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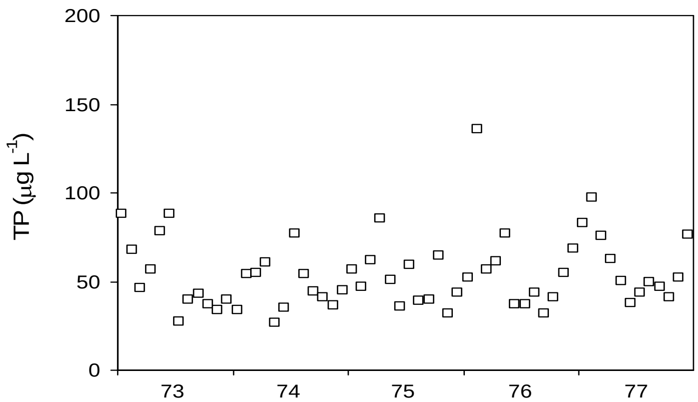
APPENDIX A - LAKE OKEECHOBEE PHOSPHORUS EXAMPLE

The following example, using total phosphorus (TP) data collected in Lake Okeechobee, can help to illustrate some of these points. The data considered here are monthly surface water samples from eight pelagic stations, averaged by month for two time periods: 1973-1977 and 1997-2001.

Raw Data – the following graphs contain the raw data collected from the sampling program (Figure A-1). These data are of the type that might be entered into the CERP database. In this case, there was considerable seasonal variation, and 5-year blocks of data were considered suitable for representing the two periods of time. Visual inspection of the graphs suggests that TP has increased, but this requires formal statistical testing.



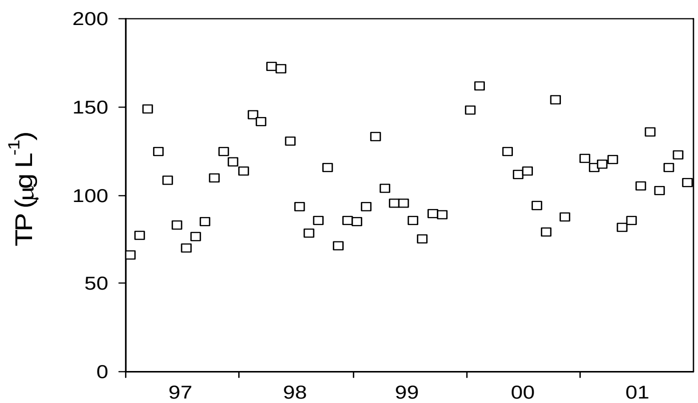


Figure A-1: Raw data collected from sampling in Lake Okeechobee.

Basic Statistics – the following table contains the basic statistics for the two time periods (Table A-1).
Table A-1.

Period:	73-77	97-01
N	60	55

Mean ($\mu g\ L^{-1}$)	54	108
Median ($\mu g\ L^{-1}$)	50	107
Max ($\mu g\ L^{-1}$)	136	173
Min ($\mu g\ L^{-1}$)	27	66
Standard Deviation ($\mu g\ L^{-1}$)	20	27
Skewness	1.5	0.6
Kurtosis	3.5	0.3

These data support the notion that TP has substantially increased. However, the 73-77 data are significantly different from normal, based on standard tests of Skewness and Kurtosis, whereas the 97-01 data are not significantly different from normal.

Data distributions – the following graphs indicate the distribution of data into deciles for the two time periods (Figure A-2).

1973-77 1997-01

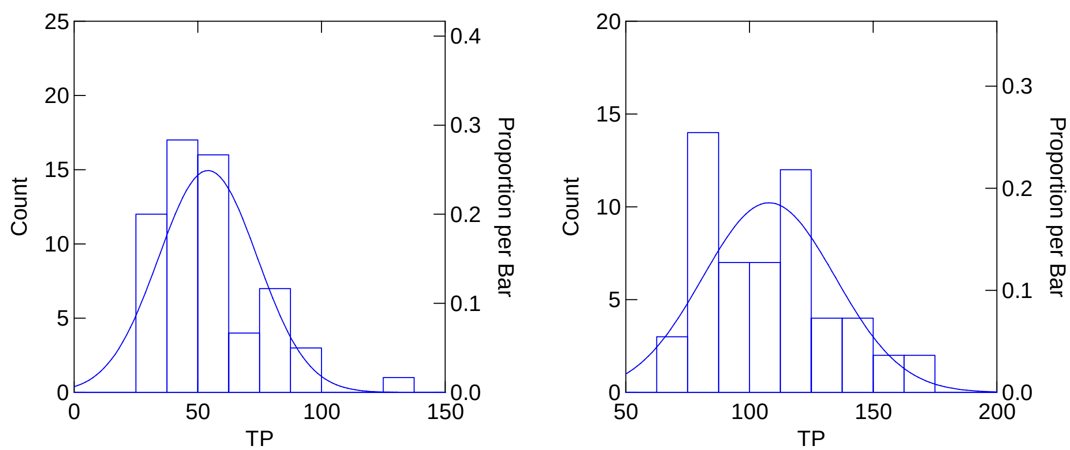


Figure A-2: Distribution of data into deciles for two time periods

Power Test – a formalized power test could be run on the data from either time period, to identify how much the mean concentration of TP would need to change in the next period of record for it to be significantly higher or lower. Most statistical packages have this feature, and a general explanation can be found in any basic statistics book. As a general rule of thumb, and a quick preliminary test, the power to detect a change is approximately twice the 95 percent confidence interval of the data, assuming that the distributional characteristics do not change. In the case of the 1997-01 data from Lake Okeechobee, this corresponds to approximately 20 μg L-1 TP.

