AgEcon SEARCH
RESEARCH IN AGRICULTURAL & APPLIED ECONOMICS

The World's Largest Open Access Agricultural & Applied Economics Digital Library

This document is discoverable and free to researchers across the globe due to the work of AgEcon Search.

Help ensure our sustainability.

Give to AgEcon Search

AgEcon Search
http://ageconsearch.umn.edu
aesearch@umn.edu

*Papers downloaded from **AgEcon Search** may be used for non-commercial purposes and personal study only. No other use, including posting to another Internet site, is permitted without permission from the copyright owner (not AgEcon Search), or as allowed under the provisions of Fair Use, U.S. Copyright Act, Title 17 U.S.C.*

Changing Perspectives in Fisheries Management

G. Waugh & J. Brown
Department of Economics
University of New South Wales

Annual Conference of the Australian Agricultural Economics Society
La Trobe University, Melbourne
9-11 February 1988

Fisheries management has, in recent years, called for a blend of economic and biological inputs. A striking aspect of this linking of the two disciplines has been the degree of antagonism between the economist and biologist. Conferences have often degenerated into bitter debates, and the degree of cooperation between the two schools of thought has been, at best, minimal. As a result, advice to management has nearly always been conflicting.

The heat generated sometimes has been explained as an emotional defence of territory (the magpie syndrome). There is no doubt that economists have been, and continue to be, persistent and successful invaders of other professional domains; a fact that is somewhat surprising to some as successive invasions have been effected from what many see as an extremely vulnerable base.

On reflection, the dearth of cooperation is not surprising given the bitter debates that continue to take place. It seems that this antagonism goes much deeper than can be explained by an emotional defence of the respective professional territories. In the broad, the biologist and the economist work within different philosophical frameworks. The biologist is basically empiricist, the economist to a large extent metaphysical. These philosophical positions are poles apart, and even debating them may well be only a waste of time. The empiricist's position is orientated towards objective things, and there is a feeling that the human mind comes properly into second place. When it comes first it may impose distorting presuppositions upon the outside world. For metaphysical philosophers the functional forms appear to be fixed in a timeless realm, divorced from individual mind and objective things: the metaphysical domain is basically one of functional forms and empty categories.

It is not difficult, for example, to view general equilibrium analysis, with its trappings of perfect competition, economic man, the frictionless state and the like, as metaphysical. This position is an anathema to the natural scientist. Furthermore, the economist's claim of positivism (what is, not what ought to be) to a large degree is an essay in mystification. When the demystifier - and to a large extent this is the role the biologist is playing in this game, even though he may not readily recognize it - comes into conflict with the mystifier, heated bitter debates must necessarily flow. But these debates may well have little to do with the fish. It is essentially a philosophical issue.

At this point it needs to be stressed that much of what appears here it tentative, and to a large extent is an attempt to draw a bead on a problem that, by its very nature, is extremely elusive. It is recognized here, that in drawing these distinctions between the two schools of

thought, there is a real danger that the differences may be overstressed. It is not denied, for example, that valuable empirical work has been carried out by economists. However, the real and fundamental philosophic differences between the two disciplines remain, and prove to be a stumbling block to effective cooperation and the progress of fisheries management. The explicit recognition of these fundamental difference could well lead to a more effective interdisciplinary approach.

It needs to be said there are no hangups here about metaphysical positions. Metaphysicians may speculate as they wish. Whether the forms are metaphysical or are merely abstractions, historically, culturally or are even ideologically driven, is not really of the essence. However, a problems emerges when such positions are implicitly suggested as natural - that is when the changeable has been disguised as necessarily permanent. Such mystification tends to steal from the individual and society the right and ability to change the institutional framework. Economists, at least implicitly, often view their models as eternal and natural. Fish matter only to the extent that they are amenable to the functional form of the model and the elusive shortrun, longrun time span of economic theory.

