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Challenges in adaptive management of riparian and coastal ecosystems

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ABSTRACT

Many case studies in adaptive−management planning for riparian ecosystems have failed to produce useful models for policy comparison or good experimental management plans for resolving key uncertainties. Modeling efforts have been plagued by difficulties in representation of cross−scale effects (from rapid hydrologic change to long−term ecological response), lack of data on key processes that are difficult to study, and confounding of factor effects in validation data. Experimental policies have been seen as too costly or risky, particularly in relation to monitoring costs and risk to sensitive species. Research and management stakeholders have shown deplorable self−interest, seeing adaptive−policy development as a threat to existing research programs and management regimes, rather than as an opportunity for improvement. Proposals for experimental management regimes have exposed and highlighted some really fundamental conflicts in ecological values, particularly in cases in which endangered species have prospered under historical management and would be threatened by ecosystem restoration efforts. There is much potential for adaptive management in the future, if we can find ways around these barriers.

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INTRODUCTION

There is growing case experience in adaptive management of riparian and coastal marine ecosystems. Most management plans now contain at least passing reference to the need for an adaptive approach, especially in settings where mandates for ecosystem management have brought attention to "new" policy options with which we have little historical management experience, such as regulation of river flows . Adaptive management forms a highly visible element in policy planning for major river systems, including the Columbia (Lee 1993) and Colorado (Collier et al. 1997). A major planning exercise in adaptive management is under way on the upper

Mississippi River (S. Light, Minnesota DNR, *personal communication*), using the Adaptive Environmental Assessment and Management (AEAM) process (Holling 1978, Walters 1986). The AEAM process has played a role in current plans for restoration of the Florida Everglades (Walters et al. 1992, Ogden and Davis 1994). A large−scale management experiment is now in progress on the Great Barrier Reef in Australia, using designs developed with an AEAM process and aimed at testing effects of fishing on reef ecosystems (Mapstone et al. 1996).

Although some peculiar and myopic definitions of adaptive management have appeared in a few settings (see review in Halbert 1993), today we generally use the term to refer to a structured process of "learning by doing" that involves much more than simply better ecological monitoring and response to unexpected management impacts. In particular, it has been repeatedly argued (Holling 1978, Walters 1986, Van Winkle et al. 1997) that adaptive management should begin with a concerted effort to integrate existing interdisciplinary experience and scientific information into dynamic models that attempt to make predictions about the impacts of alternative policies. This modeling step is intended to serve three functions: (1) problem clarification and enhanced communication among scientists, managers, and other stakeholders; (2) policy screening to eliminate options that are most likely incapable of doing much good, because of inadequate scale or type of impact; and (3) identification of key knowledge gaps that make model predictions suspect. Most often, the knowledge gaps involve biophysical processes and relationships that have defied traditional methods of scientific investigation for various reasons, and most often it becomes apparent, in the modeling process, that the quickest, most effective way to fill the gaps would be through focused, large−scale management experiments that directly reveal process impacts at the space−time scales where future management will actually occur.

The design of management experiments then becomes a key second step in the process of adaptive management, and a whole new set of management issues arises about how to deal with the costs and risks of large−scale experimentation (Walters and Green 1996). Indeed, AEAM modeling so regularly leads to recommendations for management experiments that practitioners like myself and colleagues at the University of British Columbia have come to use the terms "adaptive management" and "experimental management" as synonymous. In short, the modeling step in adaptive−management planning allows us, at least in principle, to replace management learning by trial and error (an evolutionary process) with learning by careful tests (a process of directed selection).

Unfortunately, adaptive−management planning has seldom proceeded beyond the initial stage of model development, to actual field experimentation. I have participated in 25 planning exercises for adaptive management of riparian and coastal ecosystems over the last 20 yr; only seven of these have resulted in relatively large−scale management experiments, and only two of these experiments would be considered well planned in terms of statistical design (adequate controls and replication). In two other cases, we were unable to identify experimental policies that might be practical to implement. The rest have either vanished with no visible product, or are trapped in an apparently endless process of model development and refinement. Various reasons have been offered for low success rates in implementing adaptive management, mainly having to do with cost and institutional barriers (Halbert 1993, Ludwig et al. 1993, Gunderson et al. 1995, Castleberry et al. 1996, Van Winkle et al. 1997).

This paper discusses four reasons for low success rates in implementating policies of adaptive management, based on my case experience. All, in some sense, are institutional reasons. Further, they are challenges that proponents of adaptive management will have to face routinely in future. First, modeling for adaptive−management planning has often been supplanted by ongoing modeling exercises, apparently based on the presumption that detailed modeling can be substituted for field experimentation to define "best use" policies. There is a further presumption, in such exercises, that best use policies can be corrected in the future by "passively adaptive" use of improved monitoring information. Here, I point out a variety of rather obvious reasons why such modeling exercises will probably fail. Second, effective experiments in adaptive management often have been seen as excessively expensive and/or ecologically risky, compared to best use baseline options. Although I agree with this concern in many settings, I note that it is often a fallacy to presume that some sound baseline option can be found in the first place. Third, there is often strong opposition to experimental policies by people protecting various self−interests in management bureaucracies. I suggest that proponents of adaptive management will have to be forceful about exposing these interests to public scrutiny. Fourth, there are some very deep value conflicts within the community of ecological and environmental management interests. These conflicts have become more of a barrier to policy change than the traditionally recognized conflicts between ecological and industrial (e.g., power production) values.

To some readers, this paper may raise more questions than it answers; that has certainly been my experience in writing it. I have listed some unanswered questions in the conclusion section, in hopes of stimulating further discussion and analysis.

BARRIERS TO MODELING FOR RELIABLE ASSESSMENT OF BEST USE POLICIES

In seven of the adaptive−management planning cases previously mentioned, in which experimental management policies have not yet been implemented, the initial AEAM model development has been followed, instead, by very substantial and continuing investment in baseline information gathering and complex simulation modeling. These investments have ranged from three−dimensional hydrodynamic models for coastal water advection, to individual−based models (IBMs) for population dynamics, to high−resolution landscape models based on GIS information. What probably drives these investments is the presumption that sound predictions (and, hence, good baseline policies) can somehow be found by looking more precisely, in more mechanistic detail, at more variables and factors.

At one recent AEAM modeling workshop, an agency representative referred to the models being developed in the workshop as "toy models" that might be valuable starting points for analysis, but eventually should be supplanted by "real models." Such peculiar terminology (particularly the oxymoron "real model") certainly suggests a belief that models can somehow be much more than just toys to help us think more clearly about problems. Van Winkle et al. (1997) suggest that combining individual−based fish population models with improved physical habitat models can "produce instream flow assessments that are reasonably accurate and far less expensive than an adaptive management approach." However, they base this assertion on results from models tested by experimental changes in water flows, an obvious adaptive management experiment.