Evaluating Change – a variety of methods may be used by the investigator to determine whether or not there has been a change in the measured attribute over time, and/or whether the data are approaching some quantitative performance goal established in the Monitoring and Assessment Plan. No specific method is recommended by the AT;

however, for any method that is used the investigator must ensure that critical assumptions are met and the method has been documented in the peer-reviewed literature as being appropriate for purpose for which it is used.

In the case of Lake Okeechobee, there actually is a 30-year record of pelagic TP concentrations that has been examined by trend analysis (there has been a significant increase). The investigators dealing with that dataset used a non-parametric trend test because of the seasonal variation in the data. In the example provided here, however, we could compare the TP concentration of the two time periods with a simple Student's *t*-test, working with log-transformed TP concentrations (log-transformation substantially reduces the skewness and kurtosis of the 1973-77 data, such that both distributions approximate normality).

The following are results of the T-test, including a graphical display of the data from the two time periods, which better allows for a sense of data overlap (Figure A-3).

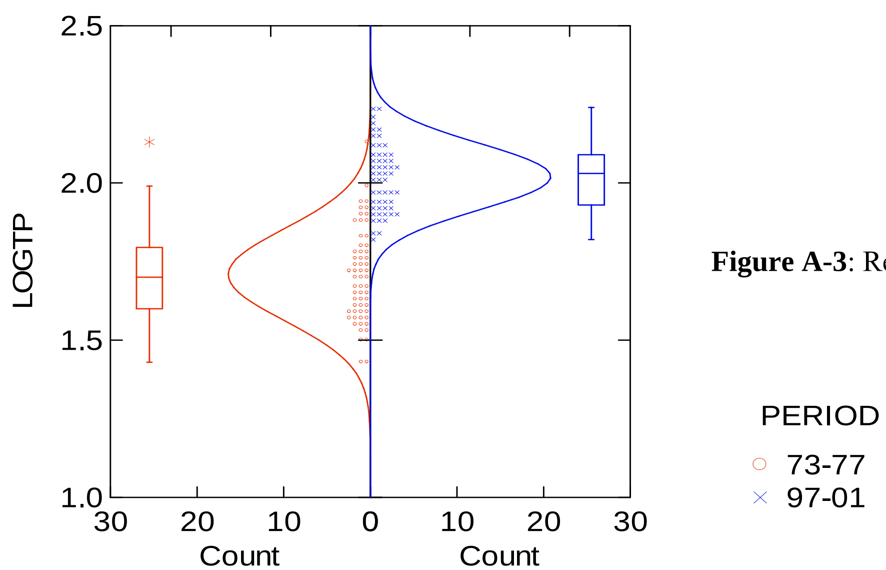


Figure A-3: Results of T-test

T-test results:

T	13.3
Degrees of Freedom	113
Significance	0.001

Variability Issues with Long-Term Trends – in addition to a need for identifying seasonal patterns for statistical purposes, it is critical that the investigator have good knowledge of long-term natural variability in his/her dataset, so that erroneous trends are not reported to the AT. The Lake Okeechobee dataset for TP concentrations serves as an excellent example (Figure A-4):

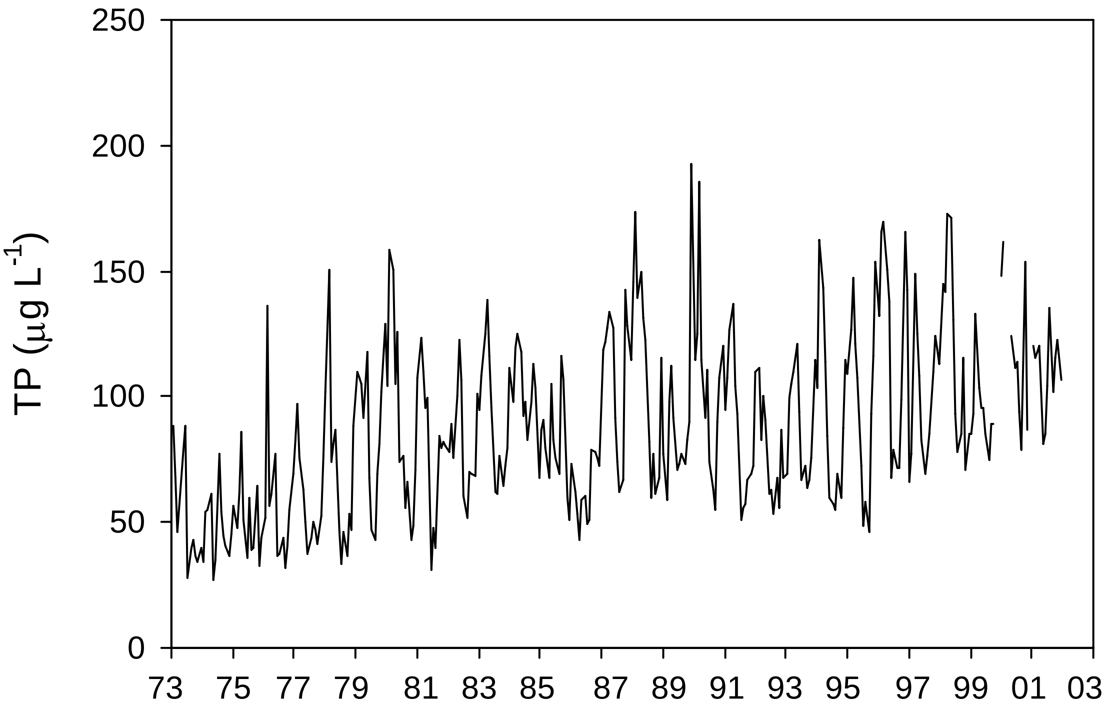


Figure A-4: Variability in long-term trends for Lake Okeechobee

These data have seasonal spikes, associated with wind-resuspension of P-rich bottom sediments during winter months, year-to-year variation associated with differences in rainfall, inflow and intensity of windstorms, and multi-year variation that in part tracks changes in water level. If one considers as an example the time period from 1980 to 1987, which is quite long (7 years) by most standards, investigators may have reached the erroneous conclusion that water quality had been substantially improved by certain P control programs implemented in the watershed in the early 1980s. In reality, the lake TP was displaying a transitory excursion and later increased.

