Experimental approaches to shorebird habitat management

Chris S. Elphick

Elphick, C. S. 1996. Experimental approaches to shorebird habitat management. *International Wader Studies* 9: 20-28.

All land management is experimental. Land management "experiments" can be exploited to benefit wildlife managers and ecologists. Benefits can be attained best through cooperation between managers and academics in the implementation of experiments that compare management options. I summarize key aspects of a rigorous experimental approach, with examples to demonstrate the importance of following experimental principles when designing research on shorebird management. I also provide examples demonstrating that experiments addressing management issues are feasible at a range of spatial scales, from microcosms to entire management units. Finally, I list topics where experiments would enhance our ability to manage shorebird populations.

Chris Elphick, Ecology, Evolution & Conservation Biology Program, 1000 Valley Road, University of Nevada, Reno, NV 89512, USA; elphickc@scs.unr.edu

"... unfortunately, adequate experiments in ecology may demand unrealistic resources." (Furness *et al.* 1993:1)

"...a management scheme is always an experiment..." (MacNab 1983:398)

Introduction

The benefits to be obtained by studying land management with an experimental approach have been stated repeatedly (*e.g.*, Romesburg 1981; MacNab 1983; Kamil 1988; Walters & Holling 1990; Gutzwiller 1991; Cooper & Barmuta 1993). Ecologists can gain because habitat management often occurs at large-scales and at multiple sites, thus allowing them to study phenomena that otherwise would be logistically difficult to address. Land managers benefit because they typically perform manipulations with a specific goal in mind, and it is in their interests to know a) which manipulation best achieves stated management goals and b) whether the results are likely to be repeatable in the future or at other sites. In addition, the better the biology of an ecosystem, or species, is understood the more effective management is likely to be (Shrader-Frechette & McCoy 1993). Wildlife managers and ecologists, however, have not exploited management activities to their full potential (Romesburg 1981; Murphy & Noon 1991). Land management experiments still are uncommon in ecology (though see examples below). Although much experimentation is done by wildlife managers, it rarely is done in a rigorous manner, greatly reducing its scientific value (MacNab 1983).

Limited communication between wildlife managers and academic ecologists and the failure to recognize the mutual benefits probably explains the lack of concerted effort. Managers may not recognize the benefits of adhering to experimental design principles, or may lack the time or the mandate to plan and conduct detailed experimental studies. Ecologists, on the other hand, often do not know enough about management issues and constraints to plan experiments that provide the information that would be most useful to managers. Increasing the number of management experiments, therefore, requires the cooperation of those with jurisdiction over how land is manipulated and the expertise to conduct those manipulations (wildlife managers) and those with the training and mandate to conduct experiments (academic ecologists). Fortunately, such cooperation is increasing as is evidenced by the rising number of experimental studies addressing management issues concerning a wide array of taxa and habitats [*e.g.*, waterfowl in wetlands (Kaminski & Prince 1981; Murkin *et al.* 1982; Ball & Nudds 1989); plants and invertebrates in reedbeds (Cowie *et al.* 1992; Ditlhogo *et al.* 1992); invasive plant control on heathlands (Marrs & Lowday 1992); geese in pastures designed to attract them away from agricultural fields (Vickery *et al.* 1994); controlling gulls on airports (Buckley & McCarthy 1994)].

Interest in managing habitats for shorebird populations has increased considerably in recent years (*e.g.*, Rundle & Fredrickson 1981; Hands *et al.* 1991; Eldridge 1992; Helmers 1992). With the exception of studies addressing predator control (*e.g.* Nol & Brooks 1982; Melvin *et al.* 1992), however, there have been few shorebird management studies that have incorporated rigorous experimental principles. Even in the area of predator control, experiments have been restricted in scope, with most investigating the value of nest exclosures for a few species. A recent book on the management of shorebirds (Helmers 1992) barely mentions the results of experiments designed to investigate habitat modification, restoration, and creation (Oring & Elphick 1993), presumably because there are so few. Similarly, this topic is omitted from a recent paper on shorebird conservation research needs (Morrison

1991). Recognizing that experimental approaches have value to management situations could therefore lead to vast improvements in our understanding of how shorebird populations can be managed, and hence conserved.

My goals in this paper are to a) outline different approaches to studying land-management questions, emphasizing the ease with which they can be achieved and the types of information that can be obtained from them; b) summarize the main components of a rigorous experimental approach, explaining how they can improve management and our understanding of ecology; c) demonstrate that an experimental approach is both feasible and informative through examples from waterbird management studies; and d) identify situations where experimentation could improve shorebird management.

The range of experimental approaches

I have categorized approaches to studying the effects of management on wildlife into five types, depending on the extent to which experimental rigor is incorporated into the study's design (Table 1). The first two approaches involve examining management methods sequentially in a trial-and-error manner. That is, a certain type of management is used to achieve a desired goal. If the management works, its use is continued. If it does not work a new method is attempted. This process continues until a satisfactory outcome is achieved. Approaches 1) and 2) differ in the manner in which the outcome is evaluated. In 1), a judgment is based on anecdotal observation and is necessarily subjective; in 2), some form of post-hoc monitoring is conducted and objective criteria are used to evaluate management success. Both approaches suffer because alternatives are not compared under equivalent conditions. In addition, once the success criteria are attained, new methods cease to be used, leaving the possibility that the best method never was attempted.

