ANGLER SURVEY METHODS

AND THEIR APPLICATIONS IN FISHERIES MANAGEMENT

Support for the publication of this book
was provided by

Sport Fish Restoration Act Funds

administered by the

U.S. Fish and Wildlife Service
Division of Federal Aid

ANGLER SURVEY METHODS
AND THEIR APPLICATIONS IN FISHERIES MANAGEMENT

Kenneth H. Pollock
Professor of Statistics, Biomathematics, and Zoology
Department of Statistics
North Carolina State University, Raleigh

Cynthia M. Jones
Associate Director,
Applied Marine Research Laboratory, and
Associate Professor,
Department of Biology
Old Dominion University, Norfolk, Virginia

Tommy L. Brown
Senior Research Associate and Leader,
Human Dimensions Research Unit
Department of Natural Resources
Cornell University, Ithaca, New York

American Fisheries Society
Special Publication 25

American Fisheries Society
Bethesda, Maryland 1994

The American Fisheries Society Special Publication Series is a registered serial. A suggested citation format follows.

Pollock, K. H., C. M. Jones, and T. L. Brown. 1994. Angler survey methods and their applications in fisheries management. American Fisheries Society Special Publication 25.

00 99 98 97 96 95 94 6 5 4 3 2 1

Library of Congress Catalog Card Number: 94-070490

ISBN 0-913235-88-1

ISSN 0097-0638

Printed in the United States of America

This publication is printed on acid-free paper

Published by the American Fisheries Society
5410 Grosvenor Lane, Suite 110, Bethesda, Maryland 20814, USA

This publication may be ordered from the American Fisheries Society
Publication Fulfillment, Post Office Box 1056, Evans City, Pennsylvania 16033, USA
Telephone (412) 772-2651

Dedication

We dedicate this book to Douglas S. Robson, Professor Emeritus, Cornell University. Doug has been a mentor for two of us, and he remains a valued colleague of us all. He has been a pioneer and leader in advocating the sound design and analysis of angler surveys for more than 30 years. Several of his papers are landmarks in the field.

Contents

Preface

Nearly all fisheries agencies in North America survey anglers and their catches in some way, on some scale. The methods vary from simple counts of creeled fish to sampling designs of great complexity; the scales range from small lakes or streams to an entire country. The aggregate annual cost of these surveys is in the millions of dollars, most of it paid for by anglers through license fees and equipment taxes. The number of angler surveys—and hence the public expenditure on them—has increased steadily over the past two decades as fisheries agencies have sought more and better information to cope with growing demands for finite recreational resources. The scope of surveys has grown as well, as social, economic, and political factors have assumed importance alongside traditional biological concerns in fisheries management. Few agency budgets, however, have kept pace with the demands placed on fisheries management over this period. The tension between information needs and fiscal realities has placed a premium on making surveys efficient in design and execution so that the data obtained will be as reliable as possible for the dollar spent. Research and field trials have produced marked improvements in existing survey designs and provided some new designs. Many of these improvements have been published in literature not normally scanned by fisheries workers, however, and others have remained poorly accessible in unpublished agency reports. A consolidated techniques manual for fisheries surveys has long been needed.

In recognition of this major gap, the American Fisheries Society (AFS) and the Division of Federal Aid of the U.S. Fish and Wildlife Service undertook a three-part program to produce a book of fisheries survey techniques. The first step was to convene an International Symposium and Workshop on Creel and Angler Surveys in Fisheries Management, which was held in Houston, Texas, on March 26–31, 1990. This conference brought 300 biologists, managers, statisticians, economists, sociologists, and theoreticians together for 5 days of intensive presentations and discussions, and it exposed a great deal of new research and recent experience relevant to fisheries surveys. The second step was peer review and publication of the symposium's 528-page proceedings, *Creel and Angler Surveys in Fisheries Management* (American Fisheries Society Symposium 12, 1991). The third and final step is publication of this techniques book, which draws heavily on work presented in the symposium and proceedings, as well as on other sources familiar to us.

A book on survey methods could be pitched to several audiences of greater or lesser specialization. We have chosen to address this one primarily to midlevel fisheries managers who have responsibility for survey administration within their agencies. Such people must know a great deal about all aspects of fisheries surveys. In particular, they must understand the elements of survey choice, design, execution, and analysis that determine whether or not objectives will be met to appropriate standards, on schedule, and within budget. They must know when to seek specialized expertise within or outside the agency, how to evaluate the expert advice received, and when (and if) cost-cutting compromises in design can be made. Surveys require both rigor and judgment, and we believe this book will allow survey managers to exert both.

Survey administrators are not the only people who can benefit from this book, however. We refer to "survey teams" throughout the text, for very few modern surveys can be handled by a single person. To a core group consisting of survey manager, planners and designers, and field and data-processing supervisors may be added biologists, regional managers, statisticians, economists, human dimensions experts, and others whose knowledge is relevant to particular surveys. The book demonstrates the broad context for their individual efforts, and it shows how their contributions must be integrated and coordinated for surveys to be successful. We also believe this book will be a useful teaching text at the graduate and midcareer levels.

Creating this book has been a team effort, as well. All three of us collaborated on the book's organization, and each of us critiqued the others' writings. Pollock was the overall coordinator and also wrote Chapters 1, 3, 5–9, 13, 14, 18, and 19. Jones wrote Chapters 10–12, and Brown wrote Chapters 4, 16, and 17. Pollock and Jones were coauthors of Chapter 15, and Pollock and Brown wrote Chapter 2 together.

We had a great deal of outside help. Pollock and Jones's long research collaboration with Douglas Robson (Cornell University) and John Hoenig (latterly of Canada's Department of Fisheries and Oceans) made writing this book much easier. Hoenig also made important suggestions for improving Chapter 18. Don Hayne and David Turner (North Carolina State University) have provided us and others with many important insights into creel survey design and analysis, and their contributions to state survey efforts in the southeastern United States over 30 years deserve special recognition. David Wade and H. Lakkis (Old Dominion University) and Scott Cone (North Carolina State University) generated many valuable ideas during their student days with us. Mark Holliday and John Witzig (National Marine Fisheries Service) provided helpful discussions about the Marine Recreational Fishery Statistics Survey. Barbara Knuth (Cornell University) and Stephen Weithman and John Stanovich (Missouri Department of Conservation) generously provided good examples of survey instruments for use in the book. Unpublished research results reported in Chapter 11 were supported by grants from the Chesapeake Bay Stock Assessment Committee and the National Marine Fisheries Service (#NA 89EA-H-00060), and Virginia Sea Grant (VGMSC-RIMG-91-2).