APPENDIX B - POWER ANALYSIS RESOURCES

The importance of power analysis can be illustrated by examining the possible outcomes associated with hypotheses testing (U.S. EPA 1992). The alternatives, familiar from statistical hypothesis testing but applicable to our performance measures, are as follows:

- Type I error: the hypothesis is actually true but it is rejected. By convention, the investigator accepts a small probability, ≤ 0.05, of incorrectly concluding there are differences when in fact there are no differences. Lower or higher confidence values can be selected, depending on the severity of the consequences for rejecting the hypothesis. In the case of a hydrologic parameter that is critical to the recovery of an endangered habitat or species we might want to set the confidence level at 0.01 for example.

- Type II error: The hypothesis is actually false yet it is accepted. Interestingly the probability of accepting the hypothesis when it is not true is almost never reported with statistical test results. Furthermore, the consequences of a Type II error are not always fully understood by many investigators. The compliment of the Type II error is often referred to as the power of the test. Therefore, *statistical power can be viewed as the probability of correctly detecting an effect.*

Consideration of these outcomes from examining hypotheses leads to the following considerations. First, the failure to reject the hypothesis does not justify its acceptance for the following reasons: 1) there really is no effect; 2) the power of the test is too low due to the high variance in the variable of interest or insufficient sampling (i.e., "n" is too small); and 3) the expected power of the statistical test has not been evaluated prior to implementing the sampling program.

Most statistical texts address the concepts of hypothesis testing and power analysis. The power of all statistical tests is dependent upon the following design parameters:

- Significance level of the test (alpha)
- Level of sampling effort (i.e., number of sampling stations and sample replicates)
- Minimum detectable difference or change in the performance measure that can be detected
- The "natural variability" of the performance measure within the sampling environment

Therefore the relationship between power and design parameters affords the opportunity to conduct a variety of power analyses and can be determined as a function of any of these design parameters. Likewise, the value of any individual design parameter required to obtain a specified power can be determined as a function of the other parameters.

B-1 Fixed Design Power Analysis: PCB Fish Tissue Concentrations

Power analyses generally fall into two broad categories: 1) determining the minimum difference in the variable of interest (i.e., performance measure) as a function of the level of sampling effort (size of "n" or degree of replication); and 2) the power of the test (probability of detection) is illustrated as a function of the minimum detectable difference that can be detected between samples over time.

This example of a fixed design power analysis design illustrates the relationships between the minimum detectable changes as percent of the mean PCB tissue concentration (U.S. EPA 1987). This study used historical data for liver concentrations of PCBs in winter flounder to evaluate the expected performance of alternative sampling designs and illustrates the case of minimum detectable differences vs. number of replicates for a fixed set of design parameters.

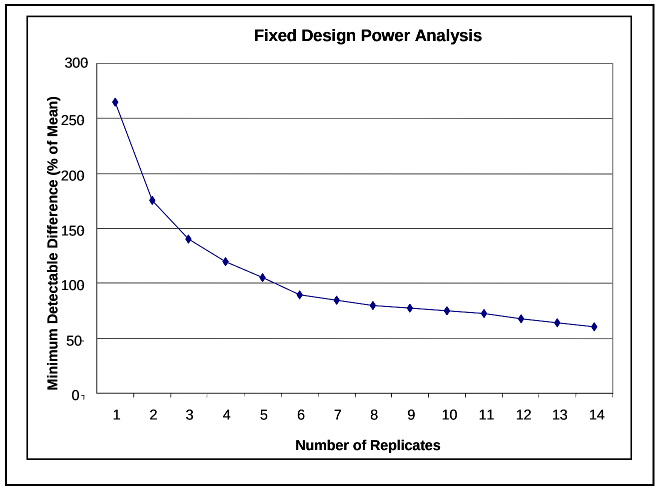


Figure B-1: Minimum difference in tissue concentration of PCBs

The results, illustrated in Figure B-1, indicate that the minimum difference in tissue concentration of PCBs (as percent of the mean – 4.9 mg/kg) that could be detected between sampling locations for different numbers of replicates. The data on individual fish show substantial variation over all the stations as would be expected from a mobile fish whose historical exposure is likely to be highly variable. Thus using a sampling design of five fish from each location would only be able to detect a difference of approximately 120 percent of the overall mean value of 4.9 mg/kg. To detect a 50 percent change in tissue PCB concentrations with this sampling design would required in excess of 15 individual fish from each sampling location which would be prohibitively expensive (U.S. EPA 1992). However, additional power analyses indicated that the collection of replicate composite samples would reduce the within sample station variance significantly thus permitting the detection of substantially smaller differences (percent of mean PCB tissue concentrations) among stations at a much lower cost.

B-2 Power as a function of minimum detectable difference

This example is taken from the analysis of water quality data that were collected in Chesapeake Bay (U.S. EPA 1992). In this case a minimum performance criterion for the monitoring program was that ability to detect a difference in dissolved oxygen equal to 1.6 mg/l. Historical data were used to estimate measurement variability and power analyses were conducted using estimates of the maximum and minimum variance. The results indicate the minimum trend in dissolved oxygen concentration that can be detected with a probability of 0.80 and ten years of historical data is on the order of 0.06-0.13 mg/l-yr which is within the existing power of detection for the current monitoring program (Figure B-2).