In what follows there has been an attempt to review the contribution of certain biologists and economists in the area of fisheries management in the light of the differing philosophical frameworks under which each was working. For it is this legacy which has had a permanent effect on the attitudes of successive generations of both economists and biologists working in this field. Explicit recognition of these differences may well be a necessary first step in the resolution of much of the conflict.

The Beginnings

The nineteenth century saw the growth of the empiricist philosophy. The biologists of the time did not, in general, show a great deal of interest in fisheries. They were more interested in life in the sea, where its was found and how it was nourished. Michael Graham (1943), for example, describes the activities on the voyages of the H.M.S. *Challenger* in the 1870's: Then the naturalists sounded unplumbed depths, read temperatures, hauled dredges, and fished for microscopic organisms with fine cloth nets: indeed they enjoyed themselves greatly.' Apart from enjoying their activities they were also reaping the successes of the empiricist philosophy. Things that 'existed' were primary and the raison d'etre of science.

Thomas Huxley, who was a fierce defender of Darwinisn, did however, take an active interest in commercial fisheries. In the Conference of International Fisheries Exhibition of 1883, as President of the Royal Zoological Society, he energetically attacked regulation in fisheries. He proceeded by estimating the relative effects of fishing and nature on fish populations and concluding with his belief 'that the cod fishery, the herring fishery, the pilchard fishery, the mackerel fishery, and probably all the great fisheries are inexhaustible: that is to say that nothing we do seriously affects the number of fish. And any attempt to regulate these fisheries seems consequently, from the nature of the case, to be useless.'

One can only speculate on Huxley's motivation here and there are various views. At the time of writing he was probably correct to the extent that natural mortality of fish stocks far exceeded fishing mortality; the immense increase in fishing power with steam trawlers was still to come. Huxley was also aware of the gross excesses of vested interests, and their ability to secure legislation that protected their own fishing interests. But basically Huxley was an empiricist: there was simply no empirical evidence, at that time, for overfishing. However, the world does, and did change.

Now it is instructive to see how Marshall, as an economist, reflected on the same overfishing problems. In 1890 in his *Principles of Economics* he makes an unusual number of references to the fishing industry, and he was aware of Huxley's stand. However, he takes what is immediately recognized as an economist's approach: '.... some think that a practically unlimited supply can be drawn from the sea by man without appreciably affected the numbers that remain there; or in other words, that the Law of Diminishing Returns scarcely applies at all to sea fisheries: while others think that experience shows a falling-off in the productiveness of those fisheries that have been vigorously worked, especially by steam trawlers...'.

Whether fisheries were being depleted or not was only of secondary importance to Marshall. He is more anxious to apply economic tools (here the law of diminishing returns). Functional forms were primary, objective things (fishes) only secondary. Whether Marshall is correct or not in applying diminishing returns in this case is, of course, another issue.

What is important is that both Huxley and Marshall are locked into philosophical positions that limit their conclusions Huxley cannot legitimately draw the conclusion that regulation would be useless since the functional relations between fishing effort and fish stocks were undetermined. Thinking as an empiricist he could argue that fishing was

not affecting fish stocks at that time. Marshall was demonstrating that different functional forms lead to different conclusions that have different policy implications. But this does not tell us what to do without knowledge of particular circumstances. It certainly should not limit the biologist to functional forms imposed by economic theory.

The point to be made is not so much that one needs a blend of thinking, or that both biological and economic thinking have their place, although both these conclusions may have considerable merit. Rather, the points to be made arise out of a recognition that the two disciplines, biology and economics have different philosophical bases, that the philosophical bases have to some extent shifted in emphasis through time, that these bases may well be historically and culturally determined, and that they of necessity limit our thinking and reflect our value judgements.

Biologists are essentially empiricists. They see objective things as primary - and tend to look for value free solutions to problems - of course the values are built into their thinking, but values have little explicit place in their thinking. Economists have been essentially abstractionists - functional forms and relationships are primary with things and events only secondary. Value judgements are often very important in their models, but rarely explicitly stated leading to a degree of positiveness that is not logically justifiable.

