

## CHAPTER 11

## Rethinking Research For Fishery and Ecosystem Management

ALEC D. MacCALL  
*Fisheries Biologist*  
*Southwest Fisheries Center*  
*National Marine Fisheries Service*  
*National Oceanic and Atmospheric Administration*  
*La Jolla, California*

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### WHY DO RESEARCH?

It is appropriate to preface this discussion with some general comments on the objectives of, or justifications for, fishery or ecosystem research. Probably the most generally accepted justification for research is the prospect of improved management, whether it be for increased yield or value, or perhaps for a predictive capability which decreases risk. The assumption is the more we know, the more closely we can approach our management objectives. Unfortunately, this argument is at times perverted in order to postpone difficult decisions, or to rationalize poor management performance: "We don't know enough about the resource to..."

As we move from consumable resources to those which traditionally are not consumed (for simplicity, I call these "non-consumable"), such as seabirds and marine mammals, research is often justified by legislative mandate. Several U.S. legislative acts, such as the National Environmental Protection Act (NEPA), the Marine Mammal Protection Act (MMPA) and the Endangered Species Act (ESA) require an ecosystem understanding of interactions among species and impacts of man's activities. This category also includes progressive international treaties such as the Convention for the Conservation of Antarctic Marine Living Resources (CCAMLR), which specifically calls for an integrated ecosystem approach to management. To some extent, these mandates include an accounting for indirect effects on consumable resources. For example, contaminants and habitat destruction addressed by NEPA affect both consumable and non-consumable resources. On the other hand, the MMPA includes fishery impacts on the food supplies and mortality of marine mammals, but in its intent of protection rather than management, excludes consideration of those mammals' impacts on fishery values. Importantly, these legislative mandates for management and protection of non-consumable resources have mostly focused on

large, visible high-level predators (at least in the marine realm), and that is where most of the attendant research has been directed.

### HOW MUCH RESEARCH?

Given the various justifications or motivations for conducting research, the equally important but difficult question remains as to how much research is appropriate. In the case of consumable resources, the value of research is potentially quantifiable. In keeping with the assumption that more information should allow closer approach to optimum utilization, we can draw two important quantitative inferences: First, there is a limit to the value of information, as the resource itself has a limit to the benefits it can provide under ideal management. Second, the value of information conforms to the rule of diminishing returns. Initial information on a resource is valuable in establishing reasonable expectations by management, whereas additional information serves mainly to refine the approach to optimum utilization. It is arguable whether there is a limit to information in the way that there is a limit to fishery value, but in any case, accumulation of useful information also conforms to the rule of diminishing returns as a function of cost.

The relationship between information, its cost, and fishery value is instructive (Figure 1). Given ideal management, maximum net value from a fishery (benefit less cost) is achieved at relatively low levels of information (Figure 1a). Here, ideal management is characterized as low-expectation, low-cost, and robust or self-limiting (low-risk). For example, the fleet size would be limited to that which is able to harvest an amount well short of the estimated maximum sustainable yield, and quota determinations, monitoring and enforcement would be avoided. Ideal management is somewhat of a fiction, but serves mainly to contrast with actual management (Figure 1b). Performance of actual management has been variable, but most often fishery value has been dissipated by excess fleet capacity (so-called "overcapitalization") and excess fishing pressure, placing the fishery on the descending limb of the production curve (i.e. increased fishing intensity leads to decreased average yield). Ironically, actual fishery management often has led to relatively low fishery value partially as a consequence of aggressively but haphazardly trying to maximize that value.

Information costs can be evaluated in terms of fishery economics: research is in effect a form of capital investment, and monitoring incurs an operating cost. From this viewpoint, actual management often tends to overinvest (or "overcapitalize") in information, and this habit has been a further source of dissipation of total fishery value. It is easy to see the forces which cause this problem: managers, researchers, and the various interest groups which vie for allocations of the resource all agree on the need for more information to support their individual arguments or interests. Researchers may claim that their work relates to management even when the connection is negligible—such claims may improve prospects for funding. The cost of the research is seldom a consideration; usually it is not borne by the interest group requesting it or benefiting from it, but rather by government agencies. Private interest groups may add to this total expenditure by hiring consultants to provide them with new information. Furthermore, such expenditures are often matched by the cost of government or opposing interest groups' responses to such efforts.

### WHAT SHOULD WE DO?

Unfortunately, the solution is not simply to spend less on monitoring and research. Most present fishery management institutions have locked themselves into policies requiring intensive research and monitoring efforts. For example, most interpretations of the Magnuson Fishery Management and Conservation Act of 1976