The remaining three approaches all involve directly comparing different management methods. Approach 3) is simply a comparison of the effectiveness of whatever management options are in use, with no experimental design (*i.e.*, little or no replication and no control over the treatments compared or how they are assigned). The restricted experimental approach of 4) refers to situations where there is no control over how experimental treatments are assigned, but where enough sites and management options are available that choices can be made as to which treatments are compared and how experimental plots are selected. This approach enables principles such as replication, randomization, and interspersion (see below) to be applied, although in a restricted manner. Finally, there is the rigorous experimental approach 5) common to laboratory or agricultural studies, where there is complete control over the application of management treatments.

The boundaries between these categories often are not distinct and one should view them as five points along a continuum. Although experimental rigor is the primary factor defining this continuum, a number of other factors vary as one moves from 1) to 5) (Table 1). Most obviously, the ease with which the research goals can be accomplished decreases. This is why the majority of habitat management studies fall into the first three categories. An important corollary of this decrease in logistical ease from 1) to 5), however, is that there is an increase in the degree of confidence that can be placed in the conclusions and in the predictive value of the results. In other words, the price for logistical ease often is less confidence in the interpretations and a reduced ability to apply research findings in other situations (Romesburg 1981).

Uses of the results from each approach also change along the continuum. All approaches can be used to generate ideas about what might be the effect of a certain management method. Approaches 2) and 3) also can be used to make tentative tests of these hypotheses, although these tests should be viewed

Table 1. Approaches to studying the effects of management on wildlife.

Approach	*Experimental rigor*	*Logistical ease*	*Uses*	*Quality of information**
1. Trial & error Subjective correlation	Low ↓	High ↑	Generating hypotheses	Low ↓
2. Trial & error Objective correlation			Generating hypotheses, Tentative tests	
3. Comparative No experimental design control			Generating hypotheses, Tentative tests	
4. Comparative Restrictions on experimental design			Generating hypotheses, Testing hypotheses, Predictions	
5. Comparative Unrestricted experimental design	High	Low	Generating hypotheses, Testing hypotheses, Predictions	High

*Measured in terms of the confidence that can be placed in the conclusions and predictive value.

with great caution due to the high potential for unknown biases. Approach 4) and, especially, 5) minimize the chance of systematic bias (see below) and can be used to make strong inferences about the relative merits of the management practices used. These last two approaches currently are rare in wildlife management research, and it is on these that I will concentrate in the remainder of this paper.

Planning a management experiment

A number of factors go into planning a good experiment and a large literature exists on the subject (*e.g.*, Cox 1958; Fisher 1971; James & McCulloch 1985; Kamil 1988; Mead 1988; Hairston 1989). It is not my goal here to explain in detail how to design an experiment but to summarize the main concepts and to demonstrate the potential management consequences of ignoring them. I have divided the discussion into four general issues. The first two deal with planning the experiment, the third with its implementation, and the last with interpreting results.

1. Know the question

The most important component of any experimental design is to identify the specific question(s) the experiment is meant to answer. A common question is, what type of management will best enhance habitat for a population of interest? Once the question has been identified, explicit and detailed hypotheses can be formulated. These should take the form of a null hypothesis of no treatment effect and an alternative hypothesis which can be either a general statement that differences will exist, or can explicitly state the expected alternative. For example, if one were interested in determining the best water depth for foraging Black-necked Stilts (*Himantopus mexicanus*), one might test the null hypothesis that stilts spend equal time feeding in water 0-10 cm deep and water 10-20 cm deep against the alternative hypothesis that they spend more time feeding in the latter treatment. The validity of these hypotheses then can be tested by an experiment. Going through this process is essential to ensuring that a proposed experiment is capable of testing the hypotheses and hence answering the question. Proceeding with a body of research without clearly stating which question(s) need to be answered too often leads to the discovery, at a later date, that the data cannot be analyzed because they violate test assumptions (see below); that they suffer from unanticipated biases; that they lack consistency in the way the data were collected; or that some essential variable was not measured (Fraser 1985). Typically, the result of this approach is that much money and time are spent learning relatively little.

2. Make useful comparisons

For an experiment to be completely successful, it is essential that the comparisons made maximize the amount of information gained. In many habitat management situations, there are several potential treatments that could be compared. Logistics often limit the number that can be tested. When this is the case, most information is likely to be obtained by comparing treatments that are expected to be most different from each other (Kamil 1988). If no difference is demonstrated for this comparison, it is reasonable to assume that there will be no differences between the other treatments. If a difference is detected, additional comparisons can be made in the knowledge that they probably will be informative. If, however, two treatments that are adjacent on a continuum are compared and found not to differ, the scientist has no insight as to the value of conducting more comparisons.

In any experiment, it also is important to incorporate control treatments into the design (Romesburg 1981). A control serves as a baseline against which other treatments can be compared. Two types of control can be identified (Hurlbert 1984). First, a control treatment can be a "do nothing" treatment. In terms of habitat management this simply entails leaving a certain number of sites alone so that the assessment of other treatments can be quantified against the option of no management. This allows an absolute assessment of a treatment's impact, rather than a purely relative one. The second type of control tests for procedural effects and helps researchers to understand why a given result occurs. For example, an experiment to examine the effect that putting electric fences around shorebird nests has on hatching success (*e.g.*, Mayer & Ryan 1991) could include an additional treatment that controls for the effect of the fence alone, by using unelectrified fences. This treatment would allow the biologist to determine whether any differences in nest predation were due to the physical barrier or to the electrification.

