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THE NATURAL HISTORY OF FISHERIES MANAGEMENT

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The history of attempts at management of fisheries is presented. Current knowledge of the biology of fish populations is critically reviewed as such data have been the basis of schemes of management in the past. It is concluded that social, political, and economic factors are at least as important in fisheries management as the scientific knowledge of the resource.

Introduction

The 1970's have seen a global upsurge of interest in the subject of fisheries management to a degree exceeding anything previous. The fisheries of Atlantic Canada have played a leading role in the sequence of events that have generated this interest and, perhaps for the first time, there is now an awareness and a growing expectation here of the extent to which fisheries offer economic opportunities for the area as a whole. As a result, the management question is being studied by more and more individuals and groups representing a wide range of interests and disciplines. Inevitably, a "science" of fisheries management is being erected and attempts are being made to reduce the general question of fisheries management to elements that are amenable to formal analysis.

It is my belief that fisheries management represents such a complex mixture of certainty and uncertainty, dreams and realities, theory and actuality, that it is premature and even misleading to apply the term science to its practice. We should be content with the less prestigious but far more appropriate terms "natural history", with its connotations of judgement rather than deduction, assessment rather than analysis, and fore-sight rather than prediction. Why we should be relatively modest in our expectations, or rather realistic in our aims, can best be demonstrated by referring to 4 kinds of questions that can be asked of a fishery.

These can be arranged in a hierarchial series beginning with the simplest:

1) How many (or how much) fish? the biological resource question.
2) How many dollars? the economic question.
3) How many jobs? the social question.
4) How many votes? the political question.

At each of these levels, we can ask a further question: How are they allocated? This adds another degree of complexity, but even without this added degree, it must be conceded that one soon reaches the stage where formal scientific analysis must fail as a guide to decision and action. This is another way of saying that the theory becomes more and more divorced from reality.

Management Theory

Indeed, formal theory becomes less and less structured as you pass from one level to the next. Biological resource theory is by now very rich and detailed and has become tailored to deal with a wide range of situations.

There are two kinds of model in general use, the "biomass" or "logistic" type and the "analytical" or "dynamic pool" model, of which the second is the more detailed and sophisticated. The main stumbling block to a precise analytical assessment of stock resilience under fishing pressure is the "stock-recruitment" question. Every fishery reduces population size but there is no universal agreement on the effect of this on reproductive success and the ability of the reduced stock to replenish itself by "recruitment".

At the second level, the economic information required is both more demanding in its scope and less precisely defined. In anything less than an efficient totalitarian state it is also less easily available. It is not surprising then that the formal economic models of the analytical kind that are most often employed tend to be derived from those originally used at the resource level and do not usually go beyond the primary level of fishermen's costs and profits (Schaefer 1957).

One of the main kinds of difficulties that the numerical economist has to face can be demonstrated by reference to an attitude displayed by some herring fishermen in 1976 in the Bay of Fundy, when for the first time, the opportunity of "over the side" sales to foreign vessels at a premium price to the fishermen was made available. On several occasions, given an option of selling a particular catch to the foreign vessels or landing the fish at a Canadian plant at significantly lower prices, the catch was landed in Canada. As it was the objective of a major government sponsored project to increase the catch value to the fishermen, subjected to an intense cost-price squeeze, this decision was inexplicable to one of the organizers of the scheme. To the individual fisherman, the explanation for his decision was eminently rational. He knew that the particular Canadian market would still exist in 1977 but could not be sure that the foreign boats would be coming back. Faced with a choice whose outcome would be widely and publically known within the industry, he chose the Canadian plant. The assumption of optimization of profit, the basis of economic models, would not have predicted this as an outcome. Moreover, a full explanation of the reason for the choice would involve a detailed analysis of the history of the fishery and of the social and political attitudes of interest groups within it, of current policies relevant to conflicts of interest at both national and international levels, the current market situation, etc. When all this was properly dealt with, there is still the question of individual choice by people of notoriously independent attitudes. Analysis could not pretend to predict the decision that was made even in a statistical model. It might account for the actual decision after the fact.

Limits to the Relevance of the Modelling Process

There are more fundamental reasons why the potential for the analytical modelling of fisheries is severely limited. These are related to the mechanics of the decision making process at each of the levels; the extent to which the authority to make decisions is concentrated, the ability to ensure that a decision is binding on the interest groups involved, and so on. This is usually a function of the political system and this can be illustrated by referring to the Canadian Federal system. It becomes immediately obvious, on inspection of the 4 questions listed above, that authoritative decisions can be made, even in principle, only at the level of the biological resource. At the higher levels, there are political, legal, institutional, and other restraints that do not allow direct intervention to control and determine events. Instead, reliance is placed on incentive or disincentive, the creation of aims common to interested parties or other persuasive tactics. At the third and fourth level, both as optimization and allocation problems, the questions tend to become aims whose achievement results from the mobilization of effort within the system as a whole. For example the following quotation, from a Ministerial speech made in March 1980 at a meeting of the United Maritime Fishermen at Moncton, N.B., represents a policy statement with the clearest implications:

> "The allocation of fish must come from a mixture of reasons: not just what is "economical" but also the survival of communities, the preservation of a way of life, the needs of the market place, the reasonable well-being of fishermen and plant workers, and so on."

The Historical Perspective

What happens in the next 10 years will establish a pattern that will determine and constrain developments that will affect the future of almost the whole of eastern Canada. No other part of the country is as sensitive to the need for good fisheries management. It is important, then, to appreciate how we arrived at the current position so that we can discriminate between constraint and opportunity. "Those who do not study history are forced to repeat it" is a more accurate comment than "history is bunk".

And, to emphasize the main characteristic of the current situation, I may quote from Ophuls' (1977) book, *Ecology and the Politics of Scarcity*, p. 8:

> "The habitual condition of civilized man is one of scarcity. Goods have never been available in such abundance as to exhaust men's wants."
>
> "The institution of Government, whether it takes the form of primitive taboo or parliamentary democracy therefore has its origins in the necessity to distribute scarce resources in an orderly fashion."[1]

The first quotation applies with particular force to the fisheries situation because fish move freely in a 3-dimensional continuum that surrounds all of the continents. By the same token the crossing of administrative borders creates problems that increase exponentially with the number of borders crossed. Witness the management scene in European waters (Korringa 1978)!

The Development of the Resource Problem

It can easily be demonstrated that "official" fisheries biology, especially in eastern Canada, has been directed towards obtaining acceptance of a very simple biological thesis.