Biological Perspectives on Management

Although the conclusions of Huxley and his supporters may have been correct at the time of writing, the rapid introduction of steam trawlers soon led to falling yields in some of the major fisheries such as haddock and plaice fisheries in the North Sea and the halibut fishery off the west coast of North American

At the outset most scientists subscribed to a view that the key to management was to keep an abundant supply of mature fish to ensure sufficient eggs and larvae to maintain the stock (Russell, 1942, p 71), the so called propagation theory (often associated with the British biologist E.W.L. Holt). Here the theory was that a fish should have the chance to spawn at least once. Mesh size on trawls was seen as the appropriate management tool that would allow the smaller fish to escape and grow.

The Danish biologist Johannes Petersen argued, on empirical grounds, against the propagation theory; more strictly speaking he saw the propagation of the species as being only one aspect of the overfishing problem, and in the fisheries he had examined (particularly North Sea

plaice) it was not a particularly important aspect of the problem. Rather, he found that there was always a large number of young plaice, even when spawners had been heavily fished; further, he argued that the observed decline in the catch was due to smaller numbers of large fish. What was needed was the correct distribution in size of fish: too many small fish reduced the total weight of fish caught, too many large fish reduced their rate of growth.

This concentration on the size of the individual fish was to be a focus of attention by biologists for the remaining part of this sixty year period (although there was to be a large blind alley in the form of what has since become known as surplus yield models). Basically the problem for the biologist was to find the mathematical equation that allowed a calculation of the best size. With hindsight on can see that the difficulty for the empirical biologist was the contrast between the concepts of 'size' and 'best': size was something he could measure, objective and fitting neatly into the empirical framework; best involves value judgements and fits better into the framework of the economist.

Petersen's position then was that the growth of the individual fish was of greater importance, in many cases, than the numbers of spawning fish , a proposition that has since been verified by many other age-distribution studies. In 1931, Russell provided a simple algebraic form of the problem. Russell argued that the stock of fish at the end of a period (S_2) is determined by three basic factors: the stock of fish of fish at the beginning of the period (S_1), the increase in stock due to new recruits (A) and growth increments (G), and the loss of stock due to natural mortality (M) and fishing mortality (C). The equation then becomes

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

In equilibrium, when the stock remains unchanged from one period to the next, the equation reduces to

$$A+G = C+M.$$

At this stage the concept of equilibrium became enmeshed in the biologists' framework. Russell (1931, p 10) warned that the 'ideal of a stabilized fishery yielding a constant maximum is impracticable' However, he was more concerned with the practical task of how to measure stock abundance, concluding (p 20) that '.... catch or landing per unit of fishing time can, if certain conditions be fulfilled, be used as such a measure or index.'

In a strongly empirical paper Graham (1935), used a logarithmic form of the Russell equation to examine the North Sea trawl fishery; for

the fish stock to be in equilibrium it is the rates of increase due to growth and recruitment which must be equal to the rates of decrease due to natural mortality and fishing mortality. Graham (p 272) found that 'the maximum sustainable yield [was] 15 percent greater than the present yield and that this yield would be obtained by 75 percent of the present effort.'

There had been other attempts to measure equilibrium yield. Hjort et al (1933), for example, look for the best age at which to take whales and other animals in order to get an equilibrium optimal yield. But again it was physical entities such as catch and fishing effort which dominated thinking, and the Russell equation provided, in part, only a means to that end.

This equilibrium approach was brought to completion with the publication (in 1947) of the now called Beverton & Holt Yield Equation, an equation which relates yield to age of first capture, fishing mortality, natural mortality and recruitment. Beverton and Holt's full version with application to North Sea fisheries did not appear until 1957. In this model the equilibrium concept is bound tightly into the fabric of the model. By following one year class over its life, and summing catch for that year class, Beverton and Holt were able to estimate yield per recruit in a single fishery if...if the fishery had been given time to establish equilibrium, an heroic assumption for a species that lives for more than a few years.