The following subsections suggest several reasons for pessimism about our ability to substitute modeling for field experimentation in the near future. These reasons represent warnings to both scientists and managers, and extend warnings offered previously by Hilborn and Walters (1981). Scientists are warned that more research does not necessarily mean better models, or that someone else will know how to integrate research results into a useful model, no matter how fragmentary those results may be. Managers are warned that it is not yet possible to purchase sound "best use" policies just by investing more in modeling and research.

Cross−scale modeling problems: from physics to biology

Riparian and coastal ecosystem models that have been developed for adaptive−management planning typically have at least four basic submodels: (1) a hydrodynamic submodel for space−time variation in water flows; (2) a hydrochemistry submodel for transport and transformation of key chemical variables such as nutrients and sediments; (3) "lower trophic level" submodels for primary, invertebrate, and small "forage" fish production; and (4) population dynamics submodel(s) for key animal indicator species, expressed as IBMs or at least as age−size−space structured abundances. In some cases, we have also developed successional submodels for changes in plant community composition. Generally, these models do not presume an ability to use any single currency (e.g., energy) for ecosystem description, or to fully describe all physical−chemical−biological features and interactions that constitute ecosystem "function." In general, the models are restricted to processes and mechanisms that link specific water management actions (flows, chemical inputs, harvest regulations, etc.) to specific indicators of ecological performance (plant community structure, abundance of "valued" vertebrate indicator species). Most often, the hydrodynamics and hydrochemistry submodels simulate not only physical and chemical processes, but also "tactical" or "operational" behavior by people who operate water regulation structures, sewage outfalls, etc., on short space−time scales.

The most difficult technical issue in developing and using these models has been the cross−scale linkage between physical/chemical and ecological processes. Generally, we must solve the hydrodynamic and chemical equations over very short time steps (minutes to hours) on fine spatial scales (dozens of meters to a few kilometers), to maintain basic physical continuity (mass balance) in the calculations. Calculations are further complicated in marine and estuarine settings by the need to account for transport and mixing due to tides. Thus, the physical submodels create an enormous computational burden in running a linked, overall model for much longer ecological time scales (years, decades). Sometimes we can decouple the physical/chemical and ecological submodels, run the physical scenarios "offline," and then "drive" the ecological submodels with results from these scenarios. Walters et al. (1992) used this approach in screening water management alternatives for the Florida Everglades. Yet, decoupling the physical and ecological submodels makes it very difficult to play with the model, i.e., testing its sensitivity to various parameters and exploring management alternatives by trial−and−error. In my experience, such play is critical to develop understanding of a complex model and to search for better management policies. We simply do not learn much by grinding out a few, very detailed management scenarios and comparing them using various quantitative performance indicators.

Even when we can find ways around the technical difficulties, there are fundamental conceptual difficulties in representing how the rapid, localized physical and chemical changes "feed upward" to influence change at larger ecological scales. Simon Levin (1992) expressed the cross−scale issue in modeling very well: "In some cases, the patterns must be understood as emerging from the collective behaviors of large ensembles of smaller scale units. In other cases, the pattern is imposed by larger scale constraints." For example, when we see a fish growth rate, we must understand that it emerged from many prey capture events and a complex temporal regime of changing metabolic rates, driven by changes in temperature, water currents, site selection choices by the fish, etc. We cannot pretend to model every member of this ensemble of events, even in the most detailed "mechanistic" models of fish growth. In practice, we represent the collective effects of many microscopic ecological events by models that (1) calculate space−time averages or totals over at least some minimum averaging scale, and (2) selectively ignore many events, concentrating attention on a subset of situations that we presume to be critical, such as physical−chemical conditions in fish spawning areas when eggs are present. In short, we assume that the organisms that we are trying to model act like natural averagers, smoothers, and selectors of events in their environments. *We must rely on empirical experience, not modeling or physical principles, to tell us how much averaging and selecting we can safely do.*

No physical/ecological linkage model developed to date is close to being a "complete" description of the linkage, even for simple processes like growth. Things get even nastier with processes like natural mortality and recruitment that arise from more complex behavioral interactions distributed over (arising, accumulating over) larger scales. In older modeling terms, it is silly to pretend that there are "black box" and "white box" models; our models are collections of black box representations of phenomena that take place at scales too small (and large) for practical observation and simulation. Obviously, we cannot assure policy makers that our models will give accurate predictions: they are incomplete representations of managed systems.

Further, we cannot assume that increasing model detail (more complete representation of space−time event structure) will result in progressively more accurate predictions and/or reduced risk of making a very bad prediction. There are at least three reasons for distrusting detailed models as much or more than simple ones. First, it is possible for critical interactions or events to be highly concentrated in space and time at scales/locations/times that we have ignored, or over which we have incorrectly assumed a simple averaging process. For instance, Bakun (1996) points out that marine fish recruitment may be determined by transient conditions at very localized fronts, eddies, and other concentrating structures in the ocean. Second, adding more detail adds more parameters to the model structure, yet each of these parameters is likely to be less well supported by field data; this "overparameterization" can degrade the predictions of a mechanistic model in exactly the same way that it can cause statistical prediction models to fail. Third, some ecological interactions result in positive feedbacks that can propagate effects of localized events across scales to produce highly variable, unpredictable spatial patterns at much larger scales (Holling 1992). Obvious examples are the spread of forest fires, defoliating insects, and exotic species. Although we may be able to predict the occurrence of such cross−scale propagation events, we seldom have accurate enough data on process rates and initial spatial pattern to accurately simulate where the propagation of each event will lead.