Earlier drafts of the book underwent extensive peer review, which was coordinated by the AFS editorial office. The following people read and commented on one or more chapters: Peter Bayley, Andrew Bindman, Edd Brown, Leon Carl, Michael Colvin, Jared Creason, Paul Cunningham, William Davies, Ronald Dent, Wolfgang Haider, Pamela Haverland, Bryan Henderson, Mark Holliday, Michael Hudgins, Nigel Lester, Stephen Malvestuto, Frank Martin, Gary Matlock, Earl Meredith, Christopher Nunan, Maury Osborn, Donald

Pereira, Steven Persons, Michael Petzold, John Stanovick, Thomas Steeger, David Van Voorhes, Stephen Weithman, Dan Witter, John Witzig, and Richard Wydoski. Their reviews were immeasurably helpful and influential; they caused us to reorganize and to essentially rewrite the book, which is considerably longer now than we originally envisioned. We thank all these reviewers, and we especially appreciate the contributions of Stephen Malvestuto, who continued to advise us through subsequent drafts. None of these people, of course, is responsible for errors or other shortcomings that may remain in the book.

We thank Robert Kendall and the AFS editorial staff for their hard work, commitment, and professionalism during the development of this manual. They are the unsung heros of so many fine publications, and we believe they deserve special recognition. Beth Staehle and Amy Wassmann handled the meticulous work of putting this book into production.

At North Carolina State University, Karla Nevils typed Pollock's many chapter drafts with patience and high standards. Without her, this book could never have been completed. Marjorie Peech provided administrative support for Brown at Cornell University.

Two of our spouses—Mary Watson Nooe (Pollock) and William Persons (Jones)—helped us through the difficult spots along the course of this project. They have earned far more than thanks.

Kenneth H. Pollock
Cynthia M. Jones
Tommy L. Brown

February 1994

Part I

OVERVIEW OF ANGLER SURVEYS

I add a word here about the hazards of copying sample designs and field instructions. There are no simple rule-books nor ready-made sample-designs, and there never will be.

—W. Edwards Deming (1960)

Chapter 1

Introduction

Fisheries agencies have but three tools to manage recreational fisheries: they can regulate harvests, they can stock fish, and they can enhance habitats (Matlock 1991). Angler surveys of sound design and implementation are necessary if all three tools are to be used effectively. Creel surveys have been used traditionally to estimate angler effort and harvest on a body of water. However, angler surveys now are being used much more widely, and they may involve telephone, mail, or aerial surveys in addition to the traditional on-site surveys. Opinion surveys may be used to evaluate angler attitudes toward harvest opportunities, seasonal closings, bag limits, stocking, habitat enhancement, and other management programs. Social and economic surveys help managers assess the value of fishing to anglers and to local and regional economies. Angler surveys in combination with other data may also be used to answer biological questions such as the contribution of fishing to total fish mortality.

Angler surveys are becoming very complex because they often have multiple objectives. They also can be costly, which brings them under agency scrutiny in times of tight budgets. Agencies should resist cutting corners on needed surveys, however. If a fishery with both recreational and commercial components is in decline, for example, an agency is likely to receive political pressure from both groups of fishers. In the absence of reliable survey data on relative harvest by the two groups, the agency may end up in court without defensible data to justify its decisions.

Angler surveys vary greatly in size and complexity. A small access point survey on an isolated lake might only involve one clerk and be funded modestly by a state agency. A survey on a large reservoir may combine aerial flights to obtain counts of anglers with access point interviews to obtain catch information. The survey might still be funded by a state agency but it would involve a much more substantial investment of personnel and money. The Marine Recreational Fishery Statistics Survey conducted by the National Marine Fisheries Service (Essig and Holliday 1991) samples marine anglers on all coasts in the United States several times every year by a combination of telephone and access point methods. The funding necessary is an order of magnitude higher than required for the surveys mentioned previously. The National Survey of Fishing, Hunting, and Wildlife-Associated Recreation is a still more complex survey of fishing and hunting participation, effort, economics, and demographics for the whole U.S. population. It has been conducted by the U.S. Fish and Wildlife Service (usually with the Bureau of the Census) every 5 years since 1955, and it has grown into a massive data collection and reporting effort that costs millions of dollars (Grambsch and Fisher 1991).

The needs of fisheries managers for information are likely to change and grow

I. OVERVIEW OF ANGLER SURVEYS
- Introduction (1)
- Planning (2)

II. BASIC PRINCIPLES
- Statistics (3)
- Questionnaire Construction (4)

III. ANGLER CONTACT METHODS
- Overview of Contact Methods (5)

Off-Site Surveys	On-Site Surveys
•Mail (6)	•Access (10)
•Telephone (7)	•Roving (11)
•Door to Door (8)	•Aerial (12)
•Logbooks, Diaries, and Catch Cards (9)	

- Comparisons (13)
- Complemented Surveys (14)

IV. APPLICATIONS
- Catch and Effort Estimation (15)
- Economic Analysis (16)
- Social and Market Analysis (17)
- Biological Analysis (18)

V. EPILOGUE

Figure 1.1 An overview of the manual's structure, showing major parts (Roman numerals) and chapters (Arabic numerals).

in the future. One important factor will be the changing demographics of the population. For example, there will be more older people living in and near cities who would benefit from strong urban fisheries programs. The types of angler surveys required will be affected by changes in age structure, wealth, and cultural diversity of the population.

State, provincial, and federal agencies in the United States and Canada fund many angler surveys with a variety of objectives and for a variety of reasons each year. Concern over whether these surveys were sound in design and analysis led to the March 1990 International Symposium and Workshop on Creel and Angler Surveys, held in Houston, Texas, to the proceedings of that conference (Guthrie et al. 1991), and to this manual on angler survey methodology.

The manual is comprehensive and suitable for use by fisheries scientists and managers. It is divided into four major parts: Overview (I), Basic Principles (II), Angler Contact Methods (III), and Applications (IV); there is also a brief Epilogue (V). Figure 1.1 provides an overview of manual structure down to individual chapter topics. Administrators and senior managers will gain from Parts I and V an appreciation of the resources necessary for proper surveys and a sense of what future survey capabilities may be. By browsing the rest of the book, they also will learn the characteristics of good survey work that should be demanded from the programs they approve. Survey managers, planners, and supervisory staff should read Parts I–III and V and the chapters of Part IV that relate to their survey objectives. The chapters in Part IV may be read in any sequence, but the chapter on Surveys for Biological Analysis (18) will be easier to master if the chapter on Catch and Effort Estimation (15) is read first. We have tried to keep this manual as accessible as possible to fisheries managers and administrators. To this end, Part II consists of chapters on basic statistical theory and questionnaire construction. We believe that most fisheries managers will be able to use Parts I–III and V with ease. The material in Part IV is more technical and hence more demanding.

To complete the overview, we next consider (in Chapter 2) the central ideas on planning, organization, and execution of angler surveys. These will establish a solid foundation on which to build the rest of the manual.