The second class of controls are most important when trying to determine the mechanism by which a treatment has its effects. In many management situations, understanding mechanisms is not the primary goal. Rather, the land manager simply wants to know what will occur if a certain activity is performed. In the previous example, the manager may not care whether it is the fence or the electrification that causes the increase in nesting success, as long as the increase occurs. Understanding why an activity causes the results it does, however, has advantages. For example, if fences alone cause an equivalent, or sufficient, reduction in nest predation, there is no point adding the costs of electrification. In general, understanding mechanisms, contributes to more efficient management (Gavin 1991).

3. Avoid confounding factors

The biggest problem in the design of any experiment is reducing the likelihood that the experimental treatments are confounded by some unknown factor. As a hypothetical example, an experiment may be designed to examine the effect of grazing on Long-billed Curlew (*Numenius americanus*) breeding success. Areas with breeding curlews could be selected, and half of them grazed. Counts of young curlews could then be used as the measure of breeding success. The results of the study may show that there were more young curlew produced per pair in the ungrazed areas. From this, one may reasonably conclude that grazing is bad for Long-billed Curlews. The true reason for the difference, however, may be that the areas that were grazed happened to have more nest predators in them for some reason that is unconnected to grazing. Hence, the conclusion that grazing is bad for curlews would be wrong and management aimed at reducing grazing to benefit curlews ineffective.

How can one reduce the chance that an unknown factor is confounded with experimental treatments? Unfortunately, it is impossible to completely eliminate the possibility. This is why confidence intervals and probabilities, which assess how likely it is that a conclusion is correct, are so important. To use Hurlbert's (1984:192) terminology, demonic intrusions (confounding factors that are correlated with experimental treatments) on an experiment cannot be avoided except through "eternal vigilance, exorcism, human sacrifice, etc."; hardly a practical solution in most cases. Nondemonic intrusion (chance events confounding experimental treatments), however, can be countered, at least partially, by ensuring that experimental units are independent, and by using the principles of replication, randomization and interspersion to assign treatments to experimental units (Hurlbert 1984; James & McCulloch 1985).

Independence: Ensuring that all experimental units are independent of one another means establishing that the characteristics of each unit (that are relevant to the question of interest) are not influenced by, or inherently correlated with, those of other units. Experimental units are defined as objects to which an experimental treatment is applied. Thus, a marsh, a shoreline, or a nest can all be experimental units. Establishing that they are independent from one another is important because it increases the degree to which observed patterns can be attributed to the treatments applied. For example, an experiment could be designed to evaluate whether chick shelters placed near avocet nests increase juvenile survival, as they do in terns (Burness & Morris 1992). In this experiment, half of the experimental nests would have shelters placed next to them and half would be left without shelters. If, however, sites are fairly close together, chicks from nests without shelters may be able to move to shelters placed by other nests. In this case the experimental nests are not independent of one another, because the outcome for one nest may depend on whether a shelter is available by a nearby nest. Assessing independence can be accomplished either using basic ecological knowledge or using statistical methods such as autocorrelation (Neter *et al.* 1989; Schneider 1990).

It also is important to ensure that experimental units are independent from one another because independence of data points is a critical assumption of inferential statistics. Violating this assumption invalidates the results of statistical tests (Kramer & Schmidhammer 1992). This problem has arisen most commonly in situations where multiple measures are taken at a single site and treated as independent data points (termed pseudoreplication and discussed below) in statistical hypothesis testing.

Replication: Replication simply means applying each experimental treatment to more than one experimental unit. In terms of habitat management this usually means having multiple sites that receive each treatment. The advantage of replicating is that it decreases the chance that randomly occurring confounding factors will be associated consistently with experimental treatments, thus improving the precision of statistical estimates (Hurlbert 1984). To use the curlew example given earlier, if there is one site that is grazed and one that is not, and there is variation in the abundance of nest predators in the region where the experiment is conducted, it is possible that one site will have a lot of predators and the other will have few. Thus, the experimental treatment is confounded with abundance of predators. If, however, ten sites were used for each treatment, the chance that all ten grazed sites also had high predator abundance and all ten ungrazed sites did not would be quite small. Variation in predator numbers would not be eliminated in the latter case, but it would be much less likely that it would influence any conclusions made about the two experimental treatments. Obviously, as the number of replicates increases the probability of treatments being confounded declines and the more confidence the experimenter can place in his conclusions. It should be equally obvious that there always will be constraints on the number of replicates that can be obtained. A common problem facing an experimenter is to determine just how many replicates are needed. If the variability of the system is known, this can be calculated for the desired confidence levels (Cohen 1988). As a general rule, the greater the likelihood of chance events influencing an experiment's results (*i.e.*, the more variability there is in a system) the more replicates should be used.

It is important to stress that replication requires the multiple application of a treatment. A common misconception is that taking multiple measurements at a single site is equivalent to replicating. This technique actually does nothing to reduce the influence of confounding factors. This practice has been termed pseudoreplication and is surprisingly common (Hurlbert 1984). Taking multiple measures at sites receiving different treatments is not, in itself, wrong. A problem arises only when each measure is used in a statistical test to compare treatments. If ten measures (*e.g.*, of shorebird prey abundance) are taken in an artificial wetland and ten in a natural wetland and a statistically significant difference found, it should not be interpreted as a difference between the two treatments (artificial vs. natural wetland). This is because the two treatments have been applied only once each - there is no replication. The correct interpretation of the result is that there was a difference between the two sites, perhaps due to the different treatments, but perhaps also due to some other factor that differed between them. In order to test the hypothesis that the two treatments differed, one would need to take measures at a number of artificial and natural wetlands and use the data for each wetland as a single replicate in the statistical test. When multiple measures are taken at each site they can be incorporated in the analysis as subsamples of each replicate.

