Article **Carl J. Walters**

Is Adaptive Management Helping to Solve Fisheries Problems?

Adaptive management has been widely recommended as a way to deal with extreme uncertainty in natural resource and environmental decision making. The core concept in adaptive management is that policy choices should be treated as deliberate, large-scale experiments; hence, policy choice should be treated at least partly as a problem of scientific experimental design. There have now been upwards of 100 case studies where attempts were made to apply adaptive management to issues ranging from restoration of endangered desert fish species to protection of the Great Barrier Reef. Most of these cases have been failures in the sense that no experimental management program was ever implemented, and there have been serious problems with monitoring programs in the handful of cases where an experimental plan was implemented. Most of the failures can be traced to three main institutional problems: *i)* lack of management resources for the expanded monitoring needed to carry out large-scale experiments; *ii)* unwillingness by decision makers to admit and embrace uncertainty in making policy choices; and *iii)* lack of leadership in the form of individuals willing to do all the hard work needed to plan and implement new and complex management programs.

INTRODUCTION

It has now been three decades since the concept of adaptive management was first proposed as an approach to dealing with extreme uncertainty about the impacts of various policy choices in renewable resource management (1–3). The concept arose from frustration in attempts to use computer modeling to integrate scientific knowledge so as to make useful predictions for decision makers. In many modeling case studies, we kept finding gross gaps in knowledge about various ecological processes that the modeling indicated to be important, and no indication of progress in dealing with those troublesome processes because they are ones that unfold at space-time scales which are inconvenient or costly for scientists to study (a notorious example is recruitment of new individuals to harvested fish populations, a complex process that typically takes place over spatial scales of thousands of kilometers and time scales of years). We concluded from such cases that if integrative models cannot be reliably developed to compare policy choices, then the only way to learn about those choices is through direct comparisons of their performance in the field, i.e. through planned experimental comparisons. As this concept of management as experimentation was further developed, we used optimization methods from the theory of optimal control to help determine when it might be worthwhile to invest management resources in potentially risky experiments rather than relying upon initial guesswork and subsequent monitoring to uncover good policies (4).

Early case studies taught us to use two main arguments to justify adaptive management experiments, which we called "probing for untested opportunity" and "coping with counter-intuitive dynamic responses". Experimental policy tests are a way to probe the dynamic responses of a system, but more particularly such tests are justified only if the experimental policy represents a possible opportunity to improve management and if historical data are inadequate to show whether the policy has already been tried (inadvertently or deliberately). Counterintuitive responses arise when scientists or managers attempt to base predictions on simple, common sense arguments (like "reducing mortality rate of the fish should cause their abundance to increase"), when in fact the complexity of ecological systems implies that responses may depend on indirect and multiple causal pathways, including pathways that are easily overlooked even when prediction is approached with formal systems modeling techniques.

The idea of an adaptive approach to management continues to have wide intuitive appeal, so that it is now routine to see claims and even legislative requirements (for example, California's Marine Life Protection Act), that it will be used on cases ranging from restoration of endangered species to management of large marine ecosystems. In many cases the claim is simply that the results of initial policy choices will be monitored so as to identify need for corrective action (so-called "passive" adaptive management), but there have also been many cases where our original approach of using computer modeling to identify critical uncertainties and to aid in design of diagnostic management experiments has been followed.

Unfortunately, the practice of adaptive management has been radically less successful than one would expect from its intuitive appeal. A decade ago, I looked back at some 30 case studies where we had worked with interdisciplinary, multi-institution teams to develop adaptive management proposals; I could find evidence of field implementation of experimental policies in only four or five of those cases (5). In a few cases, even the initial modeling step had failed to identify key uncertainties, but for most of the failures there was clear identification of needed diagnostic management experiments but recommendations to do these experiments were simply not followed. In (5), I suggested a variety of reasons for such failures, mainly related to problems with institutional incentive systems. Far more elaborate and elegant analyses have subsequently supported this finding and have suggested a variety of approaches to design of more effective institutions for management (6–10).