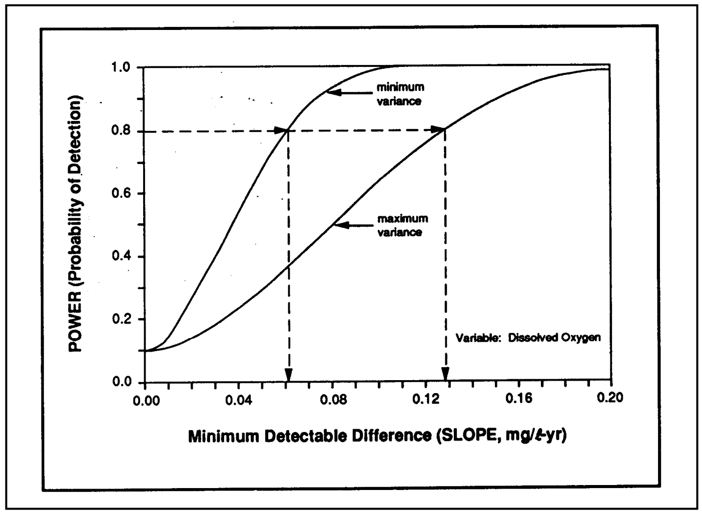


Figure B-2: Minimum trend in dissolved oxygen concentration

The relationship between power and minimum detectable difference provides the information required to evaluate the probability of a Type II error and the probability of detecting specific levels of effects in a proposed sampling program.

B-3 Power Analysis in Adaptive Management

Anderson (1998) in her paper *Errors of Inference* presents a comprehensive discussion of power analysis both in experimental sampling and adaptive management. The following is a summary of recommendations relative to adaptive management.

Several authors have surveyed experimental literature and found few examples that address Type II error (Sedlmeier and Gigerenzer 1989; Peterman 1990a 1990b; Fairweather 1991; Searcy-Bernal 1994). Although there has been some discussion about reconsidering the arbitrary limit on Type I error ($a < 0.05$), that limit is rarely reviewed in discussions of either power analysis or significance testing. However, the number of journal articles reporting new theoretical developments in the application of power analysis to ecological problems and the increasing variability of software for the purpose suggest that errors of inference will be easier to estimate and interpret in the future. The following recommendations apply to all ecological research, but especially to large-scale management experiments:

- Experimenters and decision-makers should embrace the concept that some errors of inference are unavoidable, but their frequency can be controlled by astute design of experiments and monitoring systems.

- *A priori* power analysis, with an explicit statement of desirable levels of a and b, should be included in the design process for all experiments and monitoring programs.
- All reports of non-significant results should mention the effect size and power of the experiment. Where appropriate, *a posteriori* power analysis may be used.
- Where potential costs of the errors of inference to various stakeholders can be quantified, these costs should be included in decisions about acceptable levels of a and b.
- Where currently available experimental designs lack power, research should be directed toward developing new, powerful methodologies, such as Before-After-Control-Impact paired designs (Underwood 1994).
- Resources should be allocated to pilot studies that will help to improve the power of large experiments.
- *A priori* power analyses are often difficult because not enough is known about potential response variables, biologically significant effect sizes, and spatial and temporal variability. It would be useful to carry out large-scale, long-term monitoring of these variables in ecosystems, with the express purpose of estimating them for use in future power analyses and choices about experimental design (Osenberg et al. 1994).

A few key references guide experimenters through power analysis for the most frequently used statistical tests (Anderson 1998). The classic reference to statistical power is Cohen (1988). Cohen provides clearly written instructions for calculating standardized effect size and other input parameters to power and sample size tables. He provides these tables for *t*-tests, tests involving correlation coefficients, tests involving proportions, the sign test, chi-square tests for goodness of fit and contingency, analysis of variance and covariance, multiple regression and correlation, and set correlation and multivariate methods (e.g., canonical correlation, MANOVA, and MANCOVA).

Zar (1996) presents a graph of power and sample size for analysis of variance, as well as formulas for calculating power and required sample size for a variety of other tests. While he does not include tabled values the formulas are discussed with the details of the tests themselves, including biological examples. In addition, Zar discusses examples of *a posteriori* power analyses.

Nemec (1991) introduces power analysis using examples from forest research. Included are example routines for the SAS statistical software package that compare the power of completely randomized and randomized block analysis of variance designs, and calculate power for one- and two-sample *t*-tests and one- and two-way ANOVA.

Over the last few years, the variety of software packages that perform power analysis, sample size determination, and effect size operations has greatly increased. Thomas (1997) maintains an annotated list of software packages on the World Wide Web. Many of these packages are reviewed in Thomas and Krebs (1997). For other discussions of

software, see Goldstein (1989), Borenstein et al. (1990, 1992), Rothstein et al. (1990), Steiger and Fouladi (1992), and Meyer (1995). In addition, on-line power calculations are available for ANOVA (Friendly 1996) and for correlation coefficients and tests of parameters for normal, Poisson, and exponential distributions (Bond 1996).
Numerous examples are available of power analyses for complex designs, including factorial and repeated measures—analysis of variance (Bittman and Carniello 1990; Muller and Peterson1984), moderated multiple regression (Stone-Romero et al. 1994), and multivariate general linear hypotheses (Muller and Peterson 1984). These papers focus on practical methods for addressing questions related to power, effect size, and sample size.

