Chapter 10
Synthesis: Advances in Wildlife Study Design

10.1 Introduction

In this chapter, we first briefly summarize our ideas on how to improve the way we pursue wildlife field studies through study design. We hope that our ideas, developed through the pursuit of many types of studies conducted under many different logistic and funding constraints, will serve to continue the discussion on improving scientific knowledge, conservation, and management of natural resources. We then provide the reader with a study guide for each chapter that serves as a reminder of the major points raised therein.

10.2 Suggestions for Improving Knowledge and Management

The underlying basis for wildlife research is the pursuit of knowledge about ecological systems. For this reason, researchers must understand the nature of the reality they study (ontology), the characteristics and scope of knowledge (epistemology), and what characterizes valuable and high quality research as well as value judgments made during the research process (axiology). Although there is no single prescriptive method of research in natural science, wildlife researchers employ certain intellectual and methodological approaches in common (see Chap. 1).

The goal of wildlife ecology research is to develop knowledge about wildlife populations and the habitats these populations use in order to benefit conservation. To attain this goal, wildlife ecologists draw from the fields of molecular biology, animal physiology, plant and animal ecology, statistics, computer science, sociology, public policy, economics, law, and many others disciplines when developing wildlife research studies. Using our knowledge of the species or system of interest, we ask important questions and generate hypotheses or statements about how we think the system works. We then draw on tools from many scientific disciplines to study, evaluate, and then refine our hypotheses about how ecological systems work, generate new hypotheses, ask new questions, and continue the learning process (see Table 1.2). It is critical that those implementing conservation, such as natural resource managers, also clearly understand the basics of sound methods of wildlife research; this knowledge is required to evaluate the quality of information available to them for making decisions.

Our review of wildlife study design and statistical analyses leads us to the following conclusions and suggestions for change. First, the field of ecology will fail to advance our knowledge of nature unless we ask important research questions and follow rigorous scientific methods in the design, implementation, and analysis of research and surveys. Natural resource management and conservation in general is ill served by poorly designed studies that ignore the necessity of basic concepts such as randomization and replication. More often than not, studies that ignore sound design principles produce flawed results.

Scientists must clearly elucidate study goals, and the spatial and temporal applicability of results, before initiating sampling. It is critical that managers determine how and where they will use study results so that results match needs. Researchers should carefully evaluate required sample size for the study before initiation of field sampling. Simple steps, such as sample size determination or power analysis, allow the researcher to evaluate the likely precision of results before the study begins. In this manner, researchers and natural resource managers alike can anticipate confidence in their decisions based on study results. Wildlife scientists require probabilistic samples and replication for all studies so that there is less chance that the results are biased and a greater likelihood that variation in the results can be attributable to treatment effects when they exist. Establishing replicates is often difficult in field situations, but scientists can usually achieve replication with planning. We must avoid pseudoreplication, however, so that natural resource managers do not make unsound decisions based on erroneous interpretations of data. If pseudoreplication is unavoidable (e.g., such as is often the case with isolated, rare groups of animals), we must acknowledge the implications of the sampling and account for it when interpreting results. Finally, we must interpret studies that do not employ probabilistic sampling and replication (all descriptive studies) critically. Although descriptive research can provide reliable data on such characteristics as typical clutch sizes for a given bird species, it generally cannot provide reliable data on more complex phenomena such as key factors limiting abundance of an endangered species.

In Sect. 10.3, we briefly summarize the primary points made in each of the previous chapters. We hope that these summaries will help flesh out the points made in Sect. 10.2 and refer readers back to the appropriate chapters more details where needed.

10.3 Summaries

10.3.1 Chapter 1 – Concepts for Wildlife Science: Theory

1. Wildlife scientists conduct research in the pursuit of knowledge, so they must understand what knowledge is and how it is acquired.
2. Ontologically, most wildlife scientists hold that there is a material reality independent of human thought and culture, while many social scientists and

humanists maintain that reality ultimately is a social construction because it is to some degree contingent upon human percepts and social interactions.