With more experience, it is now becoming clear that there are three main reasons for widespread implementation difficulties in adaptive management programs: *i)* failure of decision makers to understand why they are needed; *ii)* lack of leadership for the complex process of implementing an adaptive approach; and *iii)* inadequate funding for the increased ecological (and often economic) monitoring needed to successfully compare the outcomes of alternative policies. This paper discusses each of these reasons and suggests what we might do to overcome them.

Failure to Comprehend the Need for Management Experiments

Proposals for management experiments are often greeted by decision making groups (such as fisheries management councils and stakeholders with strong political influence) with blank

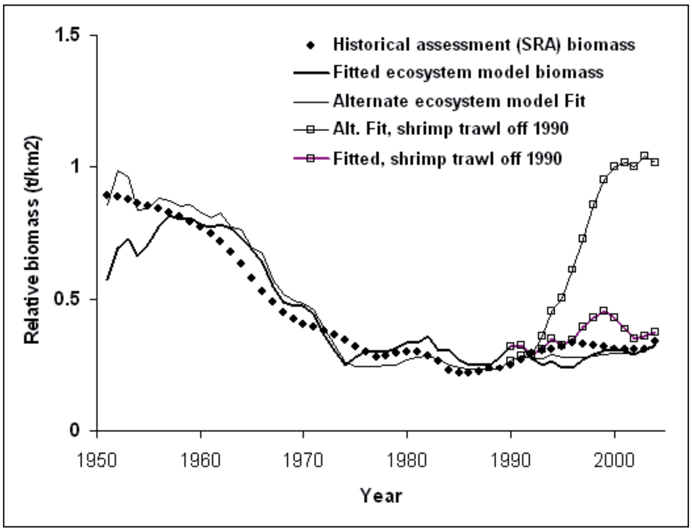

Figure 1. Two alternative fits of an ecosystem model to historical trends in abundance of an important fish species in the Gulf of Mexico, red snapper. Historical biomass estimated using stock reduction analysis which involves fitting a population model to historical catch and relative abundance trend data. The alternative ecosystem model fits to the data are obtained when different assumptions are made about interactions between juvenile red snapper and various of its fish competitors and predators like marine catfish. Note how the two different ecosystem model fits predict similar trends to the historical data (hence, cannot be distinguished on the basis of the data), yet diverge widely in predicted effect of a simulated shrimp trawl closure introduced in simulation year 1990.

stares of incomprehension, or even outright hostility and comments like "why can't you scientists make that prediction; you have gathered so much data already?" To some degree, reactions like these are the long term result of scientists having routinely oversold their capabilities to provide useful advice, so as to obtain grant funding or to gain credibility and hence authority. In highly structured settings such as fisheries stock assessments and water resource planning, where particular mathematical models have been used routinely and parameterized with historical data by many scientists, and have been somewhat successful at providing policy advice, it has become really difficult for practitioners and decision makers alike to admit that their routine calculations and predictions might be highly suspect. But beyond such obvious causes of misunderstanding, many people in key decision making positions appear to expect single, clear predictions and management prescriptions from scientists, even when it should be obvious to all concerned that the scientists should not be offering such definitive advice; the demand for single best prescriptive answers seems most often rooted in attempts by decision makers to "pass the buck" to scientists for making difficult judgments about how to cope with extreme uncertainty.