It is as important to test a detailed model as any other model against results of careful field experiments; modeling is no substitute for experimentation. Yet, concerns about simulation models apply equally well to management predictions based on direct extrapolation from small−scale field experiments (pilot studies). Many ecological processes are simply not evident or exhibited at small spatial scales, but may be critical for overall dynamic response at larger scales. This is particularly important in relation to dispersal and migration. For example, an early case in adaptive−management planning was the sockeye salmon (*Oncorhynchus nerka*) fishery of the Fraser River, British Columbia, Canada (Walters and Hilborn 1976). This fishery harvests many distinct genetic stocks, and abundance has been greatly reduced by fishing. Based on recent historical data, Walters and Hilborn hypothesized that reduced fishing would allow stocks to rebuild to much higher, more productive levels; they recommended a management experiment to test this hypothesis. Canadian Department of Fisheries and Oceans biologists countered with a proposal to manipulate fish spawning densities on just one or a few spawning sites, to test the hypothesis with much less impact on the commercial fishery. Walters and Hilborn argued that this pilot experiment would be misleading, because it would not test for large−scale redistribution of spawning and recolonization of historical spawning areas (a cross−scale propagation problem). Many spawning sites are no longer occupied in the system, and substantial increases in overall spawning density might be needed to stimulate density−dependent dispersal and recolonization of these sites. This example illustrates how highly nonlinear and dramatic the cross−scale response can be. Walters and Hilborn based their concern about dispersal and recolonization on observations of pink salmon (*Oncorhynchus gorbuschka*) in the Fraser system. This species was virtually eliminated from the upper part of the river basin by a landslide that prevented upstream migration of spawners early in the century; in the early 1950s, a few thousand spawners were seen in one upriver tributary (to which fish had presumably dispersed from lower system spawning areas). By the late 1980s, several million pink salmon were returning to upper system tributaries. A "minor" dispersal event in short−term population dynamics had profound long−term impacts on abundance.

Nonadditivity of parameter and data effects in population dynamics analysis

One might intuitively expect that more detailed models are less prone to make bad predictions due to errors in estimating any one model parameter, as each parameter relates directly to a smaller part of the overall population structure. One might also expect that when many kinds of data are used to estimate model parameters, the population dynamics assessment should be less sensitive to assumption errors about how to interpret or use each observation. Such intuitions are dangerously wrong.

Population dynamics calculations typically involve sequential products, not sums, of production and survival factors; when any one number in a product is wrong, the whole product is affected proportionally. For example, recruitment of age 1 fish per adult would be represented as a product of adult fecundity x egg survival x fry survival x early juvenile survival. In habitat management models, these survival rates would probably be broken down further into products over particular times and places where management impact could occur. All it takes to make the overall recruitment prediction very wrong is to misrepresent one of these rates. In traditional population dynamics modeling for harvest management, this problem is commonly avoided by relying on empirical analysis of overall recruitment/adult abundance relationships; this simple empirical option is not available when management interest is in the impact of manipulating particular habitat factors (unless an adaptive−management experiment has already provided empirical information on how to relate recruitment directly to the habitat factor). The product effect occurs whether the model state representation is number of animals, or an ensemble of individuals, each subject to separate risks over time.

To model fish population dynamics, one generally needs to use historical information on trends in relative abundance for parameter estimation and model validation. Typically, such trend data are very difficult to collect. In most cases, it has been necessary to use historical trend statistics based on commercial or recreational harvesting (e.g., catch per unit effort). Even when much detailed life history information, such as historical population age compositions, has been available, estimates of some key population parameters (current population size, net rate of population change) have been found to depend critically on interpretation of the crude trend data. In particular, use of catch per effort trends has a nasty way of overestimating abundance and productivity (Hilborn and Walters 1992, Walters and McGuire 1996); this effect is not diluted or prevented by having detailed auxiliary information, e.g., population age composition. For most riparian management situations, we have a legacy of weak historical data today; it would take many years to correct this situation even if better monitoring programs were started immediately.

Difficult and emergent processes

Modeling exercises for adaptive−management planning always reveal substantial gaps in knowledge about key processes and functional relationships. This may be inevitable when the modeling is directed at predicting the impact of specific policy options and actions. The specific causal linkages involved in such predictions often concern biophysical relationships that have not particularly interested scientists. Other key relationships are of general interest, but have eluded investigation by traditional scientific methods. Such difficult relationships typically describe "emergent" effects of events and interactions that accumulate over relatively large space−time scales, such that measuring the effects is costly and/or slow. Typical examples are compensatory changes in survival rates when animal abundance is reduced, changes in rates of recruitment with changes in parental abundance, and impact on survival of chronic stress. e.g., occasional flooding.

Scientists have three choices when faced with such difficult relationships: (1) try to reduce the relationship to a set of component subrelationships or processes, and study these; (2) seek empirical data on the relationship from historical experience or comparative analysis of spatial data from different situations; or (3) try to devise a large−scale field experiment to directly measure the relationship under partially controlled conditions. The reductionist approach has been useful in some settings: for example, IBMs have been used to understand "emergent" compensatory changes in fish survival rates (Van Winkle et al. 1993). However, it is dangerous as a general approach, due to cross−scale problems. Analyses of historical and comparative data are generally part of the model development process, and almost never provide the range and resolution of data needed. Large−scale field experiments aimed at particular processes and relationships are becoming more popular in ecology, but have proven technically very difficult (or we would have done them long ago).

A good example of a difficult relationship that arises regularly in riparian ecosystem modeling is the survival−flooding relationship for floodplain vegetation. In many river systems, stabilization of flows by upstream dams has allowed invasion of woody vegetation into stream bank areas where regular seasonal flooding would have prevented or limited natural vegetation development. In such settings, a common policy question is whether to restore at least some of the natural seasonality of flow. To compare flow management options, models must predict the duration/depth of flooding needed to kill (or substantially thin) vegetation. This mortality results from a complex set of physiological reactions to inhibition of photosynthesis and respiration during inundation, changes in chemistry of the root zone, mechanical damage by water, and attendant changes in vulnerability of the plant to pests and diseases, etc. If one simply reduces the modeling problem to a set of specific physiological relationships, there is little hope of correctly modeling how this complex of reactions will unfold as mortality under field conditions. Further, historical data are not likely to be much help. A typical comment from field investigators to a modeler might be "Well, we have only seen two floods in recent years; the flood of 1983 was a huge one and killed most of the plants, but the flood of 1978 only lasted a few weeks and appeared to have little impact." Obviously, the modeler is not going to build a credible functional relationship for general exploration of flooding options from such limited experience. The last resort might be to try a field−scale flooding experiment, subjecting various experimental stream bank areas to different depths and durations of flooding. The reader may wish to ponder the logistics needed to carry out (and the political acceptability of) such an experiment in, say, the Grand Canyon, without subjecting the whole Canyon to variable flooding regimes.

An emergent principle of adaptive management is that, for every difficult functional relationship, there is a scientist willing to claim the ability to measure it for you if you will provide enough research money to measure details of how the relationship arises. There is no way for management and research administrators to know when such claims are nonsense or wishful thinking, until several projects have been funded and have failed. As long as scientists can stay one step ahead of administrators in the funding game, much scarce research money will continue to be wasted.