Randomization: The scenario described above, where increased replication leads to increased confidence, assumes that treatments are not applied in a biased way such that a confounding factor becomes correlated with the experimental treatments. The logistics of many habitat management situations are such that violating this assumption could be quite easy. Assigning treatments to experimental units randomly (*i.e.*, all units have an equal chance of receiving each treatment) is the best way to ensure that such unknown biases are eliminated (Hurlbert 1984). For example, an experiment may be designed to compare how effectively different vegetation-clearance techniques (*e.g.*, burning, cutting, herbicide application), each of which uses different tools, keep islands suitable for nesting American Avocets (*Recurvirostra americana*). Logistically, the easiest way to conduct the management work would be to apply the first treatment to the first four islands, switch tools and apply the second treatment to the next four, switch tools again and apply the third treatment to the last four (Figure 1). The researcher could then go out to the islands at regular intervals to determine the rate at which plants grow back and perhaps find results such as those shown in Figure 1. From these they could conclude that burning is better than herbicide application, which is better than cutting. There may, however, also be unknown gradients in soil quality (*e.g.* nutrient supply, drainage; Figure 1) in the islands which influences the rate of plant regrowth. Thus, the researcher's conclusions would be wrong because the amount of regrowth on all islands is determined entirely by soil quality. If, instead of taking the logistically easy route, the treatments received by each island had been determined randomly, the chance of treatments coinciding with unforeseen environmental gradients would have been considerably reduced.

Interspersion of treatments: One reason for randomizing treatment assignments is to ensure that different treatments are well interspersed among each other (Figure 2), reducing the potential for bias due to confounding factors. Complete randomization, however, allows the possibility that there will be segregation of treatments, especially when the numbers of replicates is low (a likely feature of habitat management experiments). If this occurs it may be better to abandon the goal of complete randomization in favor of ensuring a reasonable level of interspersion (Hurlbert 1984). This could be accomplished by reselecting random numbers until a predetermined level of interspersion is achieved, by blocking, or by systematically assigning treatments (Cox 1958; Hurlbert 1984). Recognizing that randomization is used largely as a means by which to achieve unbiased interspersion is important because in many cases it may not be possible to randomly apply treatments to sites. In these situations, however, sites could be selected in such a way as to ensure that the treatments are interspersed, thus achieving the desired goal. Nonetheless, randomization should be used whenever possible.

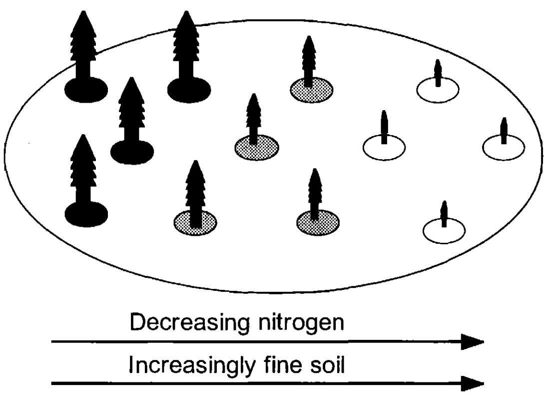

Figure 1. A hypothetical wetland containing 12 islands. Different vegetation treatments are represented by shading: solid = cutting, hatched = herbicide application, open = burning. Tree sizes represent the rate of plant regrowth. Gradients in potentially confounding factors are shown below. See text for more details.

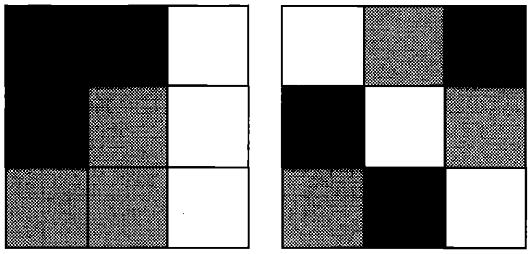

Figure 2. Two possible arrangements of experimental treatments. The one to the right has a high level of treatment interspersion; the one to the left has poor interspersion. Treatments are more likely to be confounded with other factors in the right arrangement.

4. Interpret results accurately

Most experimental studies use statistical methods to determine whether experimental treatments have an effect. Whether the conclusion made is correct or not depends on whether the statistical analysis is conducted correctly and the results interpreted appropriately. Two types of error are particularly common: misapplying tests by violating their assumptions (*e.g.*, Hurlbert 1984; James & McCulloch 1985; Kramer & Schmidhammer 1992) and misinterpreting non-significant results (Toft & Shea 1983; Rotenberry & Wiens 1985; Forbes 1990). These have been discussed in detail elsewhere and I will not dwell on them here; however, I will make two general points. First, it is important to recognize that all statistical tests have assumptions (*e.g.*, the independence of data points discussed above) and violation of these assumptions may render the results invalid, causing erroneous conclusions. Second, a non-significant result does not necessarily mean that there was no difference between experimental treatments, only that none was detected. Such an interpretation can only be made after one has determined whether the test conducted was capable of detecting a difference. This is done by calculating the statistical power of the test (Cohen 1988). If a test does not have adequate power, it is impossible to state whether a difference exists or not (*e.g.*, Reed & Oring 1993), although one still may be able to make inferences about the biological significance of the data (*e.g.*, Alberico 1995).

