A LINK BETWEEN SCIENCE AND MANAGEMENT IN FISHERIES

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ABSTRACT

In this paper a link is traced between science and management in that good conservation results from good science and that failure in management may be the result of scientific failure; but management failure is of course not the exclusive province of scientists. The argument is developed from a historical study of practice by fisheries scientists in the International Commissions.

The central problem of fisheries biology is to estimate the catch that can be safely taken from a stock. In Europe the problem was formulated by Petersen (1894) and Garstang (1900), who realized that if catches were too great they might subsequently decrease because the stock had been reduced too much. In the first decade of the present century exploratory voyages were made in the North Sea under the auspices of the International Council for the Exploration of the Sea (ICES). The high variability of the catches "precluded the possibility of any reliable combination of the trawling records" (Garstang, 1904). At the same time, Petersen said that overfishing was not the essential question and that the ICES should study the transplantation of small plaice as a method of conserving the weight of catch. Small plaice were caught in large numbers close to the continental coasts and, in summertime, the discards exceeded the retained catch by a factor of six. Petersen, Garstang, and Kyle (1907) subsequently wrote that "the plaice can be returned alive to the sea, where they . . . grow so much in size and value that the same fishermen who caught them in the first instance have a good chance of recapturing them when they have a greatly increased value." During their adult lives some demersal fish, such as plaice, grow by an order of magnitude or so, and if fished heavily the mean weight of the stock is reduced because the little fish do not have the chance to grow. The problem of growth overfishing as stated by Petersen is to conserve this loss of catch in weight.

The scientific judgment that catches were too variable led to a second judgment that management was impossible. The name of the "overfishing" committee in ICES was changed to that "investigating the biology of the Pleuronectidae and other trawl-caught fish." The solution recommended for the small plaice problem was to transplant them from the continental coasts to feeding grounds on or around the Dogger Bank. The ICES did not discuss the problem of overfishing again until after the first world war.

Management depends on the quality of scientific advice. Good science should lead to good management and failure in management is often due to scientific failure, although failure in management might be due to other causes. It has not been established yet whether the plaice stocks in the southern North Sea needed international management before the first world war, but the lack of management was not based on such a judgment; it was because the scientists could not assess the variability of catches, which was not surprising at that time because statistical techniques were not very well developed. This paper traces similar links between science and management in the subsequent history of fisheries science; the historical information is taken from a study of the development of the fisheries commissions in Cushing (1972).

THE DESCRIPTIVE MODEL

During the thirties, changes in populations were accounted for in the theory of balance; for example, a decrement in stock is compensated by an increment in recruitment per unit stock, and as fishing mortality increases a relative increase in recruitment is to be expected. Thompson and Bell (1934) and Graham (1935) stated explicitly that recruitment would not be reduced in magnitude by fishing at the stock levels normally exploited; they both worked on flatfish and their conclusion was well fitted to flatfish biology, if not to clupeids

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or gadoids. Both plaice and halibut grow by more than an order of magnitude during their adult lives. Cushing (1972) distinguished growth overfishing from recruitment overfishing. In the first the stock loses weight by too much fishing, as Petersen suggested, but recruitment is not affected. In the second, recruitment is affected; it is noticeable in the pelagic stocks at a lower rate of exploitation than in the demersal stocks because the pelagic fishes have less capacity for stabilization, being less fecund.

Populations were well described by the logistic curve, which expressed the theory of balance in stating that any change in the carrying capacity of the environment was compensated by a change in the net rate of increase of the stock. From the changes in biomass in time, average estimates of the two parameters (net rate of increase and carrying capacity) can be obtained. Such models are called descriptive because the parameters are not estimated independently but are derived from the changes in biomass. In fish populations the contributions of growth and recruitment are compounded in the application of the logistic curve, whereas it would be desirable to distinguish them. Both Thompson and Bell (1934) and Graham (1935) concluded from the application of the logistic curve that age determination was no longer necessary. Had this conclusion been applied quite firmly the distinction between the effects of growth and recruitment in fish populations would have become impossible.