Confounding of factor effects in historical validation data

Ultimately, the most telling condemnation of the presumption that modeling can be substituted for experimental management comes not from arguments about difficulties in model development, but from a much more basic argument about use of historical data to test models. No model builder is foolish enough to claim an ability to develop models so precise and complete that validation using historical data is unnecessary, and few model users would be naïve enough to believe such a claim. Unfortunately, many model users seem to be unaware of the gross distinction between a "valid" model (one that is consistent with, or fits, historical experience) and one that makes correct predictions. It is common, perhaps ubiquitous, in applied ecological modeling, to find a wide range of alternative models that are equally valid, but make violently different predictions about effects of various management policies. Alternative valid models involve different assumptions about factors whose effects are confounded in the record of historical experience (i.e., factors that have varied together over time, rather than taking on the clearly contrasting values expected if they had been varied experimentally). The issue is not about "mechanistic" vs. "statistical" models; each valid model usually has a perfectly reasonable mechanistic basis.

Wading bird populations have declined substantially in the Florida Everglades, and various models or hypotheses have been advanced to explain the decline (Walters et al. 1992). Detailed models are now being developed to aid in planning restoration of this ecosystem, including IBM simulations of wading bird responses to changes in water management policy (S. Davis, South Florida Water Management District, and D. DeAngelis, Miami International University, *personal communication*). The detailed models probably will demonstrate that declines can be explained by historical changes in water management and historical drainage of the wetland. However, this demonstration will not allow rejection of at least two valid hypotheses about the importance of events that have occurred outside the areas being modeled: wading bird distributions may be changing due to "distant magnets" that are attracting breeding birds to alternative nesting locations along the Atlantic Coast, and breeding success may have been impacted by changes in the estuarine margin of the wetland system and in adjacent Florida Bay.

There have been bitter debates about fish population impacts due to larval entrainment by nuclear power plants in the Hudson River (Barnthouse et al. 1988). At least for striped bass (*Morone saxatilis*), compensatory improvement in postlarval survival rates may essentially cancel the entrainment impacts. Recently, R. Hilborn and colleagues (University of Washington, *personal communication*) have developed population models showing that the compensatory effect may be very strong; huge increases in striped bass larval abundance following fishery closures in the mid−1980s apparently have not resulted in any increase in juvenile abundance or recruitment to older ages. In a delightfully critical review of these models, R. B. Deriso (Inter American Tropical Tuna Commission, *personal communication*) pointed out that the decline in postlarval survival that Hilborn attributed to compensatory effects is, in fact, a strong temporal pattern (declining through the 1980s) that could equally well be attributed to other changes in the Hudson River, such as reduction in fertility due to improvements in sewage treatment.

Catastrophic declines in stocks of lake trout (*Salvelinus namaycush*) usually have been attributed to the combined effects of sea lamprey (*Petromyzon marinus*) invasion and overfishing. Models for lake trout recovery have assumed that both are important (Walters et al. 1980). Yet, Milliman et al. (1987) note that the relative impacts of these two factors cannot be clearly separated (both fishing and lamprey abundance increased violently at about the same time), and other changes such as eutrophication also could have been important. Uncertainty about the role of sea lamprey could be an important policy issue in the future, especially considering public concern about the efficacy and side effects of chemical control programs for lamprey.

In British Columbia, marine survival rates of hatchery−produced coho and chinook salmon (*Oncorhynchus kisutch, O. namaycush*) have declined substantially since the mid−1970s (Cross et al. 1991). Coho salmon have also declined off the Oregon coast (Nickelson 1986, Emlen et al. 1990). Declines have been associated with changes in two major factors: (1) increased stocking rates (that may cause compensatory reduction in survival), and (2) changes in ocean temperatures and upwelling patterns. Proponents of hatchery production blame environmental changes, whereas others are concerned that there may be an ocean carrying capacity such that increasing hatchery production not only may be ineffective, but also may reduce survival rates of wild salmon (Walters 1994). In this case, experimental manipulation of hatchery releases has been suggested as a possible way to distinguish between alternative hypotheses about the survival decline (Peterman and Routledge 1983, Perry 1995).

It is not helpful, in examples like these, to point out that all of the factors identified may be important, or that it is possible to construct mechanistic models to represent the "probable" impact of each factor. Such models do not allow assessment of the relative importance of each factor (it is generally possible to make all of them fit the data equally well, by making reasonable assumptions about processes and rates for which there are inadequate historical data). To admit that a mix of factors may be important is to admit that the impacts of any policy that influences a particular factor (e.g., lamprey control policy or fishing policy in the Great Lakes) are even more uncertain than would be admitted if the particular factor were known to be dominant or not important at all. Presumption that mechanistic modeling will help in such cases is an invitation to wasteful and counterproductive "battles of models."

Inability to discriminate among alternative hypotheses based on historical data does not imply that modeling and analysis of historical data are useless exercises. Modeling can be a powerful tool to screen hypotheses that are very unlikely to have given rise to the available data and policies that are unlikely to be effective in future. A key notion in the AEAM process is that modeling can help to winnow the alternatives to a manageable set for further testing and evaluation. Modeling can act to direct more efficient field testing processes and, hence, make adaptive management fundamentally different from evolutionary adaptive processes, in which policy innovations are generated and tested more or less at random. For example, an AEAM modeling exercise is currently underway on the Kootenai River, upper Columbia River system, to seek water management policies for restoring endangered white sturgeon (*Acipenser transmontanus*) and other species impacted by the Libby Dam. In this exercise, we (Carl Walters and Josh Korman) have shown that temporal patterns of sturgeon recruitment failure following dam construction are consistent with hypotheses relating egg and larval survival to reductions in summer flows, but not with hypotheses involving reduction in nutrient loading below the Libby Dam, or loss of floodplain backwater habitats downstream from the dam. In this case, the AEAM model shows quite convincingly that recruitment failure should have occurred many years sooner if backwater habitat loss were the culprit, or several years later than it did if nutrient loss were to blame. This finding alone can result in considerable management savings, avoiding wasteful investment in fertilization programs or backwater habitat restoration. (Of course, it may be worthwhile to do these things anyway, to meet other ecosystem management objectives for the Kootenai River.)

COSTS AND RISKS OF LARGE−SCALE MANAGEMENT EXPERIMENTS

Adaptive management is generally assumed to be relatively expensive, especially if it involves large−scale field experiments. Increased cost begins with the modeling work needed to define clear hypotheses and policy options. Then, experimental options often involve substantial costs and lost income by riparian economic interests, with eventual benefits from these investments accruing to other interests well into the future. Almost always there is a need for substantial investment to improve monitoring programs. And finally, manipulative experiments always increase at least some ecological risks, in comparison to the very conservative options favored by environmental interest groups. Methodology for objective, economic comparison of experimental management options is poorly developed, and there is no general consensus about how to value or weight possible experimental outcomes (Walters and Green 1996).