Theory vs. practice

A major factor hindering the increased use of experiments in management situations is the widespread perception that conducting rigorous experiments at large scales is simply not practical. Perhaps the best demonstration that this is not true is the recent increase in the number of studies that have addressed major management questions through the use of rigorous experiments. These studies have been conducted at three spatial scales: microcosms, small plots that subdivide management units (mesocosms), and entire management units. Experiments conducted at these three scales obviously differ in the ease with which they can be accomplished and the extent to which their results can be extrapolated to real management situations (Cooper & Barmuta 1993). The results of microcosm experiments, for example, should be viewed with more caution than those obtained from experiments that manipulated real management units. In the remainder of this section I briefly describe each type of experiment and give examples of their use in addressing management questions.

Microcosms

Microcosm experiments involve creating small, simplified versions of the system of interest in order to compare the various habitat manipulations experimentally. By using simple model systems, the experimenter is better able to investigate the effects of single variables. Microcosms are, therefore, well-suited to the rigorous type (5) approach (Table 1), but may lack some realism. An example of this approach is a study conducted at Mono Lake, in the western Great Basin, which compared larval survivorship and adult emergence of alkali flies (*Ephydra hians*) at five different salinities (Herbst 1992). This experiment showed that both survivorship and emergence decline as salinity increases above 50 g/l, and that there was virtually no adult emergence once salinities reached 160 g/l. These results have considerable importance to managers because alkali flies are the only food available with a net positive energy balance for certain migrant shorebirds at Mono Lake (Rubega & Inouye 1994), and because salinities there have increased over recent years from below 50 g/l to more than 80 g/l (Patten *et al.* 1987).

Small plots (mesocosms)

Recognizing the need to make management experiments as realistic as possible while keeping them logistically feasible, various studies have used experimental plots that, like microcosms, are considerably smaller than real management units. These small plots are generally subdivisions of an area which would normally be considered a single management unit and, in contrast to microcosms, retain much of the complexity of real management units. Different experimental manipulations are then applied to the subdivisions. As with microcosms, it is usually possible to use approach (5) (Table 1) when using small plots. Examples of this type of experiment are given by Vickery *et al.* (1994), who studied whether certain management methods led to increased use of pastures set aside to attract Brent Geese (*Branta bernicula*) away from arable fields. This study involved three experiments using 0.25-0.75 ha plots that had received either different grazing, cutting, or fertilization regimes. The results showed no significant differences in goose grazing intensity between the grazing or cutting treatments, but more Brent Geese used plots that were fertilized with nitrogen than plots which were not. These results indicate that to attract Brent Geese managers may use whichever method of maintaining a short turf is most convenient and that they should consider fertilizing their pastures.

Entire management units

Ideally, one would design experiments that use entire management units as experimental plots. Logistics, however, often prevent this. One exception is the Marsh Ecology Research Project, conducted at the Delta marshes in southern Manitoba, which used ten identical wetland units to investigate the effects of water level manipulations on the productivity of northern prairie marshes (Murkin *et al.* 1985). Among the many factors considered in this study was the relationship between waterfowl and macroinvertebrate densities. In units where water depths were maintained at the level of the surrounding marsh, spring waterfowl abundance was significantly related to macroinvertebrate density. In contrast, in units managed for deeper water there was no relationship. Behavioral observations suggested that this difference arose because ducks did not use the deeper units for foraging, possibly because macroinvertebrates were less accessible (Murkin & Kadlec 1986). These results suggest that aquatic invertebrate production is an important factor influencing habitat selection by breeding waterfowl, and that deep water management of prairie marshes reduces their suitability for these birds.

When it is not possible to employ complete experimental control in the design of a management study the restricted approach 4) (Table 1) can provide a good compromise. For example, an ongoing study of the effects of rice-field management practices on birds during winter uses entire fields as experimental units (C. Elphick unpub. data). In this study, I had no control over which experimental treatment was applied to each field. The large number of available fields, however, enabled study fields to be chosen in such a way as to ensure that treatments were replicated, interspersed among one another, and spread across the study area. Thus, the chance that treatments would be confounded with some other factor was reduced. In addition, a second experiment using small plots within fields provides a more rigorous test of various hypotheses. Combining approaches in this way is a powerful way of increasing confidence in results.

Shorebird management experiments that need doing

In the final section of this paper I list several areas of research where rigorous experiments would enhance our ability to manage shorebirds in the western Great Basin and elsewhere. Many of these management ideas were discussed by Helmers (1992), who summarized a range of options for enhancing habitats for shorebirds. The efficacy of most of these options, however, has yet to be rigorously tested.

1. Island design

Building islands in managed wetlands to provide birds with relatively predator-free nesting sites has become commonplace (Swift 1982). Helmers (1992) identifies a range of factors that are likely to influence the value of nesting islands: location within a wetland (proximity to shore, other islands and foraging areas, *etc.*), timing of development, island configuration (size, shape, elevation, *etc.*), steepness of banks, and the material of which the island is made. All of these factors could be manipulated in an experimental manner during construction or modification of wetland sites.