Chapter 2

Planning, Organization, and Execution of Angler Surveys

2.1 INTRODUCTION

To plan, organize, and execute an angler survey successfully is a demanding task. Even a routine monitoring survey requires staff time and financial resources that might stress a fisheries management agency. In this chapter we discuss how to be efficient in carrying out an angler survey so that the results have acceptable accuracy and precision at a reasonable cost. An overview of this chapter is given in Figure 2.1. Important elements in the process are start-up activities (Figure 2.2), sample selection, data collection, data manipulation, data analysis, and reporting of the survey results. We were assisted by unpublished notes of Brenda G. Cox and by material in Malvestuto (1983) as we prepared this chapter.

2.2 START-UP ACTIVITIES

2.2.1 Survey Objectives

Any angler survey is an information device that must be aimed toward specified questions, problems, or issues if the results are to be meaningful. Too often this principle is forgotten at least temporarily. If the survey is a traditional creel survey (designed to estimate only effort and catch or harvest) and if it is conducted by an agency experienced with surveys of this type, people may be tempted to "just go do it like we've always done it" to minimize planning. On the other hand, if a broader angler survey is needed, it is easy for the survey to become the end rather than the means of obtaining information to meet specific management objectives, and the survey team may be tempted to draft questions before the general study objectives have been adequately defined. In each of these cases, the lack of systematic planning may likely result in a research product that is not of the quality that it could have been for the resources expended.

Planning is facilitated if survey team members are clear about the role of the survey. Surveys do not make or provide decisions about management problems. They only provide information for use, with other inputs, by the decision makers. Many management and policy decisions are made on best judgment rather than on quantitative criteria (usually because few high-quality data are available). Nevertheless, the more closely the decision criteria can be specified in advance, the better the survey can be designed to address those criteria. Our experience indicates that preliminary meetings between the survey team and the managers or decision makers who require the information can clarify the decisions to be made and the information needed to make them.

COMPONENTS OF A SURVEY
•STARTING UP
•SAMPLE SELECTION
•DATA COLLECTION
•DATA MANIPULATION
•ANALYSIS
•REPORTING

Figure 2.1 An overview of the important components of planning and organizing an angler survey. Each component is specified further in subsequent figures.

2.2.2 Cost and Type of Survey

Once it is determined that a survey is necessary for some decision-making process, the next focus is the type of survey and the cost. The cost of the survey will depend to some degree on who does the survey, the type of survey (e.g., on-site roving, on-site access, mail, telephone), the sampling design, and the sample size. Relative strengths of different types of surveys are covered in Part III; statistical sampling principles are covered in Chapter 3.

For a traditional on-site creel survey, the type of survey may be obvious. For an off-site angler survey, it may not be obvious whether a telephone or mail

STARTING UP

- •SURVEY OBJECTIVES
- •COST PERMISSIBLE
- •SURVEY TYPE
  - Off-Site or On-Site
- •TIME FRAME
  - Planning
  - Conducting Survey
  - Analyzing Data
  - Reporting
- •WHO WILL CONDUCT SURVEY
  - In-House
  - University Research Group
  - Consulting Firm
- •LEGAL AND ETHICAL CONSIDERATIONS
  - Permits
  - Confidentiality and Anonymity

Figure 2.2 An overview of the important start-up activities for an angler survey.

survey is the best choice until the material sought from the survey is itemized and the total length of the questionnaire (usually called the "instrument" in survey work) and the complexity of individual questions are determined. Thus, the first step is to lay out the information needed for the survey and determine the types of questions (simple answer, multiple choice, ranking, scale, or some other type of format) needed to obtain that information. Then it can be determined whether a mail or telephone survey is best suited to the information needs, or whether the survey is one that can be conducted by either means. Some survey research units specialize in one type of survey or another.

Once the type and approximate length of the survey instrument are determined, it becomes possible to estimate the cost of the survey for a given sample size. Precise formulae for estimating sample size exist (see Chapter 3), but the final decision often involves a trade-off between higher precision and increased costs. For example, a regional biologist for a state fisheries agency may want to know how the anglers in his or her region would react to a change in size limits for a given species. A decision about implementing the change might be made if a 95% confidence interval for regional data were of specified width, implying a particular sample size. If this species were available in lakes as well as streams, however, the manager might also like the information broken down by lake and stream anglers. If the same sample were subdivided between the two fisheries, the two confidence intervals would be wider than the one overall interval, rendering a decision impossible. The sample size could be increased to regain the required precision, but at added cost. Thus, the manager may wish to consider several alternative specifications and the cost of each before choosing one.

2.2.3 Time Frame for Planning and Conducting a Survey

Several factors determine when a survey will be conducted; among them are time of the year when the survey population can be reached, time since the events of interest occurred (which affects memory recall), planning time needed to develop and pretest the survey instrument and train the staff, funding availability, and reporting deadlines. Compromises in survey design, implementation, and analysis often have to be made. It is important that any trade-offs between survey timing and the accuracy and precision of results be made consciously during the planning process.

The length of time required to carry out and analyze most types of surveys depends on the sample size, the number of staff available, the degree to which data are verified, and other factors. For this reason, it is impossible to provide an exact time frame for conducting a given type of study. Ideally, planning for a creel survey begins 6–12 months before the survey is implemented. If a telephone or mail survey is conducted in association with a creel survey to gain more complete trip information, it can be planned within this time frame as well. Major provincewide, statewide, or national angler surveys usually need more than 1 year of planning, because their objectives tend to be diverse, requiring input from many individuals, and they often involve a contract with a survey group outside the agency. Further, any survey conducted by a federal agency in the United States must be approved by the Office of Management and Budget.

For any survey, data processing can begin before data acquisition is completed—but only if data management and analysis protocols have been established in advance, the data processing staff has been scheduled to work on the study, and the staff has been told about any special considerations. Creel surveys range in length from a few days for a fishing derby to a year for assessment of seasonal fishing trends. Mail surveys take 6–8 weeks to conduct properly (including follow-up reminders) and perhaps another 2–4 weeks if a telephone follow-up survey of nonrespondents is conducted. Telephone surveys can usually be conducted within 2–4 weeks, the duration depending on sample size and number of staff.

The time required for data processing also depends on sample size and number of staff, but it probably can be completed in 4–6 weeks (except for very large surveys). The computer programs for data analysis can be written and tested, if necessary, with a portion of the data, but full-scale analysis obviously must wait until all data are entered into the computerized database. Most primary analyses can be completed within 1–2 weeks if they are planned in advance. Questions raised by initial findings, whether they relate to data validity or to unexpected findings needing further analysis, often add weeks more to the analysis.

As suggested above, survey analyses often can be completed within about 8 weeks of the time that data entry begins. The findings must then be summarized and a full final report must be written before the results are broadly useful to many people. Depending on the size of the study and the detail of the report, this process may take as little as a few days or as much as a month of staff time. The primary author frequently has other duties, so most reports take substantially more calendar days than actual writing days to complete.

