STUDYING WIND ENERGY/BIRD INTERACTIONS: A GUIDANCE DOCUMENT

METRICS AND METHODS FOR DETERMINING OR MONITORING POTENTIAL IMPACTS ON BIRDS AT EXISTING AND PROPOSED WIND ENERGY SITES

Prepared for the Avian Subcommittee and NWCC
December 1999

NATIONAL WIND COORDINATING COMMITTEE

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December 1999

Preface

This guidance document is a product of the Avian Subcommittee of the National Wind Coordinating Committee (NWCC). The NWCC was formed in 1994 as a collaborative endeavor composed of representatives from diverse sectors including electric utilities and their support organizations, state utility commissions, state legislatures, consumer advocates, wind equipment suppliers and developers, green power marketers, environmental organizations, and state and federal agencies. The NWCC identifies issues that affect the use of wind power, establishes dialogue among key stakeholders, and catalyzes appropriate activities to support the development of an environmentally, economically and politically sustainable commercial market for wind energy.

The NWCC Avian Subcommittee was formed to better understand and promote responsible, credible, and comparable avian/wind energy interaction studies. In addition to this document, the National Wind Coordinating Committee will be placing wind energy-related materials on its web site: www.nationalwind.org

For comments on this guidance document or questions on wind energy permitting, contact the National Wind Coordinating Committee Senior Outreach Coordinator c/o RESOLVE, 1255 23rd Street NW, Suite 275, Washington, DC 20037; phone (888) 764-WIND, (202) 944-2300; fax (202) 338-1264; e-mail nwcc@resolv.org.

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Executive Summary

INTRODUCTION

In the 1980s little was known about the potential environmental effects associated with large scale wind energy development. Although wind turbines have been used in farming and remote location applications throughout this country for centuries, impacts on birds resulting from these dispersed turbines had not been reported. Thus early wind energy developments were planned, permitted, constructed, and operated with little consideration for the potential effects on birds.

In the ensuing years wind plant impacts on birds became a source of concern among a number of stakeholder groups. Based on the studies that have been done to date, significant levels of bird fatalities have been identified at only one major commercial wind energy development in the United States. Research on wind energy/bird interactions has spanned such a wide variety of protocols and vastly different levels of study effort that it is difficult to make comparisons among study findings. As a result there continues to be interest, confusion, and concern over wind energy development's potential impacts on birds. Some hypothesize that technology changes, such as less dense wind farms with larger, slower-moving turbines, will decrease the number of bird fatalities from wind turbines. Others hypothesize that, because the tip speed may be the same or faster, new turbines will not result in decreased bird fatalities but may actually increase bird impacts. Statistically significant data sets from scientifically rigorous studies will be required before either hypothesis can be tested.

Purpose and Scope of This Document

Bird mortality is a concern and wind power is a potential clean and green source of electricity, making study of wind energy/bird interactions essential. An important first step in understanding these interactions and assessing potential effects is to use the same terminology and conduct research that will produce credible and comparable results. This guidance document seeks to:

1. Provide a reference document for use by all stakeholders that will, if followed, produce a body of information adequate to:
   - assess the suitability of a proposed wind plant site with regard to birds of concern
   - assess the potential effects of a wind plant on birds of concern
   - evaluate the potential effects of wind energy technology on birds
2. Provide sufficiently detailed and clearly understandable methods, metrics, and definitions for use in the study of wind energy/bird interactions
3. Promote efficient, cost-effective study designs, methods, and metrics that will produce comparable data and reduce the overall need for some future studies
4. Provide study designs and methods for the collection of information useful in reducing risk to birds in existing and future wind plants

There is no "cookbook" approach to research. Not all jurisdictions will require information on birds or bird research in conjunction with permitting a wind energy development. Many situations will require site-specific knowledge and expert recommendations on how to proceed with study design and methods. This document provides an overview for regulators and stakeholders concerned with wind energy/bird interactions, as well as a more technical discussion of the basic concepts and tools for studying such interactions.

Organization of the Document

This document is organized in two parts.

Part I (chapters 1-2) presents the general reader with a framework for considering wind energy/bird interactions, which typically are studied within the context of wind energy development site screening, selection, permitting, and project operation.

- Chapter 1 - Introduction
- Chapter 2 - Site Evaluation Biology

Part II (chapters 3-5) provides detailed discussion of metrics, methods, and study design issues for basic and advanced wind energy/bird interaction studies. Geared toward the more technical reader, these chapters are intended to provide regulatory staff and technical advisors to the various stakeholders with a common understanding of what constitutes scientifically rigorous research methodology and its applications.

- Chapter 3 - Basic Experimental Design and Level 1 Studies
- Chapter 4 - Advanced Experimental Design and Level 2 Studies
- Chapter 5 - Risk Reduction Studies

Additional sections at the end of the document include a list of *Literature Cited,* and an *Index of Key Terms*.

SITE EVALUATION BIOLOGY

Giving adequate consideration to bird resources early in the site evaluation process can reduce expense, project delays, and stakeholder frustration, and help in complying with permitting and legal requirements. Local expertise and advice may prove quite valuable in determining what information is required by regulatory and permitting agencies. A brief written assessment for each site being evaluated should include information obtained from:

1. sources of existing information, including local expertise, literature searches, and natural resource database searches for sensitive species or for areas used by a large number of birds
2. reconnaissance studies
3. vegetation mapping, habitat evaluation and the use of information about wildlife habitat relationships.

In many cases, existing information is adequate to determine whether a site is biologically suitable or unsuitable for wind energy development. In some cases, on suitable sites, the existing information will be adequate and defensible for regulatory and environmental law purposes. If not, the developer and permitting agency may want to discuss additional information needs and specify objectives. On-site surveys and monitoring using approprate sample design, metrics, and methods can supply short- or long-term information needs effectively and efficiently. Additional on-site information-gathering may focus on:

- species of special concern
- breeding bird species
- migrating birds
- wintering birds
- nocturnal vs. diurnal bird activity
- species known to be susceptible to collision
- special situations.

Again, bird biological information must be clearly documented and sufficient for making reasonable estimates of bird impacts.

BASIC EXPERIMENTAL DESIGN AND LEVEL 1 STUDIES

Level I studies should detect major impacts on birds and assist in the design of wind energy projects to reduce these impacts where necessary. Construction of a wind plant is not a random occurrence. Potential wind plant sites are relatively unique, creating the potential for study design problems. Moreover, many of the issues related to wind plant impacts on birds are based on relatively rare events. Determination and analysis of impacts thus will seldom be based on clear-cut statistical tests, but rather on the weight of evidence developed from the study of numerous impact indicators, over numerous time periods, at numerous wind plants.

Protocols for bird studies will, by necessity, be site- and species-specific. They will be influenced by the status of the wind energy project, the area of interest, the issues and species of concern, cooperation of landowners, and also by budget considerations and available time.

Summary of Recommendations for Designing Level 1 Studies:

1. **Clearly define:** study objectives (questions to be answered), the area, the species, and the time period of interest; the area of inference, the experimental unit (and sample size), and the sampling unit (and subsample size); and the parameters to measure.

2. **Select relatively uncorrelated impact indicators, measure as many relevant covariates as feasible, and identify obvious biases.**

3. **The Before-After Control Impact (BACI) design is preferred**. Collect data for two or more time periods before and again after construction on the assessment area (wind plant) and multiple reference areas. Consider matching pairs of sampling units (data collection sites) within each study area based on criteria which are relatively permanent features.

4. **Use a probability sampling plan;** stratify on relatively permanent features and only for short-term studies. **Use a systematic sampling plan for long-term studies;** spread sampling effort throughout area and time periods of interest, and maximize the number of experimental units (sample size).

5. **Develop detailed standard operating procedures** (SOPs) prior to the initiation of field work, and **select methods that minimize bias**.

6. Make maximum use of existing data and consider some **preliminary data collection** where little data exists.

7. When data are unavailable before construction then **combine multiple reference areas with other study designs,** such as the gradient-response design.

8. **Maximize sample size** within budgetary constraints.

9. **Univariate analysis is preferred,** especially when relying on weight of evidence.

10. **Have the plan peer reviewed with an emphasis on developing comparable and credible information.**

Usually, Level I studies will serve to focus future research on areas if significant biological impacts appear likely.

ADVANCED EXPERIMENTAL DESIGN AND LEVEL 2 STUDIES

Testing hypotheses generated by the results of Level 1 studies requires more in-depth (Level 2) studies, including both manipulative experiments and modeling techniques.

Manipulative experiments. Observational studies can be used to evaluate risk reduction management options for existing and new wind plants. However, by allowing control of such factors as natural environmental variation which tend to confound observational studies, manipulative experiments could significantly improve the understanding of how these factors relate to the risk of bird collisions with turbines.

Conceptual framework for population modeling. A population is quantified in terms of birth rate, death rate, sex ratio, and age structure. The spatial structure of a population has an important role in genetics, and ultimately, survival. Because most "populations" actually are metapopulations composed of many subparts, even impacts occurring in a small geographic area can disrupt immigration and emigration between local subpopulations, resulting in a much wider effect on the population than is immediately evident. Moreover, small impacts can have serious consequences for the persistence of small populations.

Survivorship and population projections. Wildlife population projections can be made using various models which provide a numerical tool for determining growth rate and age structure of populations, facilitating growth projections.

A review of major wildlife and ornithological studies published during the past 20 years suggests that only very broad generalizations can be drawn regarding "normal" survival rates of bird populations. Because interyear variability in survivorship is large even in healthy populations, the value of short-term (1-2 year) evaluations of a population of concern is questionable. The literature indicates that even a relatively minor change in survivorship can have substantial population impacts, and that in most cases adult survivorship is critical to maintaining a viable population. These studies indicate the importance of determining survivorship in evaluating the effects of wind plants on birds, and suggest the value of modeling structures in guiding this determination.

Determining cumulative effects. The cumulative effects of a wind plant on a population over time could apply to the birds in and immediately around the wind plant, or could manifest itself in populations or subpopulations some distance away through changes in immigration and emigration. The cumulative effects resulting from the expansion of an existing wind plant also are extremely difficult to quantify in the field without a tremendous expenditure of time and funds. Establishing a rigorous and focused modeling framework becomes essential for hypothesizing the potential impacts given a variety of scenarios. In this way, inference can be drawn from data collected over the short term as it applies to likely longer-term impacts using projections of various population models.

Recommendations for Level 2 Study Design

1. **Develop a sound modeling framework** initially to prevent the pursuit of ad hoc, unfocused research studies.

2. **In many situations, quantification of adult survivorship is an essential step in determining the status of the population of interest.** Data on survival published in the literature is adequate to allow broad generalizations to be made regarding "adequate" survival for population maintenance.

3. **Determine the spatial structure of a population** to place the status of various life history parameters into context.

4. **Quantify reproductive output and breeding density.** In combination with knowledge of the population's spatial structure, this can provide a good idea of the status of the population—especially important when adult survivorship cannot easily be determined.

5. **Habitat loss usually is a factor causing the decline of a species.**

RISK REDUCTION STUDIES

Methods of assessing avian risk. In assessing avian risk with the purpose of eliminating or reducing that risk, it is essential to quantify both the use of a site and the deaths associated with that use. The ratio of death to use (risk) becomes a measure, expressed as mortality, or the rate of death (or injury) associated with bird utilization of the wind energy site. Following the epidemiological approach, mortality is the *outcome variable*—the variable that the researcher considers most likely to shed light on the hypothesis about the mechanism of injury or death. Determining the mechanism of injury or death allows the development of appropriate methods to reduce the risk to a bird of being in a wind plant.

In testing modifications to turbines or wind plants, it is important to separate bird mortality from bird utilization. Only by separating utilization from risk does it become possible to know if a modification that reduces utilization of a wind plant has a positive or negative effect on the population.

Methods of study design. There are four logical and sequential tasks that the investigator must accomplish when designing a study of wind energy/bird interactions.

1. Isolate the hypothesis of mechanism that is being tested.

2. Choose a measure of injury-death frequency that best isolates the hypothesis being tested.

3. Choose a measure of effect that uses the measure of injury-death frequency and isolates the hypothesis being tested.

4. Design a study that insures maximum statistical effectiveness within budgetary and physical constraints.

If risk is defined as the ratio of dead or injured birds to some measure of utilization, then the choice of the use factor, or denominator, is critical. The ideal denominator is the unit that represents a constant risk to the bird. Great care must be taken in identifying the factor measuring bird use of a wind energy development (e.g., bird abundance, passes near a turbine, nesting success). Indirect factors, such as changes in habitat, prey quality and quantity, and nesting sites, can affect bird use of a wind plant and must be considered in study design.

PART I

CHAPTER 1: Introduction

INTRODUCTION

Wind energy first was used to generate electricity in the United States nearly 100 years ago. Commercial wind energy developments now operate in 15 states. Additional projects are planned for completion in 1999 and 2000 in at least 16 states, including six states where commercial wind energy is not currently being used. The use of wind energy also is growing rapidly in many other countries. While wind energy, like other renewable energy resources, offers the prospect of significant environmental benefits, the effects of wind energy developments on birds have raised important legal, ecological, and often emotional issues in the permitting and operation of wind plants.

When California's first large wind plants were being developed in the late 1970s and early 1980s, state agencies raised some concerns about the potential impacts of these facilities on birds. However, the environmental effects associated with wind energy development were poorly understood in the 1980s.

In the ensuing years wind plant impacts on birds became a source of concern among a number of the stakeholder groups. Between 100 to 300 raptors including 40 golden eagles (*Aquila chrysaetos*) were estimated to die annually in the Altamont Pass Wind Resource Area (WRA), California (Orloff & Flannery 1992). This attracted the attention and scrutiny of some groups and has directly affected the permitting of some wind plants. Studies conducted at sites other than Altamont Pass WRA indicated few birds were being killed (Anderson et al. 1996b, Strickland et al. 1996). Thus, we know that wind energy can be developed in a way that minimizes the potential risk to birds, either by design or as a function of the abundance and type of bird species within the general area of the WRA. However, because studies have been conducted using a variety of protocols and with vastly different levels of effort, it was, and continues to be, very difficult to make comparisons of study findings. As a result, there continues to be interest, confusion, and concern over potential impacts on birds from wind energy development.

PURPOSE AND SCOPE OF THIS DOCUMENT

This document is intended as a guide to persons involved in designing, conducting, or requiring wind energy/bird interaction studies. It is hoped that by offering guidance with respect to language, methods, metrics (units of measurement), and study design concepts, this document will lead to future studies using methods and metrics that are, as much as is practical, consistent with accepted scientific practices. Specifically, our aims are to:

1. Provide a reference document for use by researchers, biologists, and regulatory and wildlife agencies that will, if followed, produce a body of information adequate to:
   - assess the suitability of a proposed wind plant site with regard to birds of concern
   - assess the potential effects of a wind plant project on birds of concern
   - evaluate the potential effects of the implementation of wind energy technology on birds.
2. Provide sufficiently detailed and clearly understandable methods, metrics, study designs and definitions for use in the study of wind energy/bird interactions.
3. Promote efficient, cost-effective study designs, methods, and metrics that will produce

comparable data which could reduce the overall need for some future studies.

4. Provide study designs and methods for the collection of information useful in reducing potential risk to birds in existing and future wind plants.

Using generally agreed upon methods and metrics should help to enhance both the *credibility* and the *comparability* of study results, including the results of studies conducted at different sites with different study objectives.

The benefits of achieving these objectives are manifold. If study methods and metrics are generally agreed-upon, stakeholders can focus on the implications of study results rather than on debating the validity of the data and how it was obtained. If different studies generate comparable results, the total set of wind energy/bird interaction data will be increased. This in turn should help in understanding the differences and similarities between wind energy developments, in anticipating potential avian issues at yet-to-be-developed wind energy sites, and in generating a body of knowledge about how wind energy development and operation affects birds that can be disseminated to the public. It also should lead to a more efficient use of research and monitoring budgets.

It is neither possible nor appropriate to provide a detailed "cookbook" approach to every site-specific situation. Not all jurisdictions will require information on birds or bird research in conjunction with permitting a wind energy development. In jurisdictions requiring bird research, the information in this document can be used to develop standard operating procedures (SOPs) and/or study designs. However, many situations will require site-specific knowledge and expert recommendations as to which study design and methods are most appropriate.

This document provides an overview as well as a more technical discussion of the basic concepts and tools for studying wind energy/bird interactions. Establishing standard metrics, methods, and study designs does not reduce the potential for adverse impacts, mitigate impacts, or guarantee a siting permit. It can help ensure that credible, acceptable, scientifically rigorous bird information is gathered wherever such information is required for wind energy site development.

While the list of metrics provided in this document is not exhaustive, the technical and biological information needs and approaches presented in this document can support informed decisions regardless of the size of the wind energy development project or the number of birds potentially affected. The metrics, methods, and study designs described in this document can be used for a range of projects, and some attention will be given to the difference between large and small projects.

For each of the metrics we describe, we will attempt to point out their relative advantages, disadvantages, and underlying assumptions. Project-specific protocols should be developed to accomplish specific study objectives. The optimal protocol will vary depending on the study objective.

The basic concepts presented here apply to all bird species; however, the appropriate study methods implemented will vary depending on whether the primary species of interest is large (e.g., raptors) or small (e.g., passerines), nocturnal or diurnal, migratory or resident, and so on. Methods also will vary depending on the objectives of the study. Study objectives must be clearly defined in order to determine the appropriate study design. The intent of this document is not to advise regulators on what the objectives of a study of avian impact should be, but rather to give guidance on how to conduct a scientifically defensible study that achieves specified objectives, using methods and metrics that can be meaningfully compared against an agreed-upon benchmark.

HISTORICAL PERSPECTIVE

Studies have established that wind energy generation systems can sometimes kill birds. Depending upon the situation, this may be viewed as a serious problem. Although many bird species have been affected, raptors have received the most attention in the U.S. (Anderson and Estep 1988, Estep 1989, Howell and Noone 1992, Orloff and Flannery 1992, Hunt 1995, Luke and Watts 1994, Martí 1994, Howell 1995).

The detection of dead raptors at the Altamont Pass WRA (Anderson and Estep 1988, Estep 1989) triggered concern on the part of regulatory agencies, environmental and conservation groups, resource agencies, and wind and electric utility industries. This led the California Energy Commission and the planning departments of Alameda, Contra Costa, and Solano counties to commission the first extensive study of bird fatality at the Altamont Pass WRA (Orloff and Flannery 1992).

Other North American and European research of wind energy/bird interactions have documented deaths of songbirds (Orloff and Flannery 1992, Pearson 1992, Winkelmann 1994, Higgins et al. 1995, Anderson et al. 1996b) and waterbirds (Pearson 1992, Winkelmann 1994). Research at Tarifa, Spain identified a high griffon vulture (*Gyps fulvus*) fatality rate (Martí 1994). Bats also have been killed at wind energy facilities (Higgins et al. 1995). Of the numerous commercial sites in operation in the United States today, bird fatalities of any significant level have been identified only at Altamont Pass WRA, the largest U.S. commercial wind energy site with over 6,000 turbines. Two other large California sites, in the Tehachapi and San Gorgonio WRAs, do not appear to have the same problem with bird fatalities.

In 1992, the California Energy Commission and Pacific Gas and Electric Company sponsored a wind energy/bird interaction workshop focusing on wind energy effects on birds. This workshop brought many interested parties together to discuss the issue and its evaluation, thus taking an initial step toward the development of a nationwide approach. A research program directed by Kenetech Windpower, Inc. focused on the sensory and behavioral aspects of wind energy/bird interactions and represented another significant early effort to address the avian fatality issue. At the same time, the U.S. Department of Energy/National Renewable Energy Laboratory (NREL) initiated a program to identify and prioritize research needs, provide technical advice, and fund or cost-share numerous research projects.

In July 1994, a national workshop was held in Denver, Colorado. Sponsored by NREL, the U.S. Department of Energy, the American Wind Energy Association, the National Audubon Society, the Electric Power Research Institute, and the Union of Concerned Scientists, that workshop attempted to bring together existing information and concern about wind energy/bird interactions. One major focus was on systematizing the search for the factors responsible for avian deaths from wind energy facilities, and on placing efforts to reduce avian fatality on a firm, scientific basis (Proceedings, National Avian Windpower Planning Meeting [NAWPM Proceedings] 1995).

Shortly afterward, the National Wind Coordinating Committee (NWCC) formed an Avian Subcommittee to carry forward the work, begun at the NREL workshop, of identifying and setting priorities for wind energy/bird interaction studies. The Subcommittee has provided advice to funding agencies, promoted communication among participants in wind energy developments regarding approaches to resolving wind energy/bird conflicts, and facilitated the development of standard protocols for conducting wind energy/bird interaction studies.

The NWCC felt that the interested parties needed a better understanding of the effect of wind energy development on birds and that they needed to understand whether fatality levels and risk vary from one WRA to another around the nation. Yet definitive research results on this complex question require numerous studies over a period of several years — studies that often are field-intensive, time-consuming, and costly.

In September, 1995, the Avian Subcommittee sponsored a second national workshop in Palm Springs, California, to facilitate communication among avian researchers, regulators, and groups needing good scientific information to review wind energy development proposals. An outcome of this meeting was the recommendation that a group of ornithologists, statisticians, and environmental risk specialists develop a set of study protocols and measures of wind energy/bird interactions that could be adopted by the NWCC. *Studying Wind Energy/bird Interactions: A Guidance Document* is the result of that effort. It is hoped that this document will facilitate the comparison of results from wind energy/bird studies in different areas, and that it will lead to improved understanding of potential causal factors in wind energy/bird interactions.

Produced by the Avian Subcommittee, this document has been reviewed by a wide range of stakeholders and has been endorsed by the NWCC as a valuable reference that could be used throughout the nation. A separate NWCC document, *Permitting of Wind Energy Facilities Handbook* (NWCC 1998), was developed "to help stakeholders make permitting decisions in a manner which assures necessary environmental protection and responds to public needs." The *Handbook* provides an overview of the basic features of a wind project and discusses the permitting process. It also describes many of the issues that may arise in the permitting process and provides tradeoff considerations and strategies for dealing with the issues. The potential impact of wind development on bird resources of concern is one of these issues. *Permitting of Wind Energy Facilities* also provides information on the steps and participants involved in the permitting process of a wind plant project.

Results of the early research at the Altamont Pass increased scrutiny and caution during the permitting of new wind plant developments, often resulting in costly delays. Until recently, there were no research results from other U.S. avian studies to conclusively provide support for the belief by some that not all wind developments would result in the same level of bird fatalities as was happening at the Altamont WRA. However, recent research at Tehachapi, California, has indicated that the Tehachapi Pass WRA (Anderson et al. 1996b) and the Altamont Pass WRA may differ — most importantly, that raptor use may be much lower in the Tehachapi Pass WRA. Yet, this comparison suffers from the fact that a common set of metrics were not used in these studies. More recently, early results from avian research at other wind sites where many of these metrics are comparable suggest that wind turbines can be sited in a manner that reduces the potential for impact on bird resources of concern (see Table 1-1).

METRICS, METHODS AND STUDY DESIGN

The information contained in this document provides guidelines to conduct most required wind energy/bird interaction studies. In addition, one of the goals of this document is to provide common terminology for those involved in conducting wind energy/bird interaction studies. Three commonly used terms in this document are metrics, methods, and study design. *Metrics* are measurements, concepts, and relationships, such as miles per hour or, in the case of wind energy/bird interactions: bird utilization rate, mortality (a rate of fatality), risk, and so on. *Methods* refer to observational or manipulative study techniques used to document bird location, numbers, use, behavior, and other associated parameters. *Study design*, which is part of methods, sets forth how, what, when, and where samples will be selected. The study design will need to be tailored to the specific project, whereas the metrics and other methods may not require modification from study to study.

For research to be found defensible, the metrics and methods should be scientifically credible and comply with the needs of legal and regulatory processes.

ORGANIZATION OF THIS DOCUMENT

This document is organized in two parts. Part I (chapters 1-2) presents the general reader with a framework for considering wind energy/bird interactions. Following this chapter **(Introduction), Chapter 2 - Site Evaluation Biology** discusses information needs and sources as well as the on-site inventory and monitoring work that may be conducted at a wind energy site.

Part II of the document (chapters 3-5) provides detailed discussion of metrics, methods, and study design issues for basic and advanced wind energy/bird interaction studies. Geared toward the more technical reader, these chapters are intended to provide regulatory staff and technical advisors to the various interested parties with a common understanding of what constitutes scientifically rigorous research and its applications.

Chapter 3 - Basic Experimental Design and Level 1 Studies discusses basic monitoring and research that may be conducted prior to construction and operation or during operation when potential impacts are poorly understood. Level 1 research typically includes observational studies designed to detect potential effects of large magnitude. Such studies look at risk and cumulative effects.

Chapter 4 - Advanced Experimental Design and Level 2 Studies discusses more specific (Level 2) studies focusing on manipulative experiments and bird population effects, including field and/or model-based studies. This type of research is conducted if a regulator is sufficiently concerned that a population problem exists or could result from a wind resource area. This concern might arise from the results of a Level 1 study. This research can use various forms of population modeling.

Chapter 5 - Risk Reduction Studies discusses Level 2 studies that focus on ways to reduce bird risk. This is research in applied problem solving; it addresses an acknowledged problem and normally involves designs of manipulative studies including treatments.

Additional sections at the end of the document include a list of *Literature Cited*, and an *Index of Key Terms* defined or explained in this document.

Table 1-1. Examples of Recent Avian Research at U.S. Wind Energy Sites

Site Name and Location	Purpose of Study	Status	Results*	Size**	Length of Study
Altamont WRA, CA	Evaluate impacts of perching and mortality on a wide variety of turbine types. Conduct behavioral observations of all bird species on a wide variety of turbine types.	On-going	Many bird species killed annually; particular concern for raptor fatalities	5400 turbines at site/940 turbines observed; 800 turbines serached for fatalities	~2.5 years
Tehachapi WRA, CA	Compare bird use, behavior, and mortality at small and large turbines; compare bird use, behavior, and mortality at tubular and lattice towers; compare bird use, behavior, and mortality near and away from turbines	Field work completed	A variety of bird species are killed annually, but not at levels that are deemed biologically significant	5000 turbines at site/ 2700 turbines included in research	~4 years
San Gorgonio WRA, CA	Compare bird use, behavior, and mortality at small and large turbines; compare bird use, behavior, and mortality at tubular and lattice towers; compare bird use, behavior, and mortality near and away from turbines	Field work completed	A variety of bird species are killed annually, but not at levels that are deemed biologically significant	3500 turbines at site/2700 turbines included in research	~2 years
Ponnequin, CO	To document: avian use of a relative abundance on the project area pre- and post- construction; use of existing power line poles and fence posts; raptor nesting populations; burrowing activities of ground squirrels; any avian fatalities. Research will be conducted pre- and post-construction.	1st year field work completed; post-construction monitoring on-going	Little avian activity noted; post-construction monitoring will seek to assess impact of turbines	Phase I – 21 turbines installedto date Phase II – up to 27 turbines/none yet installed	Up to 3 years
Searsburg, VT	To assess impacts of wind development on breeding and migrating birds. Before/after study design was used.	Study completed; final going report is through NREL peer-review process	No major changes in species composition were found, but the numbers of several interior forest breeding birds were lower after construction than before and several edge species were more numerous after construction. Further study was recommended.	11 turbines at site/11 turbines included in research	~3 years
Buffalo Ridge, MN	Uses control/impact on Phase I and before/after and control/impact (BACI) on Phase II to evaluate impacts on wildlife from each phase of development and the cumulative impact to wildlife from all wind energy development. To provide information that can be used to reduce impacts to wildlife of subsequent developments	3 years field work completed; 2 years pre and 1 year post-construction monitoring for Phase II	Use by some avian groups is lower than expected within wind plants, likely due to behavioral avoidance and reduced habitat effectiveness. Mortality low in comparison to other wind plants in the U.S. Relatively high incidence of bat mortality appears unique among wind plants.	Phase I – 73 turbines installed/ sampled Phase II – 143 turbines installed/ sampled	~3 years
Foote Creek Rim, WY	Uses BACI to evaluate impacts on wildlife from each phase of development and the cumulative impact to wildlife from all wind energy development. To provide information that can be used to reduce impacts to wildlife of subsequent developments	2 years field work completed: post-construction monitoring and fatality searches beginning	Turbine strings placed back from rim edges to minimize impact with raptors; post-construction monitoring continues	Phase I – 69 turbines installed/all surveyed Phase III – up to 33 turbines/none yet installed	3-4 years

* Results are based on draft documents that will soon be published.
** Total number of turbines at site/number of turbines in study

CHAPTER 2: Site Evaluation Biology

INTRODUCTION

The risk to birds is among the many considerations weighed during site evaluation of a wind plant development project. This chapter discusses types of avian biology information that may be helpful in estimating risk for birds associated with proposed wind plant site development. The information provided herein includes sources of information and field survey techniques for gathering sufficient bird information for many site evaluation decisions.

Site evaluation may start with one or more sites being considered and compared for acceptability as a wind energy development site. The site evaluation may be complete after gathering simple and straightforward existing and on-site information, or it may progress to levels of more detailed information if the developer or permitting agency deems it warranted. Site evaluation normally will utilize only a portion of the information sources and research/information gathering methods discussed in this document. The decision as to what constitutes adequate avian biology information may need to be reconsidered at more than one stage of the site evaluation. Specific information on birds may be needed to obtain compliance with local permitting requirements, if applicable, as well as state and federal environmental laws. Although this discussion focuses on birds, bat species may also be considered.

For a discussion of the various aspects of the permitting process regarding wind energy facilities, see *Permitting of Wind Energy Facilities: A Handbook* (NWCC 1998). Chapter 4 of the *Handbook* provides an overview of where, why, when, and how biological resources and bird and bat resources may be considered during the permitting process.