Fish as a renewable resource is not unlimited in abundance. This thesis can be erected into a Fundamental Law of Availability.[2] "As long as the world remains civilized it will always be technically possible to take more fish from the sea than is either desirable or advisable." The corollary of this law ought then to be: "pressure will *always* develop to encourage overexploitation." It could be said that the essence of sound management is to prevent this expectation from being realized; sophisticated management absorbs pressure rather than reacts to it.

In the northwest Atlantic the FLA was finally accepted, or at least was paid lip service by "foreign" fishing interests, in the 1970's. This happened because Canada and the United States of America could argue jointly and persuasively from the privileged position of neighboring coastal states united in a common cause. The forum in which the drama was played out, ICNAF,[3] was an international one, especially designed to achieve this goal. But the same lesson has to be reinforced nationally if we are to benefit fully from the jurisdictional vantage point we now enjoy.

There is neither space nor reason to review 1,000 years of history in detail and I will, instead, list a series of statements that conveniently brings us up to the twentieth century (see McFarland 1911 for an excellent treatment).

(1) Fisheries have been the subject of international controversy for 1,000 years or more but, until recently, not at the resource level.

(2) The initial colonization of eastern North America and the exploitation of the northwest Atlantic fisheries were very closely inter-related.

1 It will be noted that he says "orderly" and not "equitable."

2 In the Age of Acronym (AA) this becomes FLA.

3 International Convention for the Northwest Atlantic Fisheries—now replaced by NAFO, Northwest Atlantic Fisheries Organization.

(3) The current social structure of Newfoundland still reflects the events of the period when European "English" fishery interests were determined to prevent local colonizers from competing successfully to exploit the rich Grand Banks fisheries.

(4) The New England fishermen in the first half of the 18th century (and by their direct intervention) helped create eastern Canada and Nova Scotia as we know it today. This was because they wished to protect their fishing interests threatened by Continental (and pre-revolutionary) France.

(5) The American Revolution - or War of Independence - was caused largely by the almost catastrophic economic effect the Sugar Act of 1764 had on the New England fish trade to the West Indies, on which their economic base largely depended.

Despite all this, the question was not primarily a *resource* one. Some individual whale stocks and other marine mammal stocks had been decimated in some areas, but "finfish" stocks were not depleted. Indeed, as recently as 1883, Thomas Henry Huxley, Darwin's Champion, who was reporting on fisheries for the British Government, could be quoted[4] as saying:

> "The cod fishery, the herring fishery, the pilchard fishery, the mackerel fishery, and probably all of the great sea fisheries are inexhaustible. Nothing we can do can seriously affect the numbers of fish. Any attempt to regulate these fisheries seems consequently to be useless."

He was wrong, of course, but why did he say it? And why was he wrong? He said it because finfish tend to produce large numbers of eggs; from thousands for a large salmon, tens of thousands for large herring, hundreds of thousands for a large mackerel or haddock, a million or so for a large cod, 5 to 10 million for a large tuna and hundreds of millions for a large sunfish. He thought this large, even enormous, investment in reproduction conferred immunity from man on fish populations. It does not. Fish do not produce large numbers of eggs for man's benefit but for their own, and extinct herring fisheries provide mute evidence that this is so.

So that is was not until the 20th century that any real note of caution was sounded and incorporated into an instrument designed to achieve specific resource-oriented aims.

The Twentieth Century Fishing Treaties

On October 21, 1924, Canada and the United States ratified a treaty for the preservation of the halibut fishery of the northern Pacific Ocean, including the Bering Sea (Babcock et al. 1931). This treaty was remarkable in 2 ways. The fishery had begun about 1890 but for the first 35 years or so of its existence it had undergone a period of uncontrolled expansion. As with most renewable natural resources at that time, development and exploitation were the prime aims, and expectations were limited by market demands only; the conservation concept was almost unknown (Bell 1970). However, at about the time of the First World War, the continued expansion of the fishery to new grounds failed to maintain the annual yields that had been previously enjoyed. Prolonged negotiations followed between the United States and Great Britain and the resulting treaty had the honor of being the first entered into by Canada as a nation. For in the meantime, and as a result of her exertions during the First World War, Canada's sovereignty was extended to include authority to act independently in external affairs. It was, in addition, the first treaty that had as its objective the conservation of a high seas fishery that appeared threatened. The relevant portion of the text can be quoted:

[4] See Graham (1943). *The Fish Gate.* Faber and Faber Ltd., London, p. 109.

"The commission shall make a thorough investigation into the life history of the Pacific halibut, and such investigation shall be undertaken as soon as practicable. The commission shall report the results of its investigation to the two Governments and shall make recommendations as to the regulation of the halibut fishery of the North Pacific Ocean, including the Bering Sea, which may seem desirable for its preservation and development." (Babcock et al. 1931, p. 7)

The commission referred to was the International Fisheries Commission, created under the terms of Northern Pacific Halibut Treaty. The Treaty or Convention was revised 3 times, the last time being 1953 when the Commission was renamed as the International Pacific Halibut Commission (Bell 1970).

However, you will note that the original Convention did *not* mention anything that implied a specific quantitative or "scientific" aim; no term such as maximum sustained yield was used. This was not mentioned until the 1953 revision of the treaty, and by then a lot had happened.

The Twentieth Century Fisheries Theory

It is now recognized that much of modern quantitative fisheries theory was anticipated by the Russian scientist Baranov in pre-revolutionary Russia (Baranov 1918; Larkin 1977; Ricker 1940). However, to trace the *effective* history of fisheries theory, we must look to developments that began in the 1920's by an American economist, Pearl (Pearl & Reed 1923). He had been studying the history of the development of industry and of population demographic changes associated with it. Pearl found that increases in human population size over a period tended to follow a flat S-shaped Sigmoid curve.

The particular kind of quantitative analysis to which this led, entered fisheries biology by the back door. In fact, it is probably more correct to say via the brewery, because it was next heard of in connection with a study of increases in cell numbers of laboratory populations of Brewer's Yeast, *Saccharomyces cerevisiae* (Klem 1933). These were produced by the Frydenlund Brewery in Oslo for a student of Professor Johan Hjort, a famous name in 20th century fisheries biology. It was Hjort who realized that a very suggestive model was at hand to develop badly needed quantitative theories to answer a question he himself had formulated:

How can we determine"the relation between the toll levied by man on a given stock of animals and the capacity for renewal shown by that stock" (Hjort et al. 1933)?