Another serious cause of difficulty with decision makers is that calls for adaptive management experiments often appear to contradict conventional wisdom or obvious intuition. An excellent example of this problem has been in the fisheries of the Gulf of Mexico (Walters, Martell, and Mahmoudi; under review). The Gulf supports several of America's largest fisheries and in particular the largest regional shrimp trawl fishery. That colorful fishery has been notorious as an example of "bad" ecosystem management, because of its obvious negative impact on marine benthic habitats and its large bycatch of finfish (the weight of fish discarded is typically several times the weight of shrimp actually landed, see [11]). Conservation groups have repeatedly demanded that it be severely restricted or replaced with a fishery based on use of more selective fishing methods. Further support for such restriction has come from data showing the trawl bycatch of small juveniles of another valued species, the red snapper (*Lutjanus campechanus*), likely exceeds the landed catch of that species by several fold (11, 12). The red snapper has historically been quite severely overfished and still faces high fishing mortality rates especially from recreational fisheries that are very difficult to regulate. Trawl bycatch reduction, initially via so-called "bycatch reduction devices" (BRDs) has become a favored policy choice since it appears to have three types of benefits: to restore the snapper stock, to demonstrate concern for conservation of benthic habitats and biological diversity, and to avoid imposing further restrictions on fishers who target snappers.

While it is apparently self-evident and certain to many people that BRDs will be beneficial to Gulf of Mexico fishes, the scientific evidence based on analysis of historical population trends of red snapper suggests a counterintuitive possibility, namely that there is compensatory mortality in juvenile red snapper such that higher juvenile densities resulting from BRD protection will lead to increased natural mortality rates and hence to loss of much or most of the surplus juveniles saved by using BRDs (12). Recent developments with a complex ecosystem model of the Gulf also support this more pessimistic prediction (Walters, Martell, and Mahmoudi; under review). In fact, the ecosystem model predicts two quite different possible outcomes of using BRDs, depending on assumptions made in the model about how other fish species besides red snapper react to reductions in bycatch mortality (Fig. 1). We get equally good fits to the historical data with ecosystem model parameter values that predict no recovery of red snappers, as with parameter values that predict dramatic recovery. The more pessimistic prediction (lack of recovery of red snapper) results partly from compensatory mortality changes, and also from predicted increases in juvenile snapper mortality rate due to recovery of some species that prey upon it, like marine catfishes, and have also been negatively impacted by trawling in the past. Unfortunately, little historical data are available on such predators, since they are not valued species for harvest and have thus not been priority species for research.

When scenarios like Figure 1 are shown to stakeholders in the Gulf of Mexico, the immediate reaction has been to ask the modelers which prediction is most likely, as though we surely must have a single best hypothesis about what will happen. When we deny being able to make such a judgment based on the historical data and recommend instead the obvious policy of closing some experimental areas to trawling and thus obtaining direct experimental evidence about which scenario is correct, the reaction has then been to ask why we do not just get better data on the species that the ecosystem model predicts might increase in abundance so as to prevent increases in red snapper juvenile survival. Blank expressions reappear when we explain that there is simply no way for us to get such data on the other species; such data were not collected historically as the shrimp fishery developed, and there is no way for us to predict, just using recent data on abundances and life history characteristics of the other species, how they will react to reduced trawling mortality.

For decision makers in situations like the Gulf of Mexico to support our recommendation for management experiments like closed areas, they would need to abandon intuitive, simplistic arguments (BRDs will save fish) and make a considerable intellectual investment in understanding why we obtain different or ambiguous predictions when we examine causal pathways and mechanisms more closely. Virtually all failed adaptive management cases have had this character, where in the end it is just easier for decision makers to scoff at the elaborate analysis and models while trusting simpler intuitive predictions, and to defer difficult decisions about management experiments by instead calling for further research (data collection, modeling).

Lack of Leadership in Implementation

I have been involved in a large number (over 30) of cases where the Adaptive Environmental Assessment and Management (AEAM, [3]) workshop process appeared to result in general consensus among stakeholders, scientists, and regulatory agency staff about the need for an experimental management program, but where the recommended program was simply never implemented. In the few cases where implementation did occur, there was at least one singular individual (usually a middle-level staff person from a regulatory agency) who made a very large personal investment of time and energy to make sure that the program actually succeeded.