Thus, the overall process of planning, conducting, analyzing, and reporting a study can easily take a year or more. Planning the entire survey process carefully is the best way to minimize the time required while maximizing the quality of the study. Portions of the planning process are ongoing and occur only one or two steps in advance of implementation. However, a general schedule of the entire process should be laid out in advance to determine when the study findings will be available to decision makers.

If a study needs 12 months from conception to the final report, several factors may prevent those 12 months from being consecutive. For example, it is difficult to conduct mail or telephone surveys during holiday periods, especially the period from mid-November (in the United States) to January, because people are preoccupied with other activities. Also, summer is not a good time to conduct such studies because people spend much less time at home and indoors where they are accessible to telephone or mail surveyors. Staff holidays and vacations (including those of contracted data processing staff) may interrupt activities at other times. Finally, if the survey is conducted by agency staff, other priorities may divert people from the project at various times.

Figure 2.3 depicts a timetable for a fall creel survey with a follow-up telephone survey. It is for illustration only; the times for any phase of a particular study may vary from those shown.

2.2.4 Who Will Conduct the Survey?

Whether the study is conducted in-house or not depends primarily on the expertise and staffing of the fisheries agency. Most fisheries agencies have staff with a basic understanding of creel survey methods, and agencies often conduct on-site surveys of river, lake, or limited coastal fisheries. This approach may be inadequate in two situations.

Date	Creel Survey Portion	Telephone Follow-Up
Jun 1-10	Hold agency meetings to discuss study. Get approval to proceed.	Contact university human dimensions researcher for guidance on setting up.
Jun 11-20	Determine availability of field staff; recruit any new staff needed. Choose survey methods and approximate sample sizes. Consult with statistician.	Begin contract procedures, if needed for university or other contractor. Determine specific objectives for telephone follow-up.
Jun 20-30	Continue work on methods and procedures. Draft field forms.	Determine sample size needed. Begin work on survey instrument.
Jul 1-15	Inactive - vacations.	Schedule use of telephone bank. Finish draft of survey instrument.
Jul 16-31	Do field check to make sure methods and procedures will work on site. Revise as necessary.	Inactive - vacations.
Aug 1-20	Hire any needed temporary agents. Provide training and orientation to study and site. Training includes interviewing and dealing with the public as well as biological techniques.	Obtain names and phone numbers of a few current anglers and pretest survey instrument. Modify as needed. Schedule data processing staff.
Aug 21-31	Implement creel survey Aug 25. Set up data processing procedure.	Print forms. Train phone interviewers. Set up data processing procedures.
Sep 1-10	Continue creel survey. Send first batch of names and addresses to telephone survey staff.	Start phone survey Sep 6.
Sep 11-30	Continue creel survey. Hold training session for data processing staff.	Hold weekly meetings with phone survey staff to discuss questions and problems. Hold training session for data processing staff.
Oct 1-Nov 30	Start data processing Oct 1. Meet periodically with data processing staff to discuss questions and problems. Write computer analysis programs in Nov. Discontinue creel survey on Nov 30.	Start data processing Oct 1. Meet periodically with data processing staff to discuss questions and problems. Write computer analysis programs in Nov.
Dec 1-31	Finish data entry. Outline report.	Discontinue phone survey Dec 15. Finish data entry. Outline report.
Jan 1-31	Complete analysis. Begin writing report.	Complete analysis. Begin writing report.
Feb 1-15	Complete draft report.	Complete draft report.
Feb 16-28	Complete report. Begin using findings in meetings as appropriate.	Complete report. Begin using findings in meetings as appropriate.

Figure 2.3 An example of a planning schedule for a fall creel survey with a telephone follow-up.

The On-Site Angler Survey is Complex. A large regional survey of a major estuary or a long section of coastline, for example, would tax most state, provincial, or even federal agencies.

The Survey Includes an Off-Site Component. Fisheries agencies often do not have a person trained in off-site survey methods (telephone or mail, usually) or, if they do, this person is too overworked to handle the survey alone.

In these cases at least part of the work must be contracted to a university research group or a private consulting firm. In general, whenever an agency lacks sufficient expertise, staff, or equipment to carry out all or part of the survey, outside assistance must be obtained. The assistance purchased can range from advice on the design of the survey to full responsibility for the design, analysis, and reporting of the survey. Whatever the contract scope, close coordination between the agency and the outside group is essential if the survey is to be successfully completed.

2.2.5 Legal and Ethical Considerations

2.2.5.1 Obtaining the Necessary Permits

Federal agencies in the United States that conduct any type of human surveys, including creel surveys, always need the clearance of the Office of Management and Budget (OMB), and they often need the clearance of their federal departments as well (e.g., Department of the Interior) before the survey can be conducted. The OMB requirement was put into effect in the 1970s to ensure that the privacy of the public will not be invaded by federal agencies without sufficient cause. Regulations give the OMB 60 days to complete a survey review and a 30-day extension when needed. The OMB reviews the overall purpose of the study and weighs the nature and wording of each question against the study's objectives.

The OMB regulations have at least two implications for federal agencies that might want to do fisheries research that involves anglers or other human populations. First, the regulations add at least 2 or 3 months to the time it would otherwise take to complete the survey, time that must be planned into the study. Second, the person in charge of the survey must develop the instrument carefully around specific objectives. Surveys often are expanded beyond the primary objectives to include a lot of "nice to know" questions. Such questions may not survive a department review, and they almost certainly will not survive the OMB review. Nonfederal agencies may be subject to state or provincial requirements similar to those of the OMB.

Survey grants and contracts from the federal government to universities and consultants may not require OMB clearance. Universities typically have "human subjects" committees that review surveys or survey procedures, however. These reviews are more limited than an OMB review, and they primarily address whether or not the survey will be carried out with integrity and in an ethical manner (concerns addressed in the next section). We suggest that anyone who conducts human surveys set up procedures that would pass a university "human subjects" review, whether this is required or not.

Carrying out an on-site survey often requires permits from landowners, marina operators, and other private persons, so good public relations are very important. If a state agency has such poor relationships with marina operators, for example. that some of them refuse access to agents, the survey may be doomed before it begins.

