russian engineer as the motto of this essay. The mathematical models that are customarily used today to determine the appropriate catch from a fishery do not compute the probability that a proposed TAC will breach the boundary of sustainability – and I believe there is no immediately obvious way in which this can be done quantitatively.

Lest by now you think that I have succumbed to the temptation of joining those who insist that poor management alone destroyed the Labrador–Newfoundland cod, let me hasten to assure you that such is not my intention. In earnest of which, I remind you that there is plenty of evidence that a decadal-scale change in flux of cold-water onto the Grand Banks after the late 1970s changed the size-structure of populations of demersal fish having very low rates of fishing mortality, such as American plaice (*Hippoglossoides platessoiodes*). Towards the end of this period, a tendency was observed for plaice and perhaps other species to shift their population centres into deeper, warmer water refuges. Note that all this occurred during the same period in which the age-structure of the cod stock was truncated, apparently by fishing mortality (Bowering et al., 1997; Haedrich and Barnes, 1997).

So finally, let us compare how unfished and fished stocks of northern cod might have reacted to exclusion from the northern Grand Banks during a period of anomalous entrainment of Arctic water in the Labrador Current. Although we cannot be sure, we can guess that both would have retreated to thermal refuges, remembering that there is a lower limiting temperature that constrains their distribution. This fact was fixed in my memory forever by spending five weeks in 1956 with Ray Beverton off Spitzbergen, counting green blips on the echo-sounder and reading reversing water bottles.

An unfished stock that would have fled to warmer water would have included >20 year-classes, of which four or five would have been strong ones, and it could have withstood a good long siege, perhaps to come back in force from deeper water when conditions over the shelf improved. But how different the case of the remnant stock of 1992, able to flee to a thermal refuge with only half a dozen year-classes, of which perhaps only one was the remnant of a strong recruitment. Such an age composition seems self-evidently to be beyond the envelope inherent in Lotkas argument within which we can expect return to the *status ante quo* after unusual stress is removed. It will certainly be instructive to see what will become of the remnant population of 20 000 tons presently holed up in the fjords of Smith Sound and Trinity Bay.

And there I rest my case: there is, it seems to me, an urgent need – given the unsatisfactory state of many North Atlantic stocks of groundfish – for a debate on the iconoclastic question: “Is it is reasonable to use, in all seas, routine and standard stock assessment procedures, or must we significantly adjust our norms for acceptable fishing mortality for cold water species?” If my thesis (which surely cannot be original?) has any validity at all, then we may conclude that industrial fisheries are unlikely to be indefinitely sustainable at high latitudes, so that other ways of exploiting cold-water fish will have to be devised.

However, given how stock assessment scientists trust mathematics and seem diffident of simple ecological reasoning, and given also how skilled the fishing industry is in political manipulation, it is predictable that cold water fisheries will continue to be managed as if they were indefinitely sustainable. So I expect that social and political crises will recur when other cold-water stocks unexpectedly collapse, and that fisheries administrators will be left scrambling to explain what went wrong, just as they are in Ottawa today.

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