Here we must leave the biologists' story. True, fisheries biology has moved on since that time. However, it is the strong empiricists flavour of the biologists, coupled with an equilibrium approach to problems, that has moulded much of their thinking on fisheries management. We view the world through our culture, and our culture dictates, what we see, the way we see it, the way we think, and the way we operate. Subsequent fisheries biology has largely been concerned with purging the discipline of its equilibrium concept (some of Ricker's work can be viewed in this light, for example his 1963 and 1973 articles), looking at strategies of adaptive management (for example Walters 1987), and the ecology of the non-equilibrium system (for example Larkin 1977, or Lett & Kohler 1976). But one suspects that little of this has found its way into the thinking of fisheries managers, that maximum estimated yields still dominate goals, and that biologists giving advice are strongly influenced by the philosophical approaches of their predecessors.

Economic Perspectives on Management

The next task is to look at the way economics influenced our thinking on fisheries management over the same period. In the first instance we can answer the question simply: very little indeed. In fact most of the economic thinking was done by biologists who were illequipped to take this role, not only because they lacked the skills, but also because their type empiricist philosophy was not well suited to problems where functional forms, and relations are far more important than objective things. Nevertheless, almost without exception the biologists attempted to tack a few economic (or in most cases financial) parameters onto their work.

Petersen (1903) went further than merely introducing a growth component into the fisheries equation, noting that there would be some optimum size at which to take the fish should be taken. Having introduced a 'should', and maintaining his empiricist stand, he refrained from saying what that optimum size should be. However, he does given us some clues to his thinking. 'This problem depends on the value of the fish in [age class III] compared with that of the fish in [age class IIII], and upon the mortality in the third year. If we only knew the rate of growth, the mortality during the year, and the price of fish, the question might be exactly solved by mathematics. Although, Petersen wished to introduce value, he would have done it only to the extent that value was represented by price in the market place. Recognizing that profits were falling in the fishing industry, he saw an answer through, increasing mesh size that would bring bigger and more valuable fish onto the market. However, there was little if any recognition of the essential free-access nature of the problem.

Alone among the economists, Jens Warming (1911) concentrated on the heart of the matter. He argued that free access (or common property) was the cause of falling returns in fisheries, that we should not strive for the greatest possible production but rather 'make sure that the marginal productivity of fishing corresponds to that in other industries', by charging a rent for the right of use. Warming's article (a translation from the original Norwegian is given by Andersen and Hannesson, 1981) has no empirical content, it essentially concentrates on the functional relationship between number of fishermen and wage costs, and yield and revenue. This work remained largely unknown and does not appear to have had any impact on fisheries management.

Russell's economic contribution (1942, p 94) was well founded, but fails to go far enough. He is satisfied to say the 'rate of fishing which gives the maximum steady state is of course not necessarily the most economical rate of fishing - this depends upon economic factors as well, such as the cost of coal, cost of gear, level of wages and so on, in relation

to the price of fish obtained.' However, it is unlikely that Russell would distinguish between financial and economic analysis.

Graham's 1935 contribution shows considerable concern for the economic gains from management, and in a dynamic setting. He estimates that, for the North Sea trawl fishery, a reduction in fishing effort to 75 percent would give initial losses in revenue for 3 years before a continuous gain of 13 percent is attained, and this all has to be matched by a saving of about 16 percent in coal costs plus some other savings in expenses. However, the real nature of his ability to change gears is shown in his 1943 book, The Fish Gate. In chapter thirteen he outlines his great law of fishing: 'Fisheries that are unlimited become unprofitable (inefficient)'. Here he sees profit as an index of efficiency in much the same way as an economist would argue today. But he goes further than this, striking at the heart of the type of thinking required for successful fisheries management: 'It has not been difficult ....to produce the Great Law of Fishing as a 'law' in the scientific sense, that is summarizing experience only..... A law so based is widely acceptable because it belongs to the world of fact. ..... [However,] the Great Law is only fruitful in its converse form , that is, *limiting effort will restore profit to a fishery*, and of course the men who acted on that opinion, for the first time, had no experience to support them.