Permitting processes often have a defined time line, usually beginning with the formal filing of a permit application. Avian information will normally be collected during the pre-application period and may be simple and straightforward or more complicated depending on the avian resources and specific situation. In cases where more detailed information is needed, timing becomes important. Certain types of biological resource information are seasonal and can only be gathered in the field at certain times of year. Bird activity may need to be looked at during more than one season. Depending on the species present and the information requirements, more than one year of data-gathering may be required. All site-specific information should be well documented and more detailed information, if needed, should be obtained using standard, credible techniques, metrics, methods, and study design. (See chapters 3, 4, and 5 of this document for specific guidance.)

This chapter focuses on where and how to obtain biological resource information, mostly about birds. How much information is desired and over what time period it will need to be gathered is a decision made by the project proponent or the permitting authority, possibly with other stakeholder input. (See the "Biological Resources Tips" section on page 41 of the *Permitting of Wind Energy Facilities: A Handbook* [NWCC 1998].) It is valuable to understand the bird resource-related laws, standards, regulations, and ordinances of the project site areas. It is also useful to clarify early in the wind plant site evaluation process any project-specific and jurisdiction-specific legal and biological information that may be needed (NWCC 1998).

SITE EVALUATION

When one or more sites are under consideration, some quick and easy methods can be utilized to determine the type of bird resources on and near the site. Information-gathering at this stage can cover many variables and is intended to eliminate problematic surprises late in the permitting process and during operation. By conducting an appropriate assessment, the wind plant proponent and permitting authority will be able to estimate potential bird risk. A written assessment of each site considered should include:

1. Information from existing sources, including:
    - local expertise
    - literature searches
    - natural resource database searches for sensitive species, for bird species known to be susceptible to collision events, and for areas used by large numbers of birds
2. Reconnaissance surveys
3. Vegetation mapping, habitat evaluation, and wildlife habitat relationships
4. Consideration as to whether the existing information and site visit information will allow compliance with, and be defensible for, regulatory and environmental law purposes.

Sources of Existing Information

Local Expertise

Seeking out one or more local experts familiar with the site(s) being considered can save time as well as provide valuable information. Local experts can quickly identify potential bird and other biological concerns or issues at the site(s) under consideration. They may have an established working relationship with or knowledge of other persons or resources that can be utilized to provide valuable biological, regulatory, and legal information. Interviews should be documented in a written report. Local expertise can include the following:

- state fish and game agents/biologists
- federal wildlife agents/biologists (e.g., U.S. Fish & Wildlife Service, Bureau of Land Management, U.S. Forest Service, U.S. Geological Survey)
- university professors/graduate students
- Partners in Flight representatives
- National Audubon Society representatives
- Hawk Migration Association of North America representatives
- bird observatory representatives
- other knowledgeable parties.

In pre-permit evaluation of the Columbia Hills wind power site, the proponent for the site discovered that the State of Washington's wildlife agency had historical records of several bald eagle day roost sites near the site. A reconnaissance level survey of the site discovered a night roost used by a small number of eagles. This information was used in the final design of the wind plant and, had the project proceeded, would have resulted in the company eliminating at least one string of turbines potentially placing birds using the roost at risk.

Literature Search

A literature search can provide valuable information about bird resources using the area. Environmental documents previously prepared for the site or site area may be useful, as may other types of reports. Research results from other wind energy facilities can be used in site evaluation to identify trends or similarities that may either point to few problems anticipated, or raise concerns. As more wind energy/bird interactions research results become available, these results may be helpful for creating screening matrices or screening calculations. Many sources of literature will be gray literature, i.e., reports or studies published incidentally by an agency or stakeholder group as distinguished from articles published in independent, regularly published, peer-reviewed journals. (Gray literature may or may not be peer-reviewed.) Gray literature can provide good, useful information; however, the value of a specific piece of gray literature should be determined by an experienced biologist with knowledge of the species of special interest in the area.

Natural Resource Database Search

Most federal, state, and local agency offices and many conservation organizations (e.g., The Nature Conservancy, California Native Plant Society) maintain databases of sensitive resources in the area of their jurisdiction or focus. These databases can be valuable for determining whether sensitive bird species and other sensitive resources are known to use the potential site or vicinity. This information usually consists of known bird locations, so bear in mind that the site in question may never have been inventoried for bird resources. Therefore, a database search may come up negative because no one has looked, or because sensitive bird resources using the site have not yet been detected. It is important to understand how the databases are constructed in order to interpret and use the results appropriately.

Sensitive species (including species of special concern) are those species that are protected by federal or state law, or are listed or monitored by governments, agencies, or environmental groups for various reasons. Legal protection for these species can vary from federal and/or state endangered species laws to local agency policies. Having knowledge of the laws and policies of the project site area is very important. It is the responsibility of the developer to understand these laws and policies and their ramifications for the project. Some sources for information regarding sensitive species and other special situations (e.g. concentration areas, flyways, etc.) include:

- National Audubon Society Christmas bird counts
- breeding bird surveys/census/maps sites - available from various sources
- state heritage documents and maps
- state bird atlases/bird books
- state and federal endangered and threatened species lists and occurrence information
- federal, state, and local resource agency offices
- state wildlife habitat relationship programs
- Ducks Unlimited
- National Audubon Society state and federal watch lists
- other sources.

On-Site Information Gathering

Reconnaissance Studies

Reconnaissance studies are on-site surveys used by a biologist to get a general feel for the site, topography, habitat, bird use and potential use, and for species that may use the site. This type of survey can provide valuable information for an environmental assessment. Depending on the site and species known to occur there, reconnaissance studies combined with other easily-gathered information (such as local expertise and literature) can provide adequate information to estimate potential impacts. In other situations, it may provide information that can help focus more detailed studies of bird resources.

Vegetation Mapping, Habitat Evaluation and Wildlife Habitat Relationships

Each site should be visited by a trained and experienced biologist with specific knowledge of and experience detecting the bird species and other natural resources of the project site and vicinity. (This visit may coincide with the reconnaissance survey.) Plant and animal species observed on the project site and vicinity should be documented. The vegetation series should be identified and mapped at an appropriate scale (e.g. 1” = 500'). Wildlife habitat relationships are complex, but there may be information available that will assist with determining bird species’ use of a habitat. Many states have a “Wildlife Habitat Relationships Program.” These databases and the vegetation maps can be evaluated to develop lists of species that may utilize the site. In some situations the probability of species use/occurrence may be discussed. Specific habitat attributes and elements may also provide clues to species use. See Morrison et al. (1998) for a review of the literature on wildlife-habitat relationships.

Uses of the area may include such activities as breeding/nesting, migrating through, wintering, migratory stopover, and foraging. Signs of significant bird use, sensitive species use, or of use by collision-susceptible species are early warnings that may lead to additional investigations or to site abandonment.

Sensitive species use (or likely use) is one determinant of a project’s potential for significant impact. If the value of the site for sensitive species is well known, more detailed studies may be needed (see chapter 3). If a potential site has a high likelihood for biological conflicts, it may not be worth the time and cost of detailed site evaluation work (NWCC 1998). If the potential for bird risk is certain to be low, then very little additional information may be needed.

Is the Existing Information Adequate and Defensible?

At this stage of the site evaluation process, the developer may want to consider whether existing information is adequate and defensible for the permit application. Is the biological information adequate to make a determination of the likelihood of compliance with any applicable regulatory and environmental laws? Are there potentially any sensitive species that may be significantly affected? What additional information may be needed?

GATHERING ADDITIONAL INFORMATION

Planning to Gather Additional On-site Information

When planning to gather additional bird information for the permitting process, the permitting agency and developer may want to discuss future information and monitoring needs. Because cost and time are valuable considerations, it is important first to specify information needs and establish clear research objective(s), and then to use appropriate sample design, metrics, and methods. Monitoring, using options discussed in chapter 3, can supply short-term or long-term information needs in an effective and efficient manner. Obtaining this more detailed avian resource information requires more scientifically rigorous methods. Chapters 3-5 provide information and options for moderate to higher levels of research needs.

Short-term On-site Surveys and Monitoring

Short-term on-site surveys/monitoring refers to multiple visits to a site to document bird use or some other needed information of value. When simpler methods have been exhausted, but sufficient concern persists regarding the presence and use of the site by sensitive species or the numbers and types of species using the site, monitoring may be needed to learn more about the site's avian resources and provide information needed to make permitting decisions with reasonable confidence.

Depending on the site, short-term on-site surveys/monitoring may focus on one or more of the following:

- species of special concern
- breeding bird species
- migrating birds
- wintering birds
- nocturnal bird activity
- species known to be susceptible to collision
- special situations.

Not all of these variables and efforts may be needed on a given project site. The effort expended in gathering the following types of information will depend on the knowledge of the site and perceived risk to birds. It may be useful to discuss which types of information are warranted for each project with the permitting agency, and possibly with others, early in the pre-permit application process so that the issue does not arise late in the process and cause costly delays. See chapter 3 of *Permitting Wind Energy Facilities: A Handbook* for a discussion of the permitting process.

When needed, this information should be gathered using the appropriate research options discussed in chapters 3-5.

Species of Special Concern

A comprehensive literature and on-site search may be conducted to predict the likelihood the site is used (or not used) by species of special concern. Species of special concern are species listed by the state or federal governments as threatened or endangered, and those species that are afforded other legal protections. Other species may also be considered in this category. If species of special concern are known to use the site, then additional inventory efforts may be required to better understand their use of the site, time of use, and to estimate potential adverse affects. Established species-specific inventory protocols are sometimes required by regulatory or resource agencies to ensure that adequate inventory methods and techniques are employed. These protocols can be obtained from the permitting authority. In situations where the species may use the site but this has not been determined with certainty, it is important for the developer to work with the permitting authority to determine the next steps needed.

Breeding Bird Species

Part of inventorying a site for bird resources includes the identification of bird species of concern that breed and nest on *or near* the site. Birds nesting nearby may include the site in their home range. The value of the area may be for nesting, foraging (feeding), or other common or irregular uses related to nesting. An example would be golden eagles nesting near but off the site and regularly foraging on-site.

Bird species come in different sizes, and exhibit different behaviors. Large species such as golden eagles may range up to 15 kilometers (km) from the nest area (Hunt 1995); smaller species may have a much smaller activity area (e.g. < 1km). It is valuable to understand the species under consideration in order to assess potential effects. Identification of these breeding species is season-constrained, so it is important to identify the need for this type of information early in the pre-permit application period in order to conduct a site evaluation at the appropriate time of year.

Migrating Birds

Many birds are migratory. Migrating species' use of a site depends on many variables. Some birds may pass through the project area only during the fall and spring migratory periods. During the time they are in the project area they may or may not exhibit behavior that puts them at risk. Some birds (including many passerines, or song birds) mostly migrate at night, while other birds (such as raptors and vultures) mostly migrate during daylight hours. Waterfowl and shorebirds migrate during day and night. Most migrating birds fly at higher altitudes above the ground than wind turbines are tall. However, migrating birds fly at other than normal altitudes for different reasons. Examples include birds flying closer to the ground surface during bad weather conditions or due to topographical features such as ridge tops. This type of information may be used to assist in estimating possible risk to birds. If on-site migratory bird information is gathered, sampling and method options from chapter 3 (Level 1 sampling techniques) should be considered (e.g. random sample sites using bird utilization counts at some regular frequency during the migratory months).

Wintering Birds

Wintering birds are those that spend their winters in the project area. These birds leave for nesting grounds in the early spring. Wintering birds (and bats) normally are leaving a colder place with limited food for a warmer locale with an adequate food supply. Many birds, such as raptors, become more social or at least more tolerant of other raptors during the winter. During nonbreeding periods, birds often flock or form groups. They forage and/or move together in ways they would not during breeding season. Concentrations of wintering birds and their specific behavior may or may not put them at risk. Surveys for wintering birds may need to be conducted during the winter. It is valuable to choose sample designs and methods that will provide adequate and defensible information (see chapter 3).

Nocturnal Bird Activity

Many birds are active mostly during daylight, but some birds are more active during the low light periods at sunset and sunrise. Owls, a few other bird species and bats, are normally active at night. Birds active during low light and at night may be resident, breeding, migrating, or wintering bird species. Activity during low light and night-time periods can result in collision with wind turbines (Anderson et al. 1996b). There have been no known incidents of large numbers of bird kills in wind plants during the nocturnal period. Currently, the reports in wind plant developments have been of infrequent kills. Current technology for nocturnal bird utilization monitoring can be costly and is not as well established as daytime observational methods. We discuss nocturnal techniques for the reader's information in the case it may be applicable.

Information about nocturnal bird activity may be obtained using remote sensing methods such as radar (Gauthreaux 1996a), ceilometers, acoustic monitoring (Evans and Mellinger 1999) or other night-time inventory techniques. Except in specific situations, existing techniques for nocturnal surveys may not be adequately refined nor validated to provide needed information at the level of confidence required. For example, marine radar can be used to get numbers, altitudes, flight speeds and directions of birds at higher altitudes above the ground surface, but has difficulty at lower altitudes above the ground surface where the information may be important in assessing the potential for collision risk with wind turbines. Species identification cannot be made with marine radar. Acoustic monitoring techniques hold promise, but are still under development at this writing.

Species Known to be Susceptible to Collision

Some species or species groups, such as raptors, have shown a greater tendency to collide with wind turbines than have other species, such as ravens (Anderson et al.1996b). As the results of more research and monitoring studies become available, additional species may be identified as being more or less at risk of collision. Such information should be employed to evaluate the acceptability of sites.

Special Situations

Birds may utilize specific areas more than other areas on the proposed wind plant site. Understanding those activity areas and modifying the project commensurately can be very valuable. Avoiding high use areas or areas used by species of special concern can be effective in minimizing impacts. Another example of a special situation is a species of special concern utilizing the potential wind energy development site during the time of year that coincides with a low wind period (e.g. Altamont Pass gets less than 2% of its wind in December and January). Sensitive species utilizing the area during a low wind period may be presented with fewer turning turbine blade collision possibilities than when turbines are operating a greater percentage of the time. This type of information can be useful in specific siting decisions and site suitability decisions. Gathering this type of information will normally require scientifically rigorous methods (see chapter 3) in order to obtain results that provide the confidence needed for these type of decisions.

DOCUMENTATION

It is important to document in writing how, what, when, and where all biological information was obtained throughout the site evaluation process. Written documentation ensures that credibility can be determined for both the bird biological information and how it was gathered. The integration of the site evaluation information into a written report that describes the resources and estimates potential impacts is valuable and often required.

Is the Biological Information Adequate?

Has adequate biological information been gathered? *Adequate* information is the amount and type of information needed to be in compliance with regulatory and environmental laws, ordinances, regulations, and standards of the jurisdiction(s) involved. Meeting the test of adequacy requires that the biological information (written report) is both sufficient and sufficiently clear to allow for reasonable estimates of bird impacts. The types of information discussed in this chapter should be adequate to assist with making many project decisions.

ESTABLISHING MORE RIGOROUS RESEARCH DESIGNS

Part II of this guidance document addresses the scientific methods useful when looking at general and specific issues relating to wind energy/bird interactions. These scientific methods can be used in studies that may be short-term or long-term, and that may be small or large in magnitude, time, and cost. It is important to match study designs/methods with needs regarding project size and perceived impacts. In particular, chapter 3 offers the reader several alternative designs for studies to provide permitting information as well as data needed to satisfy possible operational monitoring requirements. Using the information in this document will help to ensure that pre-permit application, and operational monitoring research, if needed, uses credible study design, methods, and metrics.

Chapters 4 and 5 present concepts and discuss issues related to advanced studies and modeling techniques that may be needed in some situations to assess population effects and risk reduction strategies. The site evaluation process should alert the developer to problematic sites. However, if problems arise later, chapters 3-5 contain information that will assist with their resolution.

PART II

CHAPTER 3: Basic Experimental Design and Level 1 Studies

INTRODUCTION

Public interest in the impact of the wind industry on birds has led some state and federal agencies responsible for permitting wind plants or protecting potentially affected avian species to require studies to:

- predict the potential effect of proposed wind plants on birds
- evaluate the actual effects on birds of wind plants in operation
- determine the causes of bird fatalities
- evaluate methods for reducing risk of bird fatality.

This chapter provides guidance to regulators, industry developers, scientists, and interested members of the public as to how such studies may be conducted in a manner that will withstand scientific, legal, and public scrutiny. While wind power presents somewhat unique environmental perturbations, the principles involved in designing studies of its effect on birds are the same as for other environmental perturbations.

This chapter does not provide detailed design protocols and standard operating procedures for quantification of impacts in all wind plant situations. Instead, it discusses how the quantification of effects fits into the various philosophies of design, conduct, and analysis of field studies. This chapter draws heavily from guidelines for design and statistical analysis of impact quantification studies of oil spills by McDonald (1995). Examples of two existing protocols measuring wind power effects on bird species are discussed.

In a perfect world, impacts would be measured without error. For example, bird fatalities on the site of a wind plant could simply be counted. However, when a complete count or census is impossible then impacts must be detected by the use of scientific study and statistics. The ultimate objective of statistics is to make inferences about a population (group of units) from information contained in a sample (Scheaffer, et al. 1990). Statistical or inductive inferences are made properly in reference to:

1. the design and protocol by which the studies are conducted in the specific study areas;
2. the specific time period of the study; and,
3. the standard operating procedures (SOPs) by which data are collected and analyzed.

If either the design protocol or the SOP is inadequately documented, then the study is not replicable and its validity is uncertain. In such a situation, it is impossible to know the proper extent of statistical conclusions and there would necessarily be less scientific confidence in the statistical inferences. This would result in less confidence in inferences based on expert opinion, as opinion in this case should follow statistical inferences. A common practice in ecological studies is the extension of study conclusions beyond the specific study areas to unstudied areas. This practice is acceptable and often necessary, albeit risky, as long as the assumptions are specified and it is clear that the extrapolation is based on expert opinion. When the extrapolation is presented as an extension of statistical conclusions it is an improper form of data analysis. *Deductive inferences* that extend beyond the specific study areas to draw general conclusions about cause-and-effect aspects of operating a wind plant may be possible if enough independent studies of different wind plants identify similar effects. However, *statistical inferences* beyond the study areas are not possible; nor should this be the primary objective of quantification of impact, given the unique aspects of any development.

The Traditional Experimental Design Paradigm

The traditional design paradigm for the true experiment is defined in terms of the following principles (Fisher 1968; Pollock 1996):

- **Control.** The scientist tries to control (standardize) as many variables as possible except for those associated with the different treatment conditions that are to be compared.
- **Randomization.** The scientist randomly allocates treatments to experimental units so that the values of variables not controlled are allocated equally over units (at least on average).
- **Replication.** Each treatment is allocated to multiple independent experimental units so that unexplained or inherent variation can be quantified. Information about the amount of inherent variability is needed for valid statistical testing.

Two additional methods are useful for increasing the precision of studies when the number of replicates cannot be increased:

2. Use analysis of covariance when analyzing the response to a treatment to consider the added influence of variables having a measurable influence on the dependent variable.

The study of wind energy development impacts is made difficult by the relatively large area potentially affected, the relative scarcity of many of the species of primary concern, and the relative scarcity of the events being measured (e.g. mortality, use of a particular turbine by a particular species). Quantification of the magnitude and duration of impacts due to a wind plant necessarily requires an observational design, because the area to receive the wind plant and the areas designated as the references (controls) are not selected by a random procedure. *Observational studies* also are referred to as "sample surveys" (Kempthorne 1966), "planned surveys" (Cox 1958), and "unplanned experiments / observational studies" (National Research Council 1985). See Manly (1992) and McKinlay (1975) for a discussion of the design and analysis of observational studies. *Impact studies* typically are large field studies, as opposed to manipulative experiments or observational studies in subjectively selected small homogenous areas. Data are collected by visual detection of an event in time and space.

Finally, in studies of wind plant effects on birds, as with many environmental impact studies, conclusions concerning cause-and-effect of wind plants are limited. Practically speaking, identical "control" areas seldom exist; similar "reference" areas must be used instead. Moreover, there is no random assignment of treatment, and replication is usually impossible. Wind plant sites are selected because they are very windy, there exists a market for the power produced, and there is an existing infrastructure (e.g. a power grid). These sites tend to be relatively unique topographically, geographically, and biologically, and are difficult to duplicate, at least in a relatively small area. Even if all the potential wind sites are known in an area, the decision regarding where to locate the plant is never a random process. Finally, the expense of a wind plant makes replication impractical. Thus, one does not have a true experiment.

In all studies of impact, including wind plant impacts, it is essential that a few basic study principles be followed. The following is a brief discussion of some of the more important principles. For a detailed discussion of these principles see Green (1979) and Skalski and Robson (1992).

Know the Question

It is essential that the question being addressed by the research be clearly understood. Research questions form the basis for developing research hypotheses, and help to define the parameters for comparing hypothesized outcomes with actual research results. (See section on Data Analysis for a more direct discussion of hypothesis testing.) The design of the study protocol depends on the question being addressed. The protocol that addresses the question of wind plant risk to individual birds is substantially different from a protocol addressing the risk to a population of birds. A clear understanding of the question increases the efficiency of the research. It is a waste of time and money to collect vast quantities of data with the idea that their meaning will become obvious after the data are analyzed. The outcome of a study is more likely to be useful if an appropriate study design is followed and all interested parties have a clear understanding of the research question. Studies of wind plant impacts on birds should allow the research question to be addressed through inductive (statistical) inferences as well as deductive inferences (expert opinion). These inferences should help provide a sound scientific basis for development of protocols for quantification of wind power impact.

Replicate

Replication means repetition of the basic experiment (Krebs 1989) within each time and location of interest, producing multiple sets of independent data. Essential for statistical inference, replication allows the estimation of variance inherent in natural systems and reduces the likelihood that chance events will heavily influence the outcome of studies. Proper statistical inference must also keep the proper experimental unit in mind. In studies of wind power the experimental unit may be a turbine, a string of turbines, or the entire wind plant. Using the wrong experimental unit can lead to errors in the identification of the proper sample size and estimates of sample variance. Confidence in the results of studies improves with increased replication; generally speaking, the more replication in field studies the better.

The concept of replication often is confused in the conduct of environmental studies; what constitutes replication of the basic experiment depends on the objective of the study. For example, if the objective is to compare bird use of a wind plant to bird use in a similar area without the wind plant, replication may be achieved by collecting numerous independent samples of bird use throughout the areas and seasons of interest. In this case the sample size for statistical comparison is the number of samples of bird use by area and season. However, if the objective is to estimate the effect of another wind plant — or wind plants in general — on bird use, then the above wind plant constitutes a sample size of one, from which no statistical comparisons to other sites are possible. The statistical extrapolation of data from one study site to the universe of wind plants is one of the more egregious examples of pseudoreplication as defined by Hurlbert (1984) and Stewart-Oaten et al. (1986).

When estimating the appropriate sample size in an experiment, a good rule to follow is that the analysis should be based on only one value from each sample unit. If five sample plots are randomly located in a study area, then statistical inferences *to the area* should be based on five values — regardless of the number of birds which may be present and measured or counted in each plot. If five animals are captured and radio-tagged, then statistical inferences to the population of animals should be based on a sample of five values, regardless of the number of times each animal is relocated. Repeated observations of birds within a plot or repeated locations of the same radio-tagged animal are said to be dependent for purpose of extrapolation to the entire study area. Incorrect identification of data from sampling units is a common form of pseudoreplication that can give rise to incorrect statistical precision of estimated impact. It becomes obvious that replication is difficult and costly in environmental studies, particularly when the treatment is something as unique as a wind plant.

Randomize

Like replication, an unbiased set of *independent data* is essential for estimating the error variance and for most statistical tests of treatment effects. Although truly unbiased data are unlikely, particularly in environmental studies, a randomized sampling method can help reduce bias and dependence of data and their effects on the accuracy of estimates of parameters. A systematic sample with a random start is one type of *randomization* (Krebs 1989).

Collecting data from “representative locations” or “typical settings” is not random sampling. If landowner attitudes preclude collecting samples from private land within a study area, then sampling is not random for the entire area. In studies conducted on representative study areas, statistical inference is limited to the protocol by which the areas are selected. If private lands cannot be sampled and public lands are sampled by some unbiased protocol, statistical inference is limited to public lands. The selection of a proper sampling plan is a critical step in the design of a project and may be the most significant decision affecting the utility of the data when the project is completed. If the objective of the study is statistical inference to the entire area, yet the sampling is restricted to a subjectively selected portion of the area, then there is no way to meet the objective with the study design. The inference to the entire area is reduced from a statistical basis to expert opinion.

Control and Reduce Errors

The precision of an experiment (density of repeated measures of the same variable) can be increased through replication, but this is expensive. As discussed by Cochran (1977) and Cox (1958), the precision of an experiment can also be increased through:

1. use of experimental controls
2. refinement of the experimental techniques including greater sampling precision within experimental units
3. improved experimental designs including stratification and measurements of non-treatment factors (covariates) potentially influencing the experiment.

Use of experimental controls. Good experimental design should strive to improve the precision of conclusions from experiments through the *control (standardization) of related variables* (Krebs 1989). In the evaluation of the effect of some treatment (e.g., an anti-perching device) on the frequency of perching on wind turbines, it would be most efficient to study the devices on the same model turbine, controlling for turbine type. One could evaluate the effect of wind turbines on bird use by making comparisons within vegetation types and thus control for the effect of vegetation. However, standardization of related variables is often difficult in field studies.

An alternative to standardizing variables is to use information that can be measured on related variables in an *analysis of covariance* (Green 1979). For example, understanding differences in raptor use between areas is improved when considered in conjunction with factors influencing use, such as the relative abundance of prey in the areas.

Precision can also be improved by *stratification*, or assigning treatments (or sampling effort) to homogenous strata, or blocks, of experimental units. Stratification can occur in space (e.g., units of homogenous vegetation), and in time (e.g., sampling by season). Strata should be small enough to maximize homogeneity, keeping in mind that smaller blocks may increase sample size requirements. For example, if vegetation is used to stratify an area, then the stratum should be small enough to ensure a relatively consistent vegetation pattern within strata. However, stratification requires some minimum sample size necessary to make estimates of treatment effects within strata. It becomes clear that stratification for a variable (say vegetation type) at a finer and finer level of detail will increase the minimum sample size requirement for the area of interest. If additional related variables are controlled for (e.g., treatment effects by season), then sample size requirements can increase rapidly. Stratification also assumes the strata will remain relatively consistent throughout the life of the study, an assumption often difficult to meet in long-term field studies.

Minimizing bias. Sampling (study) methods should be selected to minimize bias in the outcome of the study. Green (1979) provides several examples of bias introduced by study methods. In field studies it is probable that study methods will always introduce some bias. This bias can be tolerated if it is relatively small, measurable, or consistent among study areas. For example, the estimation of bird use within wind plants and reference areas may be accomplished by visual observation. The presence of the observer no doubt influences bird use to some extent. However, if the observations are made the same way in both areas then the bias introduced by the study method should have little influence on the measured *difference in use* between the two areas, which is the parameter of interest. Methods introducing severe bias should be avoided.

Size and distribution of study plots. The size and distribution of study plots also is an important component of the study method. Skalski et al. (1984) illustrated how field designs that promote similar capture (selection) probabilities in the different populations being compared result in comparisons with smaller sampling error. Green (1979) points out that plot size makes little difference if organisms are distributed at random throughout the study area, but that use of a larger number of smaller plots increases precision with aggregated distributions. Since aggregated distributions are the norm in nature, it generally is better to use a larger number of smaller plots well distributed throughout the study area or stratum.

Cost, logistics, the behavior of the organism being studied, and the distribution of the organism will determine plot size. Use of larger plots usually allows the researcher to cover more area at a lower unit cost (e.g. cost/hectare sampled). Also, plots can be so small that measurement error increases dramatically (e.g. is the study subject in or out of the plot) or the variance of the sample increases because the detection of the organism is rare, resulting in a data set with a lot of zeros. As a rule, the smallest plot size practical should be selected.

Shape of study plots. The shape of study plots is an important consideration. For example, fixed plot and line-intercept sampling work well with common plant and animal species. In fixed plot sampling there is an attempt at complete census of some characteristic within selected units. Assuming some form of unbiased sampling is conducted, *fixed plot sampling* should result in equal probability of selection of each plot. *Point-to-item* and *line transect sampling* are more effective when sampling less common items. However, *line-intercept*, some point-to-item methods (e.g., plotless estimates of basal area), and some applications of *line transect* methods (e.g. when larger objects are more easily seen) are biased in that larger individuals are more likely to be included in the sample. *The selection of the appropriate size and shape of study plot must be made on a case by case basis and is an important component of the study protocol*.

Pilot Studies

A few data can do wonders for the design of environmental studies. Environmental studies should make maximum use of existing data. When little or no data exist, a pilot study can provide preliminary data useful in evaluating estimates of needed sample size, optimum sampling designs, data collection methods, the presence of environmental patterns and other factors which can affect the success of the study. Pilot studies can vary from reconnaissance surveys to the implementation of a draft protocol in a portion of the study area for a relatively short period of time. It may be false economy to try to save money by avoiding some preliminary data collection that could dramatically improve the quality of a study. In the absence of data on the study area, the first time period of study often becomes the pilot study. If the first period of study suggests major changes in the protocol, then the value of the first data set may be relatively low in the ultimate analysis of impacts, an important consideration for designs dependent on pre-impact data. *While pilot studies are not absolutely necessary they are recommended when the lack of data and/or delay due to study requirements are major concerns*.

THE PHILOSOPHY OF STUDY DESIGN

Statistical conclusions are made under two broad and differing philosophies for making scientific inferences: *design/data-based* and *model-based*. Widespread confusion surrounds these philosophies, both of which rely on current data to some degree and aim to provide "statistical inferences." There is a continuum from strict design/data-based analysis (e.g., finite sampling theory [Cochran 1977] and randomization testing [Manly 1991]) to pure model-based analysis (e.g., habitat suitability indices/habitat evaluation procedures [HSI/HEP (U.S. Fish and Wildlife Service 1980)] using only historic data [USDI 1987]). A combination of these two types of analyses is often employed, resulting in inferences based on a number of interrelated arguments.