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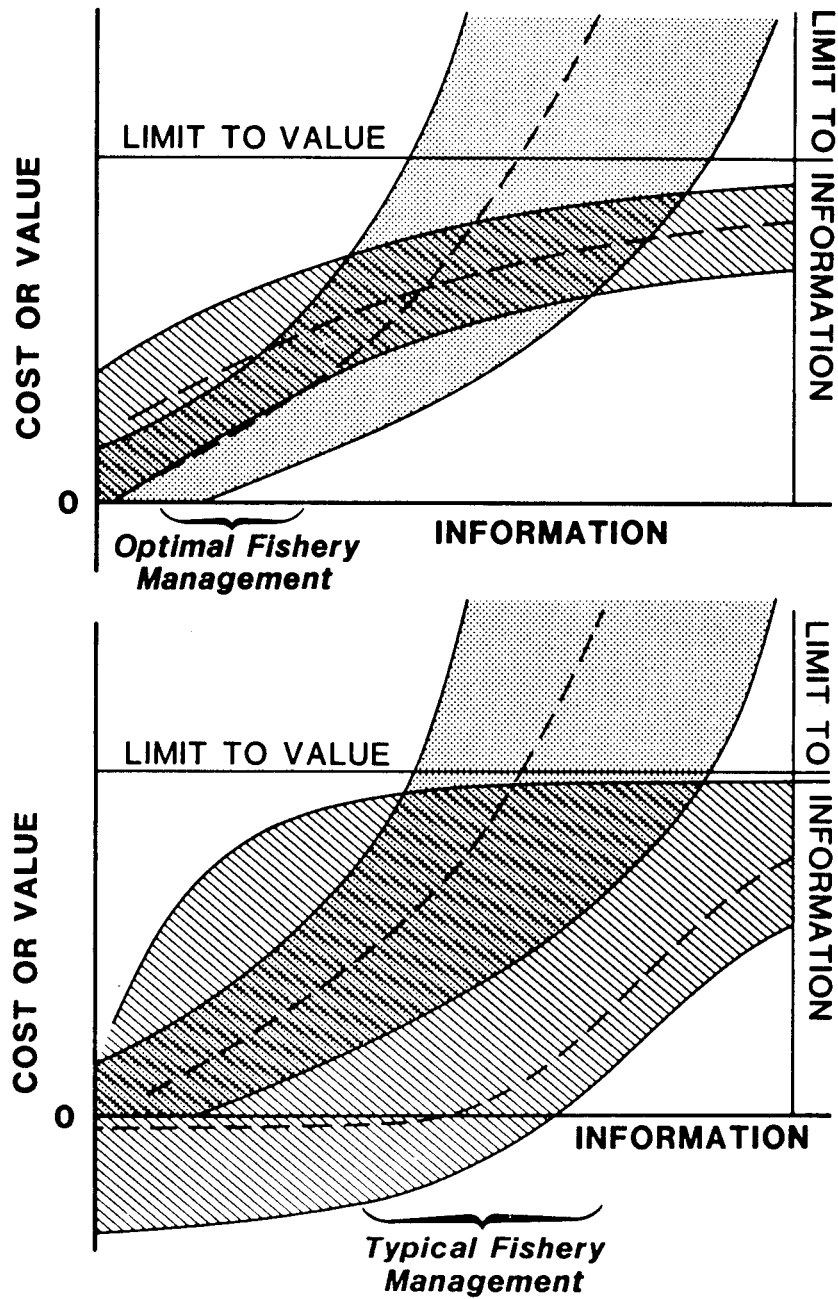


Figure 1. The relationships among information, its cost (stippled region) and its benefit (hatched region) to fishery management a.(upper): "Ideal" management; b.(lower): "Actual" management. Shading represents the range of likely outcomes. Dashed line represents the most likely outcome.

(MFCMA) have required annual setting of harvest limits, quotas or allocations, with associated requirements of information on the status of the resource, harvests and concerns of relevant interest groups. Given the existing management framework, reducing the investment in information would incur substantial risk of further declines in fishery value due to subsequent misinformed or uninformed management decisions.

Within the context of this information model, there are two courses of action which promise to improve the cost-effectiveness of information and management. The first, and less likely of the two, is to change management toward a "low-information" system. The potential value realizable from low-information management is critically dependent on establishing the appropriate management expectations and institutions. I will not go into describing such institutions, except to say that they would be substantially different from those presently governing most U.S. fisheries, and most likely would be perceived to be non-democratic, or at least non-egalitarian, requiring substantial limitation on freedom to participate in the fishery. Some candidates for these management approaches are discussed elsewhere in this symposium.

The second course of action is to keep routine fishery monitoring and research to the minimum level necessary to meet immediate management needs, and to emphasize research aimed at better understanding the workings of the physical, ecological, and human systems surrounding these fisheries. I am defining routine research to be that which follows the existing cost vs. information curve in Figure 1. The alternative, or system-oriented research, is characterized by the potential to *shift*, rather than follow, either of the entire curves: to increase the information obtained at given cost, or to increase the fishery value realizable at a given level of information, opening up new management possibilities within the existing management philosophy. I emphasize the word "potential" in the above definition, as the latter kind of research naturally contains uncertainty as to its eventual utility. If its utility were proven in advance, most likely the research would already have been done.

The distinction between the two kinds of research is not clear. Also, the appropriate level of routine research and monitoring is difficult to determine. Beyond its use in current fishery management, routine information forms an important base for system understanding, further blurring the distinction between the two types of research. Perhaps it is easier to attempt to clarify the distinction by means of examples.