2. Predator control

A major problem facing the management of shorebirds, and other ground-nesting species, is the incidence of nest predation (Nol & Brooks 1982; Paton *et al.* this volume). Various methods of reducing predation have been suggested, though not necessarily for shorebirds: single-nest predator exclosures (*e.g.*, Nol & Brooks 1982; Melvin *et al.* 1992), fences (*e.g.*, Mayer & Ryan 1991), aversive conditioning (*e.g.*, Nicolaus *et al.* 1983; Conover 1989, 1990), culling predators (*e.g.*, Greenwood *et al.* 1990), removing potential predator perches (*e.g.*, Greenwood *et al.* 1990), etc. The effectiveness of some of these methods has been evaluated through the use of well-designed experiments (*e.g.*, Nol & Brooks 1982; Conover 1990), though less rigorous approaches are more typical (*e.g.*, Deblinger *et al.* 1992; Vaske *et al.* 1994). The value of several methods has yet to be tested. For those techniques where experiments have been done, further research widening the range of species (both predator and prey), varying the design of exclosures and fences, and combining different approaches will greatly enhance our ability to manage efficiently.

3. Substrate/vegetation manipulation

Strong associations exist between the foraging and nesting sites used by shorebirds and both substrate and vegetation characteristics (*e.g.*, Baker 1979; Quammen 1982; Colwell & Oring 1990; Hands *et al.* 1991). Different methods of vegetation control also are known to have different influences on invertebrate production in wetlands (*e.g.*, Kaminski & Prince 1981; Ball & Nudds 1989). Experiments that manipulate substrate and vegetation will provide useful information on how habitats can be managed to enhance their value for shorebirds. Two lines of research would be especially useful: tests of the influence of certain characteristics (*e.g.*, substrate type; vegetation height and density; heterogeneity; native vs. introduced plants), and tests comparing different methods for achieving desired features. These methods include grazing (discussed next), water level manipulation (*e.g.*, Murkin *et al.* 1985; Reid *et al.* this volume), burning (*e.g.*, Ball & Nudds 1989), cutting (*e.g.*, Murkin & Ward 1980), and herbicide use (*e.g.*, Evans 1986).

4. Grazing

Grazing potentially influences shorebirds in both positive (*e.g.*, maintaining relatively short vegetation for foraging birds) and negative (*e.g.*, trampling of nests) ways (Powers & Glimp this volume). Effects may differ substantially between species and season. Type of grazer, timing of grazing, and grazing intensity are all factors that can be manipulated experimentally (*e.g.*, Vickery *et al.* 1994; Powers & Glimp this volume). There is great opportunity for managers to investigate the use of grazing as a management tool, rather than a management problem.

5. Water depth manipulation

Manipulating water depth, especially through the use of drawdowns, has received considerable attention in discussions of shorebird habitat management (*e.g.*, Helmers 1992). Several studies have shown correlations between water depth and invertebrate biomass and/or shorebird densities (*e.g.*, Hands *et al.* 1991; Velasquez 1992; Rehfisch 1994). Others have suggested that shorebirds select water depths according to leg and bill lengths (Baker 1979). Water depth is therefore expected to influence both shorebird food production and accessibility. In addition, the depth of water around islands used for nesting and roosting may be important in deterring predators. Many factors (*e.g.*, mean and variance in depth; timing and rate of drawdown; interactions of water depth characteristics with vegetation manipulation), therefore, may influence avian use and could be tested experimentally.

6. Disturbance

Human activities can influence the behavior of shorebirds (*e.g.*, Davidson & Rothwell 1993, and papers therein). Due to the ease with which many disturbances can be manipulated there is considerable opportunity for conducting experiments that investigate disturbance effects (Gutzwiller 1991). In a recent review, Gutzwiller (1991) summarizes statistical and biological factors that should be considered in the design of such studies. Many of the biological factors that he lists as potentially influencing such studies (*e.g.*, habituation) also could be the basis for experiments.

There are other shorebird management issues not addressed in this list and, undoubtedly, many novel ways in which habitats or populations can be manipulated that have yet to be identified. It is also important to stress that many of the management methods discussed will not only influence shorebirds, but other species as well. Hence, it often will be most informative, and cost-effective, to study the effects of management simultaneously on a variety of species. Historically, management in North America has been biased toward game birds (Peek 1986; Gradwohl & Greenberg 1989). Thus, most studies of wetland management methods have addressed effects on waterfowl alone, resulting in lost opportunities to gain information about other species. This means that studies that have already been done with waterfowl need to be repeated to assess effects on other species. If biologists studying shorebird management techniques can avoid repeating this mistake it would be of great value.

Acknowledgments

This paper benefited greatly from the comments of D. Delehanty, L. Oring, D. Palmquist, M. Reed, J. Robinson, M. Rubega and N. Warnock. The value and feasibility of conducting management experiments was demonstrated to me by the studies of various people at the University of East Anglia and their collaborations with land managers. Their body of work was instrumental to the emergence of this paper. Support while writing this paper was provided by Ducks Unlimited, Inc. through the Institute for Wetland and Waterfowl Research.

References

Alberico, J. A. R. 1995. Floating eggs to estimate incubation stage does not affect hatchability. *Wildl. Soc. Bull.* 23: 212-216.

Baker, M. C. 1979. Morphological correlates of habitat selection in a community of shorebirds (Charadriiformes). *Oikos* 33: 121-126.

Ball, J. P., & Nudds, T. D. 1989. Mallard habitat selection: an experiment and implications for management. In: R. R. Sharitz and J. W. Gibbons (eds.), *Freshwater wetlands and wildlife, CONF-8603101, DOE Symposium Series No. 61*, pp. 659-671. USDOE Office of Scientific and Technical Information, Oak Ridge, Tennessee.

Buckley, P. A., & McCarthy, M. G. 1994. Insects, vegetation, and the control of laughing gulls (*Larus atricilla*) at Kennedy International Airport, New York City. *J. Appl. Ecol.* 31: 291-302.