Finally, many ecological analyses involve specialized statistics or experimental designs for which no analytical formulas exist for calculating power. In such cases, Monte Carlo simulation can be used to produce many simulated data sets generated from distributions with known parameter values corresponding to given null and alternative hypotheses. The experimenter can then estimate statistical power by tallying the frequency with which H_o is correctly rejected by the simulated data.

Acknowledgement: This is to acknowledge that this material on power analysis was taken in large measure from Anderson (1998).

APPENDIX C - INTERMEDIATE SCALE MANAGEMENT MODELS

The concept of AM (Holling [editor] 1978) is steadily gaining wider acceptance in ecosystem management, especially in Canada and the United States (e.g., Schmiegelow and Hannon 1993; Nyberg and Taylor 1995). As a hybrid of scientific research and resource management, AM blends methods of investigation and discovery with deliberate manipulations of managed systems. Through observation and evaluation of the ways that human interventions affect managed systems, new knowledge is gleaned about system interactions and productive capacities. This new knowledge is then applied to future decisions in a cycle of continuous improvement of policies and field practices.

In adaptive management, statistical methods also play a critical role. Adaptive managers will often want to measure the initial state or status of the systems they administer, and they will usually need to monitor trends over time that show the system's responses to management policies or practices. In evaluating outcomes, they will want to draw inferences about the causes of any changes that are detected in the system to decide how and when to adjust actions in the future or at comparable sites. Statistical analyses allow managers to discern small but important differences in data sets, and to distinguish patterns of correlation and interaction from background variation and sampling errors.

Careful design of management experiments is the first step towards gaining data from which reliable inferences can be drawn. Whenever possible, AM studies should include experimental controls, unbiased sampling and allocation of treatments, and replication of treatments. However, it is important to recognize that the operational scale and setting of AM studies may constrain the level of statistical rigor that can be achieved. It may be impossible, for example, to find multiple areas that are sufficiently homogeneous to serve as replicates of operational-scale treatments. In other cases, it may not be feasible to meet some of the critical assumptions of the classical methods of statistical analysis, including random allocation of treatments, homogeneity of variance, and independence of sample variances.

Perhaps even more significant is the fact that statistical methods such as ANOVA and regression analysis are not designed to answer common management questions such as "What is the probability of a 50 percent increase in wading bird densities after increasing the hydroperiod by a factor of two?" As a result, classical methods will be useful in some AM studies but not in others. When classical methods are not appropriate, a proposed study may still be worthwhile if alternative types of analyses can reveal important insights from the data.

The following examples illustrate three approaches to developing intermediate scale models specifically designed to address issues of scale and changing management priorities.

C-1 Adaptive Environmental Assessment Model

One example of an "intermediate" level model that might be applicable for integrating the diverse spatial and temporal scale processes characteristic of the Everglades is the AEAM approach (Walters et al. 1992; Walters and Gunderson 1994). This approach employed a "simplified" hydrology model that was computationally faster that the detailed numerical models to "screen" broad water management options. The AEAM approach utilized "scenario" analysis which focused on simple indicators comparing simulated water depth patterns in the natural system to water depth and phosphorus distribution patterns under various management and restoration strategies. The AEAM was used to evaluate both the reconstruction of the historical system and for screening restoration policy options. The lessons learned from using the AEAM for examining policies for water management in the Everglades suggest that the success of this screening approach is that different models should be used for different purposes and that policies should be robust to the uncertainties inherent in both approaches. One limitation of the AEAM approach in this application is that little effort was devoted to defining biological indicators or objectives for restoration, such as increasing biodiversity of maintaining particular endangered or threatened species, habitats, or ecosystems (Walters et al. 1992).

C-2 Simple Models for Ecosystem Management

These simulation models were developed to explore and illustrate interactive dynamics of socio-ecological systems (Carpenter et al. 1999). The ecosystem used to illustrate these integrated models is a lake subject to phosphorus pollution. Phosphorus flows from agriculture to upland soils, to surface waters, where it cycles between water and sediments. The ecosystem is multi-stable, and moves among domains of attraction depending on the history of pollutant inputs. The alternative states yield different economic benefits. Agents form expectations about ecosystem dynamics, markets, and/or the actions of managers, and choose levels of pollutant inputs accordingly. Agents have heterogeneous beliefs and/or access to information. Their aggregate behavior determines the total rate of pollutant input. As the ecosystem changes, agents update their beliefs and expectations about the world they co-create, and modify their actions accordingly. For a wide range of scenarios, we observe irregular oscillations among ecosystem states and patterns of agent behavior. These oscillations resemble some features of the adaptive cycle of panarchy theory (Carpenter et al. 1999).

These models can also be used as caricatures of reality that spark imagination, focus discussion, clarify communication, and contribute to collective understanding of problems and potential solutions (Holling 1978, Walters and Gunderson 1994). The role of such models is similar to the role of metaphor in narrative. T he models are designed to illustrate general patterns of system behavior, rather than to make specific predictions. They should be usable and understandable by diverse participants, and easily modified to accommodate unforeseen situations and new ideas. This paper presents models of the metaphorical type. The interactive software for these types of models are available for download, on line from Conservation Ecology at http://www.consecol.org/vol3/iss2/art4.

C-3 Bayesian Network Model for the Neuse Estuary

There are several "intermediate-scale" modeling approaches that might be applicable for evaluating changes resulting from the implementation of CERP. The recovery of the

Everglades is the result of a number of interacting processes operating at multiple spatial and temporal scales. Thus, the individual models developed to appropriately represent each of these processes are not easily combined into a single predictive model. Borsuk et al. (2001) suggest that a system based on Bayesian networks can provide a possible solution to this scale problem. The Bayesian network employs a graphical structure, analogous to the CEMs, to explicitly represent the variables and causal relationships involved in the relevant processes. In so doing it provides a framework to integrate a variety of models representing a number of interacting processes operating at multiple spatial and temporal scales. This graphical approach explicitly represents cause-and-effect assumptions between system variables that may be obscured under other approaches. These assumptions allow the complex causal chain linking management actions to ecological consequences that characterize the CERP, to be factored into an articulated sequence of conditional relationships. Each of these relationships can then be quantified independently using an approach suitable for the type and scale of information available. In addition, probabilistic functions describing the relationships allow key known or expected mechanisms to be represented without the full complexity, or information needs, of highly reductionist models.