#### EXAMPLES OF SYSTEM-ORIENTED RESEARCH

Elsewhere in this symposium, I was informed that the Atlantic surf clam fishery is presently opened for a few hours, once a week, when a disproportionately large number of vessels descend on the resource. Accuracy of the details are unimportant to my example: currently the abundance is estimated annually, whereas it was also suggested in the symposium that this routine monitoring could reasonably be reduced to a bi- or tri-annual effort. This would be an improvement in the cost-effectiveness of routine research. However, if we consider a system view of the geography of surf clam productivity, we can hypothesize the following model which stands in contrast to routine homogeneous fishery models. Mobility of the resource occurs only during the planktonic phase, when the spawn diffuses along the coast. Clams are immobile once they settle. The edge of the population is determined by physical or competitive conditions, and clams at the edge of the population contribute very little to the population's productivity—their offspring are mostly lost to uninhabitable locations. Clams at the center of the population have the greatest probability of contributing surviving offspring because they are surrounded by inhabitable locations. This

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suggests that an unrestricted fishery could be allowed at the edges of the resource, and that the center of the population should be managed for maximum spawn production rather than maximum yield. Because of filter-feeding cannibalism, the density in the center might have to be reduced somewhat to achieve optimality. The point is that research dedicated to developing a geographic understanding of the system could lead to a rather different pattern of fishing within the present management philosophy, with an increase in realized productivity.

Some examples of system-oriented research on ecosystems include study of the cause and biological effects of prolonged changes in physical conditions. Bakun (In press) has shown some striking changes in the physical conditions off Peru (Figure 2). Changes of this kind, though perhaps not always this extreme, occur in all ecosystems. There is little reason to doubt that these physical changes are associated with changes in spawning and survival of fish eggs, larvae and juveniles, and hence sustainable yields. A system understanding will be necessary to replace the inappropriate static equilibrium yield models presently governing fishery management with models which account for these changes. A related type of ecosystem change is the apparent replacement of one species by another, for example, the anchovy-sardine "flips" which have occurred in California and Peru, and the replacement of herring by sand lance in the north Atlantic. To the extent that these replacements are mediated by interspecific competition, there are good prospects for improved, coordinated management; alternatively, if these replacements prove to be the result of largely independent responses to the physical changes described above, management will have fewer viable options.

The increasing concern over the effects of contaminants and habitat loss on fish production is not being answered by current simplistic single-species fishery models. Again, a system-oriented understanding is necessary to address these problems. An adequate model must contain a representation of the temporal and spatial structure of the population or ecosystem in order to estimate the impacts of local perturbations. These models may provide additional information useful to fishery or ecosystem management, as in the case of the surf clam model proposed above.

Another important reason for system-oriented research stems from the way we address management problems. Traditional academic research has a well-known method: after a problem is posed, the researcher conducts research, gaining *new* information by which the question is answered. It is very rare that management questions or problems can be solved by this method. After the management problem is posed, the fishery or ecosystem researcher must sort primarily through information which has *already* been gathered, such as time series of abundances. There is very little in the way of research to collect new information which can help in solving the problem. Thus our ability to answer management questions is constrained by existing knowledge, by the nature and quantity of past research. The information which will be of greatest long-range use in answering management questions will be gained by system-oriented research rather than by routine research devoted to "fine-tuning" current management.

The remainder of this discussion will focus principally on the research (and monitoring) needed to improve our understanding of marine ecosystems, rather than on research specifically intended to support fishery management. Nonetheless, fisheries are clearly elements of these ecosystems, and fishery research remains important to the discussion in several respects. Fisheries have demonstrated the capability to influence the target species' abundance, sometimes to the point of virtual elimination as a functional element of an ecosystem [this has been the case with the Pacific sardine (*Sardinops sagax*) off California]. Also, fisheries are one of the few ecosystem processes which are nominally under man's control. Indeed, fisheries represent ecological "experiments" of extraordinarily large scale, albeit without proper

