Centennial Lecture III: Fisheries Research Strategies for the Future

PETER A. LARKIN

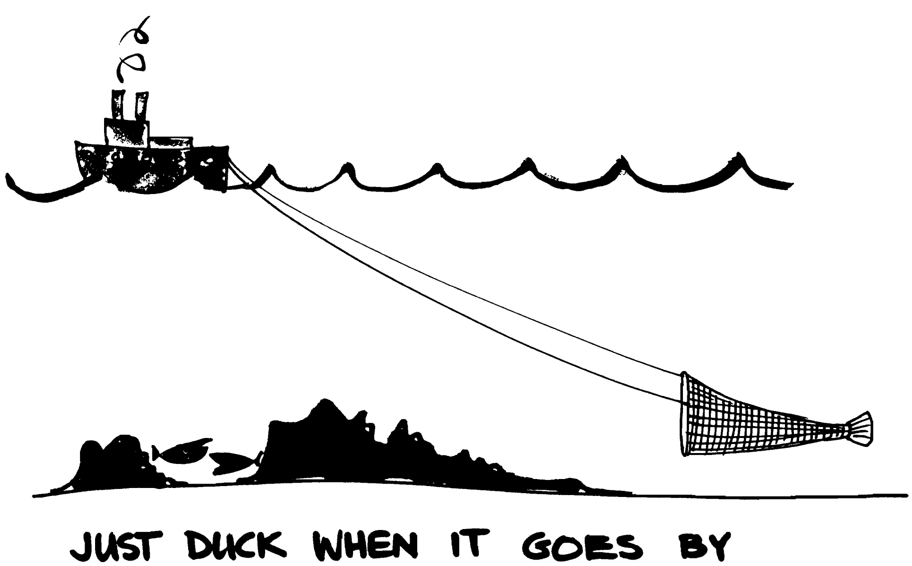
The title I was given for my remarks was "Fishery Research Strategies for the Future," and I confess to have had some difficulty with it. I am not sure I know what "research strategy" is. In Ottawa, our Canadian national capital, whenever someone uses the word "strategy," you know he is talking about what he does as distinct from that which lesser mortals do. At many levels of the hierarchy, but especially higher up, there is the delusion that research must be seen as part of a comprehensive scheme for achieving the "goals" implied by "policies."

The truth, of course, is that the kind of research that is done is driven partly by what research is being done elsewhere in other parts of the world and in other branches of science; and partly by what is seen as the current social challenge or opportunity. Except for providing a great deal of support to research, the only real strategy is to hire good researchers.

So, I am not going to talk too much about strategy. Instead, I shall do some guessing about where fisheries are going and about where science is going in the next century. From that, I will indicate where I would guess the research action will be.

Hunting Fish

Fish, at least those that live in the ocean, are far more fortunate than land animals, for in the breadth and depth of their environment they are far less likely to be rendered extinct at the hand of man. The marine mammals are somewhat less fortunate because they must surface to breathe or come to land to reproduce. But, most species of fish, it seems, will persist either because they have a refuge where they are inaccessible, or they are, or they become, so scarce as not to attract attention. This is a heartwarming thought because it implies that by contrast to what happens ashore, our mistakes in the oceans may not have such irreversible effects. It also implies that there will always be fish out there to catch and, if we eventually learn enough, we will have all of the natural ingredients to manipulate as we wish.



Peter A. Larkin is with the Institute of Resource Ecology, University of British Columbia, Vancouver, B.C. V6T 1W5, Canada.

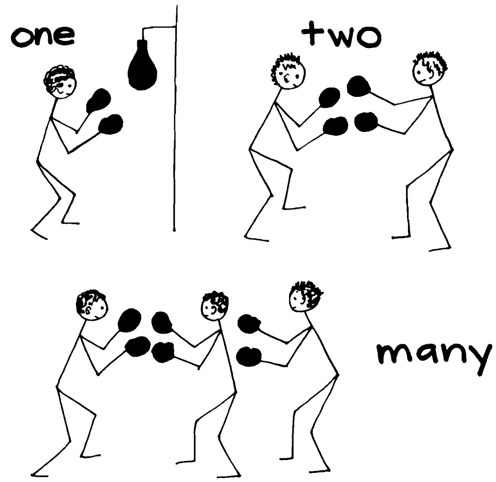
It has to be conceded that, at present, we are not very good at manipulating natural assemblages of fish populations. We have a well-developed theory of harvesting to achieve various levels of yield from single stocks, but the theory concerning the harvesting of mixtures of species is very much in a primitive state. We have theory for single species and for two species engaged in competition or predation, but beyond that, it is heavy going.