3. Several major perspectives toward the nature and the scope of knowledge, or epistemology, have developed in Western philosophy and each influence wildlife science to greater or lesser degrees. These include:

   - Empiricism
   - Rationalism
   - Pragmatism
   - Logical positivism
   - Postpositivism
   - Social constructionism

4. Regardless of the epistemological perspective one employs, logical thought, including inductive, deductive, and retroductive reasoning (Table 1.1), remains an integral component of knowledge acquisition.
5. At least three aspects of value or quality (axiology) influence wildlife science: scientific ethics, values researchers bring to their projects, and how both scientists and society determine the value and quality of scientific research.
6. Differences in ontological, epistemological, and axiological perspectives among natural scientists and many social scientists and humanists (e.g., postmodernists) have resulted in radically different interpretations of natural science since the 1960s as recently exemplified by "the science wars."
7. Because there is no single philosophy of science, there can be no single method of science either. Regardless, the natural sciences employ certain intellectual and methodological approaches in common (Table 1.2).
8. Because of the complex nature of scientific research, multiple researchers using a variety of methods often address different aspects of the same general research program.
9. Critiques of how natural science is conducted, written by scientists have helped wildlife researchers hone their study approaches; particularly regarding analytic methods.
10. Wildlife science commonly employs study designs (Fig. 1.1.) that are not consistent with Popper's (1959, 1962) falsification model of science (see postpositivism and the hypothetico-deductive model of science).
11. Epistemologically, wildlife science is better described by Haack's (2003) pragmatic model of natural science, where research programs are conducted in much the same way one completes a crossword puzzle, with warranted scientific claims anchored by experiential evidence (analogous to clues) and enmeshed in reasons (analogous to the matrix of completed entries). Under this pragmatic epistemology, truth, knowledge, and theory are inexorably connected with practical consequences, or real effects. The pragmatic model:

    - Permits any study design that can provide reliable solutions to the scientific puzzle (e.g., descriptive research, impact assessment, information-theoretic approaches using model selection, replicated manipulated experiments attempting to falsify retroductively derived research hypotheses, qualitative designs).
    - Does not imply that each of these study designs is equally likely to provide reliable answers to specific question in a given situation.

- Suggests that researchers must determine the best approach for each individual study given specific constraints; it does not provide or condone a rote checklist for excellent wildlife research programs.

12. Wildlife scientists use biological and statistical terms to represent various aspects of what they study. This sometimes can be confusing as the same word often is used in multiple contexts. Key biological and statistical concepts discussed in Chap. 1 and used in subsequent chapters include:

    - The term "significant" is particularly problematic as it can mean that something is biologically, statistically, or socially significant. Based on a particular study, not all statistically significant differences matter biologically, and just because we cannot find statistically significant differences does not imply that important biological differences do not indeed exist. Further, if wildlife scientists find something biologically significant does not imply that society will reach the same conclusion (and vice versa). Researchers must clearly stipulate what they mean by "significant."

13. Finally, we can divide wildlife studies into (1) those where the objectives focus on measuring something about individual animals or groupings of animals, (2) and those where the objectives focus on the habitat of the animal or group. This differentiation is critically important as appropriate study design hinges upon it.

10.3.2 Chapter 2 – Concepts for Wildlife Science: Design Application

1. Sound wildlife study design relies on the ability of the scientist to think critically when developing a study plan. Critical thought about the question of interest, the system under study, and potential methods for separating and evaluating sources of variation is necessary to ensure that we successfully define the causal and mechanistic relationships between variables of interest.
2. Disturbing variables limit our ability to examine the impacts of explanatory variables on the response variables of interest. Disturbing variables should be removed from the study through controlling for them by design using appropriate probabilistic sampling methods, or in the analysis by treating then as controlled variables or covariates.
3. Random selection of experimental study units permits us to use probability theory to make statistical inferences that extend to target population. Random assignment of treatments to study units helps to limit or balance the impacts of disturbing factors. Replication of experimental treatments is necessary to capture the full variability of treatment effects.
4. When developing a study, determine what type of design is most appropriate for the ecological question of interest. Determine whether a true experiment or quasiexperiment is feasible, whether a study is best suited to a mensurative approach, whether adaptive resource management is more appropriate, or whether the study is limited to description alone.