Graham, here, does not claim that the law was derived totally empirically. However, it is quite clear that he believes empirical thinking to be dominant. He further reveals that he believes that fisheries management may not require a totally empiricist approach. Graham may well agree with the current strategies to fisheries management which have their roots in the writings of Scott Gordon(1953,1954). Other major biologists were following suit. Beverton (1953) and Burkenroad (1953) also sounded warnings that economic factors may be important, but neither of the latter two authors were able to provide a convincing economic analysis, being merely content to place fisheries in a financial framework; for finance is concerned with objective measurable 'things'.

With the work by Warming (1911) largely overlooked Gordon (1953,1954) started with a clean slate. Gordon showed clearly and concisely how the root cause of the problems of overfishing was the common-property nature of the resource, that open access would ensure that the best and closest grounds were overfished first, and that in time all fishing grounds would be overfished with the value of average product coming into equilibrium with the average cost. The 'best' position was where marginal revenue equals marginal cost with optimal fishing effort being less than the equilibrium level of effort, and rent from the fishery being maximized. Therefore regulation of effort was required.

Gordon's analysis is Marshallian in flavour. There is little if any empirical evidence for the findings, things such as fishing vessels and fish are only secondary, and the major concern is with concepts and functional forms such as average and marginal products, opportunity costs and their relationships to fishing effort.. Even the term fishing effort is vague and unmeasurable, being some omnibus variable embodying capital and labour; this is a stark contrast to the biologist's use of fishing effort where it is used in a different sense and defined explicitly in a measurable form for each fishery. Gordon's conclusions and models rest on the validity of the perfect competition, general equilibrium model, with all its inherent assumptions and value judgements. One can accept that position if he were only writing for economists, but for a general audience of fisheries biologists and fisheries managers it is expected that his position would be misunderstood. It was.

The double-equilibrium notion of Gordon is confusing. For biological equilibrium is a concept far removed from economic equilibrium. The former is concerned with a natural process, with fish stocks existing in natural harmony with their environment. The latter is a man-made convention, and exists only to the extent that man is postulated to behave in certain ways and conform to certain man-made maximization rules. It is tacitly assumed that this is natural and it is difficult to argue with nature.

It is timely to include a reminder here. No criticism of the economic method is implied, although a classical trained biologist would probably read it as such. Empirical studies in fisheries economics have been increasing in the fisheries literature, and applied economists such as Huppert (1979), Hannesson (1975) and others, have made a mark in this field. However,the intention, here, is to look for the dominant methodology in the two fields, fisheries economics and fisheries biology, in an attempt to understand differences in attitude to fisheries management.

Anthony Scott followed fast on the heels of Scott Gordon, also highlighting the effects of an open access. Scott (1955) again confirmed that lack of property rights was sufficient to cause overfishing. There is no reference to a particular fishery in Scott's analysis. He proceeds by assuming function relationships between fish population, landings, cost and effort (effort defined loosely as inputs) and, given certain market conditions, he establishes revenue, cost and user cost curves. He is then able to show that sole ownership would provide the optimal result. If, however, Scott's assumptions are relaxed, his conclusion that sole ownership provides the optimal outcome no longer holds.

No empirical evidence is used in either Gordon's or Scott's analysis. Neither is it needed Once certain assumptions are made the conclusions

stand on their own, and hold independently of real world observations. The models are conditional generalizations. This philosophy differs radically from that of biologists who, it is argued, would hold that observations of things and events are paramount. For the biologist, the flow of information is from the world to the mind, not the other way around. That is to say, we observe what is and what happens and organize and interpret these events. There is a stark contrast between Gordon and Scott on the one hand, and Russell and Graham on the other.