There are certainly cases in which experimental management changes of the scale needed to resolve key uncertainties would be unacceptably costly or risky. For example, Parma and Deriso (1990) evaluated alternative experimental harvesting policies for the important Pacific halibut (*Hippoglossus stenolepis*) fishery, for which the relative importance of fishing vs. environmental factors in causing recruitment fluctuations has long been debated (the so−called "Thompson−Burkenroad debate;" see Skud 1975). In this case, the critical experimental regime would be to maintain high fishing pressure during periods of stock decline, a very costly and risky policy. However, most debates about cost and risk have not been so well founded, and appear instead to be mainly excuses for delay in decision making. The next section reviews some fallacies about cost arguments that have been used in some major riparian cases.

Direct costs to riparian economic interests

Some proposals for experimental manipulation or restriction of water uses would be quite costly to economic interests. For example, proposed restoration of seasonal−flow peaking in rivers like the Colorado or upper Columbia would involve losses in annual power production valued at several million dollars per dam affected (Collier et al. 1997; C.J. Walters and J. Korman, *unpublished data*). Synchronization of refuel cycles for nuclear power plants on the Hudson River, to provide a 2−yr cycle in plant outages and, hence, entrainment rates of larval fish, would also cost several million dollars (in lost fuel, outside power purchases) per year. Experimental reduction in hatchery salmon releases off the British Columbia coast (Perry 1995) could result in lost commercial and recreational harvest values totaling between $10 million and $100 million per year, depending on the method used to value recreational harvests.

Although such costs appear superficially large, it is important to compare them to costs that users might face under other policy options or proposals. In the Columbia and Colorado cases, there is strong pressure from environmental and endangered species interests to require massive changes in (or even removal of) hydro dam operations. In the Hudson case, environmental interests have demanded that the New York Power Authority install cooling towers, which could cost as much as $1 billion. In the Pacific Northwest, there is growing concern about the efficacy and side effects of salmon hatcheries, as well as pressure to shut down hatchery production entirely. Thus, it is by no means clear that economic interests can count on public support or authority to maintain "business as usual" much longer. If, in each of the examples listed, there is even a 10% chance that legislative or legal decisions will result in massive and permanent policy change, the expected cost (0.1 x cost of massive change) of trying to maintain current policy would be radically higher than the cost of an experiment to demonstrate that radical change is unnecessary.

Unfortunately, there is no simple, objective way for economic interests to decide whether the odds of being forced into radical policy change (e.g., 10%) are high enough to justify switching to a cooperative, experimental approach. It does no good for experimental−management proponents to point out to economic interests that the legislative and judicial track record for environmental interests has improved considerably in recent years, because this trend could reverse at any time, with public recognition of the indirect costs of improved environmental management. Further, it is not clear that environmental interests will support compromises in the form of experimental policies. For example, environmental groups in Australia have bitterly opposed experimental plans to test effects of fishing on the Great Barrier Reef, arguing that more reefs should be closed to consumptive users and that effects of fishing can be evaluated with computer models (Mapstone et al. 1996).

Intergenerational trade−offs: short−term pain for long−term gain

Ecological responses to experimental management regimes generally occur over a wide range of time scales, from seasonal to decadal. Some useful observations may occur within days or weeks, e.g., sediment transport and beach formation effects seen in recent flooding in the Grand Canyon (Collier et al. 1997). More often, management concern centers on population dynamics responses of vertebrates, which are seldom exhibited fully in less than a decade or two. Therefore, most management experiments involve a strong element of intergenerational trade−off in value. Treatments initiated today generally have substantial costs to present resource users and the public, but the legacy of response information from these treatments will mainly be useful to the next generation of managers and users. Management experiments seldom appear economically worthwhile when expected benefits are computed with the relatively high discount rates (3% or higher) usually used in economic development planning (Walters and Green 1996).

Perhaps the best counter to myopic cost−benefit arguments against long−term experimentation is simply to point out that we consider it an ethical responsibility to husband renewable resources and ecosystems for future generations, even if this husbandry forces considerable restraint in how we use resources today. Such ethical arguments have been critical in developing laws that require sustainable use of slowly renewing resources (forests, long−lived fish), and in justification of substantial public expenditures for land purchases and use closures to expand the North American base of parks and protected areas. Presumably, it is as important to invest in gathering better information for future management as it is to provide a habitat base for that management, especially if we cannot count on accumulating better understanding through ongoing research investments alone (see previous section).

High monitoring costs

Well−designed management experiments can have extremely high monitoring costs, particularly with requirements for replication and comparison of contrasting treatments. Further, ecosystem management objectives usually result in demand to monitor a far broader set of response variables than has been traditional in fisheries and wildlife population management. Physical and chemical variables are usually fairly cheap to measure, but ecosystem "support service" variables, such as primary production, net CO_2 uptake, and trends in rare and endangered species, can be very expensive to monitor, even in terms of simple trend indices suitable only for comparing experimental treatments. Monitoring multiple response variables at multiple sites and time scales does result in some economies of scale, but costs are often still prohibitive if monitoring is done using traditional methods of ecological field measurement.

Development of affordable monitoring programs for adaptive management will typically involve substantial, scientifically risky innovation in methods and approaches. Spatial monitoring is expanding rapidly with improvements in remote sensing and satellite information−gathering capabilities. Many temporal monitoring methods, such as recreational use counts, can now be automated using new techniques in digitial control systems (e.g., robotics), video recording, and event sensing. In situations where human observers and judgment are needed, scientists must learn to better exploit opportunities for developing cooperative working arrangements with a variety of people, mainly resource users and other stakeholders, who are already out in the field in large numbers. For example, experimental monitoring of changes in reef fish densities in response to fishing on the Great Barrier Reef may involve training local people from sport−diving clubs to do standardized transect counts and paying them on a per−count basis (Walters and Sainsbury 1990, Mapstone et al. 1996). In a current experimental study of methods to improve sport fishing in small lakes of British Columbia, fishing lodge owners are paid to collect fishing effort, harvest, fish size distribution, and tagging information on >12 experimental lakes.

Substituting remote and automated−sensing methods for traditional field observation usually increases the startup, capital costs of experimental programs, which increases the apparent risk of such programs compared to traditional monitoring by agency staff and consultants. Likewise, substituting local, part−time labor by field site users for labor by professional field staff creates a variety of obvious risks (e.g., deliberately incorrect or erratic reporting) and some complex logistical problems, ranging from on−site training to verification sampling. Thus far, natural resource managers and scientists have been quite conservative about these costs and risks, generally preferring to reduce experiment sizes (variety of treatments, replication, duration, complexity of monitoring set) rather than investing in innovative monitoring approaches. Unfortunately, future generations will pay the price of such false economies.