5. If conducting an observational study or sampling within experimental units, sampling design should account for variation over space or time and for the probability of missing subjects (e.g., animals, species, or plants) within a sampling unit.
6. Statistical inference methods are tools and should be treated as such. Generation of scientific hypotheses based on critical thought about the system and species of interest are paramount to developing defensible research studies. Estimation of population parameters, confidence intervals, tests for significance, and application of model selection should each be used, when appropriate, for evaluating scientific hypotheses. One should focus on statistical significance only when the observed biological effect is also deemed significant.
7. When sampling wildlife populations, the process of inference relies on using the appropriate sampling approach for the question of interest. The objective of inference is to extend the characteristics of the sample to the population from which it came by identifying the distribution of the estimator as it relates to the parameter of interest. We also should take additional properties of estimators into account when attempting to make inferences such as bias, precision, and accuracy.
8. Most wildlife research revolves around development of methods to assist with monitoring populations and evaluating those factors that influence population trajectories. Wildlife research requires not only well thought out questions, but also appropriate sampling designs that support the inference desired.
9. After project goals and data collection is accomplished, there is a wide variety of methods available for the analysis of ecological data. Programs for data storage, manipulation, and analysis are readily available and many are suitable for ecological data. However, the presentation of the results of ecological studies should be carefully considered, given the wide array of graphical methods available.
10. Finally, wildlife ecologists should identify and acknowledge to the extent possible any limitations on the strength or applicability of their inferences due to lack of randomization, replication, control, or violations of other statistical assumptions.

10.3.3 Chapter 3 – Experimental Designs

1. Wildlife studies may include manipulative experiments, quasiexperiments, or mensurative or observational studies. With manipulative experiments there is much more control of the experimental conditions; there are always two or more different experimental units receiving different treatments; and there is a random application of treatments. Observational studies involve making measurements of uncontrolled events at one or more points in space or time with space and time being the only experimental variable or treatment. Quasiexperiments are observational studies where some control and randomization may be

possible. The important point here is that all these studies are constrained by a specific protocol designed to answer specific questions or address hypotheses posed prior to data collection and analysis.

2. Once a decision is made to conduct research there are a number of practical considers including the area of interest, time of interest, species of interest, potentially confounding variables, time available to conduct studies, budget, and the magnitude of the anticipated effect.
3. Single-factor designs are the simplest and include both paired and unpaired experiments of two treatments or a treatment and control. Adding blocking, including randomized block, incomplete block and Latin squares designs further complicates the completely randomized design. Multiple designs include factorial experiments, two-factor experiments and multifactor experiments. Higher order designs result from the desire to include a large number of factors in an experiment. The object of these more complex designs is to allow the study of as many factors as possible while conserving observations. Hierarchical designs as the name implies increases complexity by having nested experimental units, for example split-plot and repeated measures designs.
4. ANCOVA uses the concepts of ANOVA and regression to improve studies by separating treatment effects on the response variable from the effects of covariates. ANCOVA can also be used to adjust response variables and summary statistics (e.g., treatment means), to assist in the interpretation of data, and to estimate missing data.
5. Multivariate analysis considers several related random variables simultaneously, each one being considered equally important at the start of the analysis. This is particularly important in studying the impact of a perturbation on the species composition and community structure of plants and animals. Multivariate techniques include multidimensional scaling and ordination analysis by methods such as principal component analysis and detrended canonical correspondence analysis.
6. Other designs are frequently used to increase efficiency, particularly in the face of scarce financial resources or when manipulative experiments are impractical. Examples of these designs include sequential designs, crossover designs, and quasiexperiments. Quasiexperiments are designed studies conducted when control and randomization opportunities are limited. The lack of randomization limits statistical inference to the study protocol and inference is usually expert opinion. The BACI study design is usually the optimum approach to quasiexperiments. Meta-analysis of a relatively large number of independent studies improves the confidence in making extrapolations from quasiexperiments.
7. An experiment is considered very powerful if the probability of concluding no effect when in fact effect does exist is very small. Four interrelated factors determine statistical power: power increases as sample size, α-level, and effect size increase; power decreases as variance increases. Understanding statistical power requires an understanding of Type I and Type II error, and the relationship of these errors to null and alternative hypotheses. It is important to understand the concept of power when designing a research project, primarily because such

understanding grounds decisions about how to design the project, including methods for data collection, the sampling plan, and sample size. To calculate power the researcher must have established a hypothesis to test, understand the expected variability in the data to be collected, decide on an acceptable α-level, and most importantly, a biologically relevant response level. Retrospective power analysis occurs after the study is completed, the data have been collected and analyzed, and the outcome is known. Statisticians typically dismiss retrospective power analysis as being uninformative and perhaps inappropriate and its application is controversial, although it can be useful in some situations.