Design/Data-Based Analysis

In strict design/data-based analysis, basic statistical inferences concerning the study areas are justified by the design of the study and data collected (Cochran 1977, Scheaffer et al. 1990). Computer intensive statistical methods (e.g., randomization, permutation testing, etc.) are available without requiring additional assumptions beyond the basic design protocol (e.g., Manly 1991). *Design/data-based statistical conclusions stand on their own merits for the agreed-upon:*

- impact indicators
- procedures to measure the indicators
- design protocol.

Re-analysis of the data at a later time cannot declare these basic statistical inferences incorrect. The data can be re-analyzed with different model-based methods or different parametric statistical methods; however, *the original analysis concerning the study areas will stand and possess scientific confidence if consensus is maintained on the conditions of the study* (bulleted items above).

Model-Based Analysis

Predictive methods estimate risk and impact through the use of models. In the extreme case of model-based analysis where no new data are available, all inferences are justified by assumption, are deductive, and are subject to counter-arguments. The more common model-based approach involves the combination of new data with parameters from the literature or data from similar studies by way of a theoretical mathematical/statistical model. An example of this approach in the evaluation of wind plant impacts on bird species is the demographic modeling of a bird population combined with use of radiotelemetry data to estimate the influence of the wind plant on critical parameters in the model. This approach is illustrated by the telemetry studies in Altamont Pass, California, as described by Shenk et al. (1996).

Mixtures of Design/Data-Based and Model-Based Analyses

Often inferences from study designs and data require mixtures of the strict design/data-based and pure model-based analyses. Mixtures of study designs would include those analyses where:

1. design/data-based studies are conducted on a few important bird species
2. manipulative tests are conducted using surrogate species to estimate the effect of exposure to wind turbines on species of concern (Cade 1994)
3. deductive professional judgment and model-based analyses are used to quantify impacts on certain components of the habitat in the affected area.

Strict adherence to design/data-based analysis in quantifying injuries often may be impossible, but it is recommended that the design/data-based analysis be adhered to as closely as possible. *The value of indisputable design/data-based statistical inferences on at least a few impact indicators cannot be overemphasized in establishing confidence in the overall assessment of impact due to wind plants*. However, in some circumstances model-based methods provide a suitable alternative to design/data-based methods. The advantages, limitations, and appropriate applications of model-based methods are discussed further in chapter 4 and in Gilbert (1987), Johnson et al. (1989), and Gilbert and Simpson (1992).

Levels of Studies

To simplify discussion of studies of wind plant effects on birds, we have divided the discussion of specific study protocols into discussions of broad, less intensive (Level 1) studies and more detailed (Level 2) studies.

Level 1 studies include pre-permitting baseline studies, risk assessment studies, and monitoring studies designed to detect the relatively large effects of operating wind plants. Studies to determine the relative risk of wind plants to species and communities, as well as monitoring studies, normally would be Level 1 studies. *Level 2 studies* involve detailed studies of one or more bird populations and manipulative studies designed to determine the mechanisms of fatality or risk. Basic research on fatality pathways, the quantification of risk to populations, and the evaluation of risk reduction management practices normally involve Level 2 studies. This chapter provides an introduction to the basics of study design and discusses Level 1 studies in some detail. For the remainder of this chapter, the wind plant is considered to be a treatment in a scientific study and the area affected by the wind plant to be the assessment area.

Level 1 studies generally will be useful in the following situations:

- A wind plant site is selected but the distribution of turbines and turbine strings has not been determined and turbine siting could be influenced by information on potential risk to bird species.

- The decision to construct a wind plant has been made, but development will proceed in phases based on assessment of impacts of construction and operation of the initial phase.

- A wind plant exists but the extent and importance of bird impacts is unknown.

- Other studies or credible information on bird use and habitat suggest impacts are likely.

Once the decision is made to conduct Level 1 studies, the following potential issues must be identified and considered.

1. **The area of interest** (area to which statistical and deductive inferences will be made). Options include the plant site, the entire WRA, the local area used by birds of concern, or the bird population potentially affected (in this case population refers to the group of birds interbreeding and sharing common demographics).

2. **Time period of interest**. The period of interest may be (for example) diurnal, nocturnal, seasonal, or annual.

3. **Species of interest**. The species of interest may be based on behavior, fatalities in existing wind plants, abundance, or legal/social mandate.

4. **Potentially confounding variables**. These may include landscape issues (e.g. large scale habitat variables), biological issues (e.g. variable prey species abundance), land use issues (e.g. rapidly changing crops and pest control), weather, study area access, etc.

5. **Time available to conduct studies**. The time available to conduct studies given the project development schedule will often determine how studies are conducted and how much data can be collected.

6. **Budget**. Budget is always a consideration for potentially expensive studies. Budget should not determine what questions to ask but will influence how they are answered. It will largely determine the sample size, and thus the degree of confidence one will be able to place in the results of the studies.

7. **Project magnitude**. Project magnitude will often determine the level of concern and the required precision.

Level 1 studies to quantify risk and impacts of wind plants typically will use an observational design with study areas not selected by random procedure. Observational studies also are referred to as "sample surveys" (Kempthorne 1966), "planned surveys" (Cox 1958), and "unplanned experiments/observational studies" (National Research Council 1985). The objective of observational studies is usually an estimate of parameters necessary to describe the statistical population, such as density, survival rates, natality, and habitat use (Skalski and Robson 1992). In this case, the statistical population is defined as the group of animals or other objects of study. See Manly (1992) and McKinlay (1975) for excellent discussions of the design and analysis of observational studies.

An observational study of the impacts of a wind plant on bird species is not a true experiment because selection of the area to receive the wind plant and selection of the areas to be the references are not by a random procedure. The wind resource assessment area may consist of several disjoint subregions affected by wind turbines. These disjoint segments of the wind plant may be further stratified into major vegetation types. A potential undeveloped reference site may have areas within its boundary that appear similar to the wind plant and may also be stratified by the same major vegetation types. Even though the logic used in the study of these areas is that both the *assessment area* and the *reference area* are stratified into vegetation types, and study sites are randomly selected from within strata, these subregions are not independent replicates of the wind plant. Random selection of study sites/organisms from assessment and reference areas is known as *subsampling*. In the end, in an Impact-Reference study design, only one wind plant in one area is available for comparison to one or more subjectively selected reference areas.

DESIGN/DATA-BASED STUDIES

Both design/data-based and model-based methods may be used in Level 1 studies. Both methods benefit from historic and current data collected according to repeatable and reliable field studies. This section contains designs that are most appropriate for Level 1 studies, but can be used in Level 2 studies. Studies following the recommended designs are repeatable. Statistical results from repeated sampling following the same design would apply to the same universe of study; whether the universe of study is an assessment area, an assessment population, or a time period of interest.

There are several alternative methods of study when estimating impact. The following designs are arranged approximately in order of reliability, for sustaining confidence in the scientific conclusions. It must be understood that no one method is always best; the method selected for a particular study will depend on a number of issues, as discussed below.

We also discuss designs for studies that make comparisons between assessment areas and areas with similar physical and biological characteristics. These areas often are termed control areas but are not true controls in the experimental sense (i.e., a near perfect match to the assessment area). Since good control areas seldom exist in field studies, we will use the term *reference area* instead. The term is defined in the same way as Stewart-Oaten (1986) and others have used the term *control area*: an area representative of the assessment area. The term "reference area" appropriately illustrates that, in observational studies, the differences between an assessment area and an area to which it is compared must be considered in light of the high degree of natural variability among any two sites.

Designs with Control (Reference) Areas

The Before-After/Control Impact Design (BACI)

The *Before-After/Control (reference)-Impact (BACI)* design is common in the literature (e.g., Stewart-Oaten 1986), and has been called the "optimal impact study design" by Green (1979). It is equivalent to the paired control-treatment design proposed by Skalski and Robson (1992). The term *BACI* is so common in the literature that the letter *C* must be retained in its name, even though we use the term "reference area" rather than "control area."

The BACI design is very desirable for impact determination because it addresses two major impact study design problems.

1. Impact indicators, such as the abundance of organisms, vary naturally through time, so any change observed in an assessment area between the pre- and post-impact periods could conceivably be unrelated to the treatment (e.g., the construction and operation of a wind plant). Large natural changes are expected during an extended study period.

2. There always are differences in the indicators between any two areas (again, consider bird abundance). Observing a difference between assessment and reference areas *following* the treatment does not necessarily mean that the wind plant was the cause of the difference. The difference may have been present prior to construction. Conversely, one would miss a wind plant impact if the abundance of the indicator on the reference area were reduced by some other perturbation concurrent with construction of the wind plant.

The BACI design helps with these difficulties. By collecting data at both reference and assessment areas using exactly the same protocol during both pre-impact and post-impact periods one can ask the question: *Did the average difference in abundance between the reference area(s) and the wind plant area change after the construction and operation?*

The BACI design is not always practical or possible. Adequate reference areas often are difficult to locate, and while preliminary analysis may satisfy the permitting agency that a project may proceed, the planning of wind plant projects does not always allow enough time for a full-scale pre-impact study period. The multiple time periods necessary for this design usually increase the cost of study. Additionally, alterations in land use or disturbance occurring over these time periods and reference areas complicate the analysis of study results. Caution should be used when employing this method in areas where potential reference areas are likely to incur relatively large alterations or changes that impact the species being studied. In the case of small homogeneous areas of potential impact and where a linear response is expected the impact gradient design may be a more suitable design. If advanced knowledge of a wind plant location exists, the area of impact is somewhat varied, and species potentially impacted are wide ranging, the BACI design is preferred for observational studies of impact.

Matched Pairs in the BACI Design

Matched pairs of sites from assessment and reference areas often are subjectively selected to reduce the natural variation in impact indicators (Skalski and Robson 1992). Eberhardt (1976) labeled designs using this matching "pseudo-experiments" because of the lack of randomization and true replication of treatments and control conditions. Statistical analysis of these pseudo-experiments is dependent on the sampling procedures used for selection of sites and the amount of information collected on concomitant site-specific variables. For example, sites may be randomly selected from the assessment area and each subjectively matched with a site from a reference area. In this case the area of inference is to the assessment area, and the reference pairs simply act as an indicator of baseline conditions.

When applied to a wind plant or other non-random perturbations (treatments), the extent of statistical inferences when matched pairs are used in the BACI design is limited to the assessment area. The inferences also are limited to the protocol by which the matched pairs are selected. If the protocol for selection of matched pairs is unbiased, then statistical inferences comparing the assessment and reference areas are valid and repeatable. The selection of matched pairs for extended study contains similar risks associated with stratification. The presumption is that, with the exception of the treatment, the pairs remain very similar — a risky proposition in long-term studies.

Primary references for design and analysis are Skalski and Robson's (1992: chapter 6) Control-Treatment Paired (CTP) design and Stewart-Oaten's (1986) Before/After-Control/Impact-Pairs (BACIP) design. If there are modifications of the basic structure of the design, then statistical analysis of the resulting data will not follow standard textbook examples.

Impact-Reference Design (After Treatment)

The *Impact-Reference Design* is for quantification of impact in studies where the impact indicators measured on the assessment area are compared to measurements from one or more reference areas. In the Impact-Reference Design, data collected after the wind plant is operational are contrasted between the assessment and reference areas. The Impact-Reference design is considered because proposed and existing wind plants often lack "before construction" baseline data from the assessment area and/or a reference area. In these cases, the BACI design is not applicable and an alternative must be found. Assessment and reference areas are censused or randomly subsampled by an appropriate observational design. Design and analysis of wind plant impacts in the absence of pre-impact data follow Skalski and Robson's (1992: chapter 6) recommendations for accident assessment studies.

Differences between assessment and reference areas measured only after the impact might be unrelated to the impact, because site-specific factors differ. For this reason, differences in natural factors between assessment and reference areas should be avoided as much as possible. However, differences usually will exist. Reliable quantification of impact must include as much temporal and spatial replication as possible. Additional study components, such as the measurement of other environmental factors that might influence impact indicators, may also be needed to limit or explain variation and the confounding effects of these differences. Environmental indicators often are termed covariates because analysis of covariance may be used to adjust the analysis of a random variable to allow for the effect of another variable.

Designs Without Reference Areas

Impact-Gradient Designs

The *Impact-Gradient Design* is for quantification of impact in relatively small assessment areas on homogeneous environments. If potentially impacted species have relatively small home ranges (e.g. passerines) and a gradient of response is anticipated, this design is a preferred approach to impact studies. When this design is appropriate, associated costs should be less than for those designs requiring baseline data and/or reference areas and impacts can be estimated with more confidence.

Analysis of the Impact-Gradient Design is based on an analysis of the relationship between the impact indicator and distance from the hypothesized impact source — in this case, wind turbines. In effect, the assessment area includes the reference area on its perimeter. This design does not require that the perimeter of the assessment area be free of impact, only that the level of impact be different. If a gradient of biological response(s) or distance is identified, the magnitude of differences can be translated into what can be presumed to be at least a minimum estimate of the amount of impact. This Impact-Gradient Design would be analogous to a laboratory toxicity test conducted along a gradient of toxicant concentrations. An example might be an increasing rate of fledgling success in active raptor nests or a decrease in passerine mortality as a function of distance from the wind plant.

In a field study, there likely will be naturally varying factors whose effects on the impact indicators are confounded with the effects of the impact. Thus it is important to have supporting measurements of covariates to help interpret the gradient of response observed in the field study. In the example of decreased mortality in passerines, an obvious covariate to consider would be vegetation type.

Data collected from these studies may also be analyzed from the philosophy of the designs with reference areas if one discovers that a gradient of response is absent but a portion of the study area meets the requirements of a reference area. The impact gradient design can be used in conjunction with BACI, Impact Reference and Before-After designs.

Before-After Designs

The *Before-After Design* is for the quantification of impact when measurements on the assessment area before the impact are compared to measurements on the same area following the impact. This design is considered because it is possible that large-scale monitoring of birds within an area might be undertaken if enough concern exists for their security within a potential WRA. Government agencies or private industry may monitor impact indicators over long periods of time, and reliable baseline data may exist. If so, measurements can be made after the incident using exactly the same protocol and SOPs. However, observed differences might be unrelated to the incident, because confounding factors also change with time (see the above discussion of the BACI design). Reliable quantification of impact usually will include additional study components to limit variation and the confounding effects of natural factors that may change with time.

Because of the difficulty in relating post-impact differences to treatment effects in the absence of data from reference areas, injury indicators can be particularly useful in detecting impacts using Before-After Design. The correlation of exposure to toxic substances and a physiological response in wildlife has been documented well enough for some substances to allow the use of the physiological response as a *biomarker* for evidence of impact. Examples of biomarkers used in impact studies include the use of blood plasma dehydratase in the study of lead exposure, acetylcholinesterase levels in blood plasma in the study of organophosphates, and the effect of many organic compounds on the microsomal mixed-function oxidase system in liver (Peterle 1991). The number of dead birds in some defined area determined by necropsy to be caused by a wind plant could be used as such an indicator. It is possible that existing biomarkers (e.g., biomarkers indicating stress) might also have some application to estimating wind plant impacts on birds.

Costs associated with conducting the Before-After Design should be less than that required for designs requiring reference areas. Statistical analysis procedures include the time-series method of intervention analysis (Box and Tiao 1975). An abrupt change in the impact indicator at the time of the impact may indicate the response is due to the perturbation (say a wind plant). Scientific confidence is gained that the abrupt change is due to the wind plant if the impact indicator returns to baseline conditions through time after making adjustments of factors in the wind plant apparently related to observed impacts (Figure 3-1).

If the impact indicator returns to baseline conditions during the operation of the wind plant, impacts would be considered short-term and the absence of long-term impacts would be suggested. However, interpretation of this type of response without reference areas or multiple treatments is difficult and somewhat subjective. This type of design is most appropriate for short-term impacts, rather than for long-term projects such as a wind plant.

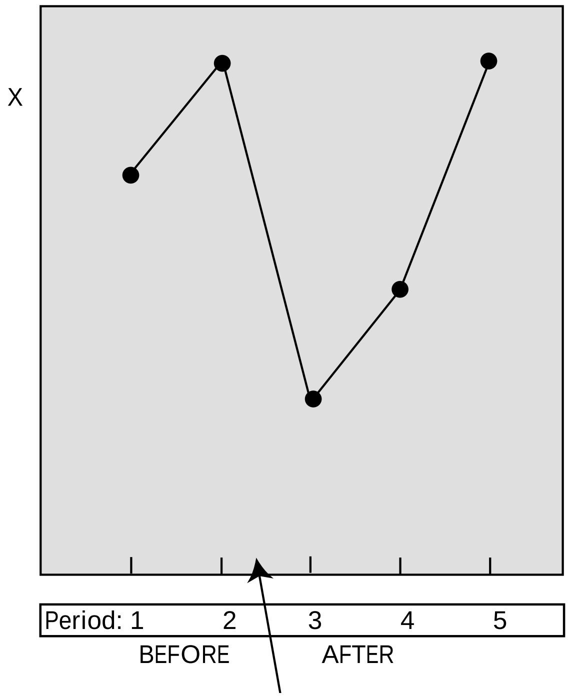

Figure 3-1. Idealized sketch of an impact indicator in a Before-After Design with five time periods (T) of interest where an abrupt change coincides with an impact and is followed by a return to baseline conditions.

IMPROVING THE RELIABILITY OF STUDY DESIGNS

Use of More than One Reference Area

Use of two or more reference areas increases the reliability of conclusions concerning quantification of impact (Underwood 1994). Reliability and validity of a scientific study for quantification of impact often will be questioned on the basis that "the reference area is not appropriate for the assessment area." Consistent relationships between the assessment area and each of two (or more) reference areas will generate far more scientific confidence in the results than if a single reference area is used. This scientific confidence will likely be increased more than would be expected given the increase in number of reference areas. This is true whether the wind plant is concluded to have "an important impact" or "no important impact." The use of multiple reference areas has the disadvantage of increased cost.

With two or more reference areas, one will be able to compare the impact indicators between different reference areas during the assessment period. Multiple reference areas also will allow a comparison of impact indicators from the assessment area with the mean of impact indicators from two or more reference areas. For example, consider a wind plant and two reference areas outside the influence of but in the same general area as the wind plant. If approximately the same differences exist between the impact indicators on the wind plant and each of the reference areas before construction, then this "replication in space" usually gives scientists more confidence when making deductive professional judgments regarding post construction impacts.

In practice, impact indicators for the three areas will be plotted and examined for relative changes before and after construction of the wind plant. Assuming all three areas have similar trends in impact indicators before impact and reference areas have similar trends after impact, tests for differences will be between the mean of the impact indicators for multiple reference areas and the value of the impact indicator for the wind plant. By studying the effect of a few important covariates on the impact indicator on the wind plant and reference areas, it may be possible to adjust raw data before comparisons of mean values are made. For example, if nestling survival is highly correlated with prey abundance it might be possible to adjust survival rates for differences in prey on reference and assessment areas before testing for wind plant effects.

Collection of Data Over Several Time Periods

Collection of data on the study areas for several time periods before and/or after the impact also will enhance reliability of results. Confidence in the relationship of assessment and reference areas is improved. Figure 3-2 illustrates results from a BACI design with two periods for data collection before the wind plant impact and two periods of data collection following the wind plant development. In this sketch there is only a slight indication of recovery after the construction of the wind plant. Statistical tests or other analyses (e.g., confidence intervals) unique to the subsampling plan used in data collection will be required for judging whether statistically significant differences exist between the point estimates.

For example, assume data on a response variable — say the number of fledglings per active nest — exist for two years before construction and two years after construction of a wind plant with one reference area. Assume also that the data meet the assumptions necessary for use of *analysis of variance (ANOVA)*. ANOVA would be used to test for interaction among study sites and years, the primary indicator of an effect due to the development. A significant interaction effect may indicate that a pre-treatment difference between a development area and reference areas is not equal to the post-treatment difference. Additional comparisons could be made, such as the comparison of the mean response pre-treatment with the response each year post-treatment or with the mean over all years post-treatment. Results would be presented graphically to illustrate point estimates and precision (confidence intervals or standard errors). The statistical inference would be limited to the two areas and the four years.

The specific test used depends on the response variable of interest (count data, percentage data, continuous data, categorical data, etc.) and the subsampling plan used (point counts, transects counts, vegetation collection methods, GIS data available, radio-tracking data, capture-recapture data, etc.). Often, classic ANOVA procedures will be inappropriate and nonparametric, or other computer-intensive methods will be required.

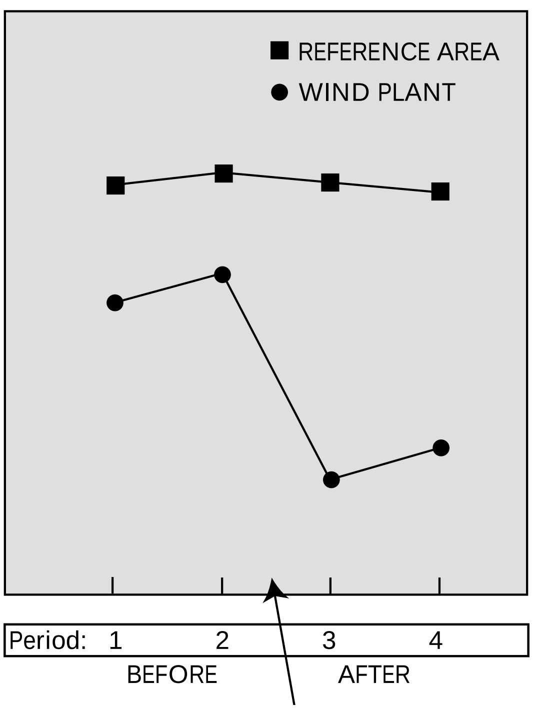

Figure 3-2. Sketch of point estimates of an impact indicator in an idealized BACI design over four time periods with slight indication of recovery after the incident.

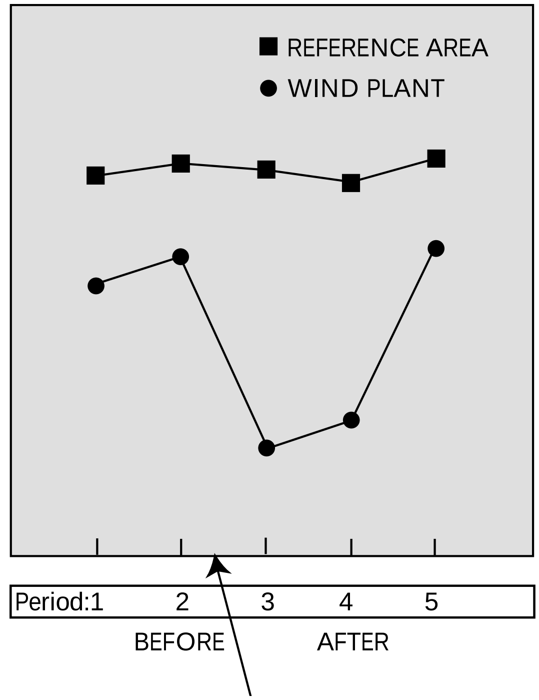

Figure 3-3. Sketch of point estimates of an impact indicator in an idealized BACI design where interaction with time indicates recovery from impact by the third time period following the incident.

Interpretation of Area-by-Time Interactions

Non-parallel responses for impact indicators plotted over time on assessment and reference areas are said to exhibit *area-by-time interaction* (Figure 3-3).

If abrupt changes in the relationship of assessment and reference areas occur following the impact and are followed by a return to baseline conditions, then scientific confidence is gained for the conclusion that the abrupt changes were due to the impact. This interaction is illustrated in Figure 3-4, where the difference between the impact indicator on the reference and assessment areas represents the magnitude of an impact. Also, a return to a relationship similar to baseline conditions provides additional scientific confidence that comparison of assessment area and the subjectively selected reference areas is appropriate for estimating impact (Skalski and Robson 1992). In the case of a wind plant, recovery suggests a change in bird behavior reducing risk, a temporary impact due to construction, or a change in the wind plant (e.g., safer turbines).

Evidence of significant area-by-time interaction is especially important in an Impact-Reference Design, because this may be the only factor which aids in estimating the difference, if any, between the reference areas and assessment area in the absence of the

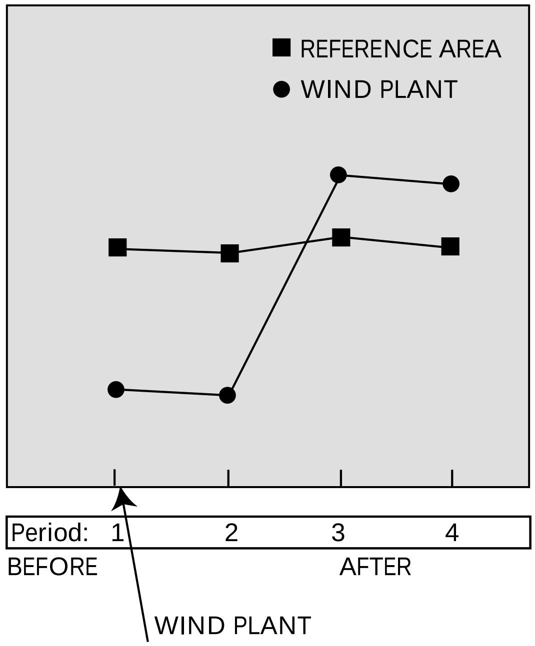

Figure 3-4. Idealized sketch of results from a Reference-Impact Design where a large initial difference in the impact is followed by a shift to parallel response curves.

impact. This situation is illustrated in figure 3-4 with an idealized presentation of a large difference between the assessment and reference area following the impact, which is followed by a return to approximately parallel responses of data plotted over time. (Note that this figure, like the others in this chapter, is an idealized hypothetical presentation. Real data points would necessarily include error bars.) This interaction could indicate that impacts were temporary or that a significant change has been made in the operation of the wind plant (say installation of safer turbines or removal of turbines responsible for the impact).

Model-Based Analysis and Use of Concomitant Site-Specific Variables

Pure design/data-based analysis often is not possible in impact studies. For example, bird abundance in an area might be estimated on matched pairs of impacted and reference study sites. However carefully the matching is conducted, uncontrolled factors always remain that may introduce too much variation in the system to allow one to statistically detect important differences between the assessment and reference areas. In a field study, there likely will be naturally varying factors whose effects on the impact indicators are confounded with the effects of the incident. Data for easily obtainable random variables that are correlated with the impact indicators (covariates) will help interpret the gradient of response observed in the field study. These variables ordinarily will not satisfy the criteria for determination of impact, but can be used in model-based analyses for refinement of the quantification of impact (Page et al. 1993, Smith 1979). For example, in the study of bird use on the Wyoming wind plant site, WEST Inc. (1995) developed indices to prey abundance (e.g. prairie dogs, ground squirrels, and rabbits). These ancillary variables are used in model-based analyses to refine comparisons of avian predator use in assessment and reference areas. Land use also is an obvious covariate that could provide important information when evaluating differences in bird use among assessment and reference areas and time periods.

Indicators of degree of exposure to the impact-producing factor also should be measured on sampling units. As in the Impact-Gradient Design, a clear impact-response relationship between impact indicators and degree of exposure will provide corroborating evidence of impact. These indicators also can be used with other concomitant variables in model-based analyses to help explain the “noise” in data from natural systems. For example, the size of turbines, the speed of the turbine blades, the type of turbine towers, etc. can possibly be considered indicators of the degree of exposure.

Sampling The Area Of Interest

In this section, the word *sample* means either *the process* by which units of observation in a specific area are selected, or *the actual collection* of units selected for study. The study area consists of either a finite or an infinite universe of sampling units. For example, a small site might be divided into a finite set of 1-m x 1-m plots, each having an opportunity to be selected in the sample. A sample of plots is selected from the area and measurements are made of indicators such as the number and biomass of plants or animals on each plot. In this case, the word sample refers more to the location of the units than to the specimen (plant, animal, sediment, etc.) collected from the unit.

If one is interested in the set of animals or plants living on (or influenced by) the assessment or reference study sites, then a second universe exists: namely, the population of animals or plants. The word *population* in this case refers to the group of organisms under study (the statistical population) and not necessarily to the biological population. This second universe also can be sampled and used to make statistical inferences to the group of organisms living in or influenced by the study area. For example, the impact of a wind plant on breeding pairs of raptors may extend >20 km from the turbines (determined by the range of the birds) and a capture-recapture model-based study may be undertaken of the breeding pairs within the WRA and a 20 km radius. In this case the marked animal is treated as the sample unit. All of the techniques for study of animal or plant populations in field ecology (plotless methods from forestry, capture-recapture methods from wildlife science, etc.) become candidates for study of the impacts of a wind plant. Chapter 4 provides more detail on the detailed study of wind plant impacts on mobile wildlife.

Two Levels of Sampling

For a smaller wind plant with a less extensive assessment area, the entire area may be the study site, resulting in only one level of sampling. However, wind plants may affect relatively large areas, in which case the WRA as a whole is "sampled" for study sites, and each of these sites is then sampled, resulting in two levels of sampling. (In a study of raptor use on the 60,619 acres of the Wyoming WRA, 18 study sites were selected for a second level of sampling.) In addition, present technology does not allow direct measurement of some environmental indicators (e.g., the number of passerine nests by species) on even moderately large areas. Destructive sampling, which permanently changes the sampled point or line (e.g., by removing vegetation), also may be required; and only a small part of the site can be destructively sampled without changing the very nature of the site.

If this second level of sampling (subsampling) is conducted according to an acceptable randomized design, then statistical inferences can be made *to the study sites*. But inferences beyond the study sites to the assessment and reference areas will be *deductive* unless the first level of sampling (study-site selection) also was conducted according to an acceptable randomized procedure.