2.2.5.2 Confidentiality and Anonymity

In some human surveys, valid reasons may exist for identifying and associating the survey respondent with his or her data, although such associations would rarely be reported to the public. In a court case, for example, an opposing party may be given the right to examine the data and even to recontact a subsample of respondents to ascertain that the data are correct and were obtained legitimately. Generally, however, respondents can and should be promised confidentiality. Telephone and face-to-face interviewers know the identities of the respondents, of course, but respondents should be assured that only the interviewers and their supervisors will be able to connect data with identity, and that the connection will not be made public. Furthermore, interviewers should be told in the strongest terms that confidentiality is a strictly observed policy that is to be maintained no matter how interesting or unusual the information. In creel surveys, clerks may spot violations of fishing regulations. Even if the interviewer has the authority, we believe that a citation should not be issued in such a case because it violates the confidential relationship between interviewer and interviewee that is necessary to obtain accurate data. We do not condone law violations, but survey research and law enforcement should not be connected. At most, the interviewer should mention the violation (after the interview) and suggest that although this violation will not be reported, its continuation could lead to a citation from enforcement officers who are in the area. If an agency charges its surveyors to do enforcement, not only may data from a cited angler be jeopardized, but word is likely to spread among anglers that police are posing as interviewers to sneak up on people. One or two enforcement actions thus could jeopardize an entire survey and the public funds invested in it.

In mail surveys, questionnaires are usually numbered consecutively so that nonrespondents can be identified and sent reminder letters or postcards. Ideally, the staff member who checks in the responses and mails out the nonrespondent follow-ups is not the same person who enters the data. If this is not practical, returned questionnaires can be logged in and set aside for several days. When the data entry technician has forgotten the association between questionnaire and respondent, the data can be batch-entered into the computer. The logbook should not be referenced during data entry, of course. Once the anonymous data have been fully entered and there is no reason to recontact respondents, the separate file of names and addresses can be destroyed.

Implementing procedures such as those suggested above and rigorously following them will help the surveying organization obtain any clearances necessary to conduct surveys. Over time, these procedures will help establish the surveying agency or organization as reputable and trustworthy in public eyes. This will benefit other organizational functions as well as survey research.

2.3 SAMPLE SELECTION ACTIVITIES

Sampling choices critically affect the accuracy and precision of survey estimates (Figure 2.4). For a creel survey, the choices of days for interviewing, sections of a stream or lake to sample, time to begin interviewing, direction of movement of the interviewer, amount of time spent with each angler surveyed, and which anglers to survey (if the survey is not a census) all influence the efficiency of the sampling design and the degree of bias that may be present in the results. Sampling considerations for off-site surveys are usually less complex, but still require considerable understanding of sampling theory. Chapter 3 introduces sampling theory relevant to designing on-site and off-site surveys. If an agency or organization does not have a professional statistician or biometrician, we recom-

SAMPLE SELECTION

- •FRAME CONSIDERATIONS
- •CHOICE OF DESIGN
  - Simple Random
  - Stratified Random
  - Systematic Random
  - Multiphase
  - Others
- •SAMPLE SIZE DETERMINATION
- •SELECTION OF THE SAMPLE

Figure 2.4 An overview of the important sample selection activities for an angler survey.

mend strongly that the proposed sampling design be discussed with a university statistician before it is implemented.

2.3.1 Frame Considerations

In classical sampling theory, a complete frame is assumed; that is, all population units are presumed to be available so that a sample can include any member of the population. Frames are essentially lists—lists of all population members, all fishing access points, or all possible sampling days, for example. We briefly discuss frames here and treat them more fully in Chapters 3 and 5.

When off-site surveys are not follow-ups to on-site creel surveys, the sample usually must be obtained in advance from a fishing license file frame or from another list of anglers. Obtaining a probability sample from these sources may take considerable time, and it may not even be feasible in time to complete the survey. Most agencies still do not computerize fishing licenses. Records of license sales from dispersed sales outlets often go to an auditing and accounting bureau before they are returned to the fisheries agency. Thus, a year's license stubs may not all be available until nearly a full year later. Because of the amount of space they occupy, an agency may keep the stubs for only a short time if it is not saving them for a specific survey. Thus, it is important to determine at the outset of planning a study whether a license or some other sampling frame is available. Then, an appropriate amount of time must be allocated to obtaining the sample and getting it ready for the study. For example, names and addresses for a mail survey must be entered into a computerized data base from which mailing labels can be generated and on which a record of responses can be maintained.

The frame for on-site surveys is usually a list of fishing areas and fishing days in the fishing season. Sampling whole fishing days often is not feasible, in which case multiphase sampling (two or more phases) is used. First a sample of fishing days might be chosen, and then a portion of each sampled day is selected for fieldwork. Sometimes the day is divided into morning and afternoon, sometimes into early, middle, and late parts. The day length that can be sampled is affected by agency labor policies regulating workdays and workweeks. Treatment of weekend days and holidays also must be decided, as discussed in Chapter 3 and later chapters.

2.3.2 Choice of Design

Once an angler contact method (mail, telephone, access, roving, etc.) has been established and available frames have been considered for their adequacy, the next step in sample selection is to decide on the sampling design. Common designs used are simple random sampling without replacement, stratified random sampling, systematic random sampling, and multiphase sampling (Chapter 3). These all are versions of probability sampling—sampling when all possible samples have a known probability of being drawn. With simple random sampling, for example, all samples have equal probability of being drawn.

The design choice is often constrained by the contact method. For a telephone survey, the most practical design often is systematic random sampling starting at the front of a telephone directory or a list of licensed anglers. For a lake survey, the sampling frame likely will encompass space and time and require multiphase sampling (day, part days), stratification (day of week, area of lake, etc.), or both.

2.3.3 Sample Size Determination

The second step in sample selection is to decide on sample sizes. This may be quite complex, especially for multiphase sampling designs (How many days to sample? How many mornings or afternoons?). Sample size decisions will be based on the trade-off between desired levels of precision and the resources the agency can allot to the survey. Sample size choices are treated in Chapter 3.

2.3.4 Selection of the Sample

Once all the background decisions have been made (choices of frame, design, and sample size), the actual sample of angler contacts can be drawn. Doing so requires the use of some randomization device. Special software may be developed for large surveys so a computer can select the sample. In other cases it is more practical to select the sample manually with a table of random numbers.

Sometimes additional sampling units are specified that are used only if necessary. For example, an aerial survey design might specify two flights over a lake each month. To guard against loss of a flight due to weather or equipment problems, an extra flight could be drawn for use only if one of the primary flights had to be cancelled or aborted.

Once the sample has been selected, the sample list must be distributed to the appropriate survey agents. This probably is best done during training sessions, which are among the preparations for data collection.

2.4 DATA COLLECTION ACTIVITIES

2.4.1 Preparation

Many tasks have to be completed before the data collection system is in place (Figure 2.5).

Preparing Letters, Forms, and Measurement Protocol. The exact nature of the material to be prepared will depend on survey objectives, survey contact mode, and other factors. Questionnaire design is discussed in detail in Chapter 4. On-site surveys often use only brief questionnaires to establish effort and catch, whereas off-site telephone or mail surveys can have elaborate questionnaires.