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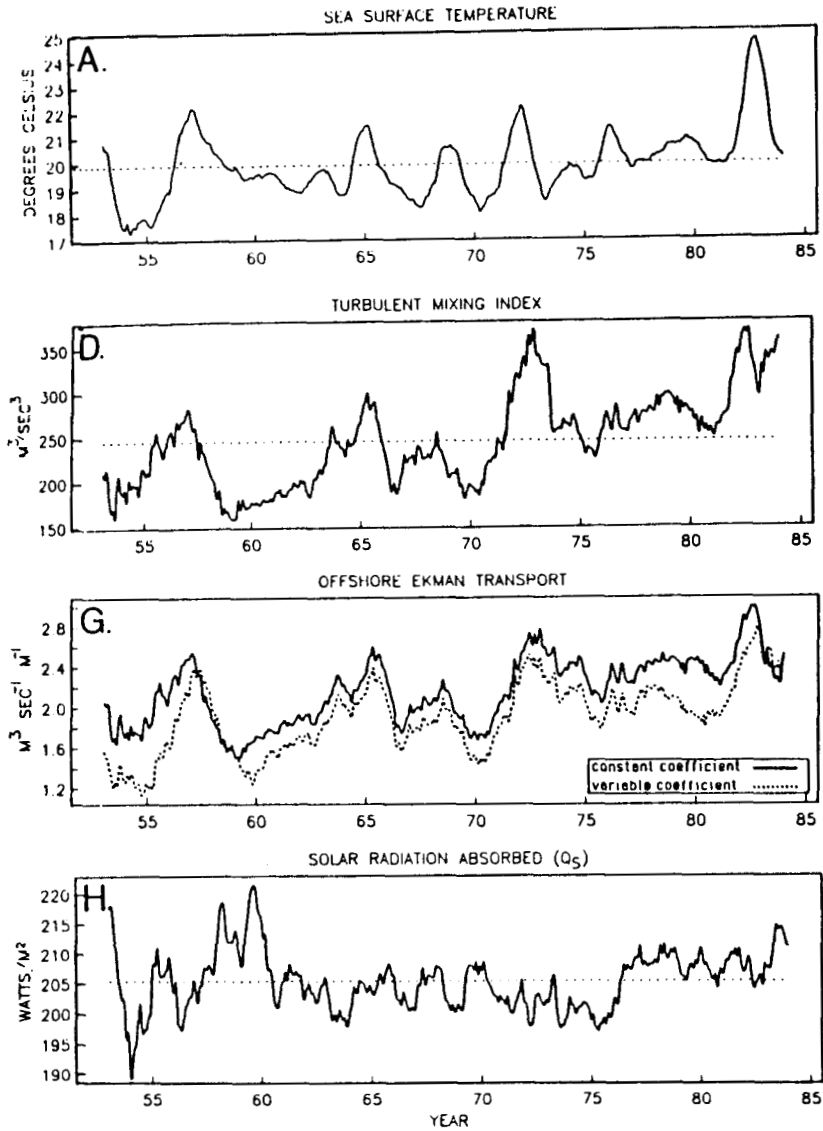


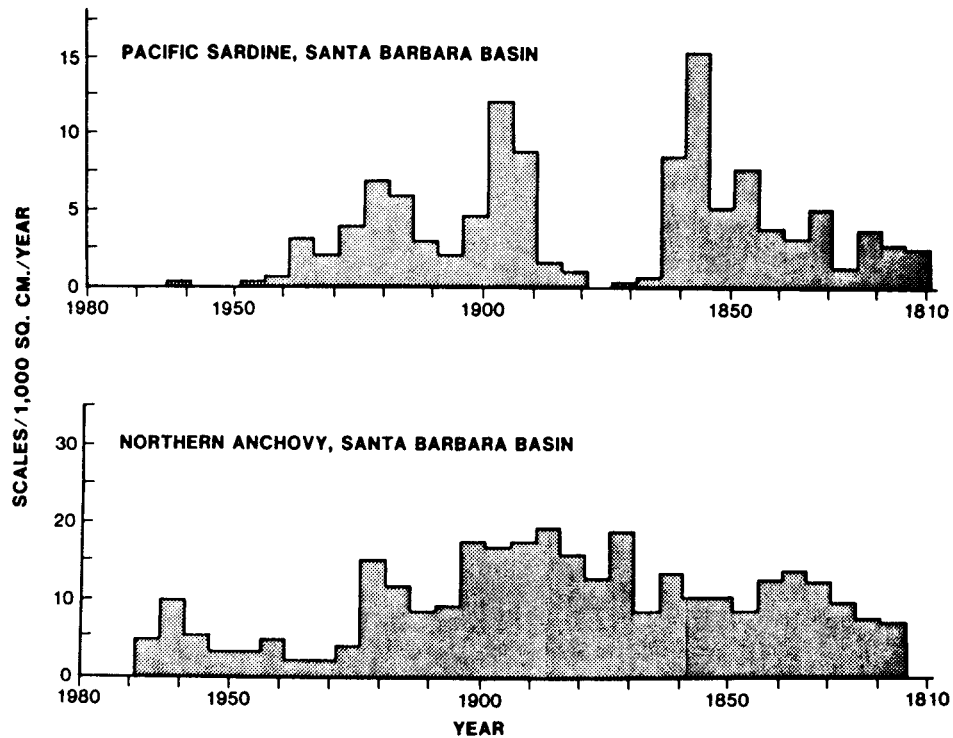
Figure 2. Time series of several physical oceanographic variables off the coast of Peru, showing changes in patterns over time (from Bakun, in press).

experimental controls. Finally, given that a fishery exists, it can be a source of large amounts of information at relatively low cost.