Graham's (1935) major explicit achievement was the application of the logistic curve to fish populations. Another achievement, an implicit one, was to encourage the application of the methods of operational research arising from the second world war to fish population dynamics by Beverton and Holt (1957), which led to the solution of the problem of growth overfishing. The logistic curve was developed more fully by Schaefer (1954, 1957). He derived a catchability coefficient from the relation of stock density to fishing effort and used it to obtain catch, which he then related to effort in the form of a parabola. Over a long time period, enough annual observations give an estimate of maximum sustainable yield. The advantage of this method is that the result can be expressed simply and convincingly. The disadvantages are 1) that at least a patient decade of data collection is needed to establish the position of the maximum, given a sufficient spread of fishing effort and 2) that upward or downward trends in recruitment would be distinguished with difficulty.

THE ANALYTIC MODEL

The first analytic model was Russell's (1931) in which the changes in stock were separated into components of growth, recruitment, and mortality. Beverton and Holt (1957) devised a series of models, including the well-known yield per recruit one and the less well-known self-regenerating yield curve, in which they incorporated their stock and recruitment relationships. The catch, or yield, was expressed as a function of fishing mortality and of the age at first capture. The most important point about the yield per recruit model is that the maximum yield is obtained from information on growth and fishing mortality, independently of the catches. There is no need to wait for a long time to establish the curve, and management decisions can be taken quickly, other considerations being equal.

The yield per recruit model was the theoretical solution to the problem of growth overfishing, and the practical solution was to increase the age at first capture with increased mesh size in the trawls. For management it is a clear solution and it is likely that the present agreement on management in the North Atlantic originated in its simplicity. There were lengthy discussions on the science and on the technology, but there are now agreed minimum landed sizes and minimum mesh sizes for a number of species throughout the North Atlantic. It must be said, however, that conservation by mesh regulation is least conservation because it is adapted to the smaller and numerous species like the haddock in the North Sea; larger species (for example, cod or turbot) are not necessarily conserved there as well as they might be.

In the yield per recruit model it is assumed that recruitment does not decline under the pressure of fishing. The argument presented by Beverton and Holt was that recruitment is so variable that the downward trend at low stock would be very difficult to detect. In management there was an unforeseen consequence: that fishing could continue until recruitment was seen to fail. Then, because of the same high variability of recruitment, fishing would continue until it was too late. However, with care, the yield per recruit model can be used when the stock and recruitment relationship is unknown; for example, if fishing is reduced, the yield per recruit will not decline and may even rise if recruitment rises with reduced fishing.

It is likely that if fishing mortality is restrained to the maximum of the yield per recruit curves of most demersal fishes, recruitment would not be very much impaired. However, the curve for the herring is asymptotic and it was tacitly believed that the herring could be fished very hard merely because of the shape of the yield per recruit curve. Another consequence was that the maximum for the cod in the Barents Sea was overshot because in international terms a high catch was needed at the cost of a decreased catch per effort. The recruitment to the Barents Sea stock was severely reduced.

The collapse of herring fisheries throughout the northeast Atlantic and the failure of recruitment to the Arcto-Norwegian cod stock have been attributed to failure in the Commissions. These Commissions, however, are only as good as their scientific advice, and the two great failures are attributable to the unstated concept that fishing could continue until recruitment was seen to fail. Then failure had to be attributed either to natural causes or to fishing, and stocks collapsed while the scientists disputed the two possibilities. If recruitment overfishing had been recognized as a problem, perhaps collapse might have been avoided. The solution to the problem of growth overfishing inadvertently generated the problem of recruitment overfishing.

STOCK AND RECRUITMENT

The dependence of recruitment on parent stock was formulated by Ricker (1954, 1958), but the variability of recruitment is very high and the curve can only be fitted when decades of annual data have been collected. Any decision on how much fishing should be allowed is perhaps delayed for statistical reasons and management cannot proceed. On the other hand, if recruitment were considered to be independent of parent stock, management could start to take decisions more quickly. With hindsight, the assumption made by Thompson and Bell (1934) and Graham (1935) can be justified because their work formed the scientific basis of all international conservation in the North Atlantic and North Pacific. However, the danger of such a procedure is that any decline in recruitment tends to be attributed to natural causes rather than to fishing, and this step has sometimes been taken without evidence.