Other examples of economic thinking in fisheries could have been taken from the fisheries literature. Turvey (1964) showed that regulation of both effort and mesh size was but a specific case of the general theory of second best, Anderson (1975) showed how technical relationships between fisheries will change optimal strategies, Anderson (1976) incorporated the traditional perfectly competitive theory of the firm into fisheries economics, Plourde (1970) and Clark & Munro (1975) introduced dynamics into the analysis and established golden rules and optimal time paths. Our intention is not to provide a literature review: the interested reader is referred to Anderson (1987) or Waugh (1984) for that. However, in all the economic works cited here there are rich insights into the general problems of fisheries management, and in each case the reasoning is that of conditional generalization, with objective functional forms holding dominance over objective things.

The Battle Grounds

With this background the two antagonist, the biologists and economists, met first with a view finding solutions to the problems that had plagued the Pacific halibut fisheries. Pacific halibut is distributed along the Pacific northwest regions of Canada and the United States from northern California to the Bering Sea. The main mode of fishing is long-lining which consists of lines (or skates) about 100 metres in length carrying a large number of baited hooks. The species is a slow growing one, females reaching sexual maturity at about 12 years, and medium size fish (10 to 60 pounds) which are caught commercially span an age of 7 to say 18 years. The stocks of halibut are relatively immobile, tend to congregate annually, are easily located, and the type of gear used is not age selective. All of this means that the fishery was (is) especially vulnerable to depletion.

The history of the fishery is provided in a number of works (see for example Crutchfield and Zellner 1962, or Waugh 1984). The development of of fishery was gradual with inshore grounds being developed first, and technological improvements providing the impetus for depletion of these grounds and the expansion to new and more distant grounds. Up until 1910 small vessels operated within 500 miles of the southern ports of Seattle and Vancouver. With the introduction of larger steam trawlers and the depletion of inshore grounds operations were extended northwards to the Gulf of Alaska. By 1912 the peak catch in the relatively northern grounds (the so called Regulatory Area 3) had been reached, but expansion the the Alaskan grounds (included in Regulatory Area 3) allowed total catch to continue to rise, reaching a peak of 69 million pounds in 1915 followed by a sharp decline.

Discussions between the United States and Canada on the halibut problems resulted in a convention in 1923 and regulations coming into force in 1932. The regulations provided for a total quota on catch and were supplemented by minimum size limits and were designed and implemented by biologists. As would be expected the regulations were concrete and explicit - a control on size of fish and a closed season when the maximum permissible catch is taken. There was on suggestion of a control on a loosely defined measure of inputs, or the creation of a free market for fishing rights, or an imposition of a tax to correct the externality, and certainly no concern about the collection or existence of economic rent, positive, negative or otherwise.

Following the regulations total catch from the fishery rose from 47 million pounds in 1933 to 58 million pounds in 1950, and to 71.2 million pounds in 1954. However, with no restriction on fishing effort, and no market for fishing rights, this larger catch was taken by an increasing number of vessels (at an increasing cost) in progressively shorter periods. Between 1933 and 1950 the number of vessels rose from 384 to 854; in Regulatory Area 2 the fishing season fell from 206 days to 32 days, and in Regulatory Area 3 the season fell from 268 days to 66 days. And the position has become progressively worse through time, even until the present day. This massive overinvestment in vessels, for just use a few days of the year, clearly represents a serious social problem.

Gordon (1957, p 69) in an international conference on fisheries management described this management programme as one which 'must go down in economic annals as one of the clearest cases of failure' Other economist comments include ones like 'The Halibut programme has questionable value biologically , is a palpable failure economically, and is a scheme that has led to the setting up of arrangements which benefit biologists and possible fish, but not humanity - either individually or collectively'. There would appear to be two other possibilities: benefit

humanity but not the fish, or benefit both. The former position is surely untenable, the latter the nub of the issue. But the different philosophical frameworks, and a different value system, at present, seems to preclude the ideal position.