**RESEARCH FOR MANAGEMENT OF MARINE ECOSYSTEMS**

Ecosystem understanding necessarily requires a foundation of knowledge about the physical setting: physical oceanography, climatology etc. This foundation includes continuity of monitoring. Experience has shown that there are major shifts in physical patterns and associated biological patterns as progressively longer time periods are considered. Moreover, these shifts can be sudden, cannot be anticipated, and are difficult to recognize until well after the fact. Bakun (in press) presents time series of a suite of physical measurements for the Peruvian coast, some of which are reproduced in Figure 2. While the presence of dominant events such as the 1954-55 cold period and the 1982-83 El Nino have highly visible effects at the time they occur, the prolonged changes such as the shift in patterns following 1977 are more likely to cause changes in the structure of the ecosystem. Accordingly, it is important to supplement monitoring and research of the present system with information on the past behavior of the system. These sources include historical archives such as newspapers and journals, and natural chronological records such as tree rings and laminated sediments (Figure 3). It is often the biological information in these records that elucidates the changes that must have occurred in the physical system.

Another aspect necessary to understanding ecosystem functions is an appreciation of the historical development of man's impacts on the ecosystem. It is tempting to



**Figure 3.** Recent scale deposition rates for Pacific sardine and northern anchovy off southern California (from Soutar and Isaacs 1974).

think of ecosystem impacts beginning with the major industrialization of fisheries in the early 20th century, but substantial impacts may have resulted from low-technology exploitation in the 18th and 19th centuries. For example, nearly all species of pinnipeds on the Pacific coast of the United States were reduced to very low abundances by the fur and oil trades during the last century and by predator control during the early decades of this century (MacCall 1986). Also, many of the large predatory fishes such as the tunas were depleted off California by 1920, when the tuna fleet began moving southward toward tropical waters. Natural mortality rates of the prey fishes must have been below the historical average as industrialized fisheries on these small pelagic fishes were expanding, a supposition which has never been addressed in the single-species fishery analyses and management.

A third area necessary as background to ecosystem research is biogeography. While it lacks glamour and is time-consuming and costly, an inventory of species, abundances and distributions (especially over time) is particularly valuable to multispecies or ecosystem management decision making. Of course the effort put into this work will vary according to species or trophic groups, both due to accessibility and interest on the part of researchers or managers. Government seems to be a necessary agent in this task, either by doing the work itself (e.g., the egg and larva surveys conducted in association with the California Cooperative Oceanic Fisheries Investigations, CalCOFI, Kramer et al., 1972), or by requiring such information to be part of environmental impact statements (EIS) or similar reviews. It is notable that fishery management plans developed under the MFCMA now must contain an EIS to meet the requirements of the NEPA.

Some marine ecosystems are unique, but most have several parallels. For example the eastern boundary currents off California, Peru, South Africa and North Africa contain remarkably similar assemblages of pelagic fishes, suggesting functional similarities in key oceanographic processes (Parrish et al., 1983). Comparative oceanography and biology of equivalent ecosystems not only provides insight into the workings of those ecosystems, but comparative history of exploitation may provide a rough replication of the massive fishery "experiments" mentioned above.