His model was built up by considering a hypothetical closed area of sea, suitable for supporting whales but without any present. A few are introduced, being careful to ensure that there is at least one of each sex; these increase in number as time goes on until a certain number is reached that represents a limiting population level, the saturation level for that enclosed area of sea. The rate of increase in numbers, he maintained, would follow the same pattern with time as had been found for enclosed flasks with limited nutrients into which yeast cells had been introduced.

The argument was: At first, because the number of animals is very low, the rate of increase is very low; at equilibrium, because the environment is saturated, the rate of increase is zero. In between the rate of increase rises to a maximum and then declines. And here is the important mental switch; the idea that Hjort, in 1933, had, and what Graham, in 1939, claimed was "a remarkable piece of insight" (Graham 1939).

The rate of increase under these hypothetical conditions can be equated to the equilibrium yield from the population, in this case, of fishes. Animals cannot be removed without depressing the population size. At environmental saturation (the "virgin-stock" state), removal of animals at any given rate will take you down the curve to a population size at which the rate of increase and the rate of withdrawal are equal. This is an equilibrium yield, and there is one unique point which defines an equilibrium yield that is a maximum. This is the origin of the *Maximum Sustained Yield* concept. This aspect of the model was developed by Michael Graham and Figure 1 is taken from his 1939 paper.

Graham made a very significant change, instead of numbers he used weight as his population criterion. This is more appropriate as weight of yield is the main economic aim of fisheries. He also derived a maximum equilibrium yield, which could be properly called a "Maximum Sustained Yield" which on the assumptions of the model, can be defined quantitatively as to the population size at which it occurs. This is 50% of the original population size. As in the simplest case the catch for a given unit of fishing effort is proportional to the population size, the point at which the catch rate has declined to half the original catch rate is the MSY point.

This kind of model has since been called the "Biomass Model," treating the population as a single unit of weight, or the "Logistic Model" because it was found that the mathematics had already been worked out in the 19th century under that name. It represents one of the 2 main types of fisheries models that are in use and was first applied successfully to analyze a tuna fishery in the 1950's by Schaefer (1954). It has since been modified and generalized and the generalized version is called the General Production Model (Pella & Tomlinson 1969). However, the Generalized Production Model does not have a single, unique MSY but a whole family of them. Far too much of a good thing some people might say!

As another point of interest, neither Hjort nor Graham refer to Maximum Sustainable Yield. They both talk of an optimum yield or, more exactly, optimum catch. Hjort says, "Here, then, there is every possibility for an intelligent community to create an industry based upon an optimum catch" (Hjort et al. 1933), the phrase also used by Graham. But Graham was also very much aware of the other aspects of management with which I began this paper. I quote him directly, "No means whatever can bring permanent profit to the industry unless the rate of fishing can be controlled" (Graham 1939). "Only temporary prosperity can be expected until there is some international arrangement to prevent the rate of fishing increase" (Graham 1939).

He was, at the time, referring to an international situation centering on the North Sea Fisheries but, meanwhile, thousands of miles away, something had happened that was to lead to what we see now as modern fisheries management in this part of the world.

The North American Scene

In the early 1930's and off the west coast of North America, a Japanese fishing vessel was sighted. This is the very area of the only international fisheries convention that existed, the I.P.H.C. Convention area. This was the first of the "Distant Water Fleets" to come fishing off the North American coast. It stimulated tripartite discussions between the United States, Canada, and the United Kingdom, all of whom had an interest in developing an international body that would control fishing. It is a rather odd fact and one to ponder over that although ICES[5], an European-based scientific body concerned with fisheries biology, had been in existence in Europe for 30 years or more, it was not then in any way concerned with management functions. Management was not even considered until 1959 when NEAFC[6] was formed.