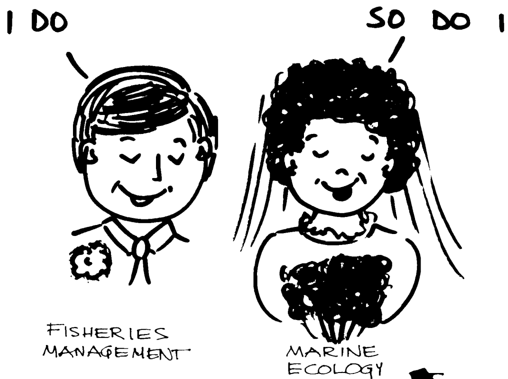
As has been remarked several times, our comprehension is somewhat akin to the most primitive method of counting—one, two, many. The May 1984 Dahlem Konferenz in Berlin is testimony to the complexities of understanding how a community of fishes achieves its characteristic mix of species. More comprehensively, although we have some knowledge of the constraints that govern the functioning of marine ecosystems and of some of the sources of environmentally induced variability in numbers of various species of fishes, the conclusions we can draw have, as yet, rather little practical utility. As John Steele recently outlined[1], we have seen what has happened when some stocks have been heavily fished (for example, in the North Sea), but why the species assemblage changed the way it did is not clear.

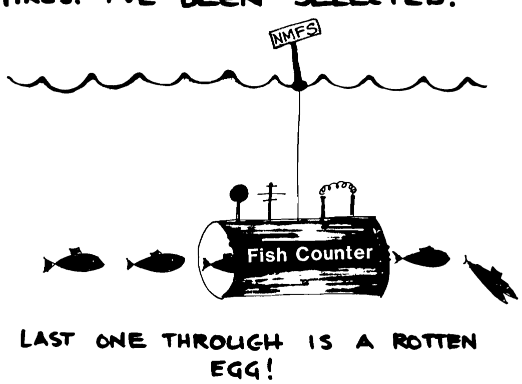
It seems unlikely that our understanding will improve a great deal from more theoretical modelling. What is needed are experimental regimes of management and continued research on the structure of marine ecosystems. The wedding of "marine ecology" and "fishery management," so strongly advocated by Warren Wooster and colleagues of the Ocean Sciences Board (National Adademy of Sciences, 1980), still has many years of productive research ahead of it. There is particular need for studies of the ecology of larval and juvenile fishes, the ages at which the interactions among species seem the most important in determining year class size. The Woods Hole constellation of institutions has led the way and should continue to do so during the next several decades. Meanwhile, there are some other kinds of research that could address some of the more immediate needs of the managers of hunted fish populations.

If we assume, for the time being, that species of fish can be treated as though they did not have anything to do with each other, like rows of different kinds of vegetables in a garden, we must still acknowledge that we are not sure how to manage them. It is widely recognized that fishing can be selective as well as intensive, which raises the question of how to harvest a stock while protecting its genetic inheritance. On this subject, we have a substantial body of theory with roots in the literature of evolution, but when it comes to management, we have only a few scraps of information (meristic, morphometric, and electrophoretic), the significance of which is not always clear, and many vague fears that the loss of some genetic diversity is bad and virtually irreversible. A strong effort to better understand the genetic structure of fish populations is a must for the future.

Even more immediate is the need for development of techniques for estimating the size of year classes before they are recruited to a fishery, thus enabling adjustment of rates of harvesting. This is essentially a technological problem and, given our modern day capabilities, it seems only a matter of time before a team will have developed some appropriate gadgets for identifying and counting young fishes in their natural environment.