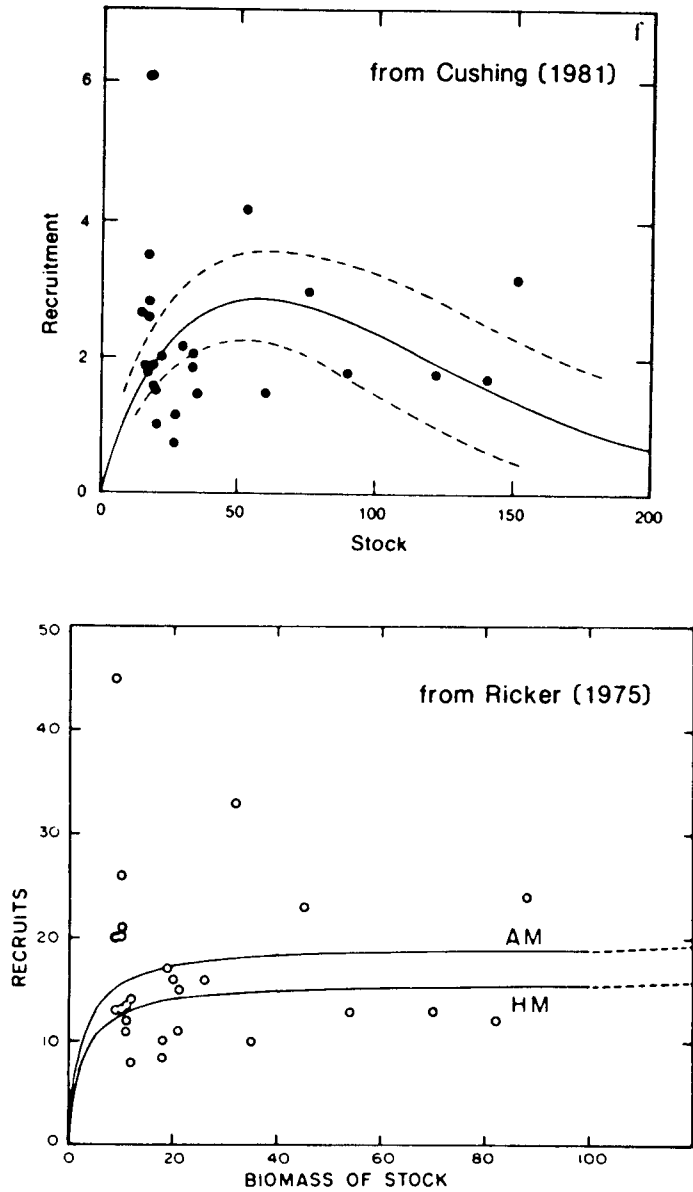
## ECOSYSTEM MODELS

The value of constructing formal ecosystem models is debatable. As a tool to improve understanding of an ecosystem, the exercise often has been of greatest benefit to the builder himself. Unfortunately, this improved understanding has not easily been transferred to non-participants. As a tool to aid managerial decision-making, complicated ecosystem models tend to produce output which is too complicated to assimilate, especially if effects of random variability are included. Also, these complicated models tend to be sensitive to assumptions, such as the functional forms used to represent non-linear relationships. An example of this kind of uncertainty is the assumed form of the stock-recruitment relationship. Two popular stock recruitment models are the Ricker curve and the Beverton-Holt curve. These two curves are shown in Figure 4, which is taken from two well-known publications. Our uncertainty is demonstrated by the fact that two well-respected fishery experts have independently based these curves on the same data! While the two curves are about equally reasonable fits to these data on North Sea plaice, the corresponding anticipated patterns of population growth and stability are quite different. For management purposes, accurate and easily interpreted analyses usually are best produced by a much less complex model which is designed specifically to address the particular issue. Of course there are some issues, such as ecosystem stability and reversibility of species declines, which may require very large and complicated

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models; accordingly, definitive answers should not be expected. Beddington (1986) provides a useful discussion of this problem.



**Figure 4. Demonstration of uncertainty in functional relationships: Independent fitting of two different stock-recruitment relationships using the same data on North Sea plaice.**

The two most successful (or at least the most ambitious) marine ecosystem models have been Laevastu's Bering Sea model (Laevastu and Larkins 1981), and Ursin's North Sea model (Andersen and Ursin 1977). The two models represent rather different approaches to the problem, and require somewhat different kinds of background research and input data. The Bering Sea model is a compartmentalized accounting model, whereas the North Sea model is constructed of simultaneous differential equations. Thus the Bering Sea model emphasizes information on states while the North Sea model emphasizes information on rates, although the two models overlap substantially in their requirements. These large models have tended to be opaque to outside observers, and the extensive "tuning" of parameters which is required to obtain reasonable model behavior can hide serious deficiencies in our knowledge. Both of these models have had the advantage of portraying relatively closed, landlocked ecosystems, unlike the open systems found along continental coastlines or in mid-ocean. A satisfactory structure for ecosystem models of open marine systems has yet to be developed.

A much less ambitious model consists of a static input-output budget for various trophic components of an ecosystem. This would seem to be a minimum requirement for ecosystem understanding, forming the basis for estimating fluxes and perhaps carrying capacities for individual trophic levels or groups. Given the biomass in each trophic category, inputs can be calculated from information on energetics or food consumption, while outputs can be calculated from mortality rates. The matrix can be constructed on the basis either of inputs or of outputs, but can be considered satisfactory only if the two approaches agree, which seldom has been the case even for individual trophic categories. Bergh (1986) developed a trophic budget for the Benguela Current system off South Africa (Figure 5), based on a Delphi method survey of experts' opinions. A severe difficulty, which is common to the study of all marine ecosystems, was his inability to obtain reliable estimates of abundance and rate parameters for the squids, which by any account must be a major element in the system.

A common problem in these models is an apparently insufficient supply of prey. Green (1978) attempted such a budget for the California Current, as a starting point for modeling the effects of fisheries on the carrying capacity of marine mammals, but found that estimated fish and squid production could not meet estimated predator needs. Given that many of those predators, especially pinnipeds, have steadily increased in abundance, she concluded that the imbalance was erroneous, and that current knowledge could not support the modeling effort. Hunter and Lynn (Southwest Fisheries Center, in prep.) have estimated total anchovy (*Engraulis mordax*) predation by mackerel (*Scomber japonicus*) in southern California, and again, estimated anchovy consumption by this predator alone nearly exceeds the total abundance of anchovies. It is clear that substantial uncertainty exists in all three quantities appearing in each cell of the matrix—abundance, consumption (input rate) and mortality (output rate)—but the consistent direction of the imbalances is disturbing. Hunter and Lynn suspect that the mackerel obtained from fishery catches are more likely to have been feeding on anchovies than the average mackerel in the population, thus biasing the samples. In the past, similar discrepancies were perceived for lower trophic levels (e.g., zooplankton vs. phytoplankton, phytoplankton vs. carbon fixation), but these are now being resolved (R. Eppley, Scripps Institution of Oceanography, pers. comm.). The keys to improved understanding have been better knowledge of rate processes, and better accounting for spatial and temporal patterns of variability. Spatial distributions of most marine organisms are characterized by a high degree of contagion (patchiness); trophic interactions must be similarly patchy, and trophic rates may be influenced as much by the spatial variance as by the mean of a species' density.