5 International Council for the Exploration of the Sea—founded in 1904.
6 North East Atlantic Fisheries Convention.

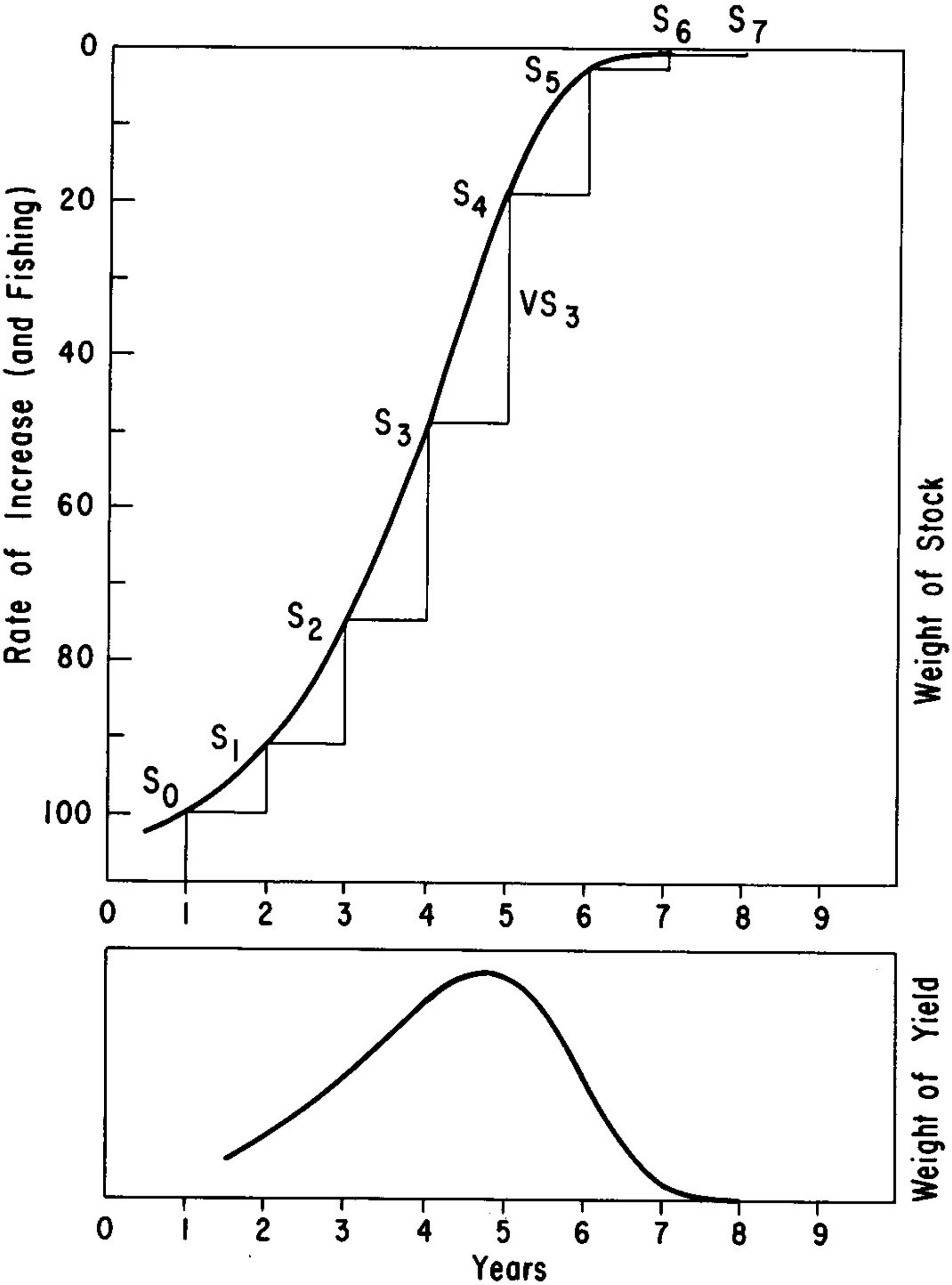

Fig 1. Graphical derivation of an S-shaped curve. (Graham 1939, p. 18)

The Formation of ICNAF

Negotiations were halted by the war but, as early as 1943, a meeting was held in London, England, between the 3 countries, to which Dr. A.W.H. Needler, a famous name in Canadian fisheries, was conveyed in a bomber being ferried across the Atlantic. This is a remarkable demonstration of how seriously the matter was being considered.

It would seem that, whereas the United Kingdom wished for an international fisheries convention with the broadest possible global approach, the United States was anxious to restrict it to the North Atlantic and to include an area off the east coast of the United States, in the general Gulf of Maine area particularly, where its traditional historical interests were concentrated. Canada blandly took the view that she would sign anything, and this, it would seem, stimulated the United States to sponsor a Convention and do all the work in its preparation as soon as possible. The Convention was ready by 1949, and was signed on February of that year at an inaugural meeting attended by 11 countries. It came into force on 3 July 1950 when the fourth of the original signatories had ratified the agreement in their parliaments. This method of activation was written into the Convention and Canada was, in fact, the fourth to ratify. And hereby hangs a tale.

The original signatories on 8 February 1949 included both Canada and, separately, "His Majesty's Government in the UK and the Government of Newfoundland in respect of Newfoundland." The ratifying Government, however, as listed in the Convention itself, was "Canada (including Newfoundland)." Newfoundland had joined the Federation in the meantime and, indeed, this was the first treaty signed by Canada as an entity which included Newfoundland. The important point is that the 4 founder members enjoyed certain privileges that later signatories did not have.

The area covered by the Convention was carefully defined and divided into 5 subareas, each of which was a management unit under its own "Panel." To be a member of a Panel and thus to initiate and influence management divisions, you had to have a substantial fishery in the subarea *unless* you were a founder member. Canada qualified as a member of Panel 5 because she was a founder member even though she had no substantial fishery in the important Subarea 5, which included the Gulf of Maine and Georges Bank. Canada and the United States could thus operate jointly over the whole Convention area. A stroke of luck. Or was it?!

The ICNAF Convention had another distinction. It was the first fisheries convention to spell out its management objectives and the means by which they were to be met. This was set out in the famous Article VIII on Regulatory Measures. The Commission, through its member governments, could recommend certain measures designed to keep "the stocks of those species of fish which support international fisheries in the Convention area at a level permitting the maximum sustained catch." The measures were (Anon. 1969):

(a) by open and closed seasons
(b) by closing certain areas
(c) by establishing size limits
(d) by prohibiting kinds of gear
(e) by establishing quotas

And, a most important point, the recommendations had to be made, specified clearly in the text of the convention, "on the basis of scientific investigations" (Anon. 1969). It became obvious in the work of the Commission that this meant *only* on the basis of scientific investigations. No social, political, or economic factors could affect or influence the advice presented by the scientists.

But this is the first mention of the term "maximum sustained catch."[7] It was not introduced with capitals as, for example "Maximum Sustained Catch," and was not defined anywhere in the Convention. The regulatory measures were "designed to keep the stocks of those species of fish which support international fisheries at a level permitting the maximum sustained catch"; no implications that single species management was obligatory. These points need to be stressed because of the enormous quantities of paper that have been used up in the discussion, analysis, and criticism of the Maximum Sustained Yield (MSY) as a management objective. A full treatment of the MSY debate at this time would be inappropriate, but it is worth pointing out that most of those pontificating about the advisability of revising management aims to avoid reliance on MSY have had very little first-hand experience in the management of complex international fisheries, such as those of the east coast, or knowledge of the efforts being made to conserve and develop them for the benefit of Canadians (and Americans one must add!). And without these efforts, the management of our fishery resources as we now know it would not have been possible.

But to deal specifically with the question of the management aims of ICNAF in relation to "MSY" or msc, it is necessary to go back to the 1930's.

Analytical Models

The logistic model, or the sigmoid curve model, was not the only kind of model being investigated, even in the 1930's. As early as 1931 E.S. Russell was examining the question from another point of view (Russell 1931). He considered a completely self-contained stock of fish of one particular kind which is being fished by a standardized gear that catches fish only when they have reached a particular length. This divides the stock into 2 groups which were called the "catchable stock" and the "non-catchable stock."

A catchable stock has, for example, an initial total weight of S, at the beginning of a year; during the year it will be joined by "recruits" that have reached the catchable size. Also, the individuals in the catchable stock will grow. Both these factors increase the stock size. During the year, fish will die from various causes and some will be caught.

We thus have a balance sheet and a means of following changes in the catchable stock.

$$S_2 = S_1 + (A + G) - (C + M)$$

where:

S_1 is the weight of catchable stock at the beginning of the year
S_2 is the weight of the catchable stock at the end of the year
A is the weight of recruits joining the stock during the year
G is the growth of all (surviving) individuals of the catchable stocks for the year
C is the weight of the catch for the year
M is the weight of all fish dying from natural causes during the year

Of course, all that this does is to simplify the picture to the degree that it becomes manageable mathematically. It is something that biologists, working in a difficult subject, often have to do for the benefit of mathematicians, physicists, and chemists who are working in the "easy" sciences.