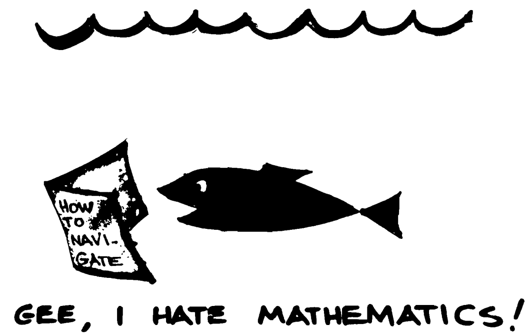


[1]Steele, John H. Manuscr. Some problems in the management of marine resources. Woods Hole Oceanogr. Inst. Contrib. 4073. 49 p.

In a somewhat related vein, it should be possible, again given our technological strengths, to devise methods of tracking fish in the course of their migrations. For example, the marine migrations of Pacific salmon, *Oncorhynchus* spp., have been reconstructed in a general way from tagging experiments (Royce et al., 1968), but there are substantial year to year vagaries which make real-time predictions of where the fish are quite inaccurate. It would be nice, too, if we knew how the fish did it and could predict where they would go. We know they have the equivalent of a clock, map, and compass, and they also have a magnetic sense, but how they put it all together and actually use these capacities remains a mystery. Suffice to say, they can surely teach us a thing or two. Work on this problem will be both exciting and rewarding.

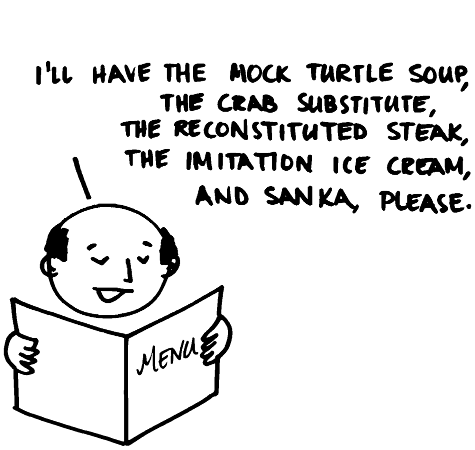
This kind of talk conjures up images of super-equipped fishermen using satellite and automated data-collecting systems of buoys to seek out and destroy schools of fish—a sort of "Sea Wars." Admittedly, we have come a fair distance in this direction with the use of fish finders and aids to navigation, but one cannot help be a bit skeptical about further advances, if only because the fishermen would probably find that costs of catching the fish would exceed that for which they could be sold.

The place where these techniques will probably be most valuable will be in the understanding of physical circulation and biological production on scales of space and time that have been beyond us. Roger Revelle's recent plea (Revelle, 1985) for support for the series of four satellite missions for oceanography from space, and the recent descriptions of phytoplankton blooming off the U.S. east coast from the Nimbus satellite (i.e., Brown et al., 1985) can only be seen as a small taste of things to come.



But this sort of high technology, even taken together with much more profound understanding, is not likely to increase production substantially from the world's oceans. Nor is it likely that superb management will accomplish much more. Hunting fish will not go out of style and world production could perhaps rise to 150 million tons per year, but to go beyond that seems a dubious proposition without drastically reducing the abundance of fishes higher up in the food chain and developing economic techniques for harvesting much smaller organisms and devising processing techniques to make from these smaller organisms products which consumers find acceptable.

The economics of harvesting, say, lantern fishes or krill, are at present discouraging. The 28 June U.S. edition of *The Korea Herald* (1985) contained a revealing little item entitled, "Antarctic krill stockpiles grow as sales outlets disappearing." According to the article, krill resources of the Antarctic are estimated at between 500 million and 1 billion tons. The Korean government had subsidized krill fishing expeditions to the tune of 600 million Won (about US$800,000). One company gave up in 1981 and a second was in debt for 1.2 billion Won (about US$1.6 million), and the 1982 catch of 2,000 tons had yet to be sold. Tantalizing as the prospect seems, distant sea fishing for krill is, at present, a doubtful economic proposition even though krill is ostensibly a fairly acceptable product in today's markets.