8. Bioequivalence testing, an alternative to the classic null hypothesis significance testing reverses the burden of proof and considers the treatment biologically significant until evidence suggests otherwise; thus switching the role of the null and alternative hypotheses. The use of estimation and confidence intervals to examine treatment differences is also an effective alternative to null hypothesis testing and often provides more information about the biological significance of a treatment.
9. Regardless of the care taken, the best-designed experiments can and many will go awry. The most important characteristics of successful studies were (1) they trusted in random sampling, systematic sampling with a random start, or some other probabilistic sampling procedure to spread the initial sampling effort over the entire study area and (2) they used an appropriate field procedures to increase detection and estimate the probability of detection of individuals on sampled units. It seems clear that including good study design principles in the initial study as described in this chapter increases the chances of salvaging a study when things go wrong.
10. Study designs must be study-specific. The feasibility of different study designs will be strongly influenced by characteristics of the different designs and by the available opportunities for applying the treatment (i.e., available treatment structures). Other, more practical considerations include characteristics of study subjects, study sites, the time available for the study, the time period of interest, the existence of confounding variables, budget, and the level of interest in the outcome of the study by others. Regardless of the environment within which studies are conducted, all protocols should follow good scientific methods. Even with the best of intentions, though, study results will seldom lead to clear-cut statistical inferences.
11. There is no single combination of design and treatment structures appropriate for all situations. Our advice is to seek assistance from a statistician and let common sense be your guide.

10.3.4 Chapter 4 – Sampling Strategies: Fundamentals

1. Clearly define issues that influence sampling organisms in an ecological system, including study objectives, study area, range of the target population, and period of interest.

2. Probability sampling in wildlife studies is necessary to use inferential statistics and the resulting data are used to estimate those parameters for the population of interest such that those values can be generalized across the population under study and hopefully to the target population. Estimators represent the mathematical formula used to determine the parameters of interest in a population.
3. Clearly define the area of inference, the experimental unit, the sampling unit, and the sampling frame. Consider the species of interest when constructing plots for sampling, as species life history should influence plot shape and size.
4. Nonprobabilistic sampling, while common, results in potentially unbiased estimates for population parameters and the biases can seldom be estimated. Probabilistic sampling provides a process by which sampling units are selected at random, thus providing a basis for statistical inference.
5. Use a probability sampling plan for short-term studies and only stratify on relatively permanent features such as topography; use a systematic sampling plan for long-term studies and studies of spatial characteristics of the study area, spread sampling effort throughout area and time intervals of interest, and maximize sample size. Systematic sampling with a random start provides a close approximation of the statistical properties of a simple random sample.
6. Model-based approaches may provide less costly and logistically easier alternatives to large design-based field studies. Data analysis can improve the quality of the information produced by these studies; however, one should not ignore fundamentally flawed design issues and limited statistical inference.
7. In model-based analysis, have the model in mind as the sampling plan is developed. In a designed-based sampling plan, clearly define the parameters to measure, and in studies of impact or tests of effect, select response variables that are relatively uncorrelated to each other, measure as many relevant covariates as possible, and identify obvious biases.
8. Maximize sample size within budgetary and logistical constraints.
9. Use model-based sampling when enumeration of variables of interest is difficult and the risk of bias is outweighed by the desire for precision. Model-based sampling also can be used to identify and evaluate nuisance parameters (e.g., variability in detection rate).
10. Incorporate designed-based estimates of parameters as much as possible in model-based studies.

10.3.5 Chapter 5 – Sampling Strategies: Applications

1. Wildlife populations and ecologies typically vary in time and space. A study design should account for these variations to ensure accurate and precise estimates of the parameters under study.

2. Various factors may lend bias to the data collected and study results. These include observer bias, sampling and measurement bias, and selection bias. Investigators should acknowledge that bias can and does occur, and take measures to minimize or mitigate the effects of that bias.
3. A critical aspect of any study is development of and adherence to a rigorous quality assurance/quality control program.
4. Study plans should be regarded as living documents that detail all facets of a study, including any changes and modifications made during application of the study design. As a rule of thumb, study plans should have sufficient detail to allow independent replication of the study.
5. Sampling intensity should be sufficient to provide the information needed and the precision desired to address the study objectives. Anything less may constitute a waste of resources.
6. Plot size and shape are unique to each study.
7. Pilot studies are critical: "Those who skip this step because they do not have enough time usually end up losing time" (Green 1979, p. 31).