7 Item 1 of Article VIII of the ICNAF Convention states: "The Commission may, on the recommendations of one or more Panels, and on the basis of scientific investigations, transmit to the Depositary Government proposals for joint action by the Contracting Governments, designed to keep the stocks of those species of fish which support international fisheries in the Convention area at a level permitting the maximum sustained catch by the application, with respect to such species of fish, of one or more of the following measures . . . ." (Anon. 1969).

It remained to express the rates of the various separate factors in appropriate form and the Beverton and Holt model (Beverton & Holt 1957) is as convenient as any to follow because of 3 things which they did.

Firstly, they *excluded* recruitment in their basic model because it was obvious that the number of fish surviving each year, out of billions of eggs produced by any reasonable sized stock, varies, apparently in an unpredictable way. This was found to be true for almost every stock and species studied. They expressed their results in terms of the individual recruit, the average fish-in-the-sea. The term "Yield Per Recruit" should be familiar to anyone having nodding acquaintance with fisheries. Secondly, they distinguished between a length at which fish entered the fishing area and the length at which they would be liable to be caught. This latter depended on the mesh size used in the cod-end of trawls, the trawl fisheries being the most important considered. This allowed them to use the model to study the effects of varying the mesh size, of controlling the size at which fish are first captured.

Thirdly, they chose a model for fish growth, already available, that gave an expression for growth and mortality in a simple mathematical form. Eventually, though not to begin with, this led to some very nice simple relationships. The model essentially balances growth and mortality and can be simply illustrated as in Figure 2. For explanatory purposes, we consider a number of individuals of a single years spawning, a "brood" or "year-class", of a stock of a species of fish at an early life history stage that is subject to a steady mortality drain from natural causes. This "natural mortality" results in an exponential decline in numbers. However, each individual is increasing in weight, and in a broadly sigmoid pattern with time. This means that the total weight of the year-class, the biomass, changes with time. A little thought should show that the biomass of a year-class must increase to a maximum and then decline, eventually to zero. The shape of the biomass curve with time will depend on the shapes of the mortality and growth curves. But as long as the numbers are large and size small to begin with, then the total weight of a year-class over the life history will rise to a maximum and then decrease.[8] If we were considering an aquaculture operation and the year-class was a batch of young all introduced into the pond at the same time, the maximum biomass point would be the ideal point to drain the pond and harvest the total crop, if maximum yield was the object.

The Analytical Maximum Sustained Yield

The situation with natural populations is different. If it is possible to selectively catch fish at the size associated with the biomass maximum, it becomes conceivable, in principle, to get the maximum yield from year-classes. A trawl net of suitable mesh size could be chosen to select the size, but to crop all fish as they reach that size would demand an infinitely high *rate* of removal—of fishing effort. This is impracticable, and the pioneer workers, like Russell, Hjort, and Graham were aware of it. Ricker also, appreciated this argument and worked out a way to calculate a size of fish smaller than this critical size, and thus to achieve an optimal catch level at a more realistic degree of effort (Ricker 1945).

The fact remains, however, that it is the biomass maximum that, in the Beverton and Holt model, best corresponds to the ideal concept of Maximum Sustained Yield. As far as I am aware, this point has never been made in discussing the concept. Nor is it at all well known that the formula defining this maximum, in terms of the length at capture that corresponds to it, was worked out by Beverton and Holt (Holt 1958; Beverton & Holt 1966) and remains one of the simplest and neatest formulas in

[8] This will be so long as the average weight of the ripe female gonad is less than twice the total weight of the female.

fisheries biology. The length is referred to as the Critical Length and is defined:

$$\text{Critical Length} = L\infty \times \frac{3}{3 + M/K}$$

Here, L∞ is a measure of the limiting (maximum) size of the fish concerned, M measures the mortality rate and K, the growth rate. If, in suitably scaled units, M and K are equal, the critical length is 75% of the limiting length.

As an "ideal" standard this Critical Length has much to commend it.

Three yield curves are depicted in Figure 3. Each of these yield curves represent the equilibrium yield at a given length at capture and over a range of different levels of sustained effort. For each length at first capture there is a maximum sustained yield which, for any length equal to or greater than the Critical Length, is approached asymptotically. Below the Critical Length the maxima are reached at intermediate levels of effort.

Figure 3 has great pedagogic value but, as must be pointed out, the model was not available at the time of the drafting of the Convention and the objective "maximum sustained catch" was not defined. Indeed, I am doubtful whether the people draft-