Burness, G. P., & Morris, R. D. 1992. Shelters decrease gull predation on chicks at a Common Tern colony. *J. Field Ornithol.* 63: 186-189.

Cohen, J. 1988. *Statistical power analysis for the behavioral sciences.* 2nd ed. Lawrence Earlbaum Associates, Hillsdale, New Jersey.

Colwell, M. A., & Oring, L. W. 1990. Nest-site characteristics of prairie shorebirds. *Can. J. Zool.* 68: 297-302.

Conover, M. R. 1989. Potential compounds for establishing conditioned food aversion in raccoons. *Wildl. Soc. Bull.* 17: 430-435.

Conover, M. R. 1990. Reducing mammalian predation on eggs by using a conditioned taste aversion to deceive predators. *J. Wildl. Manage.* 54: 360-365.

Cooper, S. D., & Barmuta, L. A. 1993. Field experiments in biomonitoring. In: D. M. Rosenberg and V. H. Resh (eds.), *Freshwater biomonitoring and benthic macroinvertebrates*, pp. 399-441. Chapman and Hall, New York.

Cowie, N. R., Sutherland, W. J., Ditlhogo, M. K. M., & James, R. 1992. The effects of conservation management of reed beds. II. The flora and litter disappearance. *J. Appl. Ecol.* 29: 277-284.

Cox, D. R. 1958. *Planning of experiments.* Wiley & Sons Inc., New York.

Davidson, N., & Rothwell, P. (eds.). 1993. Disturbance to waterfowl on estuaries. *Wader Study Group Bulletin* 68, *Special Issue.*

Deblinger, R. D., Vaske, J. J., & Rimmer, D. W. 1992. An evaluation of different predator exclosures used to protect Atlantic coast piping plover nests. *Wildl. Soc. Bull.* 20: 274-279.

Ditlhogo, M. K. M., James, R., Laurence, B. R. & Sutherland, W. J. 1992. The effects of conservation management of reed beds. I. The invertebrates. *J. Appl. Ecol.* 29: 265-276.

Eldridge, J. 1992. *Management of habitat for breeding and migrating shorebirds in the midwest.* United States Fish and Wildlife Service Leaflet 13.2.14.

Evans, P. R. 1986. Use of herbicide 'Dalapon' for control of *Spartina* encroaching on intertidal mudflats: Beneficial effects on shorebirds. *Colon. Waterbirds* 9: 171-175.

Fisher, R. A. 1971. *The design of experiments.* 9th ed. Hafner, New York, New York.

Forbes, L. S. 1990. A note on statistical power. *Auk* 107: 438-439.

Fraser, D. 1985. Piggery perspectives on wildlife management and research. *Wildl. Soc. Bull.* 13: 183-187.

Furness, R. W., Greenwood, J. J. D., & Jarvis, P. J. 1993. Can birds be used to monitor the environment? In: R. W. Furness and J. J. D. Greenwood (eds.), *Birds as monitors of environmental change,* pp. 1-41. Chapman & Hall, London.

Gavin, T. A. 1991. Why ask "why": The importance of evolutionary biology in wildlife science. *J. Wildl. Manage.* 55: 760-766.

Gradwohl, J., & Greenberg, R. 1988. Conserving nongame migratory birds: A strategy for monitoring and research. *Audubon Wildlife Report 1989/1990*: 297-328.

Greenwood, R. J., Arnold, P. M., & McGuire, B. G. 1990. Protecting duck nests from mammalian predators with fences, traps, and a toxicant. *Wildl. Soc. Bull.* 18: 75-82.

Gutzwiller, K. J. 1991. Assessing recreational impacts on wildlife: The value and design of experiments. *Trans. N. A. Wildl. & Natural Resour. Conf.* 56: 248-255.

Hairston, N. G. 1989. *Ecological experiments: Purpose, design, and execution.* Cambridge University Press, United Kingdom.

Hands, H. M., Ryan, M. R., & Smith, J. W. 1991. Migrant shorebird use of marsh, moist-soil, and flooded agricultural habitats. *Wildl. Soc. Bull.* 19: 457-464.

Helmers, D. L. 1992. *Shorebird management manual.* Western Hemisphere Shorebird Reserve Network, Manomet, Massachusetts.

Herbst, D. B. 1992. Mono Lake benthic ecosystem research: aquatic productivity component of the Environmental Impact Report. *Mono Basin EIR auxiliary report #8*. State Water Resources Control Board.

Hurlbert, S. H. 1984. Pseudoreplication and the design of ecological field experiments. *Ecol. Monogr.* 54: 187-211.

James, F. C., & McCulloch, C. E. 1985. Data analysis and the design of experiments in ornithology. *Current Ornithology* 2: 1-63.

Kamil, A. C. 1988. Experimental design in ornithology. *Current Ornithology* 4: 313-346.

Kaminski, R. M., & Prince, H. H. 1981. Dabbling duck and aquatic macroinvertebrate responses to manipulated wetland habitat. *J. Wildl. Manage.* 45: 1-15.

Kramer, M., & Schmidhammer, J. 1992. The chi-squared test in ethology: Use and misuse. *Anim. Behav.* 44: 949-955.

MacNab, J. 1983. Wildlife management as scientific experimentation. *Wildl. Soc. Bull.* 11: 397-401.

Marrs, R. H., & Lowday, J. E. 1992. Control of bracken and the restoration of heathland. II. Regeneration of the heathland community. *J. Appl. Ecol.* 29: 204-211.