In a sharp reply Bell (1969, p 64-65) states 'For full time halibut fishermen the earnings are relatively certain and greater than prevailed prior to regulation.... For both large and small dealers the larger production of halibut reduces plant overhead.... For the consumer, the larger production has more than kept pace with population... The joint regulation of the halibut fishery has been an incontestably successful experiment in management of an important source of wealth, of employment and of food.' One other biologist added during an international FAO conference in Rome that at least biologists were humble (in Hamlisch 1962)

Almost alone Crutchfield (1956) attempted to provide a balanced view. He noted the successes and the failures of the programme, drew attention to the increased production, but pointed out the serious consequences of overinvestment, distortion of the market and excesses placed on processing and port facilities, and the dissipation of economic rent.

With the divergent philosophies of economists and biologists in mind, it is easy to see why the two groups would be antagonistic. The biologist came first to fisheries science. With a strong empirical bent, their observations told them that developed fisheries had too few fish, catches were too low, and those fish were too small. Their regulations increased output and the size of the fish - what more was there to do? Economists came latter. With a background in general equilbrium theory they recognized an industry which was not conforming to the ideal. Too many resources were being allocated to fisheries compared to the rest of the economy - there was a lot to do.

The subsequent discussions never met head on, but did generate heat. Each was group working from a different prospective. It was unlikely that the economist would accept that the fish were important in themselves; and to the economist the simple increase in stock, catch or size of fish was unimportant, unless one could show that there had been a net benefit to the economy and an improvement in the allocation of resources. It was also unlikely that the biologist would appreciate the nature of the reasoning of economists and his argument from general equilbrium models, especially in the light of his total ignorance of fish.

Final comment

In 1988 the halibut fishery is little changed, and the debate still rages. The halibut fishery was used here as an example only. However, those involved in fisheries management, in either camp, will recognize the similarities with other fisheries and other regimes. It is probably true to say that biologists still look toward practical and simple management techniques, ones that can be applied easily, and ones that lead to an increase in production or employment. More abstract concepts such as rent or producer or consumer surplus play little part in their advice to management, and a closed season or restriction on gear or time of fishing can be just as effective management techniques as any. It is also probably true to say that economists are still obsessed with the economically efficient optimum that is part of general equilibrium analysis, and for some economists nothing short of the introduction of a perfect market in fishing rights (individual transferable quotas) will provide a satisfactory outcome.

The argument here has been that the debate between the biologists and economist is to be expected. They are bound to argue since each is working within a different framework, and it is not possible for one to understand the other without shifting ground. The biologist has been raised in the empiricist tradition. The flow of information is from the world to the mind and the mind is moulded accordingly. For economists the mind has been moulded by society and the way we observe the world is limited by cultural factors. For those who like to see more concrete conclusions, and who put their faith in the predictions, the words of Keynes might suffice:

'The ideas of economist and political philosophers, both when they are right and when they are wrong, is more powerful than is generally understood. Indeed the world is ruled by little else'

References

Andersen, L., 1975 Analysis of open-access commercial exploitation *and maximum economic yield in biologically and technologically interdependent fisheries, Journal of the Canadian Fisheries Research Board*, 32, 1825-1842.

Anderson, L., 1976. The relationship between firm and fishery in common property fisheries, *Land Economics*, 52, 179-191.

Anderson, L., & R. Hannesson, 1981. Translation of Jens Warming, On 'land rent' of fishing grounds, *Fisheries Economics Newsletter*, 12, V-XVI.

Andersen, L., 1986, *The Economics of Fisheries Management* (revised edition) The Johns Hopkins University Press, Baltimore.

Bell, F., 1959. Economic effects of regulation of the Pacific halibut fishery, in J. Crutchfield (editor), *Biological and Economic Aspects of Fisheries Management*, University of Washington, Seattle, 51-75.

Beverton, R., & S. Holt, 1957. *On the Dynamics of Exploited Fish Populations*, Ministry of Agriculture, Fisheries and Food, Fisheries Investigation Series, 2(19) London.