It is in the nature of the stock and recruitment problem that there should be confusion about attributing decline in recruitment to natural or to man-made causes. However, Cushing and Harris (1973) have devised a method of fitting the Ricker curve which sets confidence limits to the position of the curve itself; the standard deviation of residuals sets limits to the variation of recruitment. Then if stock is near that value which generates maximum recruitment per unit stock, any recruitment that falls below the standard deviation of the residuals has failed through natural causes. If stock is low, such a distinction cannot be made (because any recruitment below the standard deviation of the residuals is zero) and failure is attributed to natural or to man-made causes without evidence either way; however, a prudent manager might prefer to assume that recruitment declined under the pressure of fishing and to take appropriate action in the hope that the stock would recover, as recently happened with the British Columbia herring stock.

The stocks that have failed because of this dilemma are the California sardine, the Japanese sardine, and the northeast Atlantic herring; the Arcto-Norwegian cod stock might fail for the same reason. The failures of the first three were attributed to environmental factors on evidence that is only circumstantial; more recent evidence suggests that failure might have been due to fishing. To pursue the argument further is sterile. The scientific failure was the inability to make clear the distinction between natural and man-made causes. The failure in management was to delay action until the distinction could be made, whereas a prudent manager should have feared the effect of fishing upon recruitment.

In the stock and recruitment problem, long data series are needed before any management decision can be taken. When the development of a fishery proceeded slowly, this may not have mattered because the maximum yield was attained by gentle increments. Today, however, stocks are exploited rapidly, and there is the possibility that recruitment will be diminished before the data are available to describe the maximum sustained yield. What is needed is an analytic model of the stock and recruitment relationship on the lines of that of the yield per recruit one, on the basis of which decisions on management can be made quickly without the laborious acquisition of long series of data.

There are two large fisheries in the Pacific, on the Peruvian anchoveta and on the Alaska pollack. Recently the anchoveta recruitment failed, possibly due to fishing and possibly due to El Niño and perhaps due to both; because there is only about one decade of observations, the cause of failure will probably remain unknown, although it must always be admitted that the fishing mortality is high. In the Bering Sea there is a rising fishery on the Alaska pollack that also had been in existence for less than a decade. The potential managers of this fishery might like to have available now a yield curve before the data are available to describe the stock and recruitment curve. The source of scientific failure here is the inability to generate an analytic stock and recruitment model.

SCIENCE AND MANAGEMENT IN THE COMMISSIONS

When a fish stock fails the question arises whether the failure should be attributed to the Commission charged with its management or to the scientists. There is a distinction between the North Atlantic Commissions and those in the North Pacific. In the North Atlantic the two institutions (International Commission for the Northwest Atlantic Fisheries and Northeast Atlantic Fisheries Commission) are responsible for all stocks exploited in the area, whereas in the eastern North Pacific only those of interest to North American fishermen are conserved. Consequently the Commissions in the North Atlantic cannot disclaim responsibility for any failures that occur in their area, whereas the North Pacific Commissions may be able to do so.

In the North Atlantic, decline of the main demersal stocks has with one exception been prevented. The best conservation has not yet been achieved, but with the use of catch quotas and international enforcement there is considerable hope that conservation will ultimately be very effective. The scientific basis for this was the initial use of the yield per recruit model and in recent years the successful application of first, cohort analysis (Gulland, 1967), and secondly, the Clayden model (Clayden, 1972). On the other hand, the collapse of herring stocks in the northeast Atlantic was due entirely to the scientific failure to understand the nature of the stock and recruitment problem. Both success and failure in the Commissions can be linked to success or failure in the science.

In the North Pacific there are large areas of unregulated fishery, which the North Pacific Commission has not taken under its aegis. The cause of the increase in the maximum sustainable yield of the yellowfin tuna in the area of the Inter-American Tropical Tuna Commission is unknown, although a number of possible reasons have been cited. Halibut are caught by trawl by nations outside the control of the International Pacific Halibut Commission. The question of the offshore exploitation of the Pacific salmon stocks remains unresolved because the boundary between the North American and Asian stocks is not precise and the degree of mixture in the exploited area has not been established.