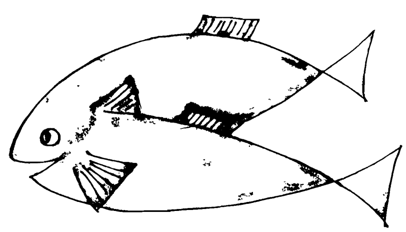
For many other such prospects, it can be speculated that modern methods of food manufacturing that can turn walleye pollock into mock crab legs and soybeans into ice cream, will someday in the future make almost anything out of cheap raw materials. There is not much doubt that these things will happen, and, it might be added, without any help from fisheries research scientists; but, whether the cheap raw material will come from what is naturally produced in the world's oceans is much less certain. It is far likely that the big increases in production will come from farming the ocean close to the population centers and their large markets.

Mariculture

The large-scale, world-wide culturing of fish, crustaceans, and mollusks is surely one of the major events in seafood production for the latter part of this century. It has been estimated that world production of seafoods by culturing will be 30 percent of the total by the turn of the century (Larkin, 1982). Enthusiasts of intense culturing systems abound, and given some investment, a little ingenuity, and some application of the erstwhile agricultural techniques of breeding, nutrition, disease control, nutrition and waste recycling, there seem to be major prospects. Indeed, there is every indication that agricultural scientists will take this over into their bailiwick, in some instances, particularly in developing countries, coupling aquaculture with agriculture in commercialized versions of traditional lifestyles.

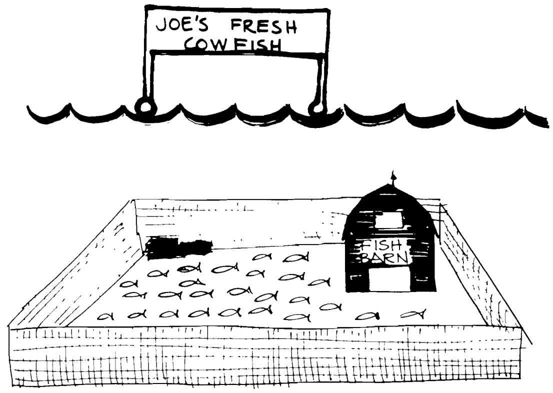
The People's Republic of China has recently announced a 5-year plan to increase aquaculture production from 6 to 16 million tons a year. An increase of 2.54 million tons is claimed for 1984 alone, when freshwater production jumped 30 percent and marine cultured production went up 10 percent (Fish Farming International, 1985). Admittedly, much of this increase is apparent rather than real as farmers bring fish to market in the newly deregulated economy, rather than eating them at home. But there can be no mistaking what the government has in mind. Many similar examples of incentives for aquaculture could be cited from many parts of the world, including North America.

The moral for fisheries research strategists is obvious: Get on board or be left behind. There are dozens of applied and basic research problems to address, particularly in the fields of physiology, behavior, and genetics of edible marine organisms. The potentials are simply enormous for applying the new know-how of recombinant DNA, animal tissue culture, monoclonal antibodies, and a host of new techniques of genetic manipulation that are just now being discovered. Fish have already been cloned (Streisinger et al., 1981). Fish growth has been stimulated by hormones manufactured by bacteria (Gill et al., 1985). Fish sex ratios have been manipulated. Pandora's box is already open and we have lots of goodies to sample.



A TETRAPLOID THAT YIELDS 4 FILLETS

There are practical questions to be addressed, such as the extent to which natural production can be exploited to raise caged fish, the feasibility of fertilizing enclosed portions of the sea, the



possibilities for "ranching" the ocean, either raising fish to release to be caught later when they are larger, or catching small fish in the wild and raising them to large size. When you think like an agricultural scientist, visualizing the ocean as a huge community pasture, a piece of which could be fenced off, you are on the way to realizing that here is a new frontier where the cowboys are soon going to have competition from the squatters.

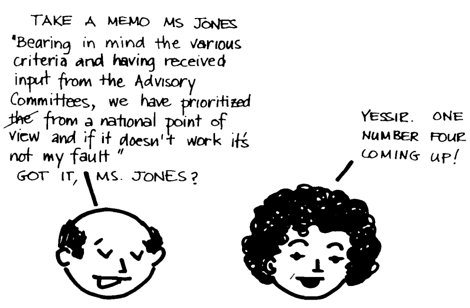
It may well be that North America is not the place where most of the development of aquaculture will take place. Historically, unless the product is very high priced, like salmon, these kinds of pursuits only pay off when they are located close to local markets. But given clean water close to shore (a good argument for pollution control measures), and given automated technologies to reduce labor costs, the economic potentials of aquaculture are still sufficiently attractive to ensure that growth in North America will be significant. Production doubled between 1975 and 1983, and although still small by world standards (about 316,000 tons), it is a signal for the future. Even a pessimist would predict steadily increasing cultured fish production, rising above 50 million tons by the middle of the next century.