It should not be surprising that two different studies on the same wind plant may yield conclusions that differ, given that:

1. study sites within the wind plant may be selected using different criteria

2. subsampling protocols and SOPs for measurement (or estimation) of indicators at a site may differ between the two studies.

This again emphasizes the importance of rigorous selection and documentation of sampling protocols and SOPs so that the conclusions drawn from a study can be defended. It also illustrates the importance of having similar protocols for the study of impacts on birds by a new technology (wind turbines) in widely separated areas. However, even if identical areas, designs, and SOPs are used, results of studies based on independent sample units will fluctuate because of natural variation within the area and variation in the application of methods. Resolution of such apparently conflicting results may require intensive investigation of sampling designs, sampling protocols, sample processing, and data analysis by experts in the specific biological areas and experts in study design and statistical analysis.

Sampling Plans

Statistical inferences can be made only with reference to the protocol by which study sites and/or study specimens are selected from the assessment and reference areas. Statistical inferences also are referenced to the protocol used for subsampling (or census) of units from sites and to the SOPs for measurement of impact indicators on subsampled units. Sampling plans can be arranged in four basic categories (Gilbert 1987):

1. haphazard sampling

2. judgment sampling

3. search sampling

4. probability sampling

Sampling plans that are most likely to be used during impact quantification associated with wind plant development are discussed below. (See Gilbert 1987:19-23, Gilbert and Simpson 1992 and Johnson et al. 1989 for other common variations of probability sampling.)

Haphazard Sampling

Gilbert (1987:19) noted that:

> "*Haphazard sampling* embodies the philosophy of 'any sampling location will do.' This attitude encourages taking samples at convenient locations (say near the road) or times, which can lead to biased estimates of means and other population characteristics. Haphazard sampling is appropriate if the target population is completely

homogeneous… This assumption is highly suspect in most environmental studies."

Haphazard sampling has little role to play in providing data for statistical inferences, because results are not repeatable. Information from haphazard sampling may be appropriate for preliminary reconnaissance of an area, but the information can be used only in making deductive arguments based on professional judgment.

Judgment Sampling

"*Judgment sampling* means subjective selection of population units by an individual [the researcher]" Gilbert (1987:19). Gilbert is not much more enthusiastic about judgment sampling than haphazard sampling:

> "If the [researcher] is sufficiently knowledgeable, judgment can result in accurate estimates of population parameters such as means and totals even if all population units cannot be visually assessed. But it is difficult to measure the accuracy of the estimated parameters. Thus, subjective sampling can be accurate, but the degree of accuracy is difficult to quantify" (1987:19).

As in haphazard sampling, judgment sampling may be appropriate for preliminary reconnaissance of an area, but has little role to play in providing data for statistical inferences, because results are not repeatable. Judgment sampling has a role to play in understanding and explaining the magnitude and duration of an impact, but inferences are deductive and depend on professional judgment.

Search Sampling

Search sampling requires historical knowledge or data indicating where the resources of interest exist. For example, a study of factors causing bird fatalities might be limited to the portion of the wind plant where bird use is common. Searching for "hot spots," which is discussed more fully under the "cost cutting procedures" section below, is a form of search sampling. The validity of this procedure depends on the accuracy of the information guiding where and when to search. The procedure also places a great deal of emphasis on the collection of accurate data over time and space to guide the search.

Probability Sampling

Probability sampling refers to the use of a specific method of random sampling of sites from the assessment and reference areas, or sampling of units from assessment and reference sites (Gilbert 1987:20). Randomization is necessary to make probability or confidence statements concerning the magnitude and/or duration of impact (Johnson et al. 1989). Examples of random sampling plans include simple random sampling (random sampling), stratified random sampling (stratified sampling), random start systematic sampling (systematic sampling), and sequential random sampling (sequential sampling).

These sampling plans (and others, especially for mobile animals) can be combined or extended to give an amazing array of possibilities. Johnson et al. (1989: 4-2) recommend: "If other more complicated sample designs are necessary, it is recommended that a statistician be consulted on the best design, and on the appropriate analysis method for that design." For example, after stratification of the impact or reference area, one might use systematic sampling within strata for location of points to mark, release, and recapture mobile animals.

Random sampling. Random sampling requires that the location of each sample site (unit) be selected independently of all other sites (units). Such sampling plans have "nice" mathematical properties, but random locations are usually more clumped and patchy than expected. Entire regions of special interest may be under- or over-represented. Some scientists mistakenly believe that random sampling is always the best procedure. Random sampling should be used in assessment or reference areas (sites) only if the area is very homogeneous with respect to the impact indicators and covariates. Because this is seldom if ever the case, researchers should try to avoid relying solely on random sampling.

Stratified sampling. Stratified sampling is a procedure designed to guarantee that the sampling effort will be spread out over important subregions called strata which are identified in advance. Important strata are identified, and sites within strata are selected for study. Similarly, sites might also be stratified for subsampling. Unless otherwise indicated, it is implicit that locations for sample sites within strata (units within sites) are randomly or systematically located.

Stratification is the division of an area into relatively homogeneous components. Strata may be subareas on a map of the known range of the species of interest. Stratification may also be by reference to some known characteristic of the species of interest (e.g., areas of high and low numerical density) or by some environmental variable (e.g., vegetation type) potentially influencing the species' response to a perturbation.

Strata must not overlap, and all impact/reference areas of interest must be included. Study sites (sampling units) must not belong to more than one stratum. Also, statistical inferences cannot be drawn toward differences in impact indicators for any portion of strata unavailable for sampling. It may be possible to make professional judgments concerning the magnitude and duration of impact on those areas, but conclusions will be made without the aid of inductive statistical results. As an example, in the studies of golden eagles in Altamont (Hunt et al. 1995) some private lands were not accessible for trapping eagles. The resulting relocation data must be analyzed with the knowledge that the radio-tagged sample is not a random sample of the population.

Stratification often will be used in impact studies for quantification of impact within strata and for contrasting the impacts of the incident between strata. For example, it may be of interest to investigate the impacts of a wind plant in different vegetation types (a potential stratification) where the objective is to make statistical inference to each vegetation type within the wind plant. This type of analysis is referred to as using "Strata as domains of study... in which the primary purpose is to make comparisons between different strata..." (Cochran 1977: 140). In this situation, the formulas for analysis and for allocation of sampling effort (Cochran 1977: 140-141), are quite different from formulas appearing in introductory texts such as Scheaffer et al. (1990). The standard objective considered in textbooks is to minimize the variance of summary statistics for all strata combined (e.g., the entire wind plant).

It is usually stated in textbook examples that a primary objective of stratification is improved precision based on optimal allocation of sampling effort into more homogeneous strata. This sounds nice, but there is a problem with this objective. It may be possible to create homogeneous strata with respect to one primary indicator (or a few indicators), but there are often many indicators measured, and it is not likely that the units within strata will be homogeneous for all of them. For example, one could stratify a study area based on vegetation and find that the stratification works well for indicators of impact associated with overstory vegetation. But because of management (e.g., grazing), understory vegetation might be completely different and make the stratification unsatisfactory for indicators of impact measured in the understory. Further, anticipated reduction in variance for the primary indicators may not occur or may be in the range of 5% to 10% and thus not substantially better than random sampling. Systematic sampling with post-classification into domains of interest (subpopulations) in the spirit of the U.S. EPA Environmental Monitoring and Assessment Program (Overton et al. 1991) may perform better than stratified random sampling. (See the discussion on systematic sampling below.)

Factors on which to stratify in quantification of impact associated with wind plant development could include physiography/topography, vegetation, land use, turbine type, etc. Strata should be relatively easy to identify by the methods that will be used to select strata and study sites within strata, and of obvious biological significance for the indicators of impact. Spatial stratification is a major help when study is of relatively short duration and very few sites (units) are misclassified. The reality is that the best laid plans go astray. Some potential study sites will be misclassified in the original classification (e.g., a pond on the aerial photo was actually a parking lot). The short-term study may turn into a long-term study in which interests migrate toward complicated analysis of subpopulations (Cochran 1977: 142-144) which cross strata boundaries, and strata may change (e.g. the corn field has become a grassland). In long-term studies investigators are likely to be happiest with the stratification procedure at the beginning of the study. Benefits of stratification on characteristics such as vegetative cover type, density of prey items, land use, etc. diminish quickly as these phenomena change with time.

A fundamental problem is that strata normally are of unequal sizes and, thus, units from different strata have different weights (importance values) in any overall analysis to be conducted. Consider the relatively complex formulas for computing an overall mean and its standard error based on stratified sampling (Cochran 1977: 87-95). In the analysis of subpopulations (subunits of a study area) which belong to more than one stratum (Cochran 1977: 142-144), formulas are even more complex for basic statistics such as means and totals. The influence of these unequal weights in subpopulations is unknown for many analyses such as ordination or multidimensional scaling. Many analyses of studies ignore these unequal weights and assume the units from different strata are selected with equal probability.

Stratification is often based on maps, but studies usually suffer from problems caused by inaccurate maps or data concerning impact sites, reference sites, and vegetation types at the time study sites are randomly selected. The basic problems are two:

1. misclassified sites have no chance of selection in the field SOPs used by investigations

2. unequal probability of site selection is introduced *within strata*.

It may be necessary to stratify with little prior knowledge of the study area; but if possible, stratification should be limited to geographic stratification with excellent maps, and the minimum number of strata should be used (preferably no more than three or four). Covariates that are potentially correlated with the magnitude and duration of impact should be measured on the study sites (or on subsampling units within sites). Some analyses such as ordination and multidimensional scaling may require additional original mathematical research for justification of their use.

Systematic sampling. Systematic sampling distributes the locations of sites (units) uniformly over the area (site) with a random starting rule. Mathematical properties are not as "nice" as for random sampling, but the statistical precision generally is better (Scheaffer et al. 1990). Systematic sampling has been criticized for two basic reasons. First, the arrangement of points may fall in step with some unknown cyclic pattern in the response of impact indicators. This problem is addressed a great deal in theory, but is seldom a problem in practice. If there are known cyclic patterns in the area, one should use them to one's advantage to design a better systematic sampling plan.

Second, in classical finite sampling theory (Cochran 1977), variation is assessed in terms of how much the result might change if time could be backed up and a different random starting point could be selected for the uniform pattern. For a single uniform grid of sampling points or plots (or a single set of parallel lines) this is impossible, and thus variation cannot be estimated in the classical sense. Various model-based approximations have been proposed for the elusive measure of variation in systematic sampling (Wolter 1984).

Aside from the criticisms, systematic sampling works very well in the following situations:

1. design/data-based analyses conducted as if random sampling had been conducted (effectively ignoring the potential correlation between neighboring locations in the uniform pattern of a systematic sample [Gilbert and Simpson 1992])

2. encounter sampling with unequal probability (Otis et al. 1993, Overton et al. 1991)

3. the model-based analysis commonly known as "spatial statistics," wherein models are proposed to estimate impact using the correlation between neighboring units in the systematic grid (see, for example, Kriging [Johnson et al. 1989: chapter 10]).

The design and analysis in Case (1), above, is often used in evaluation of indicators in relatively small, homogeneous study areas or small study areas where a gradient is expected in measured values of the indicator across the area. Ignoring the potential correlation and continuing the analysis as if it is justified by random sampling can be defended, especially in impact assessment, primarily because from a statistical perspective the analysis is conservative. Estimates of variance treating the systematic sample as a random sample will tend to overestimate the true variance of the systematic sample (Hurlbert 1984, Scheaffer et al. 1990, Thompson 1992). The bottom line is that systematic sampling in relatively small impact assessment study areas following Gilbert and Simpson's (1992) formulas for analysis is a good plan. This applies whether systematic sampling is applied to compare two areas (assessment and reference), the same area before and following the incident, or between strata of a stratified sample.

One of the primary reasons given for preference of stratified sampling (see above) over systematic sampling is that distinct rare units may not be encountered by a uniform grid of points or parallel lines. Hence, scientists perceive the need to stratify, such that all units of each distinct type are joined together into strata and simple random samples are drawn from each stratum. As noted above, stratified random sampling works best if the study is short term, no units are misclassified, and no units change strata during the study. Systematic sampling has been proposed to counter these problems in U.S. EPA's long term Environmental Monitoring and Assessment Program (EMAP), as described by Overton et al. (1991). Unequal probability sampling is almost inescapable, but the problems associated with misclassified units and units that change strata over time can largely be avoided. For long-term impact assessment, monitoring, or when problems with misclassification and changes in land use are anticipated, one should consider systematic sampling strategies similar to those proposed for EMAP (Overton et al. 1991).

As an example, in the case of the Buffalo Ridge Site in Minnesota (Strickland et al. 1996), a potential strategy was to stratify the WRA and reference area by vegetation type. However, the two major vegetation types present on the study area were fallow lands, reserved from tillage under the Conservation Reserve Program (CRP), and lands being actively farmed. These vegetation types are both very influential on bird use. Nevertheless, both vegetation types are likely to change within the next year or so. Thus, stratification on vegetation might be attractive in year 1 of the study, only to be completely inappropriate in year 2 or 3. To overcome the problem, a systematic grid of points with a random starting point was established covering each study area. For a given year, bird use within vegetation types on the assessment and reference areas are compared statistically as if the points were randomly located, even if vegetation types change from year to year.

Cost-Cutting Sampling Procedures

One of the biggest problems with large-scale field studies is that they are very expensive. Estimating the number of birds of a large number of species using an area is a prime example. Some of the standard sampling procedures that may reduce costs of fieldwork are presented below. These techniques should be considered in design of all field studies.

Double sampling and Smith's two-stage sampling procedure. The basic idea of double sampling is that easy-to-measure/economical indicators are measured on a relatively large subset or census of sampling units in the assessment and reference areas. In addition, the expensive/time-consuming indicators are measured on a subset of the sampling units from each area. As always, easily obtainable ancillary data should be collected. Analysis formulas are available in Cochran (1977). The ideas for double sampling are simple to state and the method is easy to implement.

Smith's (1979) two-stage sampling procedure is a variation of the general double sampling method. Basically, Smith's suggestion is to over-sample in an initial survey when knowledge concerning impacts is most limited and record economical easy-to-measure indicators. For example, bird use (an index to abundance sampled according to a probability sample) might be taken during a pilot study, allowing one to identify species most likely affected. In the second stage and with pilot information gained, the more expensive and time-consuming indicators, e.g., the actual number of individuals, might be measured on a subset of the units. If the correlation between the indicators measured on the double-sampled units is sufficiently high, precision of statistical analyses of the expensive/time-consuming indicator is improved.

Ranked set sampling. Ranked set sampling is a technique originally developed in estimation of biomass of vegetation during study of terrestrial vegetation; however, the procedure deserves much broader application (Muttlak and McDonald 1992, Stokes 1986). The technique is best explained by a simple illustration. Assume 60 uniformly spaced sampling units are arranged in a rectangular grid in a WRA. Measure a quick, economical indicator of bird risk (say bird use) on each of the first three units, rank-order the three units according to this indicator and measure an expensive indicator (say bird fatalities) on the highest ranked unit. Continue by measuring bird use on the next three units (numbers 4, 5, and 6), rank order them, and measure fatalities on the second-ranked unit. Finally, rank order units 7, 8, and 9 by bird use and measure fatalities on the lowest ranked unit; then start the process over on the next nine units. After, completion of all 60 units, a "ranked set sample" of 20 units will be available on the fatalities. This sample is not as good as a sample of size 60 for estimating the number of bird fatalities, but should have considerably better precision than a standard sample of size 20.

Ranked set sampling is most advantageous when the quick, economical indicator is highly correlated with the expensive indicator. These relationships need to be confirmed through additional research. Also, the methodology for estimation of standard errors and allocation of sampling effort is not straightforward.

Sequential sampling. In sequential sampling, a statistical test is used to evaluate data after the impact indicator is measured on a subset of units or batch of units selected for sampling (Johnson et al. 1989: chapter 8, Mukhopadhyay et al. 1992). The results of each sequential test determine whether another subset of sampling units or batch of units will be collected and analyzed. The procedure has obvious advantages in certain situations where a large number of samples are collected for laboratory analysis. In field studies, the estimate of certain biases, such as the estimate of scavenger removal of carcasses by monitoring carcasses placed in the field, might benefit from sequential sampling.

Johnson et al. (1989) gives an excellent presentation of the basic formulas for sequential analysis using simple random sampling. However, any variation in the simple random sampling protocol (or simple systematic sampling protocol) results in computational requirements not described in standard textbooks. Unexpected complexities are introduced into statistical procedures, because the "sample size" is a random variable (i.e., one cannot determine in advance the number of sampling units which will be analyzed).

Adaptive sampling. In adaptive sampling the procedure for selecting sites or units to be included in the sample may depend on values of the variable of interest observed during the survey (Thomson and Seber 1996). Adaptive sampling takes advantage of the tendency of plants and animals to aggregate and uses information on these aggregations to direct future sampling. Adaptive sampling could be considered a method for systematically directing search sampling.

As an example of adaptive sampling, say the wind plant is divided into a relatively large number of study units. A survey for bird carcasses is conducted in a simple random sample of the units. Each study unit and all adjacent units are considered a "neighborhood" of units. With the adaptive design additional searches are conducted in those units in the same neighborhood of a unit containing a carcass in the first survey. Additional searches are conducted until no further carcasses are discovered. As with sequential sampling, computational complexities are added because of the uncertainty of the sample size and the unequal probability associated with the selection of units.

Sampling intensity. Usually the largest source of variation in impact indicators is due to natural variation among sampling units across study areas and time, not measurement and subsampling error (e.g., determining the cause of death through blind necropsy). Precision of statistical procedures and power to detect important changes in impact indicators usually will be most influenced by an increase in the number of independent sampling units in the assessment and reference areas. A rule of thumb for improving statistical precision is to increase the number of independent field sampling units. If preliminary or pilot data are available, optimal allocation of financial resources to increase precision in statistical procedures (i.e., stratification) should be considered.

Searching for hot spots. Methods of searching for hot spots (i.e., areas within the assessment area which have high values of the impact indicator) may be valuable under certain conditions — including the evaluation of whether impacts are significant and continuing. Johnson et al. (1989: chapter 9) model a hot spot as a localized elliptical area with values of the impact indicator above a certain standard. If a sampling study does not find hot spots, then confidence is gained in the conclusion that the area is not impacted above the standard or that impacts are not continuing above the standard. Techniques involve systematic sampling from a grid of points arranged in a certain pattern and judgment that there are no hot spots of impact if none of the points yield values above a given standard. This technique will be most applicable in wind plant monitoring studies where regulatory standards for mortality exist, the study is of limited duration, and no reference areas are available.

Johnson et al. (1989: chapter 9) provide an excellent introduction to the technique and give the analyses for two basic approaches. If hot spots are detected then a decision must be made whether it is necessary to fully quantify the impact over the assessment area or just within the hot spots. For wind plants, monitoring for mortality might consider this approach if more extensive sampling suggests hot spots (e.g., end row turbines, turbines near wetlands, etc.). However, if bird use of the wind plant changes, more extensive monitoring may be required to identify hot spots.

DATA ANALYSIS

Univariate Analyses

It is assumed that quantification of impact will be based on measurements for indicators that satisfy the criteria for determination of impact. For these indicators, conducting a series of independent univariate analyses is recommended. For example, the number of dead birds found per square kilometer (km^2) of wind plant surveyed following a year of operation might be estimated and compared to the number of dead individuals found per km^2 on a reference area. During the same year of the same study, the number of fledglings produced per nest might be estimated and compared among the study areas.

It is recommended that impact and recovery of a biological community be defined in terms of individual impact indicators. Examples of impact indicators include the number of individuals of a particular species, biomass of a particular species, and number of species present. Recovery is considered incomplete and an impact exists in the biological community as long as any differences (positive or negative) in indicators can be detected between assessment and reference areas within the particular study design used (Page et al. 1993, Stekoll et al. 1993). It is also recommended that:

- the biological community be characterized in terms of relatively uncorrelated indicators that are impact indicators; and that

- individual tests of direct and more understandable measures of community response be used rather than the multivariate indices mentioned below.

As an example, several comparisons of impact indicators — e.g., the numbers of several species and the biomass of those same species — are made between a wind plant and reference areas. The species selected should be relatively unrelated ecologically (e.g. golden eagles and several species of passerines and shore birds). In the analysis of impact the percentage of biological indicators that are significantly different (positive or negative) when tested at a given level of significance (Page et al. 1993, Stekoll et al. 1993) is used to determine the direction and magnitude of the impact. This use of a relatively large number of individual comparisons is related to the *vote-counting method* of meta-analysis (Hedges and Olkin 1985, Hedges 1986).

In spite of the recommendation above that indicators be uncorrelated, the indicators (e.g. number of individuals of a species) will always be correlated to a certain extent. Thus, the votes used in determining impact (i.e., the *P*-values from the indicators) are not independent. Admittedly the procedure is *ad hoc* if applied only once after the impact, because the expected percentage of significant differences is unknown (under the hypothesis that assessment and reference areas have the same distributions for indicators). However, impact to the community can be inferred if, for example:

- In a BACI design (with data collected before and following the impact) there is an abrupt increase in the percentage of significant differences following the incident (the inference will be more reliable if the abrupt increase is followed by a return to baseline levels, i.e., recovery); or,

- In an Impact-Reference design (with several time periods of data collected following the impact) there is a large percentage of significant differences relative to the size of the test (e.g., $\alpha = 0.05$) immediately following the impact which is followed by a reduction in the percentage (the inference will be more reliable if the percentage decreases to about 5%).

This form of data analysis increases the likelihood of Type I errors and makes the interpretation of results in studies with a large number of impact indicators difficult. The assessment of the statistical significance of differences is also more subjective than with multivariate tests, placing a greater burden on the researcher in evaluating the results. However, univariate tests help interpret results in terms of biological significance. As mentioned above, some correlation among impact indicators usually will exist and univariate analyses will help with the interpretation of the significance of this correlation in the determination of impact. In the univariate analysis the detection of obvious impacts and their cause will be more straightforward and more easily defended when compared to multivariate indices of impact.

Multivariate Analysis

There is a great deal of interest in simultaneous analysis of multiple indicators *(multivariate analysis)* to explain complex relationships among many different kinds of indicators over space and time. This is particularly important in studying the impact of a perturbation on the species composition and community structure of flora and fauna (Page et al. 1993, Stekoll et al. 1993). These multivariate techniques (Gordon 1981, Green 1984, James and McCulloch 1990, Ludwig and Reynolds 1988, Manly 1986, Pielou 1984, Seber 1984) include multidimensional scaling and ordination analysis by methods such *as principal component analysis* and *detrended canonical correspondence analysis* (Page et al. 1993). If sampling units are selected with equal probability by simple random sampling or by systematic sampling from the assessment and reference areas, and no pseudo-experimental design is involved (e.g., no pairing), then the multivariate procedures are applicable.

It is unlikely that multivariate techniques will directly yield impact indicators (i.e., combinations of the original indicators) which meet the criteria for determination of impact. The techniques certainly can help explain and corroborate impact if analyzed properly within the study design. However, data from many recommended study designs are not easily analyzed by those multivariate indices, because, for example:

- in stratified random sampling, units from different strata are selected with unequal weights (unequal probability)

- in matched pair designs, the inherent precision created by the pairing is lost if that pair bond is broken.

Meta-Analysis

Meta-analysis is a relatively new approach as applied to the analysis of ecological field studies. It involves the combination of statistical results from several independent studies that all deal with the same issue (Hedges and Olkin 1985, Hedges 1986). While many biologists and statisticians are unfamiliar with its application, meta-analysis has been well known and widely used in some fields (e.g. psychology, medical research) for quite some time. It may be extremely important for use of historical and baseline data in impact assessment. The simplest form of meta-analysis is easy to understand. If several independent statistical comparisons are made on the same impact indicator but with relatively low sampling intensity, then it is possible that none are significant at the traditional level of $P \leq 0.05$. However, all or most significance levels may be "small" (e.g., all Ps are ≤ 0.15) and suggestive of the same type of impact. The probability that, for example, three or more independent tests would, by chance, indicate the same adverse impact if there were no actual impact from the perturbation, is itself an unlikely event. The combined results may establish impact due to the incident with overall significance level $P \leq 0.05$.

For a second illustration, historic scientific studies in a given assessment area may have addressed the same basic objective, but were conducted by different protocols with varying degrees of precision. It is difficult to combine original data from such studies, but it may be possible to combine results of statistical tests using meta-analysis to establish a reliable measure of baseline conditions.

For a third illustration of potential use of meta-analysis, consider stratified random sampling, where sampling intensity within a given stratum (e.g., vegetation type) is not sufficient to reject the classical null hypothesis of "no impact." If the point estimates of effect are in the same direction and indicate impact, then the statistical results might be combined across strata (e.g., vegetation type) by meta-analysis to establish the overall conclusion of impact at an acceptable level of precision.

Discussion of all aspects of the emerging field of meta-analysis is beyond the scope of this document (see, for example, Burnham 1995, Draper et al. 1992, Durlak and Lipsey 1991, Hedges and Olkin 1985, Hedges 1986, and Hunter and Schmidt 1990). Meta-analysis should be considered if several historic or baseline studies have been conducted. It may also be of value if several independent studies point in the same direction of impact, but individually lack the usual scientific requirements for statistical inferences that the impacts are "real."

Habitat Selection

Habitat selection by birds may be of interest in evaluating potential risk associated with existing or new wind plants. Manly et al. (1993) provide a unified statistical theory for the analysis of selection studies, and a thorough review of this resource is recommended for anyone considering this type of study. In resource selection studies, the availability of a resource is the quantity accessible to the animal (or population of animals) and the use of a resource is that quantity utilized during the time period of interest (Manly et al. 1993). When use of a resource is disproportionate to availability then the use is selective (i.e. the animal is showing a preference for the resource).

Scientists often identify resources used by animals (e.g. vegetation type, food, etc.) and document their availability (usually expressed as abundance or presence/absence). These studies are usually carried out to identify the long-term requirements for the management or conservation of an animal population. The differential selection of resources provides information about the ecology of birds that should also improve the assessment of risk posed by potential wind plants. Resource selection could also be used in Level 2 model-based analyses of such things as the difference in mortality associated with turbine design. Using most of the designs previously discussed, resource selection models can be used to evaluate mortality and other metrics indicating risk to birds as a function of distance to various turbine types.

In most Level 1 studies it will be impossible to identify unique animals. However, using observations of animals seen from randomly or systematically chosen points it would be possible to use resource variables with known availability (e.g., vegetation) as predictor variables. For example, if it appears that a certain vegetation type is preferentially selected for hunting by red-tailed hawks within 0.5 km of a nest, then one could predict that the risk of impact would increase if turbines were constructed on preferred hunting habitat <0.5 km from a nest. Alternatively, the study area could be classified into available units characterized on the basis of a set of predictor variables such as vegetation type, distance to water, distance to a nest, and distance to a turbine. The presence or absence of use of a sample of units could then be used to assess the effect of the predictor variables on bird use. In the case where study plots are searched for the presence or absence of dead birds, resource selection could be used to evaluate the effect of a set of predictor variables on mortality.

Cumulative Effects Analysis

Cumulative effects are a hot and difficult topic in the evaluation of environmental impacts. Neufeldt and Guralnik (1988) define cumulative as "increasing in effect, size, quantity, etc. by successive additions." As is often the case, a relatively simple term takes on a very complicated meaning when applied to natural resources and their response to perturbations. To complicate matters, the term is defined differently by federal law such as the National Environmental Policy Act and its implementing regulations. Suter et al. (1993) classifies cumulative effects into the following categories:

- **Nibbling** - the cumulative effects of a number of actions which have similar small incremental effects. For example, the additions of individual turbines to a wind plant.

- **Time-Crowded Perturbations** - the cumulative effects that occur when actions are so close in time that the system has not recovered from the effects of one before the next one occurs. For example, if impacts from wind turbines are influenced by birds' experience with the structures, one could anticipate some learned response to the turbines over time, possibly reducing risk. One could hypothesize that rapid development of a wind plant might have a greater impact on birds than phased development of the same facility.

- **Space-Crowded Perturbations** - the cumulative effects that occur where actions are so close in space that the areas within which they can induce effects overlap. For example, bird risk may be influenced by turbine and turbine string spacing.

- **Indirect Effects** - The cumulative effects that occur when the direct effects of actions are not space- or time-crowded, but their indirect effects are. For example, the change in land use resulting from a wind plant may not affect bird use or cause increased mortality but may affect habitat quality, either positively or negatively.

Cumulative effects analysis involves the study of the interaction of wind plant structures, other land uses, and the ecology of birds. Effects of wind plants on birds may be *additive*, increasing mortality beyond what might occur without the plant; or effects may be *compensatory*, simply replacing other sources of mortality. Effects of wind plants may be *synergistic*; that is, a wind plant in combination with another land use may result in an increased rate of bird mortality greater than the sum of increased mortalities which might occur due to each individual development. Or, effects may be *antagonistic*, in which case association with some other variable would reduce impacts from the wind plant. Finally, impacts of a wind plant may increase to a limit or threshold of effect. As with testing hypotheses of first order direct effects, the key to a successful analysis is the protocol by which the data are collected.

For example, if one wishes to evaluate the cumulative effects of individual turbines then the protocol should be designed appropriately. Study designs should have individual turbines as the basic sampling units and impact indicators (say mortality) should be attributable to individual turbines. Covariates should relate to the basic sampling unit (e.g. end-row turbine versus within-row turbine). It should be obvious that cumulative effects analysis requires much more extensive study than simply measuring impact indicators associated with single turbines or even entire wind plants. Cumulative effects analysis associated with population effects (Level 2 Studies) would be even more complicated and expensive.