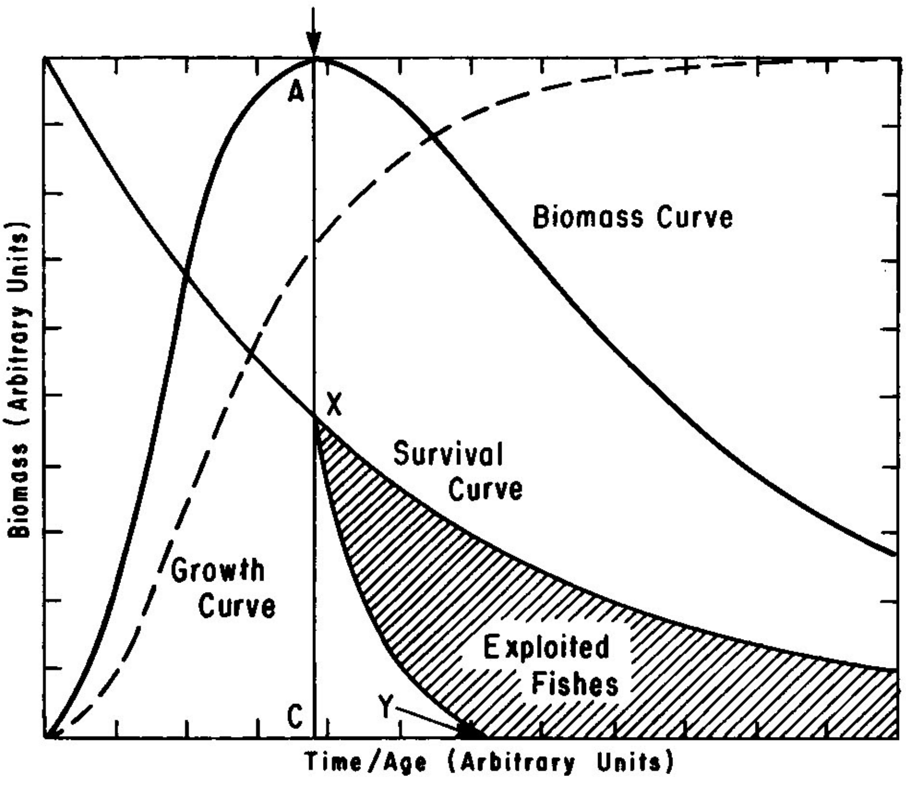

Fig 2. Model for an exploited fish population. The point of maximum biomass, A, corresponds to a particular size—and a particular age, C. At this point increase in biomass resulting from growth equal losses from mortality. The maximum possible yield would be achieved if instantaneous cropping began at X and no fishes survived beyond this point. In practise a finite fishing effort generates a new survival curve, XY, between the original survival curve and the axis XC. (Fryer & Iles 1972, p. 426)

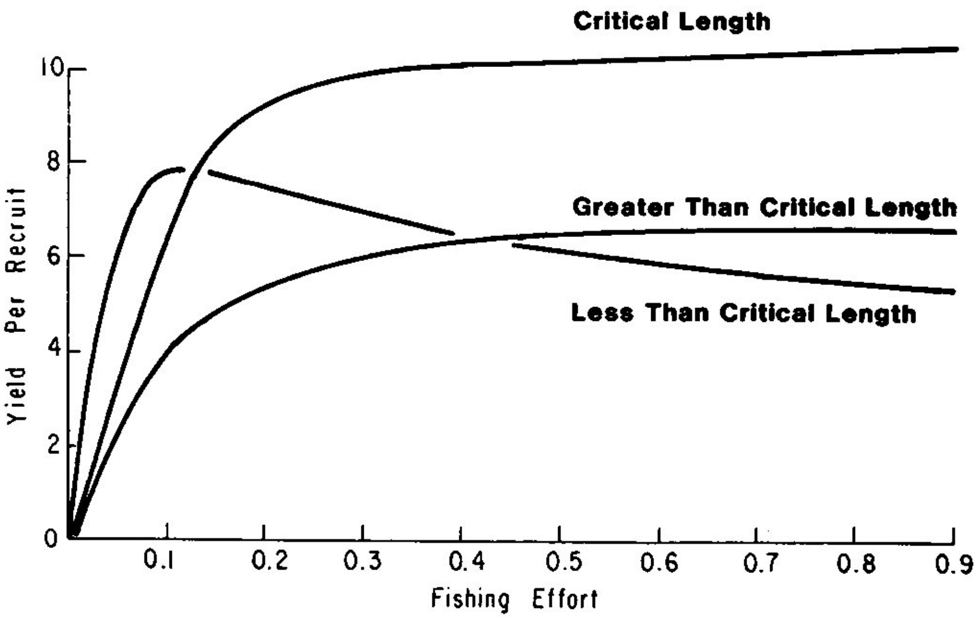

Fig 3. Yield per recruit curves at different lengths-at-first-capture relative to the critical length: Beverton and Holt (1957) model.

ing the Convention had anything specific in mind. As did both Graham and Hjort, they could well have used, instead, the term "optimum" in the sense that it represented a "best" under the circumstances.[9] And, in fact, it was not too long before any argument became academic.

The Extension of Jurisdiction and ICNAF in the 1970's

In 1970, Canada and the United States concluded an Agreement on Reciprocal Fishing Privileges. On the east coast, this was concerned largely with an industry based on juvenile herring caught along the New Brunswick and Maine coasts and which, in the mid-1960's in the United States and the late 1960's in Canada, had declined substantially; by 1971 these fisheries appeared threatened by economic collapse and the factor held responsible was an upsurge of foreign fishing on adult herring.

This was an almost classic example of traditional fisheries conducted, in this case, largely within the 3-mile limit, apparently being threatened by a recently developed offshore fisheries conducted by "distant water fleets." As a test case, they provided both the reason and the opportunity for a coordinated Canada-United States attack on the wider problem of establishing control over an accelerating expansion of effort by distant water fleets in the northwest Atlantic as a whole. In the view of Canada and the United States this was threatening the fisheries of the coastal states. The Reciprocal Agreement provided an umbrella for the preparation of joint Canada - United States proposals to ICNAF to limit herring catches in the Bay of Fundy-Gulf of Maine area, and these were presented at the Annual meeting of ICNAF in June, 1971, in Halifax. They failed to gain acceptance and to explain why this was so, introduces the important question of sub-allocation.

9 Optimum: best; most favorable or most conductive to a given end, especially, under fixed conditions (*Websters New International Dictionary*, unabridged, Second Edition, 1960).

As described above, the ICNAF Convention, as originally drawn up in 1949, had a "regulatory" section, Article VIII,. which specified what measures could be undertaken by the Commission to achieve what aims and under what circumstance.

The relevant aim was "achieving stock levels permitting the maximum sustained catch."

The relevant circumstances was "on the basis of scientific investigations" (and it will be remembered *only* "on the basis of scientific investigations").

In its early years the Commission had not relied on catch limits as a regulatory measure but on mesh size restrictions to control the minimum size of fish.[11] This determines which yield curve would be followed (see Fig 3). It was not until the mid-1960's that a scientific report was produced for the Commission by Templeman and Gulland (1965) and which challenged the validity and the éffectiveness of all 3 components of the ICNAF regulatory apparatus.

It was pointed out, firstly, that the real benefits to be gained by management were in savings in the cost of fishing rather than increased yield and, as a corollary, that "economic and technical considerations" should thus be allowed as a basis for decision making in addition to "scientific investigations." Secondly, it was demonstrated that the achievement of a Maximum Sustained Yield, and either a stable fishery or an economically efficient fishery, were incompatible. This led eventually to the substitution of "optimum utilization" for "maximum sustained catch" as a management aim. It should be noted that the phrase was "optimum utilization" not "optimum yield", and not "optimum sustained yield" either. And it should be remembered that treaties are documents in which words and phrases are very carefully weighed as to their implications. Thirdly, it was pointed out that catch limitations would not be effective unless the Total Allowable Catch[11], the "global" quota, could be suballocated, each interested group, in this case of course, each country, being given its share.