DATA COLLECTION
•PREPARATION
Letters, Forms, and Measurement Protocol
Pretests of Survey Procedures
Training Materials
Training Agents
Data Processing System
Data Sheets
Direct Computer Input
Printing of all Materials
•OPERATION
Collection of Data
Troubleshooting
Recontacts to Reduce Data Discrepancies
Quality Control

Figure 2.5 An overview of the important data collection activities undertaken before and during an angler survey.

Pretesting Survey Instruments and Procedures. Pretesting is very important, because it is the only sure way to determine whether the survey instrument is clearly worded and well presented and whether the survey procedures are complete and unambiguous. Pretesting could be as simple as asking someone to review the whole questionnaire protocol. Ideally, it should incorporate a small pilot survey so that nothing is overlooked.

Preparing Training Materials. The survey leader must prepare written and pictorial presentations for the survey agents so that they understand clearly what is expected of them.

Training Agents. Agents should be trained as a group in a workshop, where they can receive instruction and ask questions. For many surveys, however, this will not be enough, and agents also will have to be trained on the job.

Choosing a Data Processing System. In a telephone survey, data often are directly entered into the computer by the agent as the interview proceeds. In a field survey, data traditionally are recorded on a set of paper forms (waterproofed in some way) and later transferred to a computer. Whatever system is practical, it needs to be well thought out. All equipment such as tape recorders used to collect data should be backed up in case of malfunction.

Field forms can be made machine-readable, read by optical scanning devices, and transferred with custom software directly into a computer data bank (Heineman 1991). Alternatively, field interviews can be keyed electronically into a field data recorder and later downloaded into a central computer (Hammarstrom 1991). Both methods save time and key-punching errors; the data are handled only once, by the agent who collected them.

DATA MANIPULATION
•DATA RECEIPT AND LOGGING
•COMPUTER INPUT
•EDITING AND CHECKING DATA FILES
•DERIVED VARIABLES
•MISSING DATA

Figure 2.6 An overview of the important data manipulation activities for an angler survey.

Printing Survey Forms, Letters, and Measurement Protocols. Document printing should be left until very close to the time data collection begins, because pilot studies and training sessions might point up the need for modifications. Printing should be attractive so that good public relations result.

2.4.2 Operation

Various important data collection tasks occur once the survey is in progress.

Troubleshooting. The survey supervisor should have no sampling duties and should be free to move around and help agents. Important decisions always have to be made as data are being collected, and agents may not be experienced enough to make them alone.

Recontacts for Discrepant Data. Sometimes it is worthwhile to recontact respondents if discrepancies are found in their data. Often, however, recontacts are impractical and discrepant observations may have to be viewed as missing.

Quality Control. Quality control is an extremely important task that is covered in detail in the chapters on various angler contact methods. Quality control must be used in all phases of the survey. It begins with excellent training for the agents and extends through checks on data collection (by unannounced visits to watch the agents at work), on the data themselves, and on data entry, data manipulation, and statistical analysis. We cannot emphasize too strongly that a survey can be useless, even if well designed in all other ways, if the data are of poor quality.

2.5 DATA MANIPULATION ACTIVITIES

Once the data have been collected, logged in, and entered into the computer, files need to be edited and checked for unusual values (Figure 2.6). Some checking can be done by software (searches for extreme values) and some is best done manually. After the data files have been checked, new variables may be derived mathematically from existing variables (e.g., catch per unit effort from effort and catch variables) and added to the data files. At this point, problems of missing data will have to be resolved. Sometimes a value is imputed mathematically for a missing value, and sometimes the data can be left missing.

ANALYSIS
- ANALYSIS PLAN
- SOFTWARE PROCEDURES
- ANALYTICAL OUTPUT
- MODEL BUILDING

REPORTING
- MANUSCRIPT PREPARATION
- MANUSCRIPT REVIEW
- MEETINGS AND CONFERENCES
- IMPROVEMENTS FOR LATER SURVEYS
- COMPARISON OF SIMILAR SURVEYS

Figure 2.7 An overview of the important analysis and reporting activities for an angler survey.

We do not comment on types of computer hardware or software both because our comments would quickly be obsolete and because surveys differ so much in size and scope. A small personal computer may be adequate for some jobs and a very large mainframe computer may be needed for others.

2.6 ANALYSIS

It is important to have a general analysis plan laid out in advance (Figure 2.7). This will aid in refining the design to improve the validity and precision of results, and it will speed the analysis itself.

Reputable statistical software should be used so that standard errors of estimates are provided. Sometimes statistical hypotheses will be tested as part of the analysis. Analytical output should include summary tables, graphs, and charts.

After the basic survey data have been analyzed, more sophisticated analysis can begin, if necessary, to develop models. For example, an economic evaluation may be carried out to assess the importance of a fishery to a local or regional economy (see Chapter 16).

2.7 REPORTING

After data analysis is complete, the results must be written up and reported at least to the sponsoring agency, and perhaps to a professional journal or other media as well (Figure 2.7). Adequate time must be devoted to this task. Once a rough draft has been completed, it should be seriously reviewed by several other fisheries scientists and managers. Comments by these reviewers should be incorporated in the final report.

Results also should be reported at meetings or seminars. Therefore, a verbal presentation with appropriate overheads or slides should also be prepared and reviewed carefully by others before the meeting. Sometimes a nonscientific report should be presented to fishing groups to aid in public relations. A report of this type is quite different from a scientific report and requires a lot of additional effort, but it is probably an essential feature of large agency surveys.

Suggestions for improvement in the survey should be recorded and archived to benefit the next similar survey. This should be an obvious step, but it is sometimes overlooked. Without such a record, some of the previous knowledge base will be lost as the staff changes, forcing the agency to continually relearn its survey techniques with little improvement.

Finally, it may be useful to analyze a current survey in relation to similar previous surveys of a water body or region. Reports of such comparisons can provide valuable information on fishery trends, but they seem to be done rarely.

Part II

BASIC PRINCIPLES

Chapter 3

Statistical Theory of Survey Sampling

3.1 BACKGROUND

3.1.1 Populations and Samples

Populations of interest to natural and social scientists are usually too large to be measured completely. Therefore the attributes of a population have to be inferred from a sample of that population. This is the basic tenet of statistical inference.

Biological (including human) populations are not homogeneous; the attributes of a population vary among its units. A variable attribute in the population is characterized by unknown quantities termed *parameters*. Important parameters include the *population mean,* the *population total,* and the *population variance* for quantitative variables (such as age, weight, or length) and the *population proportion* for categorical variables (such as the proportion of all fish larger than a given size). The attribute in the sample is characterized by the corresponding *sample statistics,* which are known after the sample is drawn and the attributes are measured. If the sample is drawn randomly from the population (that is, if each member of the population has an equal chance of being picked for the sample) the sample statistics are estimators of the population parameters; for example, the sample mean length would be an estimator of the population mean length.