Risk to sensitive species

Management experiments in settings like the Florida Everglades have been considered risky for species that are lucky enough to be well adapted to situations created by current management (e.g., Snail Kite in the Everglades; Ogden and Davis 1994), or are rare enough to be threatened by any changes that may impact habitat or reproductive success. Usually, so−called "sensitive" species have very specialized habitat requirements; there is no assurance that experiments designed to restore natural habitat structure (e.g., mosaic of riparian vegetation types representative of complex flooding histories) will enhance particular habitat types within the overall structure. To some degree, management experiments almost always threaten at least a few sensitive species.

Perhaps the best answer that proponents of management experimentation can give to arguments about increased risk to sensitive species is one of comparative risk: baseline or default policies that would be followed in the absence of experimentation are often highly uncertain in the protection that they would afford sensitive species. Often, the best justification for experimenting in the first place is the lack of an obvious best course of action. This has become particularly common in recent years, with broadening of management objectives to include considerations like "biodiversity," for which there is little historical management experience. In many riparian settings, there may ultimately be no way to avoid hard decisions about fundamental value conflicts among ecosystem restoration, consumptive use, and protection of some rare species.

Misunderstandings about experimental design options and opportunities

Many people think of "experiments" in a simple way, as comparing measurements between treatment and control units, and think of modeling and experimentation as distinctive, mutually exclusive, ways to gain understanding. Such views invite us to consider that experiments can be done only with well−replicated systems (how can one experiment on the Everglades; there is only one of it?), and to suppose that modeling is the only way to deal with unique systems.

To understand why such views can be misleading, consider what we mean by "response to treatment" in experimental research. In an experimental unit, it is the *difference* between what happens in that unit and what it would have happened without treatment. In principle, we cannot be certain what would have happened if the treatment had not been applied (we cannot both treat and not treat a unit). To interpret or measure any response to treatment, we must *engage in modeling*, somehow predicting what would have happened had treatment not been applied. In traditional design settings, we use measurements on control or reference experimental units as models to predict what would have happened. In before−after comparisons on single systems, we use behavior before treatment to predict what would have happened. Generally, there is no particular reason to believe that spatial predictors (spatial controls) are much better than temporal ones, except in rare settings where we can "guarantee" representative predictions by deliberately selecting a large number of both treatment and reference units at random from a large universe of units. In most practical settings for applied ecologists, there is not much freedom to engage in the enviable practice of randomization in the first place; a strong onus is placed on the experimental manager to use the best model possible to predict what would have happened.

The realization that modeling is an integral, necessary part of experimental analysis opens doors to broader thinking about design options and methods for predicting baselines against which to measure response effects. Importantly, it also opens the door to understanding that there is no risk−free experiment: predictions of what would have happened without treatment can be wrong no matter how we make them (many reference units, temporal comparisons, etc.).

In this broader view, the really nasty situations are not those in which there is only one system to manage (no spatial replication), but those in which proposed management treatments are effectively *irreversible*, with no way to compare treatment alternatives, even within temporal blocks, and where any experimental unit treated incorrectly is "lost" forever. In these situations, we have no option but to predict the irreversible effects through modeling of some sort. In my view, the way to approach such situations is not through modeling in the first place; rather, we should seek creative ways to avoid irreversible treatments in the first place, if necessary by finding reversible treatments that "model" the irreversible ones to at least some degree (see the Hudson River example: synchronizing refuel cycles offers an alternative to the massive, effectively irreversible, choice of building cooling towers).

SELF−INTEREST IN RESEARCH AND MANAGEMENT ORGANIZATIONS

Adaptive management experiments underway in riparian and coastal ecosystems mainly involve relatively simple institutional settings, with a single lead management agency and a few dedicated people who have organized and maintained the experimental initiative. Experimental management planning has floundered in complex institutional settings like the Florida Everglades, Columbia River, and Upper Mississippi River, where management, research, and policy change involve collaboration among several agencies with complicated, overlapping historical responsibilities and legal mandates. An excellent review of institutional "barriers and bridges" to social learning and adaptive management in such settings is Gunderson et al. (1995). In operation, almost every management proposal or change is usually threatening to at least some organizational interest groups. Further, complex management settings seem to spawn large research investments, both because scientific work offers a possibility of certitude in decision making and because "more research is needed" is always a convenient answer in situations where bureaucratic and administrative interests are best served by delaying hard management decisions.

Leadership for experimental management should be coming from established management agencies, where knowledge is concentrated and where people are most acutely aware of deep uncertainties. This is not happening; instead, pressure and leadership for adaptive management are coming largely from nongovernmental interests, via mechanisms such as court decisions and legislative acts that more or less force the agencies into new directions.

Perhaps the best example of missed experimental opportunity is in the Columbia River basin, where adaptive−management planning via processes like AEAM has been underway for over a decade. To improve survival rates of downstream migrant salmonids, dam operations have been modified to allow higher spring freshet flows, which should reduce juvenile transit times and, hence, predation mortalities (Lee 1993). This "water budget" policy is costing >$40 million per year in lost power production, with outcomes that are extremely uncertain. Perhaps the policy is viewed by some as an adaptive−management experiment, but it is difficult to imagine a more poorly planned one. Effects of the policy will be confounded with various other changes that are occurring in marine survival rates, habitat and hatchery management practices within the system, and other water use impacts on water quality and flows. To avoid such confounding and to reduce losses in power production, an obvious experimental approach would be to deliberately vary the freshet flows from year to year in a planned−treatment sequence (or at least to use an on−off pairing of treatment and baseline flows in 2−yr time blocks).

Why are obvious win−win experimental opportunities like this being missed by management agencies? In my experience, at least three organizational factors prevent such policies from being put forward in favor of all−or−nothing policy change: (1) belief that pretense of certainty is necessary to maintain agency credibility; (2) promotion of process research approaches by scientists; and (3) inaction as rational choice by bureaucratic decision makers.