10.3.6 Chapter 6 – Impact Assessment

1. "Impact" is a general term used to describe any change that perturbs the current system, whether it is planned or unplanned, human induced or an act of nature and positive or negative.
2. There are several prerequisites for an optimal study design:
   - The impact must not have occurred, so that before-impact baseline data can provide a temporal control for comparing with after-impact data.
   - The type of impact and the time and place of occurrence must be known.
   - Nonimpacted controls must be available.
3. Impact assessment requires making assumptions about the nature of temporal and spatial variability of the system under study; assumptions about the temporal and spatial variability of a natural (nonimpacted) system can be categorized as in steady-state, spatial, or dynamic equilibrium.
4. Three primary types of disturbances occur: pulse, press, and those affecting temporal variance. Background variance caused by natural and/or undetected disturbances makes identifying the magnitude and duration of a disturbance difficult.
5. The "before–after/control–impact," or BACI, design is the standard upon which many current designs are based. In the BACI design, a sample is taken before and another sample is taken after a disturbance, in each of the putatively disturbed (impacted) sites and in an undisturbed (control) sites.
6. The basic BACI design has been expanded and improved to include both temporal and spatial replication (multiple controls; use of matched pairs).
7. Designs classified under "suboptimal" are designs without pretreatment data and most often apply to the impact situation where you had no ability to gather

preimpact (pretreatment) data or plan where the impact was going to occur. After-only impact designs also apply to planned events that resulted from management actions, but were done without any pretreatment data.

8. The gradient approach is especially applicable to localized impacts within homogeneous landscapes because it allows you to quantify the response of elements at varying distances from the impact and each gradient provides a self-contained control at the point beyond which impacts are detected.
9. A serious constraint in the design of wildlife impact studies is the limited opportunity to collect data before the disturbance. The before period is often short and beyond the control of the researcher, that is the biologist has not control over where or when the disturbance will occur. In some cases, it may be possible to improve our understanding of potential temporal variation without studying for multiple years by increasing the number of reference sites and spatial distribution of study sites such that the full range of impact response is sampled.
10. Because of the unplanned nature of most disturbances, pretreatment data are seldom directly available. Thus, the task of making a determination on the effects the disturbance had on wildlife and other resources is complicated by (1) natural stochasticity in the environment and (2) the unreplicated nature of the disturbance. To some extent, multiple reference areas can improve confidence in the attribution of impact by allowing a comparison of the condition in the impacted area to a distribution of conditions in the unimpacted (control) population.
11. Epidemiological approaches, by focusing on determining incidence rates, lend themselves to applications in impact assessment. The choice of the use factor, or denominator, is more important than the numerator. The choice arises from the preliminary understanding of the process of injury or death. The ideal denominator in epidemiology is the unit that represents a constant risk to the animal.
12. Obtaining information on the sensory abilities of animals is a key step in designing potential risk-reduction strategies.

10.3.7 Chapter 7 – Inventory and Monitoring Studies

1. Inventory and monitoring are key steps in wildlife biology and management; they can be done in pursuit of basic knowledge or as part of the management process.
2. Inventory assesses the state or status of one or more resources, whereas monitoring assesses population changes or trends.
3. Monitoring can be classified into four overlapping categories (1) implementation monitoring is used to assess whether or not a directed management action was carried out as designed, (2) effectiveness monitoring is used to evaluate whether a management action met its desired objective, (3) validation monitoring is used to evaluate whether an established management plan is working, and (4) compliance monitoring is used to see if management is occurring according to established law or regulation.

4. Selecting the appropriate variable to inventory or monitor is a key aspect of the study design, and direct measures (e.g., population numbers) are preferred over indirect measures (e.g., indices of population parameters).
5. The length of monitoring studies depends largely on the process or variable being studied, the magnitude and rate of change in the variable, and the natural variability in the variable and important covariates. The appropriate length for some variables may exceed available resources, necessitating alternative approaches, namely (1) retrospective studies, (2) substituting space-for-time, (3) modeling, and (4) substitutions of fast for slow dynamics.
6. Monitoring effects of management actions requires a clear and direct linkage between study results and management activities, often expressed as a feedback loop.