Thus the key to successful prediction lies in choosing scales at which predictable patterns emerge rather than trying to model all scales for all processes. Choosing the various scales of representation in a Bayesian network should be a dynamic and iterative process. While it is desirable to choose scales that will represent key features of the natural system, often the scales are imposed by observational, technological, or organizational constraints (Levin 1997). Finally, the scale of prediction should correspond to the needs of the decision-makers, which may change with time as they gain understanding of the problem.

To demonstrate the application of this approach, Borsuk, et al. (In Press) develop a Bayesian network representing eutrophication in the Neuse River estuary, North Carolina from a collection of previously published analyses. Relationships among variables were quantified using a variety of methods, including: process-based models statistically fit to long-term monitoring data; Bayesian hierarchical modeling of cross-system data; multivariate regression modeling of mesocosm experiments; and probability judgments elicited from scientific experts. We use the fully quantified model to generate predictions of ecosystem response to alternative nutrient management strategies.

APPENDIX D - ADAPTIVE ASSESSMENT CONCEPTS

The concept of AM (Holling [editor] 1978) is steadily gaining wider acceptance in ecosystem management, especially in Canada and the United States (e.g., Schmiegelow and Hannon 1993; Nyberg and Taylor 1995). As a hybrid of scientific research and resource management, AM blends methods of investigation and discovery with deliberate manipulations of managed systems. T hrough observation and evaluation of the ways that human interventions affect managed systems, new knowledge is gleaned about system interactions and productive capacities. This new knowledge is then applied to future decisions in a cycle of continuous improvement of policies and field practices.

Careful design of management experiments is the first step towards gaining data from which reliable inferences can be drawn. Whenever possible, adaptive management studies should include experimental controls, unbiased sampling and allocation of treatments, and replication of treatments. However, it is important to recognize that the operational scale and setting of adaptive management studies may constrain the level of statistical rigor that can be achieved. It may be impossible, for example, to find multiple areas that are sufficiently homogeneous to serve as replicates of operational-scale treatments. In other cases, it may not be feasible to meet some of the critical assumptions of the classical methods of statistical analysis, including random allocation of treatments, homogeneity of variance, and independence of sample variances. Figure D-1 illustrates the design and analysis of an adaptive management experiment (Nemec 1991). While the first several steps in the design are common, the issue of replication results in a bifurcation of the flow of the design because of its implications for the types of statistical analyses that can be performed.

The role of classical statistics in AM can, depending on the types of data and questions being asked, be very important. Adaptive managers will often want to measure the initial state or status of the systems they administer, and they will usually need to monitor trends over time that show the system’s responses to management policies or practices. In evaluating outcomes, they will want to draw inferences about the causes of any changes that are detected in the system to decide how and when to adjust actions in the future or at comparable sites. S tatistical analyses allow managers to discern small but important differences in data sets, and to distinguish patterns of correlation and interaction from background variation and sampling errors.

Adaptive management requires a suitable model for predicting transitions of a system from one state to another and a set of rules for deciding the best action at any given time. In the case of non-replicated experiments (Figure D-1), various analytical methods have been developed (see Walters 1986, Chap. 4–9). These methods are based on the theory of stochastic processes, Bayesian statistics (Bergerud and Reed 1998), and decision theory (Peterman and Peters 1998). When data arise from replicated systems (left side of Figure D-1), the problem is considerably more complicated. Responses of individual systems and the overall response of systems managed under the same plan (i.e., replicates) must be considered (see Walters 1986, Chap. 10).

Perhaps even more significant is the fact that statistical methods such as ANOVA and regression analysis are not designed to answer common management questions such as "What is the probability of a 50 percent increase in wading bird densities after increasing the hydroperiod by a factor of two?" This problem has no simple solution because the link between classical methods (e.g., ANOVA) for the analysis of replicated designs and decision analysis for non-replicated management strategies is not well developed. As a result, classical methods will be useful in some adaptive management studies but not in others. When classical methods are not appropriate, a proposed study may still be worthwhile if alternative types of analyses can reveal important insights from the data.

Meta-analysis (see Mann 1990 for an interesting and non-technical discussion of meta-analysis) or alternative methods for integrating the results from several experiments might be useful in such situations, although a piece-meal analysis of large, complex, and dynamic systems has obvious drawbacks.

Proponents of AM (e.g., Walters 1986; Walters and Holling 1990; Taylor et al. 1997) argue that successful management of complex biological systems requires full scale testing. These experiments, which are known as adaptive management experiments, are used to test entire management unit, where the management unit is equivalent to the experimental unit. In an AM experiment, one or more systems are monitored regularly over time and decisions about treatments or other interventions are made as the experiment progresses. However, because management units are large and complex, ultimately they must be broken down into suitable sampling or experimental units for observation and evaluation. In this respect, AM experiments resemble classical research experiments and therefore should meet experimental design criteria.