Belief that single best judgments are necessary to maintain credibility

Government agencies often defend particular policy initiatives as if these were certain to produce desirable outcomes, even if this defense involves such extreme measures as suppressing scientific dissent within the agency (Hutchings et al. 1997). Although such defensive positions may involve factors as simple as personal pride and deeply held beliefs by people in agency leadership positions, agency staff commonly say that they must present options with confidence and certitude to maintain credibility with political decision makers and players from other agencies. That is, many agency people apparently view admission of uncertainty as admission of weakness, and assume that the outcome of admitting weakness will be inaction or ineffective compromise policy. For example, fisheries stock assessment teams routinely present assessment results with greater confidence than the data justify, on the assumption that providing a wide range of stock size estimates will result in fishing interest pressure to use the most optimistic estimates (let us fish until you can prove that there is a conservation problem). Such pressure certainly occurs, but it is becoming less common with moves by management agencies to adopt a "precautionary principle" in decision making (when uncertain, assume the worst or at least seek a risk averse option).

It is very difficult to convince people who adopt such views that they will gain more credibility with political decision makers by openly admitting uncertainty and then suggesting positive (and sometimes less expensive) ways to deal with that uncertainty through management experiments. We have so little experience with openly admitting uncertainty that those contemplating such a move cannot look to other cases for empirical evidence of how decision makers will react. Widely publicized cases like the 1996 Grand Canyon flow release (Collier et al. 1997) are now critical in demonstrating that bureaucrats have much to gain by dealing wisely with uncertainty.

Adaptive management as threat to process research interests

It is depressingly easy for scientists to convince themselves, and bureaucratic funding agencies, that "fundamental understanding" of ther process or mechanism that they study is somehow important to predictions about impacts of ecosystem management policies. It does not seem to matter to such scientists that the things they study can only be usefully incorporated into management predictions if they can be integrated with a complex of other mechanisms, at least some of which will not be studied for funding or technical reasons. Thus, physical oceanographers argue that detailed hydrodynamic analyses are necessary to understand processes ranging from larval dispersal of fish to transport and dilution of terragenic nutrients. Phytoplankton ecologists argue that primary production is the basis of aquatic food chains and must be understood in order to make predictions about food chain responses to interventions like fertilization or sewage treatment. Fisheries biologists note that individual variation in life history patterns is critical to understanding "emergent" population dynamics phenomena like density dependence in juvenile mortality rates. Social scientists point out that resource values and decision making must be understood in terms of the complex social setting within which values and decision−making procedures have developed. At every level and scale in large management problems, people say "my concerns are important, so fund my research."

There are two ways to deal with scientific self−interest in planning adaptive management and allocating resources for management−oriented research: cooperative and antagonistic. The cooperative approach involves scientists in developing models and experimental policies in such a way that modeling reveals obvious gaps (it is clear that detailed research alone is insufficient to provide management answers)and makes it obvious that large−scale experiments will create opportunities for scientists to gain better understanding by comparative study of the field situations created in these experiments. The antagonistic approach points out that field−scale experiments can directly reveal net, overall linkages between policy and important management performance indicators, often more quickly and cheaply than studying and synthesizing all the components needed for prediction, so that much of the process research would be a waste of effort from a management perspective. Cooperative approaches will probably serve all interests better, by removing incentives for treating experimentation and process research as competing interests, and by harnessing the creativity and experience of scientists in designing better experiments.

Bureaucratic and political inaction as rational choice

Bureaucratic and political decision makers often face a nasty choice between acting decisively to initiate substantial policy change (major restriction of users, investment in restoration, conduct of large, risky management experiments) or waiting to see if the problem of the day will correct itself naturally or be resolved through research. It is wrong to assume that decisive action is the "optimum" choice for such people, whatever the weight of objective evidence about the urgency of action. Decisive action generally has immediate and obvious costs, ranging from loud outcries from affected economic interest groups to the risk of embarrassment if the policy does not perform as expected. On the other hand, the costs of inaction are seldom so immediate: there may be louder outcries from some interest groups, but these can often be alleviated by pointing out that delay allows more time for research and careful planning. Further, ecological problems often do correct themselves (e.g., recruitment failures originally attributed to overfishing may actually have been due to unfavorable environmental conditions for juvenile survival). For many decision makers, even a short delay can be enough to ensure that someone else will have to make the decision. It should not surprise us at all to see a remarkable range of excuses used to delay the difficult decisions needed to implement a significant program of experimental management.

In situations in which inaction or delay is the rational choice for decision makers, proponents of experimental management face a very difficult choice: accept the delay and hope for some natural event to create a more visible crisis that cannot be ignored, or enter the political arena and try to make inaction more costly (at least more embarrassing) via public information, advocacy for legislation requiring change, or even threat of legal action. These are difficult choices, especially for responsible managers and scientists who have become advocates of adaptive management through careful analysis, and who assume that results of the analysis will be used rationally by equally responsible decision makers. It is not my intention to suggest that responsible scientists and managers should become political advocates for experimental management, but we should understand that, in many situations, it is a waste of time to invest further effort in developing more precise and rational justifications that will simply fall on deaf ears. We should also understand just how large and perilous a step it is from analysis to advocacy.

FUNDAMENTAL CONFLICTS IN ECOLOGICAL VALUES

If developments such as dams for power production had only negative ecological impacts on riparian ecosystems, it might be relatively easy to develop public consensus about how much economic value to forego in order to mitigate these impacts. Such relatively simple trade−offs are not what we see today in major cases like the Columbia, Mississippi, and Colorado Rivers and the Florida Everglades. Stakeholder involvement processes like AEAM have often revealed considerable flexibility and constructive attitudes from "development" interests (power producers, transportation interests, consumptive water users), but intransigence and bickering among interest groups representing different "ecological" values. Conflict among ecological interests has been particularly intense where historical development has created "new" ecological values.

Regulation of seasonal flooding patterns has allowed endangered species to prosper in some places; strong legal mandates to protect these populations could thwart efforts to restore natural hydrological regimes with attendant natural ecosystem structure. In the Florida Everglades, the Snail Kite (*Rostrhamus sociabilis*) is abundant in the regulated water pool of Conservation Area 3, and the Cape Sable Sparrow (*Ammodramus mirabilis*) has invaded areas along Shark River Slough where natural flooding had prevented development of vegetation communities appropriate for nesting. In the Grand Canyon, songbirds have thrived where insect production has increased in exotic plant communities that have invaded shoreline areas that were naturally flooded, and Peregrine Falcons (*Falco peregrinus*) have become abundant; maintenance of this new food chain may conflict directly with restoration of seasonal flooding aimed at sediment management and maintenance of habitat requirements for endangered native fish species.