It remains true, of course, that the stock density of the halibut recovered from 1930 to 1960 by the action of the Halibut Commission, that the offshore exploitation of the North American salmon was prevented by the abstention principle, and that the yellowfin tuna stock is well exploited. Some of the failures in the North Pacific, like those in the North Atlantic, are rooted in scientific deficiencies (apart from the North Pacific failure to consider stocks that are outside the aegis of the Commissions).

The International Whaling Commission failed to conserve the stock of blue whales. The problem was solved in principle for the fin whale by Ruud (International Whaling Commission, 1956) but the solution was rejected by Schlijper (International Whaling Commission, 1957) who said that the age determination was faulty. The Committee of Four evaded this by expressing the results in the form of a Schaefer curve (International Whaling Commission, 1964); Schlijper never saw that age determination was used in the estimation of (recruitment less mortality) which played a considerable part in the solution. Part of the failure to conserve the stock was the delay in reaching an agreed scientific solution.

In contrast to the Whaling Commission, the North Pacific Fur Seal Commission has been well served by its scientists. A form of stock and recruitment relation has been established and the surplus stock is taken each year at somewhere near the best point for exploitation, and some progress has been made towards establishing the nature of the density-dependent control in the population. The Fur Seal Commission is the oldest of the international commissions and its records go back a long way, into the early 19th century.

From this very brief account of the role of fisheries science in the Commissions there is one general conclusion, that when the science is successful the Commissions can work well but when the scientific evidence is confused the Commissions may fail. Of course, failure may occur for other reasons; for example, a proposal in the ICES to close the small plaice grounds in 1923 was rejected by the British fishing industry in 1926.

THE NATURE OF FISHERIES SCIENCE

The organization of knowledge into science is based on the establishment of laws that interlock. Each law subsumes much information and the network of laws comprises the body of the science. Advance in science is made by the addition to or the rearrangement of the network. Any scientific conclusion is judged in relation to the general framework, and it is tested in the premises and extensions of the argument in the network. It is sometimes said that the end of a scientific procedure is to establish a correlation; without denying the use of correlation, the most important point is to establish whether the correlation is likely and how it fits into the general scientific framework.

In a highly developed science such judgments are made frequently, but in a more primitive one like fisheries biology the necessary network has not yet been established. For example, all our information on stocks depends on catches, with the various biases in availability included; independent methods of estimating stock are being developed but they are not yet reliable. In a highly developed science, a number of methods independently yield the same result; fisheries biologists are pleased to estimate fishing mortality but very rarely is more than one method used. Natural mortality is estimated as the difference between total mortality and fishing mortality, and there is very little independent evidence of its magnitude. The information needed is accumulating quite quickly but the science remains a little weak.

Because biological material is highly variable, any biologist needs a working knowledge of statistics. Without denigrating this very real need, the science needs more than statistics, more information, more hypotheses, and more insight. It has sometimes been stated that fish stocks could be assessed by the study of ecosystems rather than by the study of single populations. This is rubbish; it is my view that not enough is known of any one population primarily because we have examined adult animals to the exclusion of the juveniles. I do not mean that we know nothing of fish larvae or 0-group fish, but that we know too little of their growth rates and death rates. With more knowledge of this sort, the problems of the regulation of numbers and of competition might be solved and we might at the same time learn something of how an ecosystem itself is regulated.

It has been said that fisheries science is fully developed and that its techniques are quite reliable. Much is known about the Pacific salmon but it is only a small fraction of what is needed. It has been said that stock and recruitment is the last problem in fish population dynamics. It is the study of the regulation of numbers, of competition between species, and of the variability of recruitment. In other words, it is the central problem of population dynamics. There is a sense in which fisheries biologists have passed through a long apprenticeship before they have embarked on the real problem that concerns them.

CONCLUSION

The international management of fisheries has developed slowly since it started during the second and third decades of the present century. There are many reasons for this, economic, social, and political; indeed the agreement achieved between nations is considerable when one considers all the difficulties involved. One of the reasons for this slow development, but not the only one, is that where the science has failed, so has management. Conversely, where the science has been successful, management can proceed with confidence, other things being equal. One would expect a link to exist between science and management, as it does in other fields.

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