Mayer, P. M., & Ryan, M. R. 1991. Electric fences reduce mammalian predation on piping plover nests and chicks. *Wildl. Soc. Bull.* 19: 9-63.

Mead, R. 1988. *The design of experiments: Statistical principles for practical applications*. Cambridge University Press, United Kingdom.

Melvin, S. M., MacIvor, L. H., & Griffin, C. R. 1992. Predator exclosures: A technique to reduce predation at piping plover nests. *Wildl. Soc. Bull.* 20: 143-148.

Morrison, R. I. G. 1991. Research requirements for shorebird conservation. *Trans. N. A. Wildl. & Natural Resour. Conf.* 56: 473-480.

Murkin, H. R., Batt, B. D. J., Caldwell, P. J., Davis, C. B., Kadlec, J. A., & van der Valk, A. G. 1985. Perspectives on the Delta Waterfowl Research Station-Ducks Unlimited Canada Marsh Ecology Research Program. *Trans. N. Amer. Wildl. and Natur. Resour. Conf.* 49: 253-261.

Murkin, H. R., & Kadlec, J. A. 1986. Relationships between waterfowl and macroinvertebrate densities in a northern prairie marsh. *J. Wildl. Manage.* 50: 212-217.

Murkin, H. R., Kaminski, R. M., & Titman, R. D. 1982. Responses by dabbling ducks and aquatic invertebrates to an experimentally manipulated cattail marsh. *Can. J. Zool.* 60: 2324-2332.

Murkin, H. R., & Ward, P. 1980. Early spring cutting to control cattail in a northern marsh. *Wildl. Soc. Bull.* 8: 254-256.

Murphy, D. D., & Noon, B. D. 1991. Coping with uncertainty in wildlife biology. *J. Wildl. Manage.* 55: 773-782.

Neter, J., Wasserman, W. & Kutner, M. H. 1989. *Applied linear regression models*. Irwin, Inc., Homewood, Illinois.

Nicolaus, L. K., Cassel, J. F., Carlson, R. B., & Gustavson, C. R. 1983. Taste-aversion conditioning of crows to control predation on eggs. *Science* 220: 212-214.

Nol, E., & Brooks, R. J. 1982. Effects of predator exclosures on nesting success of killdeer. *J. Field Ornithol.* 53: 263-268.

Oring, L. W., & Elphick, C. S. 1993. Review of Helmers, Shorebird management manual. *Auk* 110: 672-674.

Patten, D. T., Conte, F. P., Cooper, W. E., Dracup, J., Dreiss, S., Harper, K., Hunt, G. L., Kilham, P., Klieforth, H. E., Melack, J. M., & Temple, S. A. 1987. *The Mono Basin ecosystem: Effects of changing lake level*. National Academy Press, Washington D.C.

Peek, J. M. 1986. *A review of wildlife management*. Prentice-Hall, Englewood Cliffs, New Jersey.

Quammen, M. L. 1982. Influence of subtle substrate differences on feeding by shorebirds on intertidal mudflats. *Mar. Biol.* 71: 339-343.

Reed, J. M., & Oring, L. W. 1993. Long-term population trends of the endangered Ae'o (Hawaiian Stilt, *Himantopus mexicanus knudseni*). *Trans. West. Sec. Wildl. Soc.* 29: 54-60.

Rehfisch, M. M. 1994. Man-made lagoons and how their attractiveness to waders might be increased by manipulating the biomass of an insect benthos. *J. Appl. Ecol.* 31: 383-401.

Romesburg, H. C. 1981. Wildlife science: Gaining reliable knowledge. *J. Wildl. Manage.* 45: 293-313.

Rotenberry, J. T., & Wiens, J. A. 1985. Statistical power analysis and community-wide patterns. *Am. Nat.* 125: 164-168.

Rubega, M., & Inouye, C. 1994. Prey switching in red-necked phalaropes: Feeding limitations, the functional response and water management at Mono Lake, CA, USA. *Biol. Cons.* 70: 205-210.

Rundle, W. D., & Fredrickson, L. H. 1981. Managing seasonally flooded impoundments for migrant rails and shorebirds. *Wildl. Soc. Bull.* 9: 80-87.

Schneider, D. C. 1990. Spatial autocorrelation in marine birds. *Polar Research* 8: 89-97.

Shrader-Frechette, K. S., & McCoy, E. D. 1993. *Method in ecology: Strategies for conservation*. Cambridge University Press, United Kingdom.

Swift, J. A. 1982. Construction of rafts and islands. In: D.A. Scott (ed.), *Managing wetlands and their birds: A manual of wetland and waterfowl management*, pp. 200-203. International Waterfowl Research Bureau, Slimbridge, United Kingdom.

Toft, C. A., & Shea, P. J. 1983. Detecting community-wide patterns: Estimating power strengthens statistical inference. *Am. Nat.* 122: 618-625.

Vaske, J. J., Rimmer, D. W. & Deblinger, R. D. 1994. The impact of different predator exclosures on Piping Plover nest abandonment. *J. Field Ornithol.* 65: 201-209.

Velasquez, C. R. 1992. Managing artificial saltpans as a waterbird habitat: Species' responses to water level manipulation. *Colon. Waterbirds* 15: 43-55.

Vickery, J. A., Sutherland, W. J., & Lane, S. J. 1994. The management of grass pastures for brent geese. *J. Appl. Ecol.* 31: 282-290.

Walters, C. J., & Holling, C. S. 1990. Large-scale management experiments and learning by doing. *Ecology* 71: 2060-2068.