In other riparian situations, exotic or naturally rare fish species have become abundant following water regulation, and now support valuable sport fisheries. In the Kootenai River of British Columbia and Montana, a rainbow trout (*Oncorhynchus mykiss*) fishery has developed below Libby Dam, in response to regulated, cold flows; productivity of this fishery could be substantially reduced by restoring spring−summer freshet flows to provide improved spawning conditions for an endangered stock of white sturgeon. In the Grand Canyon, restoration of spring freshets could similarly impact a rainbow trout fishery. In the Upper Mississippi River, the recreational fishery for a variety of species has improved with creation of many stable water pools developed for barge navigation; in this system, year−round maintenance of stable water levels may prevent maintenance of natural riparian vegetation communities and may lead to more rapid loss of the pools due to sedimentation.

Changes in species composition are not the only legacy of historical management practice; conflicts can arise from transient impacts when other cumulative effects of that practice are reversed. In the Upper Mississippi River, restoring seasonal flow/level patterns would probably cause the mobilization of sediments that have accumulated in transportation pools, with various economic (e.g., dredging) and ecological impacts on downstream pools for at least a few years. In the Everglades, increased salinity due to reduced flow from Shark Slough has probaby allowed sea grass communities to prosper along the coast to the northwest of Florida Bay. To restore higher runoff through Shark Slough could kill these communities, allowing wave and wind action to mobilize marl sediments that have accumulated in the beds. The resulting turbidity plume would probably extend southeast into Florida Bay, impacting sea grass communities and fisheries.

There is considerable danger that administrators and politicians will seek to deal with conflicting ecological values by employing compromise restoration policies, based on the presumption that there is a smooth trade−off between hydrologic restoration and species response. Thus, we may see the Libby Dam managed for modest freshet flows that are enough to substantially reduce rainbow trout spawning success, but inadequate to allow successful sturgeon spawning. Similarly, minor freshets in the Grand Canyon may be enough to restore beaches for boaters, but wholly inadequate for native fish species."Tinkering" with water regulation in the Everglades may reduce risk to endangered species, but without even coming close to restoring seasonal ecosystem function enough for many wading bird species to recover. In short, it is quite possible that compromise options will give lose−lose outcomes for all ecological interests.

Faced with such dangers and with the certainty of loud outcries from ecological interest groups (such as recreational fishers) if more decisive and extreme policy changes are made, administrators are likely to delay action as long as possible. Such delays may threaten species and system functions that are already declining under present water management policy. However, we do not generally have good enough historical data to develop convincing models and trend analyses to demonstrate the urgency of these alternative threats.

Thus, it would appear that conflicts over ecological values are likely to be one of the main impediments to policy design for adaptive management and ecosystem restoration. But there is another possibility. These conflicts are becoming more obvious and are being exposed to public scrutiny; for example, an eloquent review of ecological issues in the Grand Canyon appeared in a recent issue of *National Geographic* (Long 1997). As debate intensifies over alternative ecological values, as it almost certainly must in the next few years, it may create the sort of crisis or catalyst to promote change that Gunderson et al. (1995: 489 ff.) argue may be critical to an "adaptive cycle" of institutional change and responsiveness.

CONCLUSIONS AND QUESTIONS FOR READERS

In hindsight, it is easy to see various reasons why the simple, attractive idea of treating management as experimentation has been so difficult to put into practice. Objections to large−scale experiments range from faith in our ability to purchase answers through process research and modeling, to concerns about ecological side effects and risks of experimental policies. These objections provide a rich set of excuses to delay decisive action by those who can profit from, or find protection in, such delays. Perhaps some arguments presented here will help supporters of experimental management to counter the most superficial and self−serving objections.

The critical need today is not better ammunition for rational debate, but creative thinking about how to make management experimentation an irresistible opportunity, rather than a threat to various established interests. That is, we need to show that actively adaptive management can create win−win outcomes for scientists, bureaucratic administrators, politicians, and resource/environment interest groups. Almost every AEAM planning exercise has at least hinted at the existence of such outcomes, usually in the form of diagnostic "probes" or field trials that provide a wealth of clear response information without commitment to any permanent change in management strategy. Perhaps as we accumulate, and shamelessly publicize, examples of these outcomes, the public will come to realize that business as usual is no longer a viable option for sustaining and restoring riparian ecosystem values.

This paper has addressed a broad range of issues and has obviously left many questions unanswered. The following list of questions that I have not been able to answer might form the basis for further discussion using the interactive format of *Conservation Ecology*.

1) Even if field experimental tests are ultimately needed for any model, shouldn't we at least try to model in as much detail as possible so that we maximize the chance of identifying critical space−time scales and events?

2) Why are cross−scale problems much more than just computational inconveniences that can be overcome by modern supercomputers?

3) As scientists, we are driven to try to understand difficult processes by decomposing them into manageable details for investigation; why should we accept a different standard of understanding (e.g., direct "black−box" measurement of process effects through field−scale experiments) in applied settings?

4) Isn't there a risk, in studying difficult field processes via crude field experiments, that too narrow a range of process effects will be revealed by the experiments, whereas a more detailed research analysis might reveal functional structure that could be used to construct more widely applicable "submodels" for such processes?

5) When effects are confounded in validation data, why not proceed with management, using a worst−case or precautionary assumption, usually additivity of effects?

6) Too often we cannot "turn back the clock" to gather validation data that were not recognized historically to be important or were considered too expensive to collect; isn't a mechanistic model better than nothing for such cases?

7) Cooperation in experimental management is a gamble for economic interests, because experiments may reveal worse impacts than expected; how can we convince economic interests that the experimental gamble is a better one than going to court to fight for "rights" of use? What is the role of science in providing this advice?

8) One way to solve the problem of appropriate discount rates for environmental and ecological planning would be to carve particular rates in stone via legislative or constitutional directive; is the public legislative arena where we should go to deal with this problem?

9) Is there any way to speed up large−scale field experiments so as to avoid the discounting barrier?

10) Why is the development of innovative methods for large−scale monitoring not usually considered a good research topic, especially for aspiring graduate students?

11) Most ecological monitoring programs end up "doing the thing right" (precise, local measurement) rather than "doing the right thing;" what can we do to change this scientific culture?

12) Should sensitive species that have prospered under historical management be considered for endangered species listing in the first place, except in obvious instances where these species have shifted habitats in response to management, such that no natural population still occupies original habitat?

13) It is my impression that people and agencies that work the hardest to maintain credibility are actually the ones who end up with the least. Is this impression widely shared?

14) What is our responsibility as scientists to make inaction more uncomfortable, i.e., under what circumstances should a scientist deliberately try to orchestrate a strong public reaction to inaction?

15) There are many examples of apparent win−win options for riparian restoration (e.g., Grand Canyon water flows could be managed to maintain a far richer ecosystem than was natural); why do so many ecologists and environmentalists today equate "natural" with "best"?

RESPONSES TO THIS ARTICLE

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