Implementation seldom takes place in an atmosphere of crisis, where all concerned recognize a need for urgent policy change. Instead, regulatory agency staff in particular are usually comfortable with existing policies and programs, and will not voluntarily make additional work for themselves in terms of setting up new regulations and enforcement procedures, designing and staffing (funds, equipment, people) new monitoring initiatives, and organizing the oversight processes (committees, administrative procedures) typically required for any new management program in today's highly bureaucratized management systems. So, unless one key individual pushes all these people, through apparently endless and largely wasted time spent writing and talking to them, the implementation process will fail at one or more of the necessary implementation steps (and all it takes is for one step to fail).

In none of the successful cases would the key leader be called an inspiring or charismatic personality. Rather, the leaders have been people who *i)* have a broad overview of the decision making and implementation process, along with intimate knowledge of all the people and technical/administrative details involved in each step in the process; *ii)* are very well organized in terms of planning who, what, where, and when specific activities and actions are needed; *iii)* simply refuses to take no for an answer on the many occasions when contributors to the process offer excuses for inaction; and *iv)* are willing to devote their whole career, for extended periods of time (typically several years), to the implementation process. Early in the development of the AEAM workshop process, C. S. Holling wrote about the sorts of people who need to be involved in complex ecological analyses and referred to leaders with these attributes as the "compleat emmanuensis" of the policy process (13).

Inadequate Funding for Monitoring Programs

Lack of adequate monitoring data and historical reference information has become a universal complaint in natural resources management, independent of experimental management initiatives. A few of the worst fisheries disasters, such as the collapse of the Newfoundland cod fishery that has been called one of the most severe social disasters in Canadian history, have been attributed more or less directly to misinterpretation of inadequate monitoring data on trends in stock size (14). So even before we start to talk about enhanced monitoring in support of large-scale management experiments, we know that we will have to work with monitoring programs and data that are already inadequate. Further, in many cases analysis of costs for various improvements in monitoring quickly demonstrates that increased investment in monitoring would not be justified, in the sense that monitoring costs could easily come to be a large part of, or even exceed, revenues to resource users (It can easily cost more to monitor and manage a fishery than that fishery is worth in the first place.).

Management experiments generally involve replicated comparisons of treatment alternatives (e.g., closed versus open areas), preferably over spatial experimental units but perhaps also (or only) over blocks of time. Replication or repetition is absolutely necessary in order to demonstrate what if any differences in response are large enough to be repeatedly visible despite the many other causes of variation in space and time that are always present in complex, open ecological systems. The requirement for replication obviously has large impacts on potential monitoring costs, especially if it is assumed that traditional monitoring methods and systems must simply be expanded to collect the extra information. In fact, most large experimental management proposals in fisheries would be considered prohibitively expensive absent some special source of outside support, since as much money as possible is already being spent on routine monitoring. For example the largest single fisheries management experiment that has actually been implemented anywhere in the world today, aimed at measuring effects of fishing on the Great Barrier Reef, Australia (15, 16), would not have gone ahead without funding from sources external to the regional fisheries management agency.

Almost all management agencies now rely mainly upon in-house or contract data collection, largely by highly educated staff. So fisheries scientists with doctorate degrees now routinely go on long survey cruises, where they mainly collect simple data (fish counts, measurements) that could just as well (or better) be collected by much less costly technical staff with only modest training. Further, the measurements that we make could in most cases be equally well done by resource users (fishermen), at radically lower cost because those people are already out in the field and are skilled at the day-to-day operation of vessels and fishing equipment.

Recognizing these points about data collection, proposals for improved overall monitoring and large-scale management experiments have included a strong recommendation to develop collaborative monitoring programs where resource users collect most of the monitoring data at relatively low marginal cost (17, 18). Predictably, some traditional scientists have objected strongly to such proposals, citing difficulties ranging from costs of quality control to examples of outright cheating (stakeholders recording information that they think will be favorable to them and withholding information, such as tag recoveries, that they think may result in restriction of their activities). Fortunately, new technologies promise to deal effectively with most such objections. For example, fishermen can be provided hand-held computers with GPS and mobile phone capability, electronic charting software linked to the GPS, and data entry/error correction software (essentially an electronic logbook system). Data entered through such systems can be accurately geo-referenced and sent immediately to central data management systems for further error evaluation and use in spatial data analysis.