Statistical Power and the Weight of Evidence

Traditionally in scientific research, a null hypothesis — that there is no difference in the value of an indicator between reference areas and assessment areas or that there is a zero correlation between two indicators along their gradients — is adopted as the "straw man" that must be rejected in order to infer that an indicator has changed or that a cause-and-effect relationship exists. Although this approach has pervaded the scientific method and discipline of statistics for nearly a century, it usually places the burden of scientific proof of impact on regulators. The classical use of a null hypothesis protects only against the probability of a Type I Error (concluding that impact exists when it really does not, i.e., a false positive). Often the significance level is required to be below $\alpha = 0.05$ before the conclusion of impact is considered to be valid. The probability of a Type II Error (concluding no impact when in fact impact does exist, i.e., a false negative) is commonly ignored and is often much larger than 0.05. The risk of a Type II error can be decreased by conducting larger, more expensive studies or, in some situations, through use of better experimental design and/or more powerful types of analysis. In general, the power of a statistical test of some hypothesis is the probability that it rejects the null hypothesis when it is false. An experiment is said to be very powerful if the probability of a Type II Error is very small.

The traditional statistical paradigm is geared to protect against a "false positive," but the interest of the regulator is protection against a "false negative." A more fair statistical method is needed to balance protection against the two possible errors. The standard paradigm is clumsy at best and is not easily understood by many segments of society. For a discussion of an alternative paradigm see McDonald (1995), McDonald and Erickson (1994), and Erickson and McDonald (1995).

Scientists often are concerned with the statistical power of an experiment, that is, the probability of rejecting a null hypothesis when it is false. In the case of wind plant monitoring, the null hypothesis will usually be that there is no impact to birds. Accepting a "no impact" result when an experiment has low statistical power may give regulators and the public a false sense of security. The power of the test to detect an effect is a function of the sample size, the chosen α value, estimates of variance, and the magnitude of the effect. The α level of the experiment is usually set by convention, if not by regulation, and the magnitude of the effect in an observational study is certainly not controllable. Thus, sample size and estimates of variance usually determine the power of observational studies. Many of the methods discussed in this chapter are directed toward reducing variance in observational studies. When observational studies are designed properly, the ultimate determination of statistical power is sample size.

The lack of sufficient sample size necessary to have reasonable power to detect differences between treatment and reference areas is a common problem in field studies described in this chapter. Estimates of direct mortality can be made in a given year through carcass searches, but tests of other parameters for any given year (e.g., avoidance of wind plant by bird species) may have relatively little power to detect an effect of wind power on the species of concern. The lack of power is a concern and should be addressed by increasing sample size, through the use of other methods of efficient study design described above, and by minimizing measurement error (e.g., the use of the proper study methods, properly trained personnel, etc.). However, most field studies will result in data that must be analyzed with an emphasis on detection of biological significance when statistical significance is marginal. For a more complete study of statistical power see Cohen (1973), Dallal (1992), Fairweather (1991), and Peterman (1990). Computer-intensive methods allow estimates of variance and standard error when complicated designs make standard estimates of variance problematic (Manly 1991). Such methods can be useful in calculating confidence intervals and in tests of hypotheses using data with non-standard distributions. Computer-intensive methods also can be used with pilot data to predict necessary sample sizes to meet objectives for precision.

The trend of differences between reference and impact areas for several important variables may detect impacts, even when tests of statistical significance on individual variables have marginal confidence. This deductive, model-based approach is illustrated by the following discussion. The evaluation of effects from wind energy development includes effects on individual birds (e.g., reduction or increase in use of the area occupied by the turbines) and population effects such as mortality (e.g., death due to collision with a turbine). Several outcomes are possible from the bird studies. For example, a decline in bird use on a new wind plant without a similar decline on the reference area(s) may be interpreted as evidence of an effect of wind energy development on individual birds. The presence of a greater number of carcasses of the same species near turbines than in the reference plots increases the weight of evidence that an effect can be attributed to the wind plant. However, a decline in use of both the reference and development area (i.e., an area with wind turbines) in the absence of large numbers of carcasses may be interpreted as a response unrelated to the wind plant. Data on covariates (e.g., prey) for the assessment and reference area(s) could be used to further clarify this interpretation.

The level at which fatalities are considered significant is subjective and will depend on the species involved. Even a small number of carcasses of a rare species associated with turbine strings may be considered significant, particularly during the breeding season. A substantial number of carcasses associated with a decline in use relative to the reference area, particularly late in the breeding season during the dispersal of young, may be interpreted as a possible population effect. The suggestion of a population effect may lead to Level 2 studies.

CASE STUDIES

Proposed Plant - Buffalo Ridge, Minnesota

The following describes a monitoring protocol for evaluating the cumulative effects on passerines and shore birds of proposed wind energy development in the Buffalo Ridge area of southwestern Minnesota (Strickland et al. 1996). The initial implementation of the protocol monitors the effects of the existing 25 MW Phase I development and the proposed 100 MW Phase II wind plant. Phase I, constructed by Kenetech Wind power, Inc., consists of 73 Model 33 M-VS turbines and related facilities. Phase I is located in the approximate center of the wind resource area (WRA).

Phase II consists of 143 turbines constructed in 1997 and 1998 by Enron Wind Corporation. Phase II is located in the northwestern portion of the WRA. Facilities capable of generating an additional 100 MW are planned for the WRA by early 1999. Plans call for additional phases of development and the eventual production of 425 MW of electricity within the WRA. The results of the first year's study are presented in Johnson et al. (1997).

The primary goals of monitoring are to evaluate the risk to bird species from each phase of development and the cumulative risk to bird species from all wind power development in the WRA. The secondary goal of monitoring is to provide information which can be used to reduce the risk to bird species from subsequent developments. While the elements of the plan are somewhat technical, it was developed in cooperation with and received review from individuals and groups with an interest in the wind plant and its potential effect on birds (stakeholders). The plan also was peer-reviewed before implementation.

The sampling design is a combination of the impact-reference area and the BACI design. Bird use and mortality are measured on plots located at varying distances from turbines following the sampling Protocol A proposed by Manly et al. (1993). The sampling plan allows for mortality estimation for the entire wind plant and reference areas and an estimation of use standardized by unit area and unit effort.

The BACI design combines collection of data before and after Phase II and III of wind power development with collection of data from multiple reference areas. The impact-reference design involves collection of data from the existing development and multiple control areas. Reference areas are as similar as possible to the wind plant development areas, both physically and biologically. Four areas are studied: the existing wind plant (Phase I) (denoted EW), the northwest development area (Phase II) (denoted NW), the southeast development area (Phase III) (denoted SE), and a permanent reference area (denoted REF). The southeast site serves as a reference area prior to its development; data collected for the site also provide pre-construction data for its future development. Future development sites will be identified as soon as possible and pre-construction data collection will begin at these sites. As with the SE site, new sites monitored prior to development will act as references to developed sites. By sampling both the reference and the impact areas before and after wind power development, both temporal and spatial controls are utilized, optimizing the impact design (Green 1979). Adding future sites within the Buffalo Ridge area into the monitoring effort will address cumulative impacts of development.

Relative use by bird species is measured through point count surveys conducted during daylight hours. Two types of point count surveys are conducted. The first method involves making point counts of birds for a short duration (five minutes) at a large number of relatively small plots across the study areas. Passerines, shore birds, and other smaller birds (PSB) are the targets of these surveys, since they have smaller home ranges and cannot be detected at large distances. The second method involves making point counts of birds for a longer duration (30-60 minutes) from a systematically selected subset of the points with large viewing areas in each study area. Raptors and other larger birds (RLB) are the target of these surveys, since they can be detected from larger distances and appear to be fairly rare within the study area. In the PSB surveys, raptors and other larger birds are recorded when detected, but observers concentrate on a much smaller viewing area to minimize missing small birds. In the RLB surveys, observers concentrate on detecting all raptors and other large birds, but also record unusual or rare observations of small birds. In both surveys, distance to the observation is recorded so data can be standardized by both area and effort (time). Mortality is measured through carcass searches at turbines and within a systematically located subset of reference plots where use is estimated.

Data Analysis

The project is in its second year. The following analyses are planned at the end of each year. Species lists are generated by study period and study unit (EW, NW, SE, and REF). The number of birds seen during each point count survey is standardized to a unit area and unit time surveyed (observed density). For example, if four horned larks are seen during a five-minute interval at a station with a standardized viewing area of 0.031 km^2, these data are standardized to 4/0.031=~129 horned larks/km^2. Relative density corrected for visibility bias (Buckland et al. 1993) is estimated by species, if data are sufficient, using the program DISTANCE (Laake et al. 1993).

Data are tabulated and plotted to illustrate differences in bird use between: seasons, times of day, stations, turbine sites and non-turbine sites, vegetation type, flight height, and study areas. As data are accumulated over time, ANOVA techniques will be used to test for differences in bird use and possible interactions between: seasons, times of day, study areas, turbine sites and non-turbine sites, and pre- versus post-development. Non-parametric methods such as randomization tests (Manly 1991) will be used when assumptions for the parametric methods cannot be met (e.g., equality of variances). Statistical analysis of bird use and flight height as a function of distance from turbines, vegetation type, and other physical characteristics will be conducted using resource selection techniques (Manly et al. 1993, McDonald et al. 1995, Pereira and Itami 1991). Other standard statistical procedures will be used as appropriate. For important tests of hypotheses, the power (i.e., probability of rejecting the hypothesis of no difference in means) is calculated for various effect sizes based on baseline studies and initial data collected during monitoring, as soon as enough data exists for estimates of variance.

Resource/Habitat Selection Analyses

Statistical tools used in habitat selection studies are applied to the bird use data for investigation of habitat selection as well as the effects of the turbines on the avian resource. Data collected prior to development of the wind plant (Phase II and Phase III) can be used to determine what important factors appear to be related to presence/absence of a bird species or the magnitude of use by bird species. For example, through multiple regression techniques it may be shown that use by a species of bird is related to the amount (percentage of area) of land protected under the Conservation Reserve Program (CRP) within the vicinity of the point count or to distance from the nearest wetland. Using presence/absence data at the point count location, logistic regression (Hosmer and Lemeshow 1989) can be used to estimate the relative probability that an area will be used as a function of the characteristics of the area. For example, it may be shown that distance to the nearest wetland is related to the probability of use for a species, and that areas at (for example) 300 meters are twice as likely to have bird use by this species as areas at 500 meters. These functions may be useful in developing a data layer in a GIS system indicating those regions within a development which have the highest probability of use by the given species. This information may be useful in siting turbines in future phases.

Using bird use data collected within areas with turbines (existing wind plant and northwest site after construction), these same resource selection techniques can be used to evaluate effects of the turbines on use by bird species. For example, logistic regression models may show that a bird species has a higher probability of using an area that is far from turbines (i.e., possible avoidance of turbines). Multiple regression models may be used to determine if distance to turbines is negatively related to the magnitude of bird use.

Data collected at the point (bird use, presence/absence, habitat) are used in the logistic and multiple regression analyses. Because repeated correlated measures are made of these variables at the point, bootstrapping techniques (Manly 1991, Ward et al. 1996) will be used to estimate the precision and confidence in the coefficients of the regression analyses and to avoid pseudoreplication.

Analysis of Flight Height

Flight height categories will be recorded for every bird observed corresponding to flights below, within, and above the rotor swept area. More than one category may be recorded for each bird observed. For example, the proportion of individual birds of a given species observed to be within the rotor swept area at least once during the observation period may be analyzed. Also, the proportion of observations by species/groups within each flight height category (i.e., below, within, or above rotor swept area) can be calculated and plotted. In a similar fashion to the habitat selection and regression methods described previously, logistic and ordinary regression can be used to predict the probability a bird will use a location as a function of the characteristics of the location. In this analysis, observations used in the analysis are individual birds with potentially correlated observations within study plots, so bootstrapping techniques will again be used to incorporate point to point and survey to survey variability and to avoid pseudoreplication in reporting precision of statistical results.

This type of analysis can be used to predict the probability a bird (by species) will fly within areas of risk associated with a wind plant, say the rotor swept area of turbines, based on the characteristics of the area surrounding turbines. Use within an area of risk is considered an index of risk of exposure. True risk must consider other factors such as species ability to avoid collisions while flying close to turbines and other wind plant features.

Summary

This protocol embodies many of the sampling and analysis principles described throughout this chapter. While the overall design philosophy is a BACI design, the control/impact design is required to evaluate the effects of Phase I because of the lack of preconstruction data. In addition, there will be only two years of pre-construction data for Phase II, reducing the value of the BACI design for evaluating the effects of this phase. It is obvious that the assessment of impacts will require long-term monitoring with the strongest analysis of effect occurring after the development of Phase III. This delays the strongest detection of effect until much of the wind plant is constructed. A stronger study design would have been possible if the BACI design had been implemented before construction of Phase I. The protocol also does not include nocturnal use studies. However, the carcass searches should detect the death of nocturnal and migrant species.

Although multiple reference areas are used, the use of one reference area that is likely to be developed in the near future is less desirable than having both reference areas outside the WRA. The protocol illustrates the necessity of balancing the desire for a strong study design with budgetary constraints. Finally, the species most likely to be affected by the wind plant may be the red-tailed hawk, the only raptor species breeding in significant numbers in the area, or it may be a water bird. If Level 1 studies verify that a single or small group of species is at greatest risk, Level 2 studies may be required to quantify population effects.

Many of the weaknesses in this study relate to timing and budgetary constraints. As a general rule, the earlier research begins in the development of a wind plant the more likely the research will provide data important to decisions related to the future of the development.

Existing Plant - Tehachapi and San Gorgonio Pass, California

A number of studies have been conducted on existing plants in the United States and Europe. Colson (1995), PNAWPPM (1995), and Orloff and Flannery (1996) have summarized the results of these studies. Most of these studies were observational, involving a variety of protocols, and were conducted at what we have defined as Level 1 and Level 2. The most recent attempt at preparing a detailed protocol for the conduct of bird studies at an existing wind plant is described by Anderson et al. (1996a) and by modifications to this protocol (Anderson, personal communication). The following is a description of their protocol and modifications. Like the Minnesota protocol, the Anderson protocol was developed in cooperation with and was reviewed by individuals and groups with an interest in wind power and its potential effect on birds (stakeholders). The protocol was also peer-reviewed before implementation.

The protocol is designed to determine if an operating WRA results in an increased risk of bird mortality. The protocol was implemented at wind plants in southern California in the Tehachapi Pass WRA (Pilot Study). A modification of this protocol is being implemented in studies at Tehachapi and the San Gorgonio Pass WRA in southern California (Phase II).

Pilot Study

The purpose of the pilot study was measuring bird use and mortality in wind plants and in surrounding undeveloped areas and for developing several indicators of bird risk. The protocol utilizes standard point count methodology to determine the relative abundance and utilization rates of all bird species using study areas within the WRAs. The protocol also includes estimating mortality at each point where use is estimated. The protocol proposes the use of the ratio of mortality to use, as a function of distance from turbines, to estimate the amount of risk of bird fatality attributable to the development of wind energy. The protocol also proposes the use of this ratio, in combination with the rotor size and hours of operation, to make comparisons of risk of bird fatality among different sizes of turbines.

Results from the first year of study on the Tehachapi Pass WRA are contained in Anderson et al. (1996b). The WRA is heavily developed with numerous wind energy companies operating wind plants with a variety of turbine types. Initial studies have focused on four companies making up approximately 80 percent of the development within the WRA. The WRA is divided (stratified) into three primary study areas corresponding to three distinct geographical locations with operating wind plants.

1. *The West Ridge*, owned and/or operated by Enron, is at the highest elevation and is dominated by grasslands and Sierra Nevada foothills vegetation (scattered woodlands, subshrubs, and oak chaparral).

2. *The Middle Area*, owned and/or operated by Cannon and FloWind Corporation, contains a mix of grasslands, mountain foothills, and small patches of Joshua trees and desert scrub-shrub.

3. *The East Slope*, operated by SeaWest, Tehachapi, Inc., is dominated by vegetation of the Mojave Desert (junipers, Joshua trees, and creosote bush).

In spite of the differences in vegetation, all three areas are structurally similar and are dominated by grassland with patches of shrubs and sub-shrubs.

Each of three primary study areas is defined as a developed subregion of the WRA and a buffer is designated as a non-developed comparison area. The study areas are further stratified by natural plant communities. Bird use is determined by recording observations of birds at points established along transects within the study area using the point count method. Random starting points are selected within the study area with the starting points stratified by natural communities. From these starting points, random angles are used to determine each transect direction. Points for making bird counts are established every 300 meters along each transect where 5- and 10-minute counts are made of any bird seen, regardless of distance from the point. Transects may cross plant community boundaries. Unlike standard point counts, birdcalls are not used in detection of birds in an effort to avoid the potential bias against detecting birdcalls within the wind plant where background noise is stronger.

Point counts are made throughout daylight hours and throughout each of four seasons. A minimum of 250 points are sampled each season and a minimum of 1,000 points are sampled each year. All birds seen from a point are recorded and the horizontal distance to the bird and its height above ground are estimated. Bird activities are recorded, including flying, perching, soaring, hunting, foraging, and proximity to wind plant structures.

A circular area within a 50-meter radius of each point is thoroughly searched for dead birds or bird parts. Dead birds or bird parts of any age are counted and specimens are left in the field, potentially to be counted again if they fall within a subsequent carcass search area. Additional data (covariates) are collected at the site including estimated time since death, cause of death, type of injury, distance to nearest turbine and distance to nearest structure.

Transect length varies depending on a variety of factors (e.g., change in land ownership affecting access, fatigue, etc.). Bird utilization counts and carcass searches are made during standardized hours of the day. The proposed duration of the study is one to two years. Anderson et al. (1996a) indicate that future phases of this study and potentially other studies will be expanded to include the San Gorgonio Pass WRA in southwestern California.

Data Analysis

With the exception of some details of the measurement methodology, the measure of the above metrics, the analysis of the data, and the testing of hypotheses is similar to the Buffalo Ridge protocol. What sets this protocol apart from Buffalo Ridge is the use of the area surrounding active portions of the wind plant as a control area. The design assumes that the surrounding area is an adequate control area (impact-control design with no pre-construction data). It is also an example of the protocol for contaminant-gradient designs discussed earlier.

Measuring use and mortality at the same points for the WRA and a control is a strong point of this protocol. Providing an index to mortality as a function of use for a treatment and reference area is essential to the interpretation of the index. Attributable risk is also an important issue. In the absence of an estimate of attributable risk, all mortalities found within the WRA will be attributed to the wind plant. Plotless methods allow recording of all birds seen (i.e., observers record what they see without regard for distance), not just those within a fixed-size sample plot. However, since utilization rate may be most appropriate when considered as use per-unit-area per-unit-time, the data can be trimmed to fit within a fixed area if necessary by measuring the distance to each bird observed.

The protocol results in data that will allow the development of several indices of risk including bird utilization rate and bird mortality. The protocol also calculates: *bird risk*, defined as the ratio of bird mortality to bird utilization rate; *attributable risk*, defined as the difference between background mortality and mortality caused by the wind plant; and *rotor swept hour*, defined as the product of area swept by a turbine rotor and the hours of turbine operation. Rotor swept hour is used to estimate a *rotor swept hour risk*, defined as one (1) divided by the product of rotor swept hour and bird risk. This value is proposed as a method for comparing risk associated with different rotor swept areas of turbine sizes in relation to the time they operate when data on bird mortality and use exist.

Following this protocol a determination of the effect on bird risk of operation of a wind plants requires one of two assumptions:

1. that the surrounding area is an adequate control area (impact-control design with no pre-construction data); *or*

2. that relative risk is a function of distance from the wind plant and wind plant features, which also assumes that the surrounding area is an adequate control.

The empirical use and mortality data and indices can be analyzed using the same basic methods as proposed for the Minnesota project for comparing two or more wind plants. In the Anderson protocol, the comparisons are between the buffer (reference) area and the wind plant. The random angle introduces unequal and unknown probability of selection of sample points within the wind plants and buffer and may introduce some bias. However, if the protocol is followed from area to area and year to year, an index can be developed which will allow the desired comparisons. As with all indices, the resulting impact indicator is totally dependent on the protocol. If technology changes or methods change then the index will change in some unknown way. Absolute estimates of parameters, such as dead birds per unit area, cannot be computed because of the unequal and unknown probability of sampling. The gradient-response allows the use of logistic and ordinary regression models to evaluate indices of risk as a function of the distance to turbines by type and different portions of the wind plant.

Phase I Protocol

The Phase I studies are currently being conducted in the Tehachapi and San Gorgonio WRAs. The following discussion focuses on Tehachapi, Phase I. The objective of Phase I studies is to use the indicators of bird risk developed in the pilot study to evaluate differences in risk among six turbine types. Measurement methods are the same as used in pilot study. However the sampling plan is substantially different.

The Phase I sampling plan follows the stratified random sampling procedure. The WRA is again stratified into the three regions described for the pilot study. As described earlier, each region can also be considered a subdivision of the WRA by company (see above). Each region is further divided into three approximately equal sized subregions, along an approximately north-south orientation. For the purpose of this discussion the subregions will be termed the north, middle, and south subregions. All turbines within each region and subregion are classified into one of the following types:

- large turbine (rotor diameter > 26m) with a lattice support structure

- small turbine (rotor diameter < 22m) with a lattice support structure

- large turbine with a tubular support structure

- small turbine with a tubular support structure

- small turbine with a lattice support structure deployed as a “windwall”

- standard vertical axis turbine with two blades.

A total of 160 plots, each centered on a turbine, are selected for study. (Note that each plot includes multiple turbines.) Each plot is given a unique number and a random sample is drawn from each subregion in proportion to availability. The subregions are used to insure that the sample of turbines is well distributed (as much as possible given their patchy distribution) across the region.

Bird utilization is estimated for each plot four times each quarter-year for Phase I studies, and fatalities are estimated for each plot once each quarter-year using the methods described. Plots are visited in random order for utilization surveys until all turbines are visited and then the process is repeated for four visits. Plots are visited in random order for carcass searches and are continued until all turbines are visited. Utilization surveys are conducted during morning hours and carcass searches are conducted during the remainder of the day. Carcass searches take approximately four times as long as utilization surveys, explaining the increased number of utilization surveys during the three-month survey period.

Data Analysis

Phase I studies had not evolved to the point of data analysis at the time this chapter was written and Anderson et al. (1996b) does not include a proposed analysis. Two possible analyses of the Phase I study are outlined below. These two possibilities may not be the most desirable and are certainly not offered as a substitute for the analysis that may be used when the project is completed.

In most scientific studies, there exist several competing analyses. Which analysis to use in any one study depends in part on study objectives, responses of interest, and available covariate information. In general, it is best to analyze available data in a manner consistent with the way they were collected. For example, if "treatments" are randomly applied to wind turbines or wind resource areas, the data should probably be analyzed using accepted ANOVA techniques (Neter et al. 1985). If the experimental design of such a study is appropriate, ANOVA will provide a powerful, defensible, and easily replicated analysis.

Analysis 1: analysis of variance (incomplete block). The first analysis views the data as coming from a "blocked" experiment where there are six treatments, three manufacturer blocks, and three geographic blocks. The six treatments are: 1) large-lattice, 2) small-lattice, 3) large-tubular, 4) small-tubular, 5) windwall, and, 6) vertical axis. The three manufacturer blocks are: 1) Enron, 2) Cannon and FloWind, and, 3) SeaWest. The three geographic blocks are: 1) North, 2) Middle, and 3) South.

In order to analyze all data in one large analysis (i.e., using all the structure outlined above), there need to be certain combinations of treatments (turbine types/sizes) present in each combination of geographic block and manufacturer block. That is, if all treatments appear in all combinations of geographic block and manufacturer block (i.e., all six treatments appear in the Enron-North block, all six treatments appear in the Enron-Middle block, all six in Enron-South, etc.) then the proper combinations exist for one large analysis.

The Tehachapi Pass study was not a designed experiment and study managers did not have complete control over which turbine types and sizes appeared in blocks. For example, it may be unreasonable to expect SeaWest to remove some tubular turbines from their management area and construct Enron lattice types in their place. This lack of design control is a big issue for the analysis of Phase I data. Without the above conditions met, we must search for smaller pieces of the large analysis to analyze separately.

One potential analysis might be to combine treatments 1, 2, 3, and 4 and ignore the SeaWest block to produce an analysis of the "treatments": rotor size versus windwall versus vertical axis. This example would reduce to an incomplete block design with three treatments, two blocks, and subsampling of the rotor size treatment in both blocks.

Alternatively, one could analyze each manufacturer block separately by viewing each manufacturer's wind resource area as a separate experiment. For example, if we focus only on Enron's block, it may be that enough combinations of turbine type/size occur within each geographic block (North, Middle, and South) to achieve design requirements and allow an estimate of error. If so, the analysis would be an incomplete block design with three blocks (the geographic blocks) and three (or possibly two) treatments (large-lattice, small-lattice, and windwall). A similar approach can be taken in the other manufacturer blocks.

Analysis 2: regression (analysis of covariance). This analysis is both an alternative and a complement to the ANOVA technique outlined above. It should be noted that regression (Neter et al. 1985) and ANOVA are not disjoint analysis techniques, but rather are the same analysis technique applied to different types of covariates (or explanatory variables).

There may be many reasons to consider regression methods, two of which are described below, followed by examples.

1. When the design does not lend itself to a strict ANOVA approach, it is possible that a regression approach, which considers other (and often continuous) covariates, will yield an analysis which is essentially the same as ANOVA.

2. If the blocking structure of the study does not do an adequate job of controlling excess variation, it is possible that a regression approach will.

Suppose that even by focusing on the Cannon/FloWind block, the regular ANOVA approach does not yield an analysis because there are not enough turbines of each type in each of the north, middle, and south geographic blocks. A regression approach would allow one to disregard the geographic blocks and instead record the latitude of each turbine under study. In this approach, the latitude of each turbine becomes a surrogate for the geographic blocking and would potentially control for the same extraneous factors as the geographic block would have had they produced an analysis. Furthermore, latitude represents a continuous covariate. To test for differences between turbine types, researchers must be willing to assume that there is no latitude by turbine type interaction, or at least assume some simple form for the interaction.

As a second example, suppose that prevailing wind speed affects bird mortality at all types of turbines (suppose higher wind means higher mortality irrespective of turbine type). Furthermore, assume that the existing blocking factors (manufacturer and geographic) do not adequately control for wind speed because the blocks are too big to accurately represent wind speed in a local area around each turbine. A regression approach would solve this problem by estimating average wind speed at each turbine and by using average wind speed as a covariate instead of the blocking factors.

SUMMARY AND CONCLUSION

Protocols for bird studies will, by necessity, be site- and species-specific. However, all protocols should follow good scientific methods. Many of the issues related to avian impacts of wind power are contentious, and settling these issues will be assisted by good scientific studies. However, many of the issues related to wind power impacts on birds are based on relatively rare events. First, producing electricity commercially with wind power is a relatively recent development. With the possible exception of fatalities at the Altamont WRA, bird fatalities in existing wind plants are a relatively infrequently documented event. Many of the bird species of major concern are also rare. Second, as pointed out in this chapter, the construction of a wind plant is not a random occurrence and potential wind plant sites are relatively unique, making selection of reference areas difficult. In spite of these difficulties, bird mortality is a significant concern and wind power is a potential clean source of electricity, making study of these issues essential.

Because impact indicators normally are estimates of relatively rare events, analysis of impacts must rely on an accumulation of information. A determination of impact will seldom be based on clear-cut statistical tests, but usually will be based on the weight of evidence developed from the study of numerous impact indicators, over numerous years, at numerous wind plants. The selection of the appropriate protocol must be site- and species-specific. Protocol selection will be influenced by the status of the wind power project (existing or proposed), the area of interest, the issues and species of concern, cooperation of landowners, and so on. Decisions about methods, designs, and sample sizes will always be influenced by budget considerations.

The following is a summary of important considerations when designing Level I observational studies:

1. Clearly define the objectives of the study including the questions to be answered, as well as the area, the species, and the time period of interest.

2. Clearly define the area of inference, the experimental unit (and sample size), and the sampling unit (and subsample size).

3. Clearly define the parameters to measure, select impact indicators which are relatively uncorrelated to each other, measure as many relevant covariates as possible, and identify obvious biases. Impact indicators should allow for the determination of impact following generally accepted scientific principles and as defined by the standards agreed to by stakeholders.

4. The BACI design is the most reliable design for sustaining confidence in scientific conclusions. Data should be collected for two or more time periods before and again two or more time periods after construction of the wind plant on both the assessment area (wind plant) and multiple reference areas. Consider matching pairs of sampling units (data collection sites) within each study area based on matching criteria which are relatively permanent features (e.g. topography, geology). If the BACI design can not be implemented then other appropriate designs should be used.

5. Use a probability sampling plan, stratify on relatively permanent features, such as topography,

and only for short-term studies; use a systematic sampling plan for long-term studies, spread sampling effort throughout area and time periods of interest, and maximize sample size.

6. Develop detailed SOPs prior to the initiation of fieldwork and select methods that minimize bias.

7. Make maximum use of existing data and consider some preliminary data collection where little information exists.

8. When pre-construction data are unavailable then combine data collection on multiple reference areas with other study designs such as the gradient-response design.

9. Maximize sample size within budgetary constraints.

10. Univariate analysis is preferred, especially when determining impacts by a weight of evidence approach.

11. Have the plan peer-reviewed.

Each wind energy project will be unique and decisions regarding the study design, sampling plan, and parameters to measure will require considerable expertise. There is no single combination of study components appropriate for all situations. However, at the risk of oversimplification, Table 3-1 contains a simple decision matrix to assist in the design of wind energy/bird interaction studies.

Level I studies should detect major sources of impact on species of interest and assist in the design of wind energy projects to reduce impacts on birds. When uncertainty on avian risk exists Level I studies should also identify sites where there is a low probability of risk to these species. More often than not, the product of Level I studies will be to focus future research on areas where significant biological impacts appear likely or identify that no further research is needed.