How Much Research?

I would have preferred to have confined my remarks to science and research, but if the Woods Hole Laboratory is like some of its sister institutions, no discussion of research strategy would be complete without some reference to the social context in which the research enterprise is embedded.

The crucial questions, rolled into one, are: "How much of what kind of research should be done where?", and at one time or another in the past I have stood squarely on all sides of the fence. There must be a balance, just as there is in an investment portfolio, between what may pay off in the short term and what may pay off in the long term. The total investment should not be out of line with a realistic appraisal of the potential pay off. The portfolio should reflect the perceived needs of the various user groups, both commercial and recreational. Because fish do not recognize state or national boundaries, research and management can only achieve their potentials if there is well coordinated, interjurisdictional cooperation. Universities should be involved so that the interests of bright young people can be captured. Finally, and perhaps most important, there should be retrospective evaluation of what has been accomplished so that future investment may be guided into original mistakes. It is with these kinds of considerations that senior civil servants and their political masters concoct centralized national research strategies.

If a country is small enough and homogeneous enough, then there is a greater likelihood that a centralized policy can mean enough to be applied with effectiveness. When the country is large and diversified, bordering on several oceans, such as the United States or Canada, the proposition that the research strategy can be articulated centrally is much less sure. Social, economic and political circumstances differ regionally, and so perforce do the research needs. Large centralized operations tend to foster higher levels of abstraction, progressively further removed from the substance and more preoccupied with form. Not infrequently, those who cogitate about research strategy have never done any research. In such circumstances, national policy becomes either a bostitched, bowdlerized version of regional policies or an abstract statement of platitudes, and in either case is largely redundant, except as it is necessary to raise funds for research. Around the world there are sayings to the effect that the national capital is an island surrounded by reality, and they more or less capture the sentiment. Decisions about what kind of research should be done, should mostly be left to the locals. In an age of worldwide comprehensive and rapid communication, the locals are just as capable of seeing the broad issues and far more capable of seeing their local relevance. If it should seem that this results in duplication of effort, one can always be confident that the competition will be vigorous.



I would not wish these remarks to be construed as overly critical of senior public servants who strive to ensure that regional proposals are sold at the national level. Rather, I see senior public servants largely as captives in a much more comprehensive tendency to centralized rather than regional government. Despite the many expressions of disfavor in the performance of some of the regional fisheries councils in the United States, they remain for me a start on a good model for others to follow. The essential step is to place the responsibility for the resource in the hands of the local resource users.

Some Concluding Comments

Some things probably will not change as the future unfolds. Scientists, I expect, will continue to grumble that they are being asked to do what cannot be done and are not being provided the funds to do what needs to be done. Commercial fishermen will continue to push the regulatory agencies for more fishing time and fewer regulations. Recreational fishermen may continue to complain that their interests are not adequately considered, and, history being on their side, they will probably achieve greater recognition. They will continue to tell tall stories to the Game Warden, who will continue to explain to them why they did not catch any fish (wrong bait, lure, depth, tide, etc.; too early, late, hot, cold, calm, rough, etc.). While research on the behavior of both commercial and recreational fishermen will no doubt continue and will, perhaps, lead to better appreciation of the social and economic consequences of different patterns of management, no one should expect that it will resolve anything. People in general, and fishermen in particular are too perverse to be manipulated. At least I hope they are.

More seriously, buried in the foregoing remarks are some implied assumptions, one of which is that science is an international enterprise in which all have the opportunity, if not the capacity, to take advantage. Looking to the past, the Woods Hole Laboratory and its sister institutions have made contributions not only to the United States, but through publications and a wide ranging hospitality, contributions in fisheries research and management to the world at large.