3.1.2 Properties of Estimators

When sample estimators are used to infer attributes of the whole population, the reasoning is inductive and the conclusions are subject to uncertainty. This uncertainty can be stated in terms of probability, the basis of statistical inference. Therefore, the estimators are uncertain and we need to consider the important properties of an estimator, which are as follows.

Consistency. A consistent estimator is one that gets closer and closer to the true parameter value as the size of the sample increases. This is an essential property of any reasonable estimator.

Unbiasedness. An unbiased estimator is one whose average (or *expected*) value over many hypothetical repetitions of the study is the true parameter value. Ideal estimators have little or no bias.

Variance. The variance of an estimator is the average (or expected) value of the squared deviations of the estimator from its expected value. The smaller the variance of an estimator, the better.

Standard Error. The standard error of an estimator is the square root of the estimator's variance. The standard error is the measure of variability usually quoted, because it is in the original units of measurement; the variance is in square units and harder to interpret. The smaller the standard error, the better.

Precision. A precise estimator is one that has a small standard error (or variance). This is a desirable and almost essential property of an estimator. Estimators become more precise (i.e., they have a smaller standard error) as the sample size increases. Of course, increasing the sample size has a practical cost in time and money.

Mean Squared Error. A quantity that combines the concepts of bias and variance is the mean squared error of an estimator, the average of the squared deviations of the estimator from its true parameter value. The mean squared error (MSE) is equal to the variance (VAR) plus the bias (BIAS) squared:

$$\text{MSE} = \text{VAR} + (\text{BIAS})^2. \tag{3.1}$$

A good estimator has a small mean squared error so that, on average, the estimator is "close to" the true population parameter value.

Accuracy. An accurate estimator is one that has a small mean squared error. This implies that it has both little or no bias and a small standard error. Sometimes the term accuracy is used very loosely and misleadingly to imply low bias only.

Figure 3.1 is a graphical depiction of bias, variance, mean squared error, precision, and accuracy based on the analogy of a shooter firing at a target (Overton and Davis 1969; White et al. 1982). Bias is represented by how far, on average, the shooter's group of shots is off center. Precision is represented by how tightly the shots cluster around the shooter's average. Accuracy is represented by how tightly the shots cluster around the bull's-eye in particular, and it combines bias and variance as discussed earlier. Only case (a) in Figure 3.1 represents an accurate estimator: the shots are centered on the bull's-eye (unbiased estimator) and have a small spread (low standard error, precise estimator).

The ideal estimator is accurate because it is always consistent (it approaches the true parameter as sample size increases), has little or no bias (it averages close to the true parameter), and has a small standard error (it is precise). A desire for this ideal, however, has to be tempered by the financial, logistical, and staffing constraints of the study undertaken. For more detail on basic statistical theory and the properties of estimators, see White et al. (1982) or a basic statistics textbook (e.g., Ott 1988; Moore and McCabe 1993). A glossary of the important statistical concepts and notation used in this manual is given at the book's end.

3.1.3 Finite Population Sampling

The angler survey methodology developed in this manual is very broad, but all angler surveys involve taking a sample from a population of known size (a finite population). In this section we define the key concepts of finite population sampling from which the statistical theory of sampling designs can be developed.

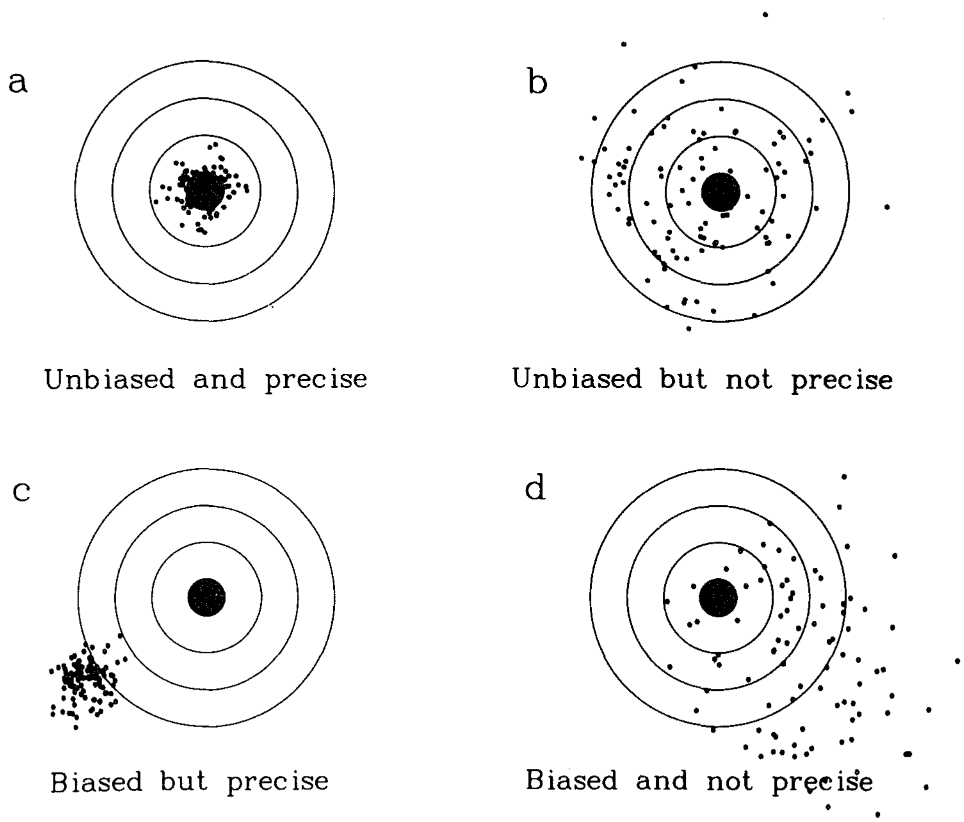


Figure 3.1 Targets and shot patterns to illustrate the concepts of bias and precision. If the bull's-eye represents a population parameter, an ideal estimator of the parameter gives values tightly clustered around the true value, as in pattern (a). Pattern (b) is less desirable, but the average (expected) value of the estimator still is close to the population value, and steps often can be taken to reduce the estimator's scatter (standard error). Biology differs from target shooting in that the population parameter value—the position of the bull's-eye—is not known in advance and it is rarely constant through time. Consequently, the undesirable patterns (c) and (d) are difficult to recognize when they occur; pattern (c) is a particular nightmare because its precision imparts a false sense of correctness. The use of sample data to infer population characteristics must be undertaken carefully. (Reproduced from White et al. 1982.)

Target Population. The target population is the population about which information is desired. For example, the target population for an opinion survey in Ontario might be all the anglers in Ontario.

Sampled Population. The sampled population is the actual population from which information is collected. All the anglers in Ontario might be the target population, but the sampled population might have to be licensed anglers in Ontario because sampling unlicensed anglers is too difficult or costly. The compromise from target population to sampled population must be decided for each survey.