DISCUSSION

Of the three main causes of implementation failure described above, easily the most important has been lack of leadership to carry out the complicated administrative steps involved in moving a new management vision into actual field practice. There have been various suggestions about how to overcome this problem, mainly involved with techniques for promoting collaborative work among stakeholders (19). Unfortunately, all the organizational theory in the world will not overcome the need for that one singular individual who must make an extraordinary personal commitment to the organizational process. The single most important thing that proponents of adaptive management must learn to do in the future is to develop skill at identifying and nurturing such people. Most of the intellectual skills needed to originate an adaptive management proposal (critical scientific attitude, knowledge of modeling and statistical design, monitoring methods experience) are just not the right ones for carrying out the hard, often boring, and generally frustrating work of implementation.

There are great opportunities for ecological scientists today to participate creatively in the development of innovative monitoring programs that utilize expertise and experience of resource users (especially fishers) to collect data more cheaply. The electronic logbook and data entry systems mentioned above are just one relatively simple example of the technologies that can now be developed for capture of ecological information. Another major research opportunity is in the development of fish tags and tag detectors that can be used by nonscientists to help collect better information on distribution, movements, and exploitation rates. Still another is in the use of advanced acoustic and video techniques, along with technologies like remotely operated vehicles (ROVs), to carry out large-scale surveys of fish and fish habitat.

There remains the fundamental problem of how to convince decision makers to support management experiments. I believe that the key to this problem is for the scientific community to agree to stop providing simplistic point estimates and management advice. That is, when we are asked for our best estimates and predictions, we should agree to offer only a strategic range of possible outcomes for each policy choice (like the extreme predictions in Figure 1), and we should simply refuse to say which outcome we think is most likely. This tactic would place the burden of risk management squarely on the shoulders of the decision makers, essentially forcing them to confront the real uncertainty and think carefully about ways to deal with it. In recent years, the fisheries stock assessment literature has been flooded with papers about how to calculate Bayesian probability distributions for key management quantities (20). Unfortunately, while such analyses certainly look impressive from a mathematical-statistics perspective, they are generally based on hopelessly optimistic assumptions about statistical variability and knowledge of structural relationships, and hence grossly underestimate how much uncertainty ought to be admitted (21, 22). Further, the results of these analyses are typically presented as probability distributions, which are difficult to interpret and are accompanied not by recommendations to do management experiments but rather with admonishments about the need for cautious (so-called "precautionary") management, a risk averse management judgment that scientists ought not to be making in the first place.

The history of fisheries management has largely involved myopic focus on single fish stocks and the direct impacts of fishing on them. Under that focus, it has been permissible to base policy on relatively simple dynamic models, and to ignore more complex interaction effects like described above for the Gulf of Mexico shrimp trawl fishery. Adaptive management has been of little help in dealing with single stock management issue. But the Gulf of Mexico example is not an isolated or unusual one; it is routine to see counterintuitive predictions (and inability to test these with fragmentary historical data) when we attempt to model trophic and habitat interactions in support of ecosystem management questions such as how fisheries at one trophic level affect productivity of fisheries at other levels, and whether natural predators should be culled in order to make fisheries more productive (23, 24). Management agencies today are under very strong pressure to adopt ecosystem-based management approaches (25, 26). With that pressure has come demands for ecosystem modeling and those models will further expose just how deep our uncertainty is about the efficacy of even apparently simple regulatory measures like making fisheries more selective to reduce bycatch. If we are honest about admitting and embracing that uncertainty in developing policy advice, adaptive management experiments will finally come to play a central role in the management of fisheries and their ecosystems. We will simply be forced to find ways to overcome the problems of selling experimental policies to decision makers, leadership in implementation, and high costs of monitoring.

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