The Origin of Sub-allocation of TAC's

These points were all accepted by the Commission in 1969, but by mid-1971 the necessary changes in the Convention had not yet been ratified by the member countries and suballocation was not available as a part of the regulatory apparatus. Thus, although everyone agreed at the 1971 Annual Meeting on the seriousness of the herring situation, it was impossible to agree on an acceptable solution. This was because each of the national fisheries concerned had its own characteristic seasonal pattern, which clearly favored any country whose season occurred early in the quota year rather than another. There is an important point to be made here. Anyone proposing regulation at ICNAF in terms of any aim, or by using any method not legally part of the Convention at that time, would have been ruled out of order. No one was naive enough to do so although alternative aims, including optimum utilization, had been discussed for years.

Attempts were made to change the timing of the quota year to help solve the problem, but these were unsuccessful and, in view of the urgency of the matter, a Special Meeting of ICNAF, the first of many, was called for January 1972, in Rome, and for which scientists were called to make "extraordinary efforts" in preparation. The Commission gave the scientists 6 whole months to solve problems that had been years in the making!

By 15 December 1971, the "suballocation" change in the Convention had finally been ratified by member countries and became part of the Convention. Agreement

[10] Incidentally, this committed them to the Beverton and Holt type model and to thinking about msc's in Beverton and Holt terms but, in fact, nobody raised the issue as to what msc meant.

[11] The origin of the famous "TAC" concept.

was reached in January 1972 at the Special Meeting in Rome for the 3 major herring stocks in the Bay of Fundy-Gulf of Maine-Georges Bank area. The ability to suballocate was, of course, the key factor allowing this. What is not fully appreciated is that this agreement became possible only because of the amendment to the Convention which removed "scientific investigations" as the sole basis for advice and decision making. There *can* be a scientifically based justification for recommending setting an overall catch limitation; the division of a quota or TAC, however, cannot be based solely or indeed, at all, on *scientific* criteria. Science and the scientist were put in their proper place in the management scheme, and I do not imply that this is any way an inferior position. Indeed, it meant that the advice that scientists had been giving for years in the interests of the safeguarding of the resource, was finally acted on. An essential flexibility of action was introduced to translate advice into administrative action.

I will not follow subsequent events at ICNAF, interesting as they were. Instead, I will consider the problems Canada was faced with in handling its own suballocation.

The Myths of Common Economic Goals and the Maximum Net Economic Yield

It is one more of the interesting paradoxes we see in the recent history of international management is that the inclusion of "technical" and "economicl" considerations as grounds for action, and the way that this then allowed suballocation as a means to achieve control over fishing effort, also ensured inevitably that ICNAF could not pursue common economic goals. Once a country had been allocated a share, it was free to do whatever it chose with it; the argument in negotiation centered on just how big was the allocation. The fundamental differences in political and economic systems amongst the ICNAF countries made the pursuit of a common economic aim impossible, and suballocation, once agreed on, removed the problem of trying to define and establish one.

Again, there was nothing new in this. In 1957 Schaefer had discussed this very point in dealing with the Maximum Net Economic Yield as an alternative approach to Maximum Sustained Yield as a management aim (Schaefer 1957). His argument is worth summarizing and commenting on.

He first maintained that MSY (he referred to the "logistic" MSY) is a property of the population and not the exploiter and, as such, is a (fixed) function of the biology and ecology. This "quasi-absolute" nature of physical yield, incidentally, is a highly desirable feature as a management aim under certain conditions, where for example conflict of interest is such as to demand a "neutral" criterion to set limits within which conflict must operate.

The Maximum Net Economic Yield, MNEY (estimated, it must be stressed, only for the primary sector, the fishermen), on the contrary, depends on the value of and the cost of catching the fish. This varies from country to country.

It is therefore impossible to establish a unique relationship between MNEY and fishing effort and, because of this, to reach agreement or a level of fishing effort based on MNEY in an international high-seas fishery. MNEY is therefore an impossible basis for joint management among nations. It is no easier to rely on within a democratic society, it may be added.

For, within a country, different interested groups have access to the same political and economic system and the quantitative resolution of conflicts of interests which, incidentally, is one of the best pithy definitions of management I know of, has to be done under a very different set of rules which are far less structured and far more equivocal than is so for an international treaty such as the ICNAF Treaty. This dumping of the management problems in the individual country's lap has been referred to as "internalization". This happened with the Canadian fisheries when the program initiated and carried through via ICNAF led to Canada's extension of management control to 200 miles in 1977.

The Canadian Problem

Both biologically and socially, politically and economically the Canadian east coast fisheries form a very complex system with actual and potential conflicts of interest that defy complete analysis. You cannot model these fisheries.

It is worthwhile at this point to emphasize a principle of some significance. No major change in any fishery that results from a management decision (or indeed from any environmental change) can affect all interested parties equally. In the international arena this principle was invoked in another way as the Principle of Equal Sacrifice. No change was acceptable to a country if it was affected adversely to a greater degree than another. The criterion for measuring degree was flexible and it is inevitable that any regulatory change must be differential in *some* way. In international fisheries debates, it allowed a vehement support of the need for resource protection, while at the same time, there could be an expression of heartfelt regret that no practical solution is emerging which satisfies the sound democratic principle of equality of sacrifice. The man in the middle, the scientist, whose advice is the basis for any decision on resource removal is, naturally, held responsible for closing the door to the only commonly accepted solution, the increase in allowable catch that would solve everybody's problems.

The Sanctity of TAC's

One of the most significant of all management developments, occurred at the 1973 Annual Meeting of ICNAF in Copenhagen (Anon. 1973). Eric Gillette had been appointed chairman of Panel 3 to replace the unexpectedly unavailable regular chairman. An experienced negotiator with Trades Unions in the United Kingdom, he was skilled in procedural matters. At a crucial stage of a discussion on the allocation of national shares of a TAC and after the usual plea by each country's delegate that their requirements could be met quite easily by an (insignificant)[12] increase in the TAC, he proposed a motion from the chair. This motion was to the effect that any proposal as to allocation of quota shares that involved increasing the TAC should be ruled out of order by the Chairman. Nobody could object to this without appearing to be careless of the accepted need for conservation, and a practical convention became established at ICNAF that simplified matters and led to greatly increased efficiency in decision making.