It is to be hoped that the next century will see this tradition maintained. The principles of natural science that govern the production of fish are the same the world over. Whatever is discovered here at Woods Hole has application far beyond Cape Cod and the adjacent waters of Georges Bank. Too often we compartmentalize our thinking and look for benefits from research on a much too parochial scale. It is often the case that what is discovered and advertised is far more important abroad than at home. The other side of that coin is that what is discovered abroad provides the context from which progress can be made at home. In science, the universal research strategy is the coupling of a bit of philanthropy with a lot of parasitism.

A more mundane question about research in the future is "How to keep abreast of it all?" Let me give you just a few statistics I collected recently in regard to water-related research. The Union List of Scientific Serials in Canadian Libraries lists 833 journals with the word "water" in their titles, 60 containing the word "limnology," 48 with the word "aquatic," 226 with "pollution," 523 with "marine," 23 with the word "freshwater," and 206 with "hydrobiology" or "hydrology." Then there are the words "fish" or "fishes" (252), "fishery" or "fisheries" (702), "oceanography," "ocean," or "oceans"(545), and "aquaculture" or "mariculture" (27). The total collected set with no duplication of these words contains 2,889 journals.

Then there are the books. In our university library alone, there are about 4,500 books or monographs on freshwater and 3,000 on the oceans. Down in the catacombs of the library, there is a collection of what is called "government publications" which no one bothers to count. Suffice to say, it is a "voluminous" collection in the literal sense.

It is commonly predicted that the world's scientific literature will double in size in the next 15 years, and such predictions often fail to consider the growing contributions from the developing countries. Keeping up with all that is being discovered elsewhere continues to shape up as a major problem for research strategists in all branches of science in the future.

It must also be remembered that research in fisheries contributes to and feeds on research in the natural sciences broadly. It was the development of concepts of maximum sustained yield in fisheries that led the way for other resource managers of forests and wildlife. Studies of birds and bees led the way for those concerned with fish migration. The advances of oceanography have been critical to much of our contemporary understanding of fisheries and the same will be true in the future (Brewer, 1983). Dozens of such examples could be cited. All would illustrate that fisheries research is both a giver and a taker in the building of knowledge.

The conclusion is inescapable that fisheries research will flourish just as science as a whole flourishes. Satellites in space, genetic engineers in laboratories, theoreticians developing models, submersibles exploring the deep seabeds, gumbooting seashore ecologists, and a host of others will together determine, along with us, just what the next century will bring as rewards in the endless enterprise that we call fisheries research.

Literature Cited

Brewer, P. G. (editor) 1983. Oceanography: The present and the future. Springer-Verlag, N.Y., 392 p.

Brown, D. B., R. H. Evans, J. W. Brown, H. R. Gordon, R. C. Smith, K. S. Baker. 1985. Phytoplankton blooming off the U.S. East Coast: A satellite description. Science 229:163-167.

Fish Farming International. 1985. China sets 16 million ton target. Fish Farm. Int. 12(6):1.

Gill, J. A., J. P. Sumpter, E. M. Donaldson, H. M.Dye, L. Souza, T. Berg, J. Wypych, and K. Langley. 1985. Recombinant chicken and bovine growth hormones accelerate growth in aquacultured juvenile Pacific salmon (*Oncorhynchus kisutch*). Biotechnology 3:643-646.

Larkin, P. A. 1982. Aquaculture in North America: An assessment of future prospects. Can. J. Fish. Aquat. Sci. 39(1):151-154.

National Academy of Sciences. 1980. Fisheries ecology: Some constraints that impede advances in our understanding. Report of an ad hoc group of the Oceans Sciences Board. Natl. Acad. Sci., Wash., D.C., 16 p.

Revelle, R. 1985. Oceanography from space. Science 228:133.

Royce, W. F., L. S. Smith, and A. C. Hartt. 1968. Models of oceanic migrations of Pacific salmon and comments on guidance mechanisms. Fish. Bull. (U.S.) 66(3):441-462.

Streisinger, G., C. Walker, N. Dower, D. Kanuber, and F. Singer. 1981. Production of clones of homozygous diploid zebra fish (*Brachydanio rerio*). Nature 291:293-296.

The Korea Herald. 1985. Antarctic krill stockpiles grow as sales outlets disappearing. The Korea Herald United States edition 3910:7 (June 28).