***Table 3-1(a-c).* Recommended decision matrix for the design and conduct of Level 1 studies.**

(a) Design Options			
Study Conditions	**Recommended Design**	**Potential Design Modification**	
• Pre-impact Data Possible • Reference Area Indicated	BACI	Matching of study sites on assessment and reference areas possible	Matched Pair Design With BACI
• Pre-impact Data Not Possible • Reference Area Indicated	Impact-Reference	Matching of study sites on assessment and reference areas possible	Matched Pair Design With Impact-Reference
• Pre-impact Data Possible • Reference Area Not Indicated	Before-After		
• Small Homogenous Area of Potential Impact	Impact-Gradient[1]		

[1]Impact-Gradient design can be used in conjunction with BACI, Impact Reference, and Before-After designs.

(b) Sampling Plan Options	
Sampling Plan	**Recommended Use**
Haphazard/Judgment Sampling	Preliminary Reconnaissance
Probability Based Sampling:	
• Simple Random Sampling	Homogenous area with respect to impact indicators and covariates
• Stratified Random Sampling	Strata well defined and relatively permanent, and study of short duration
• Systematic Sampling	Heterogeneous area with respect to impact indicators and covariates, and study of long duration

(c) Parameters To Measure	
Parameter	**Empirical Description**
Abundance/Relative Use	Use per unit area and/or per unit time as an index[2]
Mortality	Carcasses per unit area and/or per unit time
Reproduction	Young per breeding pair of adults
Habitat Use	Use as a function of availability
Covariates	Vegetation, topography, structure, distance, species, weather, season, etc.

[2]Can be summarized by activity/behavior for evaluation of risk.

CHAPTER 4: Advanced Experimental Design and Level 2 Studies

INTRODUCTION

Level 1 observational field studies can be important in making statistical inferences regarding the magnitude and extent of impacts of proposed and existing developments. Level 2 studies employ controlled manipulative experiments to determine cause-and-effect relationships or make use of mathematical population modeling to improve the basis for deductive professional judgments regarding the impact of wind energy developments on birds. This chapter discusses both manipulative experiments and population models.

Manipulative Experiments

In the case of wind energy, manipulative experiments (also known as "comparative experiments" [Cox 1958, Kempthorne 1966] and "randomized experiments" [National Research Council 1985]) usually will be conducted to evaluate risk reduction management options for existing and new wind plants. For example, turbine characteristics such as support structure type, rotor swept area, and turbine color have been suggested as factors affecting bird risk in wind plants. Observational studies, such as Anderson et al. (1996b) can be used to evaluate some of these risk factors. However, manipulative experiments could significantly improve the understanding of how these factors relate to the risk of bird collisions with turbines. Manipulative experiments help determine treatment effects by allowing control of such factors as natural environmental variation, which tend to confound observational studies.

Population Effects and Modeling

Although methods are available for making empirically-based estimations of potential impacts on populations, population models represent an alternative and sometimes complementary approach. Protocols using population models have the advantage of providing results of impact assessment with relatively limited empirical data and allow the evaluation of data needs in a stepwise fashion, and should be capable of providing at least a preliminary indication of potential responses of birds to wind energy developments. But as discussed below, population models do have many limitations. Further, it is doubtful if population modeling will be indicated in most wind developments.

The main goal of this chapter is to develop a framework for Level 2 studies that can be generalized to most bird species for evaluation of potential wind plant impacts. This is accomplished by:

- developing a conceptual framework based on the major factors that can influence the persistence of a wild population

- briefly reviewing the basic approach to manipulative experiments as well as the various models that can aid in estimating population status and trend, including methods of evaluating model structure and performance

- reviewing survivorship and population projections

- developing a framework for determining the cumulative effects of wind energy development on birds.

This chapter does not argue against rigorous design-based (field) studies. Rather, it describes how an alternative, model-based approach can assist with evaluation of wind energy/bird interaction issues. Before proceeding to a detailed discussion of population effects and modeling, a brief discussion of manipulative experiments is offered.

MANIPULATIVE EXPERIMENTS

Manipulative experiments may be useful in wind energy/bird interaction studies. They satisfy two criteria:

1. Two or more "treatments" (one of which usually is a control, or reference treatment) are to be compared for study of cause-and-effect relationships on impact indicators.

2. Treatments are randomly assigned to experimental units (Hurlbert 1984).

If treatments are not randomly assigned to experimental units, the experimental design becomes observational (as in Chapter 3), and the information gained on cause-and-effect relationships is much reduced (Cox 1958, Kempthorne 1966). Designs for studying impacts of a wind plant can never be *truly* manipulative, because the area/population to be impacted by the plant and the reference areas/populations are not randomly selected by the researcher.

In manipulative experiments the statistical inference is still the protocol by which the study is conducted, the criteria by which study sites are selected, the source of the treatment materials, and the amount of replication in time and space. For example, if two wind plants are selected for the study of some treatment and the treatment and references are randomly assigned within the two plants, there exist two independent studies. Statistical inference is limited to the effect of the selected treatment as applied in the study on the wind plant where it is applied for the time period of application. The results of the two independent studies can be used in the subjective assessment of the potential effect of the treatment on other wind plants.

Any design used in laboratory experiments or manipulative field experiments are of use in Level 2 studies of wind energy/bird interactions, and a complete discussion of these options is beyond the scope of this document. For details on study design see references such as Cox (1958), Box et al. (1978), Green (1979), and Hurlbert (1984). All of the design principles and the basic sampling designs contained in Chapter 3 are appropriate for manipulative experiments. However, it is worth repeating Krebs (1989) that "every manipulative ecological field experiment must have a contemporaneous control..., randomize where possible..., and, because of the need for replication, utilize at least two controls and two experimental areas or units."

The following example illustrates the use of common design principles (described in Chapter 3) in the evaluation of a hypothetical risk reduction treatment included in the design of a newly constructed wind plant. This is just one example from among an almost infinite number of potential designs. Suppose a new wind plant is constructed consisting of 120 turbines distributed in 12 turbine strings, each with 10 turbines. Also suppose a two-year study is conducted to evaluate a treatment applied to some of the turbines hypothesized to reduce the risk of bird collisions with turbines. Finally, assume that risk is measured by the relative amount of bird use and bird carcasses located within study plots centered around treated and untreated turbines.

The first year of the study estimates the avian behavior, use, and mortality at the newly constructed wind plant without installation of a treatment. In year two, the treatment is applied and the reduction in risk due to the treatment is evaluated through the measurement of avian behavior, use, and mortality at turbines with and without the treatment.

In year one of the study, avian use and mortality are measured on plots containing turbines without treatment; in year two, use and mortality are measured on plots containing turbines both with and without the selected treatment. All twelve turbine strings are surveyed for avian use, behavior, and mortality, so a census in space within the wind plant is achieved. It is assumed that if a bird comes into the defined critical zone surrounding the turbines (some distance from turbines), then the bird is potentially at risk of injury. If the bird does not enter the critical zone, it is assumed that the bird is not at risk of injury. Risk is thus defined as use within a certain distance of a turbine. Fatalities are measured and an estimate is made of mortality per unit of use. Risk may also be defined as a change in mortality per unit of use.

There are two basic paradigms regarding the analysis of these data. One paradigm is that the sampling design is a matched pairs design (randomized block with two treatment levels). The second paradigm is that this is a manipulative study embedded in a large observational study using a BACI design. In the first paradigm, the effectiveness of the treatment is evaluated by testing the interaction between year and treatment. A two-factor repeated measures analysis of variance is conducted using the mortality rate (number of carcasses per search divided by bird use per visit per observation point) as the dependent variable. Figure 4-1 illustrates the mean mortality per unit of bird use near turbines by year and treatment. There appears to be an interaction between year and treatment; the mean is relatively stable for the non-treated turbines, whereas the mean for the treated turbines decreased in year 2. Given that a statistical test for interaction corroborates our interpretation of the graph, statistical tests of treatment effects should be conducted within each year. Bird fatality near treated turbines is significantly less than near the

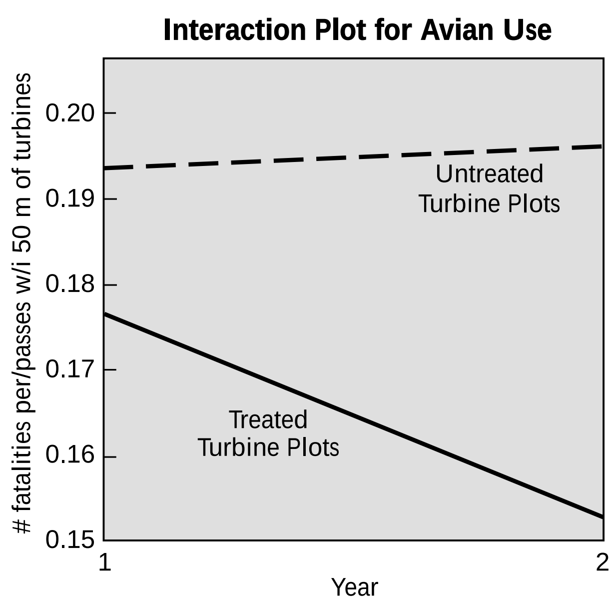

Figure 4-1. Interaction between the number of fatalities per bird passes within 50 meters of treated and untreated turbines.

non-treated turbines in year 2, indicating that the treatment does appear to reduce the risk to birds.

The second paradigm recognizes that the turbines (and turbine strings) are not random effects because the wind plant, turbine strings, and turbines are not randomly located. According to this paradigm, this is a pseudo-experiment with an unreplicated observational study over time and space. The analysis would follow statistical analyses for BACI designs (Skalski and Robson 1992) as described in Chapter 3.

In the above example, the design could be modified by applying the treatment and reference in year one to the selected subset of turbines and switching the treatment and reference turbines the second year. While this design slightly strengthens the study, it would be practical only if the risk reduction treatment were relatively easy and inexpensive to apply.

Manipulative studies can be very complex. However, because of the cost of treating wind turbines, most studies will by necessity be limited to simple designs evaluating a small number of treatments. Manipulative studies will be most valuable initially in evaluating treatments on individual wind plants. As data accumulate, subjective inference on a more global scale will be possible. However, care must be taken to avoid extrapolating the effectiveness of a treatment at one or a few wind plants to all wind plants.

CONCEPTUAL FRAMEWORK FOR POPULATION MODELING

A severe and continuing problem in ecology is determination of the proper population of study. A typical definition of population illustrates the vagueness of the concept: a population is a group of organisms of the same species living in a particular space at a particular time (Krebs 1985). A population is quantified in terms of birth rate, death rate, sex ratio, and age structure. Unfortunately, the absolute values for each of these parameters is determined in large part by the geographic boundaries the user draws. Recently, much interest has been shown in population structure, primarily because of advances in understanding the role that the spatial structure of a population has on genetics, and ultimately, survival. It is now understood that most "populations" are actually metapopulations composed of many subparts. This concept relates directly to determination of impacts on animals. First, even impacts occurring in a small geographic area can disrupt immigration and emigration between local subpopulations, and thus the impact can have a much wider effect on the population than immediately evident. Second, small impacts can have serious consequences to the persistence of small populations if population size is allowed to drop below a certain threshold point (e.g., the effective population size). Thus, the "population" or subsets thereof must be clearly defined prior to initiation of any study.

The major factors that can influence the persistence of a population are:

- demography
- genetics
- environmental stochasticity
- life history parameters
- ecological factors
- additive vs. compensatory mortality.

These factors, reviewed below, could be considered when developing a study plan for evaluating a specific impact of development.

Demography

Demographic "accidents" leading to extinction are most likely to occur in small populations. Because individuals do not survive for the same length of time, and individuals vary in the number of offspring they bear, such effects are sensitive to population size. These effects can be ignored if the population is larger than about 100 (depending on age structure). This minimum number will be inadequate, however, if the population is effectively divided into many local subpopulations, which is likely in most regions. In such cases, substantial mortality (by any cause) in one subpopulation can negatively impact adjacent subpopulations by changing rates of immigration and emigration. Boyce (1992) summarized that variation in population parameters attributable to environmental stochasticity will be more important than demographic uncertainty with regard to the probability of actual extinction.

This is not to say, however, that population projections are not an important part of evaluating a population. Leslie matrix and similar stage-structured models (described below) can give insight into the processes of population growth. For example, the sensitivity of the population growth rate, *lambda*, to perturbations in vital rates for a Leslie-type model can be solved analytically. Understanding how growth rate changes in response to perturbations at various stages in life table analysis may help direct management strategies. For example, adult survival tends to be a very sensitive demographic parameter in long-lived species, whereas fecundity can be more important in short-lived species.

At low densities, a positive relationship between per capita population growth rate and population size (known as an Allee effect) can occur. The consequences of such an effect for a population are important because it can create a threshold or critical population size below which extinction is probable. For example, limitations to juvenile dispersal can create an extinction threshold in territorial species (Lande 1987).

A model partitioned into spatial subunits can be difficult to analyze, although increasing understanding of metapopulation dynamics is forcing such efforts (e.g., Wootton and Bell 1992). Spatial heterogeneity and dispersal can stabilize population fluctuations, whether the fluctuations are caused by natural or human-induced factors. Unfortunately, no general statements can be made regarding: the influence of corridors; minimum distances between habitat patches; the ability of dispersers to locate suitable areas; the size and shape of suitable habitat patches; the ability of animals to travel across, or survive within, marginal habitats; and so forth. These limitations must be considered when interpreting results of any model.

Quantifying adult survivorship tells one a lot about population status (Lande 1988). Adult survivorship is usually very high, especially in long-lived species (such as raptors). Also, *lambda* has been shown to be most sensitive to changes in adult survivorship. In addition, in most monogamous species, it is female survivorship that is usually the most important to population persistence (e.g., Wootton and Bell 1992). Thus, in cases where information on population status is needed, quantifying adult survivorship provides a preliminary, basic indication of the status of the population.

Genetics

Models of genetic variation have a central role in the conservation of populations. We are particularly interested in how small population size can result in inbreeding depression and a reduction in genetic variation, both of which can lead to extinction (Boyce 1992). Local populations or subpopulations may contain the genetic diversity that is necessary to ensure survival of the species within a region, or even throughout its range.

Boyce (1992) concluded that modeling genetics is not likely to be as important as modeling demographic and ecological processes in evaluating population persistence. He based this conclusion, in part, on the fact that we do not yet understand genetics sufficiently to use it as a basis for management. Thus, practical considerations were the overriding factor in his conclusion. Genetics will be of priority in small, isolated populations, but are unlikely to have direct applicability in studies of wind energy/bird interactions.

Environmental Stochasticity

Random environmental events (e.g., catastrophic fires, hurricanes, disease, and so forth) can have pronounced effects on small populations. Such factors can also have pronounced effects on large populations that are spatially divided into subpopulations. Here, factors such as dispersal will determine the fate of a subpopulation driven to very low numbers by a catastrophic event. It is also important to understand the variance structure of the population; that is, how environmental stochasticity affects the organism. A major problem here, however, is that sampling variance may overwhelm attempts to decompose variance into individual and environmental components. Thus, the relative importance of environmental stochasticity must be based on an understanding of the spatial distribution of the population under study. This conclusion relates directly to the discussion above on the importance of determining the spatial structure of the population in any evaluation of potential impacts.

Life History Parameters

The animal characteristics that we collectively call life-history parameters include longevity, lifetime reproductive output, young produced per breeding attempt, age at dispersal, survivorship, sex ratio, time between breeding attempts, and various other characteristics. The absolute expression of each of these characteristics is usually determined by age of the individual; they change during the lifetime of an animal. For example, young and old individuals tend to produce fewer viable young than do animals in their prime. In addition, these factors can interact in various ways that modify the expression of other factors.

Life-history parameters are used in the development of some population-projection models. For example, combining various ranges of parameters can yield substantially different rates of adult survival. Such analyses provide guidance on the rates of mortality that can be sustained under varying expressions of life history traits. Once such relationships are understood, researchers have the opportunity to monitor selected life history traits as part of an assessment of the status of a population. For example, if previous work shows that the timing of breeding is correlated with reproductive output, and thus with population size for the year, monitoring time of breeding can provide an early warning of potential population-level problems.

Naturally, this is a simplistic view of a complex set of factors that determine population size and trend. However, it illustrates the utility of developing even rudimentary models as part of the evaluation of the status of a population. Much of the data necessary for input into such models is available in the literature (see discussion below).

Ecological Factors

Temple (1986) found that for endangered birds, 82% are listed as endangered as a result of habitat loss, 44% due to excessive take, 35% due to introductions of exotics, and 12% due to chemical pollution or the consequences of natural events. Development of a wind plant alters the environmental conditions available to birds; thus, habitat (defined as a species-specific entity) is added, removed, and modified during and after development, depending on the bird species being considered. Quantifying changes in habitat for species of concern thus may give insight into potential changes (positive or negative) caused by wind energy development.

Temple (1986) listed “excessive take” as the second most important factor causing declines in small populations. Generalizing “take” to include any artificially-induced mortality implies that mortality caused by wind turbines could be quantified to give insight into the magnitude of the effect. It might be easier to quantify and model habitat parameters, and their influence on some index of population abundance and life-history traits, than it is to adequately model demographics. As noted above, delineating population or subpopulation boundaries is extremely difficult.

Additive vs. Compensatory Mortality

Much interest has been expressed by individuals in the wind industry regarding the specific type of mortality functioning on wind plants. That is, do wind plant-related deaths add to the total number of deaths experienced by a population; or, are such deaths simply part of the total number of birds that would have died from some other cause? The former situation, termed additive mortality, is of concern because it means wind plants are potentially causing population-related impacts. The latter situation, termed compensatory mortality, would render wind plant deaths of relatively less concern because the total number of deaths has not been increased by the wind energy development.

The issue of additive versus compensatory mortality has been a central topic of debate in the ecological literature for decades. In large part, this is because hunting advocates have used the possibility of compensatory mortality to argue that hunting only removes animals that would have died anyway. Unfortunately, little quality data exists on this subject. In the case of wind plants, no studies have been conducted that help to elucidate the additive versus compensatory issue. It is not appropriate to assume that compensatory mortality takes place, especially with regard to the specific age and sex segment of the population (i.e., even if overall deaths might be the same, the segment of the population experiencing the deaths would likely be different). Thus, the issue of compensatory versus additive mortality could be further investigated, and it is probably not appropriate to assume a specific form of mortality is occurring at this time in wind plants.

MODELING

Uses of Modeling

In many model-based analyses of populations, a central part of impact assessment is development of a model predicting the survival rates required to maintain a population. The strategy is to determine survival rates required to sustain populations exhibiting various combinations of the other parameters governing population size. To be useful in a wide range of environmental situations and useable for people with varying expertise, the model must be based on simple mathematics.

The use of models (of all types) soared beginning in the 1980s. In fact, modeling is now a focus of much interest, research, and management action in wildlife and conservation biology. But as in all aspects of science, models have certain assumptions and limitations that must be understood before results of the models can be properly used. Modeling *per se* is neither “good” nor “bad”; it is the use of model outputs that determines the value of the modeling approach.

The use of population models to make management decisions is becoming common. For example, such models are playing a large role in management plans for such threatened and endangered species as the spotted owl (*Strix occidentalis*, all subspecies), desert tortoise (*Gopherus agassizii*), Kirtland's warbler (*Dendroica kirklandii*), various kangaroo rats (*Dipodomys* spp.), and so forth.

Two general uses of models should be distinguished:

1. using models to give insight into how an ecological system behaves

2. predicting the outcome of a specific situation.

In the first case, the model helps guide decisions when used in combination with other reliable data, whereas in the second case model assumptions and results must be tested in a quantitative manner (i.e., model validation).

Types of Models

Life Tables

Life tables are one of the oldest means of examining mortality in animals; simply, they summarize survivorship by age classes in a cohort of animals. A basic life table requires only that age, the number of individuals surviving to the beginning of each age classification, and the number of deaths in each age class be known; mortality and survival rates can be calculated from these data. There is only one independent column in a life table; all the others can be calculated from entries in any one column. This dependency requires that great care be taken in constructing the table, and that large sample sizes be gathered. Grier (1980) and Buehler et al. (1991) used a deterministic life-table model to calculate survivorship and population growth in bald eagles.

Simple Lotka Models

The annual geometric growth rate of a population is represented by *lambda*, also known as the finite rate of population increase. At time t the population size is *lambda* times its value at time t - 1, $N_t = lambda(N_{t-1})$. The population is increasing if *lambda* > 1, is constant if *lambda* = 1, and is decreasing if *lambda* < 1. For example, if *lambda* = 1.04, then the population was growing at the rate of 4% per period during the time sampled. For purposes of calculation, this formula is usually presented as $N_t = N_0e^{rt}$, where e is the base of the natural logarithm, and r is the instantaneous rate of population increase (Johnson 1994).

Eberhardt (1990) developed a modeling scheme based on approximations of the Lotka equations using the grizzly bear (*Ursus arctos*) as an example. Parameters used to develop the model included litter size, proportion of female cubs, breeding interval in years, and reproductive rate. The utility of this approach was that estimates of these parameters were available in the literature. Eberhardt used variations among these estimates (e.g., litter size ranged from 1.65 to 2.36) to calculate ranges of female survival rates to provide information about the scope of such rates needed to sustain populations. Each user of the Eberhardt scheme could select the particular combination of demographic parameters thought to be most appropriate for a particular situation.

Leslie Matrix Models

Matrix models subsume classical life table analysis as a special case but have capabilities that go far beyond that analysis. As summarized by McDonald and Caswell (1993), they:

- are not limited to classifying individuals by age

- lead easily to sensitivity analysis

- can be constructed using the life cycle graph, an intuitively appealing graphical description of the life cycle

- can be extended to include stochastic variation and density-dependent nonlinearities.

McDonald and Caswell present a detailed description of the formulation and application of matrix models to avian demographic studies.

The numbers in the body of the matrix are transition probabilities for survival and progression into other stages, while the numbers on the top row of the matrix represent stage-specific fecundity values. The term in any particular row and column can be thought of as the contribution of an individual in the age class represented by that column in year t to the age class represented by that row in year t + 1. The population can be projected from one year to the next by repeating the process into the future. Thus, we term this matrix the population projection matrix, or more popularly, the Leslie matrix after its developer (Leslie 1945).

A Leslie matrix can be built from estimates of fecundity and survival probabilities, and population growth may be projected for any number of time periods by pre-multiplying the age distribution at each time period by the Leslie matrix to get the new age distribution for the next time period. Population projections using Leslie matrices is a useful approach to the analysis of demography (Jenkins 1988). They provide a numerical tool for determining growth rate and age structure of populations. The Leslie matrix also is useful for illustrating and studying the transient properties of populations as they converge to the stable state.

Stage-based matrices, analogous to the age-based Leslie, can be used to analyze population growth for species in which it is difficult to age individuals, or where it is more appropriate to classify them into life stages or size classes rather than by age; these models are generally referred to as Lefkovitch (1965) stage-based models. It is extremely difficult to determine the specific age of most birds and mammals after they reach adulthood. In the case of raptors, the focus of concern in many wind energy developments, young (juveniles) and subadults can usually be aged up until adulthood (through differences in plumage and soft tissues, and sometimes eye color). Further, adult raptors can often be placed into categories based on breeding status.

Lefkovitch models assume a single, well-mixed population with no spatial structure and no density dependence in the variables. Thus, they assume homogeneous probabilities of survivorship and breeding success within each stage, independent of other factors such as age. The models can be modified to incorporate spatial population structure and analyze it in the context of different management options for a population (e.g., see Wootton and Bell 1992 for development and review; and Ruckelshaus et al. 1997 for problems with using developing models). However, such spatially-explicit models are beyond the scope of this document, and are more detailed than necessary for most wind energy applications.

Case Study

The impact of the Altamont Wind Resource Area on the golden eagle population resident there is being evaluated with the aid of a Lefkovitch stage-based model (see Shenk et al. 1996 for complete model development). The model is being developed by a team of scientists assembled by NREL; field data are being collected by a team of biologists headed by Dr. Grainger Hunt. The overall goal of the project is to determine the finite rate of population growth (*lambda*) based on birth and death rates for the defined golden eagle population around the WRA. If the estimate of *lambda* is >1, then the population will be assumed to be stable or increasing. If *lambda* is <1, then no definite conclusion regarding the impact of the WRA on the population can be made. This is because there could be many reasons for a declining growth rate. The model will, however, provide a quantitative approximation of the current status of the eagle population. Further, parameter estimates of survival and fecundity will assist in evaluating the status of the population through comparisons with the same information for other populations. For example, if the survival of some segment of the population is relatively low, and that segment has been shown to sustain turbine-related deaths, then further study would be indicated. The value of the model-based approach is that it provides a specific structure for the field studies to follow, including understanding of the sample sizes necessary to reach desired estimates of the parameters.

The group developing the Altamont model was faced with time (three years for field study) and monetary constraints. Because of these constraints, time effects would have to be ignored (i.e., interyear variability could not be determined), and sampling would have to focus only on those elements essential to model development. Regardless of the length of study chosen (i.e., relatively longer- or shorter-term), modeling helps to focus the field sampling, thus making for an efficient and justifiable expenditure of funds.

The modeling group next evaluated the sample sizes necessary to estimate survival with a minimum precision of 10%. The eagle population could be broken into four general categories: adult breeders (territory holders; >4 years old), adult floaters (nonterritory holders; >4 years old, subadults (1-4 years old), and juveniles (<1 year old). Eight total categories resulted when sex was considered. Preliminary analyses indicated that at least 25 individuals would be necessary for each of the eight classes. Based on time and funding constraints, it was determined to be infeasible to gather this many samples. Further discussion indicated that all adult floaters and subadults could be combined and considered “nonterritory holders.” Most demographic models only consider the demographics of females because of their central role in the production of young. (Wootton and Bell 1992). However, because male eagles can have a key role in determining nesting success (G. Hunt, Univ. California, Santa Cruz, pers. comm., various dates 1996-97), male eagles were also marked. Immigration and emigration were assumed to have no influence on *lambda* because of the difficulties in estimating these parameters. Of course, the potential influence that such assumptions may have on model results must be considered when drawing conclusions from the study. The assumptions and constraints necessary in the Altamont eagle study are typical of real-world modeling situations.

Effective Population Size

As discussed above, small populations are susceptible to extinction because of random loss of genetic variation and random demographic events. In the "ideal" theoretical population, the rate of loss of genetic variation is inversely proportional to the population size. Of course, the reproductive behavior of natural populations is far from ideal. To try to link natural and idealized populations, Wright (1931) defined the "effective population size" (N_e) as the size of an ideal population whose genetic composition is influenced by random processes in the same way as the natural population.

When N_e is small, the population can rapidly lose genetic variation. However, N_e has no set relationship to actual population size, and its precise estimation is complex. Two approaches have been used to estimate N_e: genetic and ecological. The genetic methods directly quantify the effects on genetics of a particular effective population size, whereas the ecological methods are indirect and depend upon the measurement of ecological parameters that are thought to influence a particular effective population size.

Although direct measurement of the effective population size by a genetic method is the most appropriate, there are several problems associated with its determination. First, the method requires the gathering of a large amount of genetic information. Although new technologies are reducing this problem, it is still beyond the capabilities of most researchers. Second, the confounding factors of immigration and population subdivision, and the possibility that even relatively low levels of some types of evolutionary selection are having a large influence on the estimated N_e.

Ecological methods depend on theory linking particular ecological parameters, usually based on demography or behavior, to changes in N_e. Wright (1938) established the relationship linking the effective population size to the population sex ratio and to the variance in reproductive success among individuals. For example, variation in family size inflates the variance in reproductive success and thus reduces the effective population size.

Various formulas have been developed to estimate the effective population size. Harris and Allendorf (1989) evaluated several of these methods. Hill's (1972) original equation and its derivatives were consistently the most accurate. Nunney and Elam (1994) developed a related approach that required a minimum amount of information while still giving a good estimate of N_e. Termed the "minimal" method, it requires the estimation of six parameters: (1) mean maturation time to adulthood for both males and females; (2) mean adult life span for each sex, and overall; (3) estimation of generation time; estimation of variation in (4) male and (5) female reproductive success per breeding season; and (6) estimation of the adult sex ratio. This method is designed to provide an estimate of the effective population size in long-lived species using the minimum of data possible derived from the literature and short-term study. The method is most effective if survivorship is age-independent, which is common in many natural, long-lived populations (not including juveniles).

Nunney and Elam (1994) argued that the minimal method (and ecological methods in general) provide data that can be used to predict changes in effective population size as the conditions confronting the population change. Thus, it functions well in monitoring populations over time. Genetic methods determine what the effective population size has been over the last or several generations, but they provide no insight into why this has been the prevailing value. Therefore, they recommend ecological methods when it is practical, so that the effect of different management options on the effective population size can be estimated. They note, however, that the demographic information needed to provide a reliable estimate of N_e can often be difficult to obtain. As noted above, it is unlikely that this level of data collection will be indicated in most wind energy applications.

There has been continuing debate over the minimum size a population must maintain to ensure long-term persistence (perhaps 100 generations). During the 1980s and into the 1990s, geneticists estimated that the minimum effective population size was 500 or fewer breeding individuals. New genetic evidence suggests, however, that this former estimate is far too low, and could easily range between 1000 and 10,000 individuals. This new estimate is based on consideration of the effect that mutations have on the fitness of the organism at low population sizes (Lande 1995, Lynch et al. 1995).

It is difficult to make broad generalizations on the effective population size of organisms. For example, small populations (<100 adults) have been shown t o persist for extended periods of time because of adaptations to local environmental conditions (e.g., Reed et al. 1986, Grant and Grant 1992, Nunney 1992). Evaluation of effective population size may be appropriate in preliminary analyses of a population. Such evaluations can help prioritize species to study and help determine the level of concern that should be placed on deaths in a population before initiating a full-scale population study.