This also had the effect of removing the scientist one stage from the controversial aspects of *who* gets the fish, a separation of roles between the scientist (as a scientist) and the manager that many people see as essential to his proper function (e.g., Gulland 1977). It also emphasizes a crucial value of TAC setting in an overall management process. A fishing enterprise in an allocation system is usually interested in how much fish of a particular species it can count on being able to catch. The captain of a Bay of Fundy herring purse seiner wants to know what is his allocation for the season, and the purse seine fleet, what its total allocation is compared, say, with that of the gillnet fleet. The 1980 purse seine allocation of 44,000 MT is still 44,000 MT whether it is allocated from an MSY, an MSC, an OSY, an MNEY, a pre-emptive quota, a by-catch from a silver hake fishery, or an allocation from a global estimate based on all finfish species in the Bay of Fundy Management Area on a Bio-Energetic Multi-Species Ecosystem Dynamics (BEMUSED) basis.

Within Canada a management system is being put together which is capable of

12 The proposed increase was usually not large enough to exceed the upper limit of the scientists' estimates of allowable catch!

dealing with these "internalized" problems and which takes into account the realities of the Canadian fishing scene. What is seen as an essential separation of "scientific" and "managerial" input takes place at the presentation of advice on a catch level. It is difficult to conceive of any other kind of system that might be developed. As it is, any improvement in the scientific basis can be incorporated without disrupting the system as a whole.

No matter what general basis for management is agreed upon, or sanctified as a desirable or essential aim, its translation into effective management action has to consider both the conflicts of interests themselves and the way in which the groups holding specific interests can influence the decision making process at the higher level of questioning introduced early in this paper.

Nor is it possible to ignore the "historical load", the natural resistance to the major changes in the status quo that many suggested alternative systems imply, but whose proponents do not usually openly recognize. How do we get to the new system from where we are?

Within the Canadian management regime the fishery biologist has to go on playing exactly the same role as he always did: objectively trying to answer the resource question stated by Hjort, "How do we determine the relation between the toll levied by man on a given stock of fish and the capacity for renewal of that stock?" There is one major difference between then and now. Then—as we were working to help Canada establish management control and prime access to the resource by getting rid of the foreigners—we were heroes. Now, we are liable to be blamed for the state of the resource as if we caused it and not merely assessed it. It is unfortunate, but true, that Canadian fishing effort, unless controlled and limited, can have as deleterious an effect on fish stocks as did an equivalent amount of foreign effort. This fact of life has not yet been fully assimilated, and because of this, and to finish on a plea, the scientist is more to be pitied than to be blamed.

References

Anonymous. 1969. Text of the Convention, p. 9-21 in ICNAF Handbook. Headquarters of the Commission, Dartmouth, 93 p.

Anonymous. 1973. Report of the 23rd Annual Meeting of the International Commission for the Northwest Atlantic Fisheries, Copenhagen, Denmark, June 5-16, 1978. *ICNAF Ann. Rep.* 23: 37-95.

Babcock, J.P., Found, W.A., Freeman, M., and **O'Malley, H.** 1931. Report of the International Fisheries Commission appointed under the Northern Pacific Halibut Treaty. *Rep. Int. Fish. Comm.* 1: 7-31.

Baranov, F.I. 1918. On the question of the biological basis of fisheries. Nauch. issledov. ikhtiol. Inst. Izv., I(1): 81-128, Moscow.

Bell, F.H. 1970. Management of Pacific halibut, p. 209-221. In: *A Century of Fisheries in North America.* (ed. N.G. Benson) Amer. Fish. Soc. Spec. Publ. 7, Washington.

Beverton, R.J.H. and **Holt, S.J.** 1957. On the dynamics of exploited fish populations. *U.K. Min. Agric. Fish., Fish. Invest. Ser. 2,* Vol. 19.

Beverton, R.J.H. and **Holt, S.J.** 1966. Manual of methods for fish stock assessment, Part II, Tables of yield functions. *FAO Fish. Tech. Pap.* 38: 77 p.

Fryer, G. and **Iles, T.D.** 1972. *The Cichlid Fishes of the Great Lakes of Africa.* Oliver and Boyd, Edinburgh.

Graham, M. 1939. The sigmoid curve and the overfishing problem. *Rapp. Proc.-Verb. Réun. Cons. perm. int. Explor. Mer,* 110: 15-20.

Gulland, J.A. 1977. Goals and objectives of fishery management. *FAO Fish. Tech. Pap.* 116.

Hjort, J., Jahn, G., and **Ottestad, P.** 1933. The optimum catch. *Hvalradets Skr.* 7: 55-91.

Holt, S.J. 1958. The evaluation of fisheries resources by the dynamic analysis of stocks, and notes on the time factors involved. *ICNAF Spec. Publ. 1: 77-95.*

Klem, A. 1933. On the growth of populations of yeast. *Hvalradets Skr.* 7: 55-91.

Korringa, P. 1978. European fisheries—an object lesson in economic and ecological mismanagement. *Interdiscip. Sci. Rev.* 3: 335-345.

Larkin, P.A. 1977. An epitaph for the concept of maximum sustained yield. *Trans. Am. Fish. Soc.* 106: 1-11.

McFarland, R. 1911. *A History of the New England Fisheries.* D. Appleton and Company, Agents, New York.

Ophuls, W. 1977. *Ecology and the Politics of Scarcity.* W.H. Freeman and Company, San Francisco.

Pearl, R. and **Reed, L.J.** 1923. On the mathematical theory of population growth. Metron (Rogivo) 3: 6-19.

Pella, J.J. and **Tomlinson, P.K.** 1969. A generalized stock production model. *Inter-Amer. Trop. Tuna Comm. Bull.* 13: 419-496.

Ricker, W.E. 1940. Relation of "Catch per Unit Effort" to abundance and rate of exploitation. *J. Fish. Res. Board Can.* 5: 43-70.

Richer, W.E. 1945. A method of estimating minimum size limits for obtaining maximum yield. *Copea* 1945: 84-94.

Russel, E.S. 1931. Some theoretical considerations of the "overfishing" problem. *J. Cons. Int. Explor. Mer* 6: 3-27.

Schaefer, M.B. 1954. Some aspects of populations important to the management of the commercial marine fisheries. *Inter-Amer. Trop. Tuna Comm. Bull.* 1: 1-329.

Schaefer, M.B. 1957. Some considerations of population dynamics and economics in relation to the management of the commercial marine fisheries. *J. Fish. Res. Board Can.* 14: 669-681.

Templeman, W., and **Gulland, J.** 1965. Review of possible conservation actions for ICNAF area. *ICNAF Ann. Proc.* 15: 47-56.