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**SOUTHERN BENGUELA SYSTEM**  
(Tonnes Carbon x 10<sup>3</sup>. yr<sup>-1</sup>)

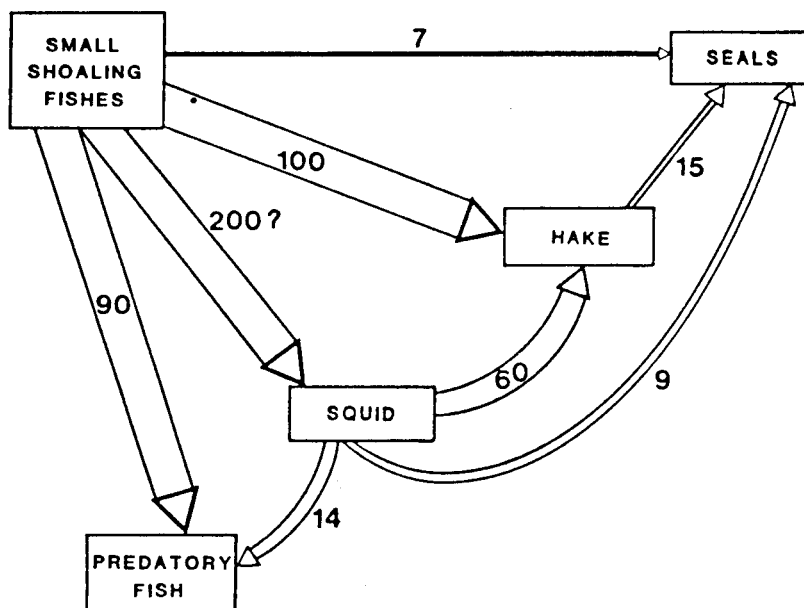


Figure 5. A trophic budget for the Benguela Current (from Bergh 1986).

#### OPERATIONAL CONSIDERATIONS

The expense of ecosystem research requires that surveys and sampling be planned for efficiency, but with emphasis on multiple purpose activity. These two objectives can conflict, as can be seen in the contrast between pelagic fishery landings, which tend to include few species but are conveniently centralized, and landings by demersal fisheries, which often include many species but are geographically diffuse (Figure 6). Another barrier to multiple purpose activity is institutional jurisdictions. For example, the National Marine Fisheries Service has responsibility for marine fishes and marine mammals, but not for seabirds, which are the responsibility of the U.S. Fish and Wildlife Service. Even within agencies, there may be psychological barriers between traditional fishery researchers who subconsciously promote consumptive uses of fish, and marine mammal or seabird biologists who stress the role of fish as forage.

An interesting possibility for low-cost ecosystem monitoring is the use of "indicator species." The reproduction or physiological state of some predators may be closely tied to the availability of prey. For example, the reproductive success of brown pelicans (*Pelecanus occidentalis californicus*) in southern California closely tracks the abundance of northern anchovy, its primary forage (Anderson et al. 1982, Figure 7). Similarly, changes in guano production by seabirds in South Africa and