Beverton, R., 1953. Some observations on the principles of fishery regulation, *Journal du Conseil International pour l'Exploration de la Mer*, 19, 56-68.

Burkenroad, M., 1953. Theory and practice of marine fishery management, *Journal du Conseil International pour l'Exploration de la Mer*, 18, 300-310.

Clark, C. & G. Munro, 1978. The economics of fishing and modern capital theory: a simplified approach, *Journal of Environmental Economics and Management*, 5, 198-205.

Crutchfield, J., 1956. Common property resources and factor allocation, *Canadian Journal of Economics and Political Science*, 22, 292-300.

Crutchfield, Z., 1962. *Economic Aspects of the Pacific Halibut Fishery*, Fishery Industrial Research 1, U.S. Department of Interior, Washington.

Gordon, H., 1953. An economic approach to the optimum utilization of fishery resources, *Journal of Canadian Fisheries Research Board*, 10, 452-457.

Gordon, H., 1957. Obstacles to agreement on control in fishing, in R. Turvey and J. Wiseman, *The Economics of Fisheries*, F.A.O. Rome, 65-72.

Gordon, H., 1953. The economic theory of a common-property resource: the fishery, *Journal of Political Economy*, 62, 124-142.

Graham, M., 1935. Modern theory of exploiting a fishery, and application to North Sea Trawling, *Journal du Conseil International pour l'Exploration de la Mer*, 10, 264-274.

Graham, M., 1943. *The Fish Gate*, Faber and Faber, London.Hamlisch, R. (editor), 1962. *The Economic Effects of Fishery Regulation*, F.A.O. Fisheries Report No. 5, Rome.

Hannesson, R., 1975. Fishery dynamics: a north Atlantic cod fishery, *The Canadian Journal of Economics*, 8, 151-173.

Harland, R., 1987. *Superstructuralism*, Methuen, London

Hjort, J., et al, 1933, The optimum catch, *Hvalradets Skrifter*, 7, 92-127

Holt, E. 1895. Present state of the Grimsby trawl fishery, *Journal of the Marine Biological Association*, 3: 339-446.

Huppert, D., 1979. Implications of multipurpose fleets and mixed stocks for control policies, *Journal of Canadian Fisheries Research Board*, 36, 845-854.

Larkin, P. 1977. An epitaph to the concept of maximum sustained yield, *Transactions of the American Fisheries Society*, 106, 1-11.

Lett, P., & C. Kohler, 1976. Recruitment: a problems of multispecies interaction and environmental perturbations, with special reference to the Gulf of St Lawrence Atlantic herring (Clupea harengus harengus) *Journal of Canadian Fisheries Research Board*, 33, 1353-1371.

Marshall, A., 1890. *Principles of Economics*, London.

Petersen, J., 1903. What is overfishing, *Journal of the Marine Biological Association*, 6: 587-595.

Ricker, W., 1963. Big effects from small changes: two examples from fish population dynamics, *Journal of Canadian Fisheries Research Board*, 20, 257-264.

Ricker, W., 1973. Two mechanisms that make it impossible to maintain peak period yields from Pacific salmon and other fisheries, *Journal of Canadian Fisheries Research Board*, 30, 1275-1286.

Russell, E., 1931. Some theoretical considerations on the "overfishing" problem, *Journal du Conseil International pour l'Exploration de la Mer*, 6, 3-20.

Russell, E., 1942. *The Overfishing Problem*, University Press, Cambridge.

Scott, A. , 1955. The fishery: the objectives of sole ownership, *Journal of Political Economy*, 63 116-124.

Skolimowski, H., 1981. *Eco-Philosophy*, Marion Boyars, New York.

Turvey, R., 1964. Optimization and and suboptimization in fishery regulation, *American Economic Review*, 54, 64-76.

Waugh, G. 1984. *Fisheries management: Theoretical Developments and Contemporary Applications*, Westview Press, Boulder.