10.3.8 Chapter 8 – Design Applications

1. Studies should follow this process:
   - Establish questions
   - Develop hypotheses and predictions
   - Design research
   - Choose variables
   - Choose recording methods
   - Establish acceptable level of precision
   - Prepare a detailed protocol that clearly lays out the design, the data to be collected, the sampling plan, sample sizes, the methods for data collection, and the anticipated analysis
   - Collect preliminary data
   - Make necessary adjustments in the study protocol
   - Complete final data collection
   - Conduct quality/quantity assurance
   - Conduct analyses/hypothesis testing
   - Interpret results
   - Report and/or publish results
2. Questions are developed using literature, expert opinion, your own experiences, intuition, and guesswork. A thorough literature review is an essential cornerstone of all studies.
3. Simply stating a hypothesis in no way guarantees that knowledge will be advanced. What is required is careful and thoughtful evaluation of the predictive value of any proposed hypothesis.
4. Delineation of the study population is critical to deciding how to sample the population so that statistical inference can be made to the target population. Making statistical extrapolation beyond the study population requires replication in multiple populations.

5. Proper distribution of sampling locations (e.g., plots, transects) is a critical aspect of all studies.
6. Variables must be selected that are expected to respond to the treatment being tested or be closely linked to the relationship being investigated. Even purely descriptive, hypothesis-generating studies should focus sampling efforts on a restricted set of measurements.
7. All proposed recording methods should be thoroughly reviewed for potential biases and degree of precision attainable.
8. Calculating sample sizes necessary to achieve specified statistical precision or power are essential aspects of study design.
9. All studies should begin with a preliminary phase during which observers are trained to become competent in all sampling procedures, sample size calculations are conducted, and recording methods are refined.
10. It is important to the successful completion of the study that a formal protocol that includes a program of quality assurance/quality control is instituted on both the data collection and data processing components to ensure that the execution of the plan is in accordance with the study design.
11. A weakness of most studies is the lack of a detailed protocol that results in ad hoc implantation of the study details and a failure to enter, proof, and analyze data on a continuing basis.
12. Conclusions must be drawn with reference to the protocol by which the study is conducted and the population of inference only.
13. Although you cannot change your initial study design after the study is completed, you can legitimately change the way you group and analyze your data within the limitations of your design. Thus, you may be able to a posteriori change your method of data processing and analysis (e.g., from a two-group treatment vs. no treatment to a gradient approach).
14. Because of design inadequacies (or insufficient or inappropriate samples), you might have to revise your initial your study goals, such as narrowing the scope and applicability of the study.
15. The fundamental resource necessary for success in any scientific study is leadership. Regardless of the rigor of the design and the qualifications of your assistants, you must provide a detailed study protocol and you must be able to train, encourage, critique the implementation of the protocol – and accept criticism and suggestions, and overall guide your study throughout its duration.
16. Report your results, preferably in a peer-reviewed scientific journal when the results warrant.

10.3.9 Chapter 9 – Education in Design and Statistics for Students and Professionals

1. All wildlife professionals should, at a minimum, be able to ask the proper questions needed to interpret any report or journal article. Such questions include

issues of independence, randomization, and replication; adequacy of sample size and statistical power; pseudoreplication and study design; and proper extrapolation of results.

2. We think that all natural resource professionals require, at a minimum, a good knowledge of the principles and underlying theory of study design and statistics. The applied approach, which minimizes formulas and mathematics, is adequate in many cases for interpretation of research results.
3. All students planning to receive graduate degrees should take at least a beginning course in calculus in preparation for advanced methods in study design and evaluation.
4. We recommend that, following the introductory two-semester statistics course, graduate students enroll in an experimental design or sampling design course.
5. The graduate student, especially the PhD student, would also be advised to take more advanced courses in such topics as spatial analysis, time series, nonparametrics or resampling statistics, or multivariate analysis. The appropriate choice of courses will depend on the student's emphasis; however, we recommend that PhD students take a graduate course in statistical theory, as it will lay the foundation for all other courses.
6. We also recommend that graduate students at least become familiar with basic modeling, including analysis of population, community, or landscape dynamics, and the estimation of the parameters associated with these models. Estimation of these parameters often involves techniques for adjusting for imperfect detectability of subjects of interest (e.g., capture–recapture methodology).
7. We recommend that all natural resource professionals, including managers, administrators, and regulators, possess the statistical training outlined for the MS graduate student, or at least avail themselves of the advice of a good statistician.
8. All professionals should have a personal library of journal reprints and books that are readily available for reference. We provide some suggestions for assembling a small personal library that provides reference for common study designs and statistical analyses.

In closing, we wish all of you great success with your current and future research endeavors. All work is important; but appropriately designed, analyzed, and interpreted work has the greatest impact. We hope that our book assists in some positive way with the advancement of knowledge and conservation.