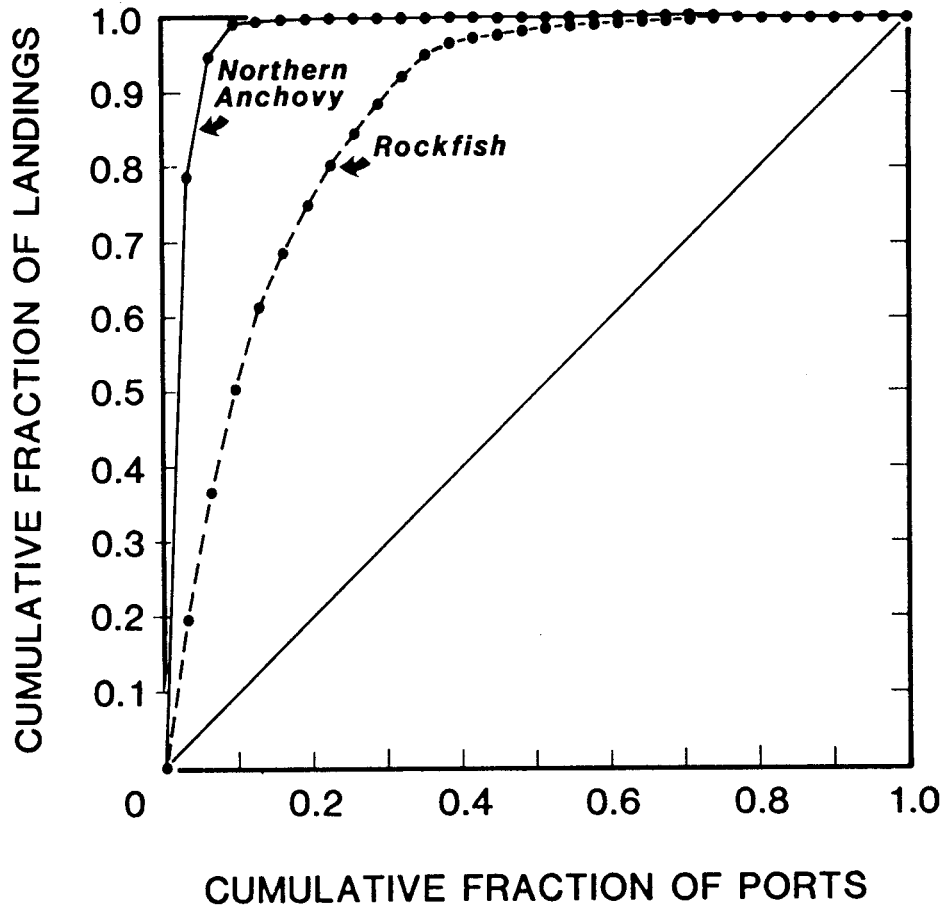


Figure 6. Comparison of relative geographic dispersion of landings of a pelagic fish (northern anchovy) and a groundfish species complex (rockfish, *Sebastes* spp.) in California in 1975.

Peru have reflected changes in abundance of pelagic fishes (Crawford and Shelton 1978). Monitoring of penguins and pinnipeds in the Antarctic has been proposed as a source of information on the abundance of forage species, including krill. Inexpensive (relative to the cost of seagoing surveys) monitoring of these "indicator species" could provide information, albeit imprecise, on changes in forage populations including a variety of forage species such as squids which have not been sampled effectively by existing methods.

Drawbacks to the use of indicator species include the difficulty of interpreting the information without verification or calibration. Use of indicator species as a source of information for fishery management is unlikely not only because of imprecision, but because of the reluctance of fishermen to allow their fishery harvests to be governed by the performance of a competitor. In contrast, indicator

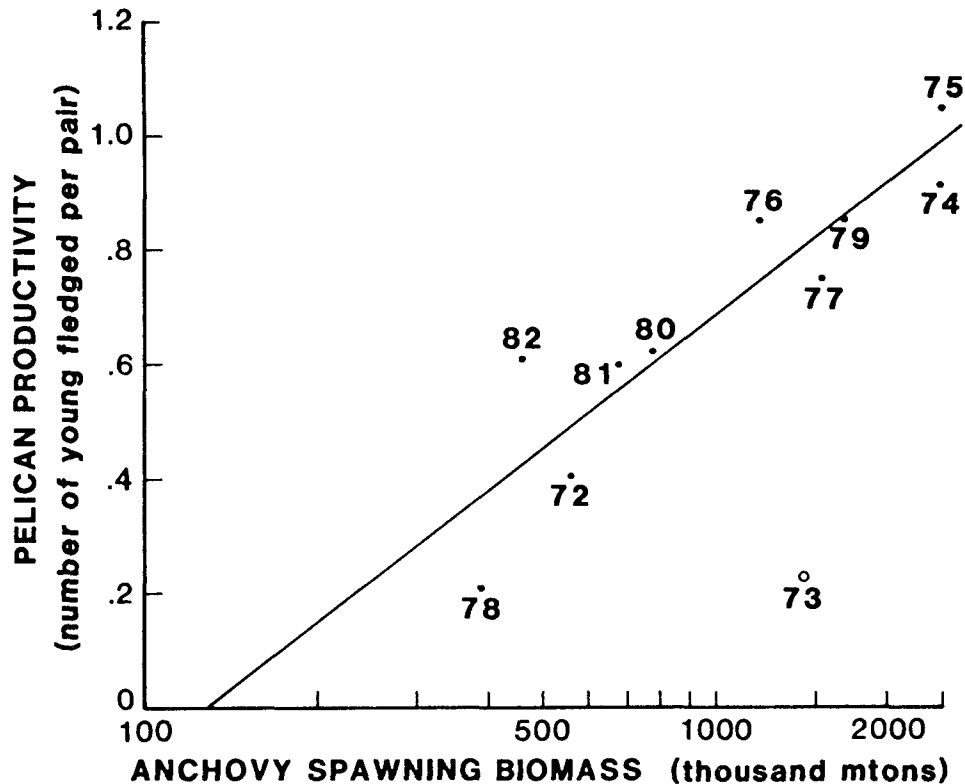


Figure 7. Relation between brown pelican productivity and anchovy spawning biomass off southern California (from MacCall et al. 1983).

species could be used quite effectively in the "low-information" management I described earlier.

#### WHO WILL DO THE WORK?

Fishery and ecosystem research is costly in time, money and manpower. For this reason alone, we must expect severe limitations on the amount of research which can be accomplished. There are additional barriers and impediments which render the work even more difficult. Areas of ecosystem research are divided into a bewildering number of jurisdictions and funding sources, with no single entity being responsible for coordination or integration. Further difficulties arise in large ecosystems which span international boundaries, where various nations may have very different policies toward research and management. The best hope for ecosystem research may lie in formation of consortia similar to CalCOFI on the Pacific coast (Baxter 1982, Reid 1982) which unite local, federal and perhaps international government agencies with academic institutions in pursuing and coordinating ecosystem studies. In the absence of incompatible goals (as might arise from implementation of the MMPA or the MFCMA) the mutual benefits should foster a strong and effective cooperative effort.

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