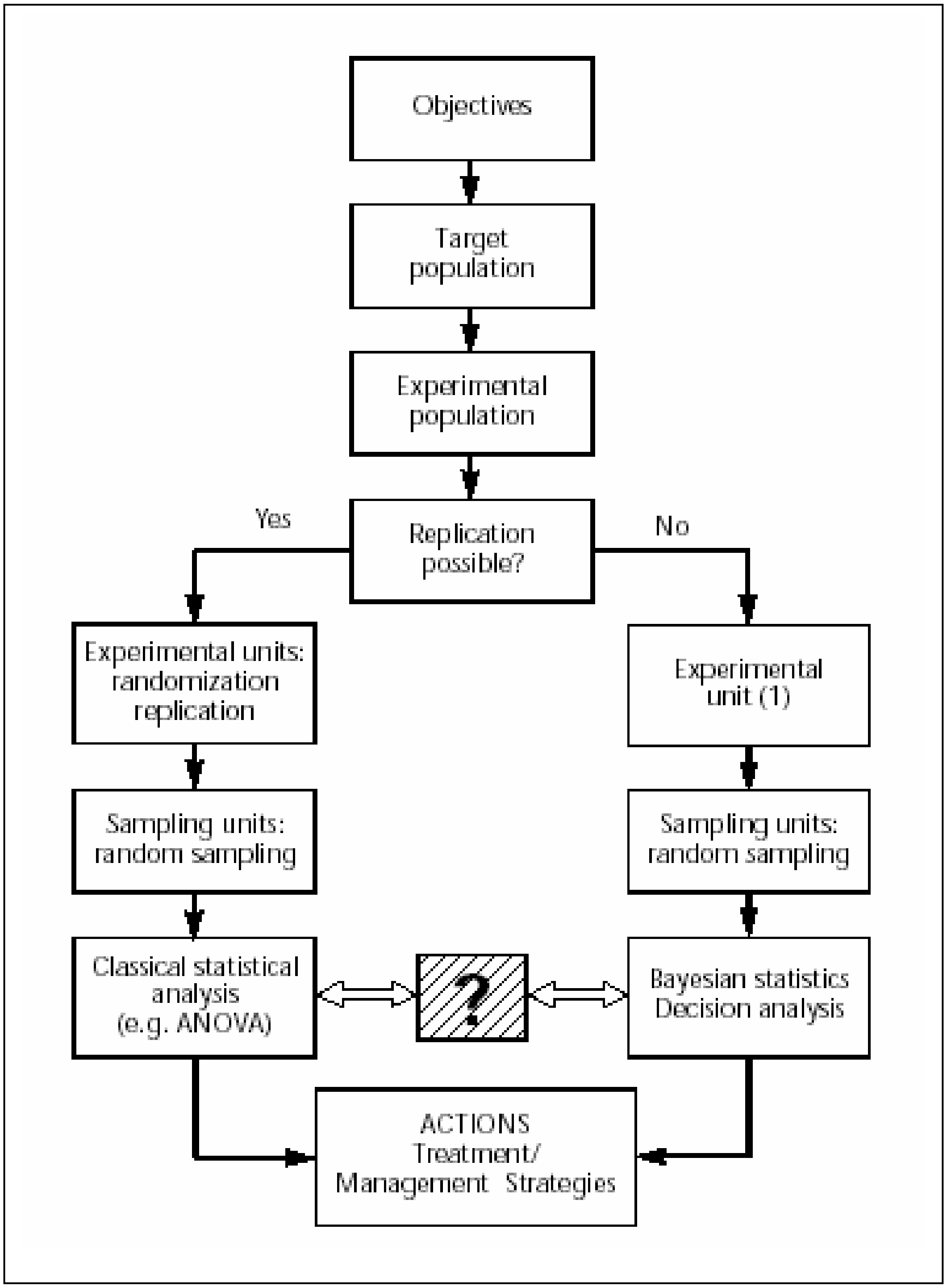


Figure D-1. Design and analysis of an adaptive management experiment (Nemec 1991).

APPENDIX E - ASSESSMENT AND EVALUATION PERFORMANCE MEASURES

Introduction

The AT guidance for conducting MAP assessments has expanded on the original CERP concept of PMs. Assessment and evaluation PMs derive from CERP's Regional and Total System CEMs. MAP monitoring/research elements, and consequently, PMs generally correspond to ecological attributes, physical and chemical stressors, as well as critical intermediate processes comprising the stressor - response pathways developed from the CEMs.

The current PM philosophy, developed for the MAP, was determined before an AT assessment strategy had been developed. Consequently, it is likely that there will be PMs that may not fit within the current assessment strategy. Currently, PMs as defined by CERP are comprised of the following components: 1) a particular physical, chemical, or ecological attribute of importance to the functioning and restoration of the system; 2) a measurable parameter of that attribute; 3) a metric by which to measure and/or model the parameter of interest, and 4) a desired target value for the parameter. The target value is the desired end-state for the parameter, which indicates when restoration is deemed to be successful in regard to that parameter. For example, an ecological attribute could be the health of the wading bird population; the parameter used to assess health could be nesting success, the metric for nesting success could be the number of nests per square kilometer and the target value could be 10 nests. The PM then is "nesting success" and the desired target value is 10 nests.

Ideally, there should be correspondence between the PMs developed for the monitoring plan, the assessments, and the evaluations. However there are important distinctions that will be discussed further detail below.

Evaluation Performance Measures

Simulation models are frequently used in CERP to estimate the effects of water management operations and features (e.g. canals, levees) on the ecosystems of South Florida. These simulation models, by definition, contain simplified representations of existing conditions (e.g. topography, vegetation) and processes (e.g. rainfall, evapo-transpiration, levee seepage). All are designed to operate at very specific spatial and temporal scales, and they are often optimized to accurately represent, at most, only a few features of the natural system. As such, these models cannot, and should not be expected to accurately simulate all the complex interactions occurring between Everglades' climate and hydrologic cycles, trophic webs, and vegetative communities. This modeling limitation is particularly relevant to the CERP planning process because it is these complex interactions that will determine the response of the natural system to restoration activities.