Model Evaluation

Bart (1995) provided an excellent review of the steps necessary in evaluating the appropriate uses of a population model. The following outline is summarized from his paper. There are three major components of model evaluation that should be included in all studies: model objectives, model description, and analysis of model reliability. The latter component is further divided into four important criteria.

Model Objectives

As noted above, all studies should list the specific objectives for which model outputs will be used, and the reliability needed for those outputs. Will the output be used only as part of a much larger set of information — or will management decisions be based on model results? The precision needed in all cases should be specified; there are no pre-established standards.

Model Description

The general structure and organization of the model should be detailed. This description should include the basis for classifying the environment (e.g., vegetation types used for analysis), the number of sex and age classes, the behavior of the animals (e.g., breeding times, dispersal), and so on. For example, if sexes or age classes are lumped (because of sample-size considerations), then the behavior of the sexes and age classes is assumed to be equal. Likewise, if data on any aspect of the model are lumped across years, then time is held constant and assumed to have no overriding impact on the model. Most decisions reduce the complexity of the model, which in turn reduces its reality. Careful consideration and justification of any such decisions must be included in model description.

Analysis of Model Reliability

There are four major types of model reliability to evaluate: structure, parameter values, secondary predictions, and primary predictions. Each type should receive attention, with emphasis on the particular type that the management will focus on.

Model structure. The realism of each assumption about the model should be fully assessed using any information available. Naturally, the first source of information here is the scientific literature about animal behavior, habitat relationships, population structure, and demographics. If little information is available on the species of interest, then data on related species should be consulted. The impact that each assumption should have on model results should be clearly discussed. Some assumptions likely will have minimal impact, while others may have potentially severe influence on the model. In some cases the decision will have to be made that insufficient information is available on this or closely related species for any meaningful evaluation of the model to be made. In such cases, the model — if developed — is of the purely descriptive form and should only function in identifying likely areas upon which field research (to fill the data gaps) should focus. However, information is usually available with which at least a preliminary model structure can be based.

Parameter values. The most reasonable estimate of mean values and ranges for each parameter should be developed. Again, the literature should first be consulted. However, field studies may have to be conducted to provide reasonable estimates of certain parameter values. Unfortunately, the wildlife literature provides little in the way of strong data on survivorship of animals, especially where data on specific sex-age classes are needed. The reality of the situation usually demands that a short-term (1-3 year) study be initiated to provide the missing data. Because these studies usually focus on either rare species or isolated populations, it may be necessary to ignore yearly variations and lump across time to achieve an adequate sample size. As discussed above, the ramifications of this type of simplification must be carefully evaluated. It also is almost always the case that certain age classes (e.g., nonbreeding adults in raptors) will have to be combined; additionally, in most animals age cannot be readily determined after adulthood is reached.

Secondary predictions of the model. Secondary predictions are intermediate outputs of the model that can be used to better understand the population and help evaluate the reliability of the final model. Each of these outputs is a function of two or more input variables. Comparing them to empirical data, to data for similar species, or just plain ecological common sense helps identify how reliable the model will be (and where weaknesses exist). Examples of secondary outputs include the distribution of age classes at first breeding, territory occupation, and so on.

Primary predictions of the model. Primary predictions are the outputs of primary interest; this is the information used to determine project impacts and make management decisions. Predicted model results should be compared to reality either by comparing them with empirical data, or by running simulations that can be compared with known (past) population values. That is, if the model fits past (known) trends, then it is more likely to be properly forecasting future values. Unfortunately, little data are usually available because few animals have been adequately studied. Evaluations of models, however, are not truly independent if available empirical data are used to develop the model in the first place; testing the model predictions with the same data results in a biased validation.

Synthesis

The goal should be to present a realistic and unbiased evaluation of the model. It is preferable to present both a best and worst case scenario for model outputs, so that the range of values attainable by the model can be evaluated. For example, with a basic Leslie Matrix Model of population growth, knowing whether the confidence interval for the predicted (mean) value for *lambda* (rate of population growth) includes a negative value provides insight into the reliability of the predicted direction of population growth.

The process of model development and evaluation may show that the predictions of the model are sufficiently robust to existing uncertainties about the animal's behavior and demography that high confidence can be placed in the model's predictions. A poor model does not mean that modeling is inappropriate for the situation under study. Rather, even a poor model (i.e., a model that does not meet study objectives) will provide insight into how a population reacts to certain environmental situations, and thus provide guidelines as to how empirical data should be collected so that the model can be improved. Modeling is usually a stepwise process. Confidence intervals can be calculated to quantify the amount of variability associated with model outputs (Bender et al. 1996).

SURVIVORSHIP AND POPULATION PROJECTIONS

Major wildlife and ornithological journals (*Journal of Wildlife Management, Condor, Auk, Journal of Raptor Research*) published during the past 20 years were reviewed for this chapter to determine if any commonality existed among species with regard to annual survivorship. Most data in the articles examined were based on either short-term (usually 1-3 years) telemetry studies, or long-term analyses of band returns. Most of the band return data were obtained from waterfowl harvested by hunters.

In summary, only very broad generalizations can be drawn regarding "normal" survival rates of avian populations. Further, interyear variability in survivorship is large even in healthy populations, which makes short-term (1-2 years) evaluations of a population of concern suspect. Bellrose (1980) summarized survival rates for waterfowl, concluding that immature ducks show 60% to 70% first year mortality, but that subsequent (adult) yearly loss is only 35% to 40% (or survival of about 60% to 65%). More recent studies confirm these general values of Bellrose. For example, Smith and Reynolds (1992) found that survivorship in mallards ranged from about 60% to 70%, and that the population should not decline in abundance. Unfortunately, most studies that present survivorship data provide no information on population trends or projected population persistence; most showed survivorship values similar to those summarized by Bellrose (1980; e.g., see Conroy et al. 1989, Chu et al. 1995, Reynolds et al. 1995). Haramis et al. (1993) and Hohman et al. (1993) found what they called "high" survivorship rates of over 90% in canvasbacks. Foster et al. (1992) examined survival of northern spotted owls in four study areas across 2-4 years by radioing 213 owls. Annual survivorship ranged between 67% and 100% with most between 80% to 94%; no information on population persistence was provided.

In glaucous-winged gulls, Reid (1988) found 85% annual survival in adults, 80% in second year, and 61% in first year birds. Using these survival values and other population parameters to construct a Leslie matrix model, he calculated a *lambda* of 1.05. This rate of population growth compared favorably to the observed rate of growth.

For bald eagles in Maryland, Bowman et al. (1995) provided survivorship data for 1-6 year old age classes (their table 1), and using a deterministic life-table model, predicted a finite population growth rate of 5.8% per year. They found, however, that a simulated 12% decrease in minimum adult survival (from 83% to 73%) eliminated population growth. Their review of the literature showed that their estimated survival rate exceeded those previously published.

Likewise, Bowman et al. (1995) found that after first year, survivorship for bald eagles in Alaska was about 90%; survivorship within the first year was 71%. They too used a deterministic life table to calculate a *lambda* of 1.02 (2% annual population growth). They illustrated through sensitivity analysis that their model was robust to changes in reproductive rates and annual survival rates for first-year eagles, but sensitive to changes in survival rates for after first-year eagles (see their fig. 2). Sensitivity analysis suggested that *lambda* turned <1 when after first-year survival dropped to only 88%; *lambda* dropped to about 93% when survival was lowered to 82% (other parameters also were modified).

Conway et al. (1995) conducted an experimental evaluation of the effects of removal of nestling prairie falcons on the breeding population in an attempt to simulate the impacts of falconry. They removed 138 of 451 nestlings (31% of natality) from 20 territories during 1982-89, along with a reference area. They found no overall difference in nesting success and productivity between treatments and references, although treatments were lower than references in two years of study. Their results suggested that intensive harvest of nestling prairie falcons may adversely affect some local population parameters, but harvests were sustainable and probably did not affect local population size. Because only about 0.2% of all prairie falcon natality is harvested annually in the United States, such a loss has no impact on population numbers or persistence (relative to the much higher level of harvest they simulated). This study is an excellent example of experimental evaluation of the impacts of loss of young, and indicates the resilience of raptor populations to loss of young.

These studies are important because they indicate that even a relatively minor change in survivorship can have substantial population impacts. They also indicate the importance of determining survivorship, as guided by a modeling structure, in evaluations of effects of wind energy developments on birds. The literature clearly indicates that, in most cases, adult survivorship is critical to maintaining a viable population.

Population Viability Analysis

A population viability analysis (PVA) is a complex process that considers all factors that affect the processes of a species' population dynamics. Such factors can include demographic, genetic, and environmental stochasticity, plus life history and habitat-use parameters; dispersal, competition, and predation also need to be considered. By formally trying to understand these processes and how they might influence the species, our general knowledge of how an environmental impact might affect the species is formed. The determination of biological effect made by the Forest Service in their biological evaluation procedure is usually based on some type of PVA, and PVAs are now being included by the Fish and Wildlife Service in some recovery plans for endangered species.

Thus, a PVA concerns birth, death, immigration, and emigration rates and how environmental factors (including a treatment) affect these rates over time. Models are used within a PVA process, ranging from simple verbal to complex mathematical versions (as reviewed above). A PVA should thus be considered a "process" rather than a specific model in and of itself. It entails evaluation of available data and models for a population to anticipate the likelihood of population persistence over some period of time. The "minimum viable population" (MVP) modeling scheme, where an estimate of the minimum number that constitutes a viable population is estimated, is closely related. PVA embraces MVP, but without seeking to arrive at an absolute population minimum. An MVP can be considered in overall determination of the PVA.

There are few published or peer reviewed PVAs, and most that are available provide only a vague outline of model structure or use general "rules of thumb" that are burdened with severe assumptions. There are no specific guides for completing a valid PVA. This is understandable, however, because each situation is unique due to differing environmental conditions and differences in the proposed impact(s). The advantage of the PVA over an MVP is found in the fact that sufficient data to derive reliable estimates for all parameters to develop an MVP is not practical in most cases (for the reasons developed above).

Hindering the fuller use of PVAs in the decision-making process is a general misunderstanding of their strengths and limitations. Lindenmayer et al. (1993) and Boyce (1992) carefully reviewed this topic, with the former outlining the following strengths of a PVA.

1. It produces an explicit statement of the ecology of the species and identifies missing data.

2. It synthesizes interacting factors and identifies trends in population behavior.

3. It identifies processes threatening to the species.

4. It can be used in defining minimum critical areas and designing reserves.

5. It can enhance on-ground management and decision making.

6. It has applications in species recovery, reintro duction, and captive breeding programs.

It is clear that PVAs have applicability in a wide range of management scenarios, which increases the need for managers from all disciplines to understand their functioning.

Of particular interest in application of PVAs to wind energy development are Lindenmayer (et al.)'s points 1, 2, 3, and 5. By describing the general ecology of the species (point 1), users from all educational backgrounds are able to better understand the problems confronting the scientist in making predictions regarding likely environmental impacts of development. For example, the needs of a raptor for certain types and sizes of prey when feeding young, or the influence of a skewed sex ratio on territory occupancy during breeding, are complicated issues that must be described. These factors might interact (point 2) because only a certain sex is able to efficiently exploit the prey available in the project area; development might change this prey availability because of disturbance to the ground and changes in plant-species composition (point 3). Development of points 1-3 naturally leads to fulfillment of their point 5, namely enhancement of sound decision-making regarding both permitting of the project, and in the case where permitting is allowed, modification of the project to avoid potential impacts on the species of concern (in the above example, avoiding unwanted changes in prey availability through habitat management). *The development of a complete PVA will seldom be necessary in permitting or evaluation of wind-energy developments. Great care must be used in developing a PVA because of the difficulty involved in gathering data and producing reliable results.*

From this review (above) of factors known to influence population persistence, it appears that we should:

- worry about adult survival in long-lived species (including most raptors), but fecundity in short-lived species

- not try to quantify genetics, but rather examine estimates of effective population size

- worry about the spatial structure of populations of concern (which by implication includes immigration and emigration)

- carefully evaluate how life-history parameters could interact to influence population persistence (e.g., sex ratio and productivity).

As noted by Lindenmayer et al., however, PVAs — as with many models — are only as strong as the data available for use in their development. And because all models are simplifications of ecological interactions, PVAs by their sheer complexity tend to conflate errors. Further, because of substantial differences in life-history parameters among species, no generic PVA model is available. This greatly complicates the use of a PVA, because one must be familiar both with the ecology of the species as well as with a complex set of mathematical formulations (Lindenmayer et al. list many of the models available). As such, most models for PVA analysis must be modified to meet the particular requirements of a given project.

The use of PVAs is thus hindered not by something inherently wrong with the concept *per se*, but rather by the inherent complexity of biological systems. PVAs are simply an attempt to formalize the complexity of nature. As such, PVAs may enhance decision-making by formally identifying the process under study, thus providing a list of the data available and the data still needed to make a rational decision regarding project impacts. In fact, researchers and managers alike are increasing their use of PVAs (Mace and Lande 1991).

Boyce (1992) and Lindenmayer et al. (1993) reviewed many of the PVAs that have been constructed. The summary presented by Lindenmayer et al. (1993: Table 1) is especially useful in highlighting the fact that each of the PVAs they reviewed were able to identify the primary cause of risk to the population. As they noted, habitat loss was the primary risk factor in most of the situations evaluated. In cases where habitat loss was not the primary factor, a species- and site-specific factor was identified as of primary concern. For example, hurricanes — which fall into the general category of environmental stochasticity — were the risk factor for several small, geographically isolated populations (Puerto Rican parrot [*Amazona vittata*], key deer [*Odocoileus virginianus clavium*]). Other site-specific impacts were found to be the primary risk agent in other isolated populations, including ski resort development, hunting, and logging.

These results from the PVA are mostly intuitive: one would expect that a small, isolated population would be negatively impacted by factors heavily impacting the location where members of the population remain. The ability of a population to adjust to changes in its habitat can be predicted through careful study of the behavior of individuals in the population; that is, through determination of the classification of individuals as either "specialists" or "generalists". By definition, habitat is a species- or population-specific phenomenon (Morrison et al. 1998). As such, changes to its habitat must have some impact on individuals in the area, given that its habitat has been properly characterized by the researcher.

The question arises, then, if results of PVAs are basically intuitive, what value does actual creation of a PVA offer? The answer is found in the fact that by constructing a PVA, the researcher is able to show in a systematic and analytical fashion that his/her intuition was indeed logical. Perhaps a proper test of a PVA would involve evaluation of a hypothesis based on researcher knowledge and intuition. Further, a PVA allows knowledge to be gained on the interactions of various life-history parameters, and their impact on population numbers.

DETERMINING CUMULATIVE EFFECTS

There are two major aspects to cumulative effect analysis that are directly related to wind energy development. The first concerns cumulative effects on a population over time. That is, are effects (positive or negative) caused by the wind plant relatively subtle over a short period of time, so that only a longer-term study will reveal the trend of impact? This impact could apply to the birds in and immediately around the wind plant, or could manifest itself in populations or subpopulations some distance away through changes in immigration and emigration. This type of influence is extremely difficult to quantify in the field without a tremendous expenditure of time and funds. Here, it becomes essential that a rigorous and focused modeling framework be established so that the potential impacts can be hypothesized given a variety of scenarios (e.g., levels of death). In this way, inference can be drawn from data collected over the short term as it applies to likely longer-term impacts using projections of various population models.

The second issue with regard to cumulative effects concerns the expansion of an existing wind plant. The comments in the preceding paragraph still apply, but the issue is complicated by the continuing development of the wind plant. No information is available on how bird populations respond to wind plant expansion. In particular, we do not know if the relationship between number of turbines and number of deaths is linear, or if it plateaus at some point. Further, we do not know if the potential benefits of a wind plant to certain bird species (e.g., potential increase in prey for raptors) reaches some optimal level given a certain size of the wind plant. Here again, the most efficient approach would be to model the likely responses of a population to simulated changes in prey abundance and deaths, and then compare the resulting population projects with what is found initially in the field. These results will indicate the level of concern that should be applied to bird deaths.

As detailed in chapter 3, proper experimental designs must be implemented for analysis of the response of birds to wind energy development. It is beyond the scope of this chapter to describe all of the various designs and analyses possible. The standard call for adequate treatments and references, including pre-treatment data, apply here as well. The advantage of designing a study of cumulative effects as a wind plant expands is that good references potentially exist in the areas that are scheduled for development at some point in the future. The only weakness here is that, if the wind plant is fully developed, the references will eventually disappear; allowances must be designed for this eventuality (e.g., locating areas that could be suitable for wind energy development, but are unlikely to be so developed).

Land uses unrelated to wind development also could impact bird populations inhabiting a wind plant. For example, residential housing, commercial development, roads, and agriculture could influence birds on or near a wind plant. It is not the purpose of this document, however, to discuss the myriad non-wind factors that could be part of a complete analysis of the cumulative effects of human activities on bird populations. Such an endeavor would involve a thorough environmental impact assessment.

RECOMMENDATIONS FOR STUDY DESIGN

Below is a summary of the primary points discussed in this chapter.

1. *Manipulative studies* can be an effective means of determining the response of birds to treatments or experiments designed to test behavior,

such as procedures designed to identify methods for reducing the risk of bird deaths.

2. Developing a *sound modeling framework* may help identify the critical aspects of the population that should be studied, even if a formal model is not calculated.

3. In many situations, *quantification of adult survivorship* is an essential step in determining the status of the population of interest. Data on survival published in the literature is adequate to allow broad generalizations to be made regarding "adequate" survival for population maintenance.

4. Determining the *spatial structure of a population* — whether it is divided into subpopulations — is important in that it places the status of various life history parameters into context.

5. *Quantifying reproductive output and breeding density,* when combined with knowledge of the population's spatial structure, provides a good idea of the status of the population. This will be especially important when adult survivorship cannot easily be determined.

6. Habitat loss is usually a factor causing the decline of a species. *Quantification of habitat use,* including factors such as food abundance, can be an important part of evaluation of a population's status.

7. *Compensatory mortality* should not be assumed to be operating with regard to wind plant related mortalities.

8. It is likely that *Leslie matrix models* will be most useful when predicting the response of locally abundant subpopulations. Here, enough individuals are present for a population trend to be estimated.

9. Determination of the *effective population size* (N_e) likely will be useful in evaluating the status of rare subpopulations. A rapid determination of the likely lower critical threshold for the subpopulation is necessary.

CHAPTER 5
Risk Reduction Studies

INTRODUCTION

This chapter outlines guidelines for studying the risk of death to birds in wind energy developments, and for studying the effect of treatments to reduce that risk. It is important to bear in mind that there are two components to this issue:

- The effect of a wind energy facility on bird *utilization* of a site

- The risk of death to birds using the site (bird *mortality*)

Thus, we are interested in quantifying both the use of a site and the deaths associated with that use. Great care must be taken in identifying an appropriate measure of bird use of a wind energy development (e.g., bird abundance, passes near a turbine, nesting success). Indirect factors, such as changes in habitat, prey quality and quantity, nesting sites, and many other factors can affect bird use of a wind energy development and must be considered in study design.

The goal of this chapter is to develop guidelines for studying methods of reducing the risk of bird death in wind energy developments. This goal will be addressed by:

1. developing methods of assessing avian risk

2. reviewing the hazards to birds in wind energy developments

3. describing methods of study design for evaluating bird risk in wind energy developments, and for assessing the effectiveness of treatments in reducing that risk.

Much of the material reviewed herein is taken from reports previously developed under NREL auspices by L. S. Mayer, M. L. Morrison, H. Davis, K. H. Pollock, R. L. Anderson, and S. A. Gauthreaux.

ASSESSING AVIAN RISK

In assessing avian risk with the purpose of eliminating or reducing that risk, it is essential to quantify both the use of a site and the deaths associated with that use. The ratio of death to use becomes a measure, expressed as mortality, or the rate of death (or injury) associated with bird utilization of the wind energy site. Following the epidemiological approach, mortality is the *outcome variable* — the variable that the researcher considers most likely to shed light on the hypothesis about the mechanism of injury or death. Mortality thus depends heavily on the mechanism hypothesized as the cause of injury or death. Determining the mechanism of injury or death allows the development of appropriate methods to reduce the risk to a bird while in a wind plant.

In testing modifications to turbines or wind plants, it is important to separate bird mortality from bird utilization in order to determine if decreased deaths were due to decreased utilization, decreased risk, or both. Separating utilization from risk makes it possible to know if a modification that reduces risk of injury or death of birds using a wind plant has a positive or negative effect on the population. For example, modifications in turbine characteristics might be accompanied by, say, a 30% reduction in the number of dead birds as compared to the wind plant before the modifications. If use has remained constant, this could be interpreted as a positive outcome from the modifications. If, however, use of the wind plant by birds drops by 50% following the modification, then the benefits of the modification are not clear. If the 50% decline in use results from some factor unrelated to the wind plant then the 30% decline in the number of deaths might actually be considered an increase in mortality. Thus, if we ignore bird use, we cannot assume that a modification that is accompanied by reduced deaths will have a positive or a negative effect on the bird population in question.

Attributable vs. Preventable Risk

Attributable Risk

Attributable risk is defined as the proportional increase in the risk of injury or death attributable to the external factor (turbine or wind plant). It combines the relative risk imposed by exposure to the external factor with the likelihood that a given individual is exposed to the external factor. Attributable risk (AR) is calculated as:

$$AR = (PD - PDUE)/PD$$

where PD is the probability of death per individual for the entire study population, and PDUE the probability of death per individual for the population not exposed to the risk. For example, if the probability of death for a randomly chosen individual in the population is 0.01, and the probability of death in a reference area for a bird flying through a theoretical rotor plane without the presence of blades is 0.0005, then AR is (0.01 - 0.0005)/0.01 = 0.95. Thus, about 95% of the risk of dying while crossing the rotor plane is attributable to the presence of blades. As noted by Mayer (1996), it is the potentially large attributable risk that stimulates the concern about the impact of wind energy development on birds, regardless of the absolute number of bird deaths. Testing a preventive measure in a treatment-reference experiment allows us to determine the change in risk due to the treatment.

Preventable Fraction

If impacts are at unacceptably high levels, and preventive measures are deemed necessary, there is interest in determining the proportion of deaths removed by a preventive step. The proportion of deaths removed by a preventive step is termed the *preventable fraction* and is defined as the proportion of injuries or deaths that would *be removed* if all birds were able to take advantage of the preventive intervention (e.g., perch removal). Preventable fraction (PLF) is calculated as:

$$PLF = (PD - PDI)/PD$$

where PDI is the probability of injury or death given the preventive intervention. For example, if population mortality in the wind plant is 0.01, and mortality for those using the area with perches removed is 0.005, then the preventable fraction is (0.01 - 0.005)/0.01 = 0.5. Thus, about 50% of the risk would be removed if all of the perches were removed. Note that the attributable risk and preventable fraction would be the same value if the intervention removed the risk.

Prevented Fraction

The *prevented fraction* is the *actual reduction* in mortality that occurred because of the preventive intervention. Prevented fraction (PFI) is calculated as:

$$PFI = (PDAI - PD)/PDAI$$

where PDAI is the probability of injury or death in the absence of intervention. For example, suppose that 25% of the perches are removed in a treatment-reference experiment. If field studies determine that mortality is 0.01 for the population and 0.015 for those living without the prevention (e.g., perches), then the prevented fraction is (0.015 - 0.01)/0.015 = 0.33. Thus, about 33% of the risk has been removed by removing 25% of the perches. This and the following analyses can be stratified by ages or sexes of concern.

It is important to remember that these three measures of effect remove emphasis from the risk to individuals and place emphasis on the risk to the population.

Measuring risk

Potential Observational Data

There are a limited number of parameters that can be measured during Level I studies. These studies normally will not use marked animals and the observational methods will not allow estimation of absolute abundance. However, observational data can be used to estimate use, which can be considered an index to abundance, where the parameter measured is an observation of an individual bird over some specific time period. Individual behaviors also can be quantified. Observations of use can be classified according to activity, and thus used to estimate the amount of time a particular species spends perching, soaring, flapping, etc. If these activities can be related to risk, they can be used to test hypotheses regarding the impact of wind plants on bird species. For example, it may be assumed that the more time a species spends flying at heights encompassed by the rotor swept area of turbines, the more risk the species faces in a wind plant. Measures of use should allow a comparison of potential wind plant sites for differences in risk to bird species. The season of use can indicate the relative abundance of migrants, wintering birds, and breeding populations.

Because many birds migrate at night, estimates of nocturnal use are of interest, but suitable methods are still in the developmental stage. To date, however, there is no strong evidence that large-scale kills are occurring at night. Some concern has been raised that bats might be killed by colliding with turbine blades. Cooper (1996) describes the use of radar for wind plant-related research. Gauthreaux (1996b) describes advances in the use of radar and infrared techniques in the study of bird use and migration. Recent work by Evans and Mellinger (1999) and Evans and Rosenberg (1999) describe the use of acoustic monitoring to estimate species composition of nocturnal bird migration. These “remote sensing methods” of bird study may provide tools for use in the estimation of impact in the future. Presently they seem most useful in early screening of wind resource areas for potential conflicts with birds similar to the study described by Hawrot and Hanowski (1997).

Mortality is the primary indicator of negative impact to individual animals from a wind plant. Mortality can be calculated from an estimate of fatalities. To use carcasses in assessing a wind plant as a cause of fatalities, all carcasses located within areas surveyed (regardless of species), should be recorded and a cause of death determined, if possible (The U.S. Fish and Wildlife Service may assign a cause of death for legal purposes). Not all carcasses will be whole animals. The condition of each carcass found should be recorded using condition categories such as:

- *Intact* - carcass that is completely intact, is not badly decomposed, and shows no sign of being fed upon by a predator or scavenger.
- *Scavenged* - entire carcass that shows signs of being fed upon by a predator or scavenger or a portion(s) of a carcass in one location (e.g., wings, skeletal remains, legs, pieces of skin, etc.).
- *Feather spot or feather tract* - 10 or more feathers at one location indicating predation or scavenging.

The estimated time of death, season of death, and species can be important in interpreting fatalities. There is always the possibility that death was not caused by striking the turbine, so care should be taken in assigning a cause of death (e.g., shooting, poisoning). In certain situations a blind necropsy may be indicated.

In addition to carcasses, observers may discover live birds that cannot fly or have other physical abnormalities due to collisions with turbines or other injuries. These birds should be captured and examined to determine the cause of injuries. For injured birds that cannot be captured, the species, location, and physical abnormalities observed should be described in the data. Injured birds should be treated in accordance with the appropriate laws and regulations.

Impacts of wind plants on reproduction can be measured. In Level I studies the most common measure of reproductive performance will be through nest surveys. For example, the number and distribution of active nests within an area potentially impacted by the placement of wind turbines over time represents an index to the status of the breeding population of raptors. The area influenced by wind turbines will extend varying distances depending on the size of the area utilized by individuals of the species of interest. Passerines may range only a few hundred meters while raptors can range 20 or more kilometers. For species with multiple nest structures the number of breeding pairs is of more value than occupied nests when evaluating breeding population status. Other factors that are changing within and around the wind development, such as roads, housing, and recreational activities, might also impact birds and should also be considered in any analysis.

Nesting surveys for smaller species such as passerines, some shore birds, and ground nesting birds are best accomplished on foot using ground surveys (Ralph et al., 1993). Unless the area is completely covered, previously described sampling protocols should be followed. For larger species, such as raptors, study areas should be surveyed initially when possible by air, preferably by helicopter, during the height of the nesting period. Aerial surveys should be followed immediately by ground surveys to confirm the species and status of each observed nest. Ground visits to occupied nests should be continued, to confirm the number of young fledged. Surveys should begin early enough to detect early nesters, such as eagles, and continue until all species of interest have begun nesting activity.

Empirical data on nesting pairs should be collected for all species of interest. In addition, the numerous reproductive parameters should be estimated to augment empirical data. The number of occupied nests within the defined area can be used to estimate relative abundance of nesting species potentially affected by the wind turbines. The following nest and territory parameters are suggested:

- *Occupancy rate* - the number of occupied territories (nests) per number of territories (nests) checked.
- *Breeding pair density* - the number of breeding pairs per area surveyed.
- *Reproductive rate* - the number of reproductive pairs per number of occupied territories.
- *Fledging success rate* - the number of pairs fledging young per number of reproductive pairs.
- *Breeding rate* - the number of young fledged per number of reproductive pairs.

Statistical comparisons of these parameters, if sufficient data exist, can be made among assessment and reference areas before and after construction.

Data on the above parameters will contain numerous biases, most of these related to the sampling method, data collection methods used (e.g., radar, visual, etc.), and observer and detection biases. Biases associated with sampling methods have been discussed previously. Biases associated with data collection methods may be found in numerous reference publications including Bibby et al. (1993), Buckland et al. (1993), Bookhout (1994), Edwards et al. (1981), Gauthreaux (1996a), and Reynolds et al. (1980).

Selection of Impact Indicators

Impact indicators should allow for the determination of impact following generally accepted scientific principles. Stakeholders should believe that the criteria for determination of impact will be satisfied by the indicators at the end of the assessment period. Of course, other indicators that are believed to provide useful information for analysis or for corroboration of results also should be measured. In the end, studies should be designed to:

- quantify indicators that will allow convincing arguments that impacts did or did not occur
- quantify the magnitude and duration of the impact with acceptable measures of precision and accuracy
- allow for standardized comparisons among populations and with results of other studies.

In an ideal world, a study of birds and wind plants would involve a direct count of birds using or passing through the wind plant, behaviors putting birds at risk, and a count of fatalities caused by wind turbines and related facilities. To count birds and behaviors one would need to identify individual birds. To count fatalities one would need to detect carcasses before removal by scavengers and be 100 percent confident of the cause of death. This level of effort is not possible in Level 1 studies.