Hydrologic simulation models such as the SFWMM are useful for approximating water levels and hydro patterns in the natural system that may result from a particular rainfall distribution and set of water management practices. In some cases, estimates of habitat suitability (HSI) and water quality are derived from the hydrologic output, though these features are not modeled explicitly within the SFWMM domain. These properties make the SFWMM (and other models) useful planning tools for the CERP process. However, because the SFWMM and other CERP models are run with a historic (36 yr or less) climate record, the hydrologic output, the HSI values and the water quality estimates can not be interpreted as predictions of actual conditions expected to occur at a given time in the future. Rather, the SFWMM and its derivatives (HSI, WQ relationships) only approximate how certain features of the natural system would have responded to the rainfall conditions present in the 36 yr period of record in combination with a fixed set of land use characteristics and water management practices. This same constraint applies to all models, in addition to the SFWMM that utilize the historic climate record in the CERP process. While the output from these models are not predictions *per se*, these models *are* intended to provide some estimates of how the average values of certain features of the natural system can be expected to respond over the long term to a particular set of water management practices and land use characteristics.

Model simulations of alternative water management strategies and land use practices are central to the CERP planning process. A primary function of the evaluation performance measures is therefore to provide metrics that quantify changes in select natural system features that result from the alternative model scenarios. As mentioned above, these features may include average water levels, hydro patterns, and in some cases water quality and habitat suitability. However, because the evaluation performance measures rely on model output, they are constrained by the same limitations exhibited by all the models currently used in the CERP process. Namely, 1) that thePMs are not predictions *per se*, but are intended only to estimate how the natural system would have responded under the 36 yr climate record combined with a unique set of control structures and operations, and 2) they are not intended to represent all the dynamic interactions between climate, hydrology, nutrient cycles, and the biota that characterize the Everglades ecosystem. It is also important to remember that the information generated by evaluation PMs on features such as water levels, hydro patterns, habitat suitability, etc. is often constrained by spatial resolution of the model domain in which they were generated. In the case of the SFWMM, the spatial resolution is 2 by 2 square miles.

Assessment Performance Measures

The RECOVER Assessment Team is charged with assessing progress towards restoration at regular intervals as CERP is implemented. The IAT (as sub-team of the RECOVER AT) could have concluded that it would suffice to simply provide a compendium of the status of each PM with respect to its individual target, which might be called a "report card." However, with a view towards the gradual refinement and improvement of the CEMs, the near certainty of unexpected ecosystem responses and the plethora of non-linear relationships amongst the great many parameters being monitored, the IAT elected a different approach.

The AT Assessment Guidance has expanded the concept of individual PMs as described in the MAP to a hypothesis-based approach which is more robust, flexible, and more consistent with the AM process. Rather than focusing upon individual PMs, the decision was made to focus upon "hypotheses" (expected responses and relationships captured in the CEM) which ecological attributes, multiple stressors, and intermediate processes in various combinations to determine if the underlying CEM hypotheses are being validated or if they need to be modified. The hypothesis based approach recognizes the complexities of ecological responses by assessing their status and trends using multiple measures (clusters of physical, water quality, and ecological attributes) which reflects a more holistic approach that attempts to capture the mechanistic interactions of multiple stressor-response pathways rather than relying on one metric to characterize ecological complexity. Further, the hypothesis-based approach is more scientifically robust and increases the likelihood of detecting unexpected responses of the ecosystem. Interpreting performance of ecological systems, then, can be best accomplished within the context of a set of hypotheses and can be reported based on the cumulative or combined performance of multiple indicators (e.g., performance measures.

The assessment performance measures, developed from hypothesis clusters, are contained in the MAP. These hypotheses clusters are intended to represent the dynamic interactions occurring between the Everglades' climate, hydrologic cycles, and biota. In effect, these hypothesis clusters represent the complex causal linkages existing between ecosystem stressors such as water management practices and the biological attributes of the Everglades' communities. These hypotheses thus form the basis of predictions about how the biological attributes of the Everglades are likely to change in the future with CERP water management scenarios and climate patterns. In turn, the assessment PMs and their associated metrics applied in the MAP are derived from these hypothesis clusters. In some cases, the metrics applied in both the assessment and evaluation PMs will be the same. For example, average annual water levels or dry season recession rates are two metrics that might apply to both sets of PMs. However, the full set of assessment PMs utilized in the MAP will necessarily be more comprehensive than the evaluation PMs that are applied to the simulation model results. This is because the assessment PMs are intended to quantify, to the fullest extent possible, both the changes in the biological and physical attributes of the natural system, as well as the causal mechanisms leading to these changes.

Summary

Ultimately, the success of the restoration will be determined by how well the indicators perform either individually or collectively. Performance measures currently are required for the interim goal process and to support adaptive management (i.e., actual performance can only be judged in the context of some desired level of performance). Clearly, individual performance measures are applicable for measuring changes in physical and chemical stressors (e.g., hydrologic features and nutrients). However, because of the complexities associated with ecological attributes it is problematic whether one can assess or evaluate ecological change using this approach given non-linearity's, natural variability, and stochasticity that characterize ecological systems. It is important to recognize that performance measures, as currently being applied in CERP, are being used for two different purposes: assessments and evaluations. The assessments are dealing with empirical data derived from MAP and non- MAP monitoring programs, historical data, and experimental data. Thus the assessments reflect the reality of the current status of the system. The evaluations, on the other hand, are modeled constructs of reality that are more simplified than those of the assessments and deal with prediction of future states of the system or its individual components.