As an alternative, studies of wind energy/bird interactions must rely on estimation of parameters that allow the test of hypotheses. These parameters are often expressed as rates, similar to epidemiological studies. Mayer (1996) provides an excellent discussion of the use of epidemiological measures to estimate the effects of wind plants and related facilities on bird species. He points out the importance of selecting the appropriate denominator when developing a rate for use in comparisons of effect. For example, a comparison of the number of bird fatalities per turbine among portions of a wind plant, between two turbine types, or among several wind plants, is much more meaningful if an estimate of bird abundance is added to the denominator.

There are a limited number of parameters that one can measure in a Level 1 study. The more likely parameter candidates and some potential risk indices are listed here and described below.

- bird utilization counts
- bird utilization rate
- dead bird search
- bird mortality
- removal rate
- observer bias
- detection bias.

There is little doubt that the presence of a wind plant will increase the risk of individual bird fatalities. This may be of great concern if the individual birds at risk have some special significance, as in the case of an extremely rare species. Risk of individual fatalities may be of interest when planning the design or location of a new wind plant, evaluating differences among turbine types, or when making modifications in equipment. *However, the risk of individual fatalities may not necessarily represent a risk to a population of birds*. Studies of risk to individuals and populations require separate study designs. Normally, Level 1 studies will be designed to make direct statistical and deductive inference to risk to individuals and indirectly indicate risk to populations. Level 2 studies normally will be needed to estimate risk to populations.

Metrics Definitions

Bird utilization counts. Utilization counts are indices of relative abundance among plots, areas, and seasons. Utilization counts represent observations of individual birds from an observation point or transect conducted repeatedly over some time period to document behavior and relative abundance of birds using the area. The observer counts the length of time the bird is within the plot and estimates “bird minutes” of use. The bird utilization counts allow comparisons among defined time periods (e.g., seasons, migration periods, or years), and areas. Bird activities should include behaviors which could be related to risk of injury or mortality from wind plants and might include flying, perching, soaring, hunting, foraging, height above ground, and behavior within 50 meters of WRA structures, etc. In situations of high bird density where it is impossible to keep track of all birds in a plot, use can be estimated for the observation period by making instantaneous counts repeatedly during the counting time period.

Bird utilization rate. This term refers to the number of birds observed or the number of bird minutes recorded per count period and/or survey plot. Like bird utilization counts, bird utilization rate may be used for comparisons among plots, areas, and seasons. One formula for utilization rate is

$$\frac{\text{\# birds observed}}{\text{time or time and area}} = \text{Bird Utilization Rate}$$

Utilization rates within specified distances of wind plant structures (e.g. large and small turbines, different tower types, etc.), subdivided on the basis of relevant environmental covariates (e.g. topographic features, vegetation edge, nesting structures, etc.) can be derived from the bird utilization counts. Rates can be developed for species, taxonomic groups, all birds observed, natural communities, seasons, distance from nearest turbine, turbine type, and other variables. Rates can be calculated for specific behaviors and risk can be evaluated in terms of the number of birds observed exhibiting behaviors that place them at greater risk. For example, birds flying at heights within the range of the rotor swept area are likely at greater risk than those consistently flying at heights above and below the rotor swept area. Evaluation of risk based on behavioral data can be used in a variety of studies of wind energy including relative comparisons of areas, turbines, and species. The choice of a utilization rate is critical; see discussion below.

Dead bird search. Searches are conducted in a defined area with complete coverage to detect bird fatalities. The number of dead birds found at each search site (e.g., a 50-meter diameter circle centered on the bird utilization count site) is documented. Information is collected which will aid in analysis later in the study. This may include bird species, sex, age, estimated time since death, cause of death, type of injury, distance and direction to nearest turbine, and distance and direction to nearest structure.

Bird mortality. The number of dead birds documented per search site may be termed “bird mortality.”

This is the rate of fatalities. Examples of indices for bird mortality are:

$$\frac{\text{\# dead birds}}{\text{turbine}}, \quad \text{and} \quad \frac{\text{dead birds}}{\text{unit rotor swept area}}$$

Removal rate. This is the rate at which bird carcasses are removed by scavengers or by other means (e.g., human removal), resulting in their loss to detection by the dead bird search. Information about removal rates is necessary when estimating the total number of dead birds in a given area. The results are used to adjust the number of dead birds detected. This rate may be determined by placing a known number of bird carcasses at randomly chosen locations and monitoring them for removal. Removal rates can be calculated as a rate or rate/area. This allows for comparison of removal levels between different locations or subareas within the WRA. If not detected, significant removal rate differences would result in misleading bird risk rates. If removal rates in different areas within the same WRA or between WRAs are equal, they will have no effect when computing and comparing mortality rates, bird risk rates, and attributable risk rates.

Observer bias. Observer bias is a quantification of the observer's ability to find dead birds or detect live birds. One study might quantify the observer's ability to find dead birds when a known number of birds are placed in the search area. Another study might compare the field crew's live bird observations in order to determine inter-observer differences.

Detection bias. Detection bias is a measure of the differences in detection probability due to topography and vegetative structure. Detection bias may be determined through a designed study which includes placing a known number of dead birds in a variety of locations with differing topography and vegetative structure. The detection success can be quantified and the probability of detection determined.

Defining Utilization

If risk is defined as the ratio of dead or injured birds to some measure of utilization, then the choice of the use factor, or denominator, is more important than the numerator (number of dead or injured birds). In fact, the treatment effect is usually small relative to the variability that would arise from allowing alternative measures of risk. The choice arises from the preliminary understanding of the process of injury or death. For example, should the denominator be bird abundance, bird flight time in the plant, bird passes through the rotor plane, or some other measure of use? Unless these measures are highly correlated with death — which may be unlikely — then the measure selected will result in quite different measures of mortality. Further, the choice of denominator should express the mechanism causing the injury or mortality. If it does not, then it cannot be used to accurately measure the effectiveness of a risk reduction treatment. There is, however, much uncertainty in the mechanism(s) leading to bird fatalities in wind plants.

Choice of utilization factor. Suppose that bird use or abundance is selected as the denominator, with bird deaths as the numerator, and painted blades as the treatment. A treatment-reference study determines that death decreases from 10 to 7 following the treatment, but use also decreases from 100 to 70 (arbitrary units). It thus appears that the treatment had no effect because both ratios are 0.1 (10/100 and 7/70). There are numerous reasons why bird use of a wind plant could change (up or down) that are independent of the blade treatment; for example, changes in prey availability, deaths on wintering grounds, environmental contaminants, change of land use, and so on. Thus, unless it can be established that there is a direct link between the number of birds using the area and flights near a turbine, this study may be seriously flawed. Recording bird flights through the rotor plane of painted blades would have yielded a more correct measure of effect. In addition, the use of selected covariates can help focus the analysis on the treatment effects. Naturally, the hypothetical study noted above should be adequately replicated if implemented. (See chapter 3 for recommendations on study design.)

Surrogate utilization variables. Utilization is an indicator of the level of at-risk behavior. Thus, adopting a measure of utilization requires the assumption that the higher the utilization, the higher the fatalities. It is, of course, prohibitive from a practical standpoint to record every passage of a bird through a zone of risk (be it a rotor plane or the overall wind plant). Further, it is usually prohibitive to accurately census the population and tally all deaths. Researchers must usually rely on surrogate variables to use as indices of population size and death. A *surrogate variable* is one that replaces the outcome variable without significant loss in the validity or power of the study. For example, researchers might use the number of birds observed during 10-minute point counts (i.e., the number of birds counted during a 10-minute observation period) as a measure of utilization (for either a treatment or reference case).

Once a measure of mortality is chosen, a measure of effect must be selected. This measure could be the *risk ratio*, defined as the ratio of mortality in one area (e.g., wind plant) to that in another area (e.g., reference). Thus, if mortality in the wind plant is 0.01 and that in the reference area is 0.001, the risk ratio is 10; the relative (potential) risk of death is 10 times greater for a randomly chosen bird in the site versus one in the reference area. Ideally, such a study should be adequately replicated, because references are not perfect matches to their associated treated sites. An alternative is to use one of the measures of attributable risk described above. These measures have the advantage of combining relative risk with the likelihood that a given individual is exposed to the external factor. This results in the proportional change in the risk of injury or death attributable to the external factor. Whereas the risk ratio ignores the absolute size of the risk, the use of attributable risk implies that the importance of the risk is going to be weighed by the absolute size of the risk.

REVIEW OF WIND ENERGY PLANT HAZARDS TO BIRDS

Direct Interactions

Collision with Turbine

The specific factors causing bird deaths in wind developments are not well understood. It has been proposed that birds die when trying to pass through the rotor plane because they cannot see the blades, turbulence, or because they are fixated on a perch or prey item beyond the blades. Birds might also be killed by striking turbine support structures (wires), or by striking part of a tower, or through electrocution by a turbine-related power line.

Layout of Wind Plants

To maximize operation time, turbines often are placed on ridges and upwind slopes. Such locations place turbines near updrafts that are commonly used by soaring birds, including but not restricted to raptors. It has also been suggested that turbines placed near valleys and end-row turbines might result in relatively higher risk to individual birds. The spacing and height of turbines also could interact to change the relative risk to individual birds. Thus, the micrositing of a turbine could be influencing avian risk, and doing so in a complicated manner that includes turbine height and spacing, location along a ridge, and the relationship to other turbines.

Plant Operation

The presence of the turbine, even with stationary blades, could increase risk to individual birds, especially in periods of poor visibility (fog, rain, night, dusk or dawn). Obviously, if birds have difficulty seeing blades, then operating a turbine during poor visibility would likely increase the risk of death for individual birds. In addition, operating during peak periods of migration, such as during spring and fall, could increase the absolute number of bird deaths simply because of the large number of individuals passing through the area. To date, however, few small, migratory birds have been shown to be killed in wind plants. We emphasize that these factors are untested hypotheses and should not be taken to represent management recommendations.

Indirect Interactions

Changes in Species Habitat

Central to understanding how a development such as a wind plant affects animal habitat is a proper understanding of the term itself. Unfortunately, "habitat" is a commonly misused term in ecology. First, habitat is a species-specific concept. That is, an area is neither "good" nor "poor" habitat *per se* except with reference to a specific species. Part of the misunderstanding comes from the term "habitat type" — originally developed for use in vegetation ecology to describe the general type of vegetation in an area — being misapplied to describe an animal's habitat. The term "vegetation type" should be used to describe the vegetative community in an area; "habitat" should refer to the environmental characteristics used by a specific animal or group of species (see below). To avoid confusion between animal and plant ecology, the term "habitat type" simply should not be used in any context.

Habitat is a multifaceted concept encompassing:

- both the structural and floristic composition of vegetation
- any number of environmental factors that influence animals, including water, soil properties, salinity, temperature, and so forth
- competitors and predators
- quantity and quality of food
- various other factors.

The amount, or presence or absence of any one of these factors can render an area unsuitable, regardless of the status of the other necessary factors. For example, a location that seems otherwise ideal for a species might, in fact, be unsuitable because of the presence (perhaps unseen to the observer) of a predator.

Thus, it is critical that the observer has a firm understanding of the habitat requirements of a species before making recommendations regarding the lessening of a real or potential environmental impact, such as development of a wind plant. As briefly reviewed below, a host of factors can affect the number and behavior of animals in an area, both directly and indirectly.

Changes in Prey Quantity and Quality

The substantial changes to an area that accompany development often cause substantial changes in the abundance and distribution of potential food for animals. Developing a wind plant requires that a network of roads be constructed, that pads be formed for siting the turbines and support buildings, that powerlines be buried, and so forth. All of these operations disturb the soil, many of them in permanent ways. These activities thus alter the potential habitat for many species, likely lessening the amount and quality for some species, while increasing it for other species. A notable example of this in wind energy development is the probable enhancement of habitat for ground burrowing animals (e.g., squirrels, gophers) due primarily to the aforementioned soil disturbance. In many areas throughout the west, such disturbance has resulted in substantial increases in ground squirrel abundance (e.g., Salmon 1981, Smallwood et al. 1998). Any activity that also results in low grass height also enhances ground squirrel habitat, including fire, grazing, and mowing. Many other species of small mammal, including those nocturnal species seldom seen by people, also react (positively or negatively) to these changes in soil and vegetation. Some of these changes could be ephemeral, or can be eliminated through habitat restoration.

Soil disturbance, along with changes in vegetative structure, can also change the habitats available for a host of other species. For example, species of grassland birds react differently to different densities and heights of grasses and shrubs. Predators can be attracted to a disturbed area because of an increase in prey density, or might avoid the area because their prey has decreased, or because of the increased activity of people.

Thus, the response of animal species can usually be predicted within at least wide bounds depending on the specific conditions present following development. However, it must be remembered that the responses are species-specific, and gross generalizations regarding the impact of development on the animal community cannot be made. For example, increasing the abundance of raptors because prey are more available is not "good" if other species have declined or disappeared, and the goal was to maintain a predevelopment animal community.

Changes in Perches, Nest Sites, and Related Items

Wind plants may increase the number of perches potentially available to birds for perching and nesting. The increasingly common use of tubular towers (rather than lattice towers) is reducing the perching opportunities on generating equipment in wind plants. Although perching by raptors is most obvious because of their size, most birds perch above the ground, including even grassland ground-foraging species such as sparrows, meadowlarks, and larks. Birds perch for a variety of reasons, including to rest, to scan for predators, or to scan for prey. Thus, use of turbines as perches could enhance the habitat of an area for a species by increasing its hunting success and/or lowering its susceptibility to predation. As noted above, there is likely some cost/benefit to the bird that is species-specific, and also probably location (wind plant)-specific. That is, decreasing starvation by increasing hunting success, while also increasing deaths due to striking blades, could result in an overall reduction in population mortality. Evaluating attributable risk is an important component in examining the cost/benefit ratio.

RISK REDUCTION

Individuals from the wind industry and the scientific community as well as individual environmentalists and regulators have postulated that bird deaths can be reduced by modifying towers to reduce perching, painting disruptive patterns on turbine blades, modifying turbine spacing, and so on. Some have suggested, however, that statistically valid analyses of such treatments are not feasible because bird death appears to be such a rare event. While it may be argued that simply reducing bird use on and around towers is sufficient to conclude that treatments have been effective, the weakness of this argument is that changes in behavior could also cause increases in death even if the use around turbines has declined. (For example, a perch guard might successfully prevent birds from perching on the tower, but might also have the effect of causing a frightened bird to fly into the blades, indirectly resulting in the very death it was designed to prevent.) Further, without quantification of dead birds, no statements can be made regarding the influence of turbines on the abundance and dynamics of bird populations — unless the turbines displaced the population (see chapter 4). If the risk to an individual per visit to a turbine stays the same, then mortality (rate of bird death) has not been reduced even if fewer birds visit. Thus, the parameter used to quantify "visit" is an absolutely critical part of impact assessment.

Plant Siting

It seems intuitive that avoiding areas of concentrated bird activity would eliminate many potential bird-turbine problems. Surveys can be used to determine if a proposed plant site is located in areas of high nesting or seasonal density, or in the range of a threatened or endangered species. Using such data in the site selection and evaluation process can reduce the absolute number of bird deaths. Many existing wind plants have avoided areas of concentrated bird use.

On-site Reduction

There are two major possibilities for reducing the risk to birds on a developed site. First, the site itself can be made unsuitable for use by birds or a specific bird species, either directly through micrositing of the turbines, or indirectly through changes in habitat parameters (e.g., changing prey type or abundance). Second, the turbines themselves can be made unsuitable for use by birds (e.g., removing potential perches on lattice towers).

Micrositing includes the position of turbines relative to a ridge, spacing between turbines, distance from potential perch and nest sites, turbine location relative to vegetated gullies or water sources, and so forth. It is important, given the concern over turbine-caused bird deaths, that micrositing include consideration of biological factors.

The height of the turbine tower, as well as the length of the blade, also could function in bird deaths. Taller towers potentially could expose a narrower or wider range of birds to impacts, although little specific research has been conducted on this factor. In addition, the length of the blade changes the rotor swept area, thus potentially changing the opportunity for collisions (Howell 1997).

Birds exhibit variations in activity both within and between days. These variations can be quantified by developing activity budgets for species of concern. Based on such data, reliable models could be developed that predict times of maximum risk to birds

Development of a wind plant likely changes prey available to birds, both increasing food for certain species and decreasing it for others. Many of these changes can be predicted, and thus the response of birds to the changes anticipated. For example, it is likely that numbers of ground squirrels will increase because of soil disturbance and decreased grass height through vegetation management that often accompany wind plant development. Because squirrels are a central part of the diet of many large raptors, it is likely that an increase in squirrel abundance could attract certain raptors to the site. It has been well documented that birds use certain tower types as perches, potentially increasing the chances of bird death because of the proximity to the blades (e.g., Orloff and Flannery 1992).

STUDY DESIGNS

Basic Experimental Approaches

As outlined by Mayer (1996), there are four tasks that the investigator must accomplish when designing a study of wind energy/bird interactions. The logic is sequential and nested; each choice depends on the choice made before:

1. **Isolate the hypothesis of mechanism that is being tested.** For example, one might be testing the hypothesis that birds strike blades when attempting to perch on a turbine.

2. **Choose a measure of injury-death frequency that best isolates the hypothesis being tested.** The two components of this choice are to choose an injury-death count to use as a numerator and a base count (likely utilization) to use as a denominator. It is critical that a relevant measure of use be obtained (e.g., passes through the rotor plane; occurrence by flight-height categories; use within a certain distance of the turbine).

3. **Choose a measure of effect that uses the measure of injury-death frequency and isolates the hypothesis being tested.** The key is to decide whether the relative risk (risk ratio), attributable risk, or another measure of effect should be used.

4. **Design a study that compares two or more groups (12 is preferable) using the measure of effect applied to the measure of injury-death frequency chosen.** The goals here are to isolate the effect, control for confounding factors, and allow a test of the hypothesis. Replication is essential.

The ideal denominator in epidemiology is the unit that represents a constant risk to the bird. The unit might be miles of flight, hours spent in the site, or years of life. If the denominator is the total population number then we are assuming that each bird bears the same risk by being alive. In human epidemiological studies, the total population size is usually used because we cannot estimate units of time or units of use. In avian studies, however, actual population density is extremely difficult to estimate and entire populations are seldom at risk from the site. If the risk is caused by being in the area, then deaths per hour in the area is probably the best epidemiological measure in avian studies. It is then extrapolated to the population by estimating the utilization rate of the area for the entire population. Measuring utilization is difficult, however, and must be approached carefully (see chapter 3).

Thus, we have two major alternative ways to calculate mortality rate:

1. number of dead birds / number of birds in population

2. number of dead birds / bird use.

The first ratio may be the ideal, but as discussed above, is usually impractical. The second ratio is feasible, but will vary widely depending upon the measure of bird use selected. In addition, if bird use is chosen for the denominator, the background (non-wind site) mortality rate must also be determined for comparative purposes. Thus, use of ratio (2) should be the focus of further discussion.

Consultations with wind industry personnel have identified the need for a measure of operation time that is easily standardized among wind plants and turbine types, preferably one that considers differences in blade size and operation time. To meet these requirements, the concept of *rotor swept area* has been developed. This is simply the circular area that a turning blade covers. Rotor swept area is then converted to an index that incorporates operation time as follows:

rotor swept hour = rotor swept area x operation hours

An index of risk is then calculated by using a measure of risk:

rotor swept hour risk = risk measure/rotor swept hour

Here, "risk measure" could be flight passes through the rotor plane or any other appropriate measure of use (as discussed above). The length of the blade and speed of rotation are factors that could also influence risk (e.g., Tucker 1996).

Treatments and References (Controls)

Study Design

There are two main options for experimental units, using either the wind plant or a small plot as the basis for study.

Wind plant-based study. In this design, a relatively large portion of the wind plant serves as an experimental unit. For example, a group of 100 turbines would receive treatment (e.g., perch guards, painted blades), and a similar group of turbines would serve as a reference. This basic approach could be applied to both existing and planned wind plants. Unless preliminary studies are first conducted, an educated guess would be necessary to determine how many turbines to include in an experimental unit. Further, it will usually be difficult to replicate the pairs of experimental units if extrapolation to the plant is desired. With a few pairs (1, 2, or 3), this design is most comparable to a series of observational studies. Even if treatments are randomly assigned to one member of the pair, statistical inference is only to the pair and the protocol by which they were selected. With this design, however, extrapolation to the entire wind plant is subjective and possible only if one is willing to assume that the study sites are representative of the wind plant (i.e., that appropriate criteria were used to select treatments and references, and that the study is adequately replicated). This approach can be considered a model-based approach.

Small plot-based study. In this design, an individual turbine, or a small group (e.g., a string) of turbines serves as the experimental unit. For example, pairs of strings of turbines are randomly selected and one of each pair is randomly selected to receive the treatment. This design has the advantage of being centered on discrete units that can readily be observed; it is a design-based experiment. The greater the area of the plant sampled (replicated), the better the basis for extrapolating results to the plant, using deductive inference (professional judgment).

This design is preferred because of the relative ease of gaining an adequate sample size. A relatively large number of pairs of units can be analyzed in the sense of a "true" experiment. Extrapolation to the entire wind plant is possible if treatment and reference units are randomly sampled.

Design Considerations

Treatments and references can be reversed after the initial experimental period. This strengthens the test, and would be useful in the wind plant-based study because of the likely small number of replicates possible.

Variable Selection

One primary variable usually will drive the study design, and the initial sample size should be aimed at that variable. It is thus assumed that at least a reasonable sample size will be gathered for the other, secondary variables. There can be a sequential analysis of sample size as data are collected.

With the small, paired-unit design, one or two primary use variables (e.g., passes through the blade plane, perch attempts) will likely be adequate. The minimum number of pairs to be sampled should be as many as can be afforded but no less than 12. However, a greater number of pairs would be desirable, at least initially. The sampling unit can either be individual turbines, or strings of turbines. It is expected that string length will range from 5 to 10 turbines, depending upon the size and configuration of the wind plant. Portions of longer strings can be subsampled; however, this complicates the analysis.

Designing treatment vs. reference studies for inferences on measures of use is feasible. Determination of mortality is possible using Ratio (2) (see p. 74), but statistical power to conclude that treatment and reference sites have different mortality rates will be low. For example, in a randomized pairs design, most pairs are expected to result in zero mortalities, with tied values and no mortalities on either member of a pair. The high frequency of zero values effectively reduces the sample size for most analyses.

Case Study Approach

Case studies have high utility in evaluating mortality. Here, one collects dead birds inside and outside a wind plant, and conducts blind analysis to determine the cause of death. Unfortunately, under most situations very few dead birds will be found outside the site.

The case study approach suggests that epidemiological analysis can often be combined with clinical analysis to extend the inferential power of a study. Here the clinical analysis would be the necropsies of the birds. Suppose we are successful at finding dead birds inside a wind plant. If we look at *proportional mortality* — the proportion of the birds killed by blunt trauma, sharp trauma, poisoning, hunting, natural causes, etc. — then the proportions should differ significantly between the plant and the reference area. The assumption is that the differential bias in finding dead birds within the two areas is uniform across the causes of mortality and thus the proportions should be the same even if the counts differ (i.e., relatively few dead birds found outside the site).

Behavioral and Physiological Studies

Obtaining information on the sensory abilities of birds should help in designing potential risk-reduction strategies for wind plants and individual turbines. Although it may seem intuitive to paint blades so birds can more readily see them, there are many possible designs and colors to select from. For example, what colors can birds see, and how do birds react to different patterns? If painting blades causes a bird to panic and fly into another turbine, then painting has not achieved its intended goal. Many of these questions are best investigated initially in a laboratory setting. Unfortunately, translating lab to field is an age-old problem in behavioral ecology. Success in the lab using tame and trained birds does not necessarily mean success in the field, where a myriad of other factors come into play (wind speed and direction, fog, presence of other birds), and the physical scales are different. However, initial lab studies can help to narrow the scope of field trials. A sequential process of initial lab testing of treatments, followed by field trials, followed by additional lab trials as indicated, can be implemented.

Researchers under the direction of Drs. Hugh McIsaac and Mark Fuller, Boise State University, have been conducting a series of intensive laboratory trials to determine the visual acuity of raptors (unpubl. data). Included are trials to determine the ability of the birds to differentiate between different painted patterns on turbine blades. In addition, the McIsaac-Fuller research team has initiated field trials to determine the ability of trained but free-flying raptors to avoid painted blades. This research is an example of how combining laboratory and field experimentation can be conducted to address bird-wind interactions.

Statistical Concepts

Experimental Units

Pairs or groups of turbines with similar environmental conditions and/or breeding (nesting) densities for the species of interest (and, if possible, similar history of mortalities) will be the basic sets of experimental units. (Blocks of groups of turbines may improve the design if there are three or more treatments.) The number of turbines or strings of turbines will be based on the configuration of the site. The researcher should attempt to sample as many as the budget will allow but a minimum of 12 pairs of strings. Treatment should be randomly assigned to a member of each pair of experimental units (treatments are randomly assigned to experimental units in a block).

Sampling Frequency

The frequency of sampling (i.e., taking measurements at the experimental units) should be based on the goal and objectives of the project; initial sampling can be adjusted after preliminary data are analyzed. Sampling should be stratified by time so that adequate samples are taken both within and between days (temporal replication).

Stratification of sampling by major weather condition (high or low wind; clear or moderate to heavy fog) could be valuable, because weather could directly influence the frequency of bird strikes. However, stratification on weather would be very difficult, and could be initiated only if funds are available for the additional observers necessary to take advantage of such conditions. A more practical design would be to use weather as a covariate. It is highly recommended that designs be kept simple and focused, as it is far better to learn a lot about one treatment than it is to gain partial information on multiple factors. Sampling can cover a complete range of environmental conditions considered relevant to treatment effects. However, such an approach often increases variability and may mask treatment effects (unless effects are very large). See chapter 4 for a more detailed discussion of these issues.

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CHAPTER 3

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APPENDIX B:
Index of Key Terms

APPENDIX C:
National Wind Coordinating Committee Members and Key Participants

*= Steering Committee Members

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NEG Micon USA, Inc.

Dick Anderson*
California Energy Commission

Larry Bean*
Iowa Department of Natural Resources

Eric Blank
Land & Water Fund of the Rockies

R.T. "Hap" Boyd
Enron Wind Corporation

Matthew Brown*
National Conference of State Legislatures

Jack Cadogan
US Department of Energy Wind Energy Program

Kim Christianson
North Dakota Division of Community Services

Steven L. Clemmer*
Union of Concerned Scientists

Steve Corneli
Minnesota Attorney General's Office

Lisa Daniels
Windustry Project

Ed DeMeo*
Renewable Energy Consulting Services, Inc.

Stephen Dryden
FPL Energy, Inc.

Steve Ellenbecker*
Wyoming Public Service Commission

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FPL Energy, Inc.

Chris Flavin*
Worldwatch Institute

Larry Flowers
National Renewable Energy Lab

Larry Frimmerman*
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US Department of Energy Wind Energy Program

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Thomas Gray
American Wind Energy Association

Rick Halet
Northern States Power

Roger Hamilton
Oregon Public Utilities Commission

Ed Holt
Ed Holt & Associates

Karen Lane
Utility Wind Interest Group

Ron Lehr*
National Association of Regulatory Utility Commissioners

Chuck Linderman*
Edison Electric Institute

Annunciato Marino
Pennsylvania Public Utilities Commission

Ward Marshall
Central & South West Services

Garry Mauro
Texas General Land Office

Rudd Mayer
Land & Water Fund of the Rockies

Chuck McGowin
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Pam Nelson
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Jane Pulaski
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Green Mountain Energy Resources

Leda Roselle
Texas General Land Office

Richard Sedano
Vermont Department of Public Service

Tom Sloan
Kansas State Representative

Charlie Smith
Electrotek Concepts Inc.

Pat Spears
Inter-Tribal Council On Utility Policy

Randy Swisher*
American Wind Energy Association

Michael Tennis*
ReGen Technologies/All Energy

Steve Wegman
South Dakota Govenor's Office

Carol Werner
Environmental and Energy Study Institute

Steve Wiese
Planergy, Inc.

Jean Wilson
PacifiCorp

NWCC members include representatives from:

American Wind Energy Association

California Energy Commission

Central and South West Services

Lincoln County Economic Development

Ed Holt & Associates

Edison Electric Institute

Electric Power Research Institute

FPL Energy, Inc.

Greenmountain.com

Hawkeye Power Partners

Inter-Tribal Council on Utility Policy

Iowa Department of Natural Resources

Iowa State Legislature

Kansas State Legislature

Land & Water Fund of the Rockies

Minnesota Attorney General's Office

National Association of Regulatory Utility Commissioners

National Association of State Energy Officials

National Conference of State Legislatures

NEG Micon USA, Inc.

North Dakota Division of Community Services, Energy Program

Northern States Power Company

Ohio Consumer Counsel

Oregon Public Utilities Commission

PacifiCorp

Pennsylvania Public Utilities Commission

Planergy

ReGen Technologies/All Energy

South Dakota Public Utilities Commission

Windustry Project

Texas General Land Office

Texas State Energy Conservation Office

Union of Concerned Scientists

U.S. Department of Energy, Wind Program

Utility Wind Interest Group

Vermont Department of Public Service

Worldwatch Institute

Wyoming Business Council, Energy Office

Wyoming Public Service Commission

The NWCC is a collaborative endeavor formed in 1994 that includes representatives from electric utilities and their support organizations, state legislatures, state utility commissions, consumer advocacy offices, wind manufacturers and developers, power marketers, environmental organizations, and local, regional, state, tribal and federal agencies. The National Wind Coordinating Committee identifies issues that affect the use of wind power, establishes dialogue among key stakeholders, and catalyzes appropriate activities to support the development of an environmentally, economically, and politically sustainable market for wind power.

For additional information or to schedule a wind-avian or siting workshop, please contact:

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