Sampling Unit. The basic element of sampling is the sampling unit. In a mail survey, the sampling unit is usually a licensed angler. In an access point survey, the sampling unit may be a particular combination of place and time: Herb's Marina on Friday, February 13, for example.

Sampling Frame. The complete set or list of all the sampling units is called the sampling frame. The list of all licensed anglers in Ontario would constitute the sampling frame for the opinion survey mentioned previously. The ideal sampling frame is complete and represented by an actual list to make the sampling process easy to implement. In practice, the frame often is not ideal. Sampling units may be missing from the list or duplicated in it. A physical list of units may not even exist. For example, some states have not computerized their license files, and the mass of paper license stubs may be very difficult to sample. Not all frames are lists of anglers. The frame for an on-site access point survey may be a list of access points combined with a list of possible days to sample; the important point is that the list of place–time combinations must be complete. In many angler surveys, alternative frames may be available. For estimating catches of a trophy species, for example, the frame might be either a list of all anglers who bought a special license or a list of all times and places at which fishing for the species will occur. The survey designs based on these frames would be very different: the license file frame would support an off-site telephone or mail survey, and the spatiotemporal frame would support an on-site access point or roving survey. Frame issues are discussed further in Chapter 5 (Section 5.4).

Probability Sampling Procedures. A sampling procedure must be consistent with sound statistical principles or it will be impossible to establish the properties of the estimators obtained from the sample in terms of bias, precision, and accuracy. Samples drawn subjectively to cut costs or to be vaguely representative are useless. In this manual we will only consider sampling mechanisms based on *probability sampling,* whereby all possible samples have known probabilities of being drawn. This allows us to use statistical inference and probability theory to establish the properties of the estimators.

The simplest form of probability sampling is called simple random sampling without replacement, in which each possible sample has equal probability of being drawn or each sampling unit has the same probability of being included in the sample. Simple random sampling theory is considered in Section 3.2. Subsequently sections treat stratified random sampling, systematic random sampling, two-stage (cluster) sampling, and sampling with nonuniform probability. After some examples to illustrate the sampling designs, we conclude Chapter 3 with a brief discussion of the important but difficult topic of finding the variance of an estimator.

3.2 SIMPLE RANDOM SAMPLING WITHOUT REPLACEMENT

3.2.1 Background

Sampling without replacement requires that after a sampling unit is drawn for a sample, it is not replaced in the pool of possible units and hence cannot be drawn again. Simple random sampling without replacement means that each sampling unit has an equal chance of being drawn at the first stage. At the second stage all remaining unselected units have an equal chance of being drawn, and so on at the third and subsequent stages. Simple random sampling without replacement also means that all possible combinations of units have an equal chance of constituting the chosen sample. An example illustrates this equivalent of definitions.

Consider a population of $N = 6$ anglers, numbered 1, 2, . . ., 6, from which a sample of $n = 2$ units (anglers) is to be drawn randomly without replacement. Any of the six anglers has one chance in six of being selected as the first sampling unit. Each of the five anglers remaining in the pool has one chance in five of being selected as the second unit. The overall probability that a particular sample of two anglers will be drawn in a particular order is $(1/6) \times (1/5) = 1/30$. That 30 possible ordered samples are available can be confirmed by listing them: {1,2}, {1,3}, {1,4}, {1,5}, {1,6}, {2,1}, {2,3}, . . ., {6,5}. For purposes of survey sampling, however, the order in which sampling units are selected does not matter; only the combination is important. In this example, each combination can be drawn in two ways (e.g., {3,5} and {5,3}), so the number of qualitatively different samples is $30/2 = 15$ and the probability of drawing any one of them is 1/15.

A general formula for random sample probability that accounts for redundancies when units are not replaced and the order of unit selection does not matter is

$$\text{probability} = \frac{n}{N} \times \frac{n-1}{N-1} \times \frac{n-2}{N-2} \times \ldots \times \frac{1}{N-n+1}.$$

In the present example, by this formula,

$$\text{probability} = \frac{2}{6} \times \frac{1}{5} = \frac{2}{30} = \frac{1}{15},$$

as already demonstrated.

We represent population size—specifically size of the sampled population—by N. Surveys are always designed so that N is known. The N sampling units in the population define the sampling frame. In Section 3.1.3 we pointed out that frames may be lists of anglers in off-site surveys or lists of places and times to sample in on-site surveys.

Members of the population (anglers, place–time combinations, etc.) are numbered 1, 2, . . ., N. An attribute (variable) measured in the population is denoted y, and its values for individual members is $y_1, y_2, \ldots, y_N$; more generally, it is y_j for the jth member. Only very rarely can y be measured on all members of the population (a census). Logistic, personnel, or funding constraints usually mean that only some of the y_j's can be studied (a sample). We represent our sample values by $y_1, y_2, \ldots, y_n$; they are a subset of size n from the population of size N. The fraction of the population actually sampled is denoted $f = n/N$.

3.2.2 Estimation of Population Totals, Means, and Variances

The usual *population parameters* of interest for an attribute are population total (Y), population mean ($\bar{Y}$), population variance (S^2), and population standard deviation (S). Their corresponding *sample estimators* are $\hat{Y}, \bar{y}, s^2$, and s. Formulas for these parameters and their estimators are presented in Table 3.1. The properties of the estimators are now discussed briefly.

Bias. For simple random sampling, the expected or average values of $\hat{Y}$, $\bar{y}$, and s^2 are equal to the respective population values, so these three estimators are

Table 3.1 Parameters and sample estimator formulas for simple random sampling without replacement.

Measure	Population	Sample
Units	$y_1, y_2, y_3, \ldots, y_N$	$y_1, y_2, y_3, \ldots, y_n$
Total	$Y = \sum_{j=1}^{N} y_j$ $= (y_1 + y_2 + y_3 + \ldots + y_N)$	$\hat{Y} = \frac{N}{n} \cdot \sum_{j=1}^{n} y_j = N\bar{y}$ $= \frac{N}{n}(y_1 + y_2 + y_3 + \ldots + y_n)$
Mean	$\bar{Y} = \sum_{j=1}^{N} y_j/N$ $= (y_1 + y_2 + y_3 = \ldots + y_N)/N$	$\bar{y} = \sum_{j=1}^{n} y_j/n$ $= (y_1 + y_2 + y_3 + \ldots + y_n)/n$
Variance	$S^2 = \left[\sum_{j=1}^{N} (y_j - \bar{Y})^2\right]/(N-1)$ $= \frac{(y_1 - \bar{Y})^2 + (y_2 - \bar{Y})^2 + \ldots + (y_N - \bar{Y})^2}{N-1}$	$s^2 = \left[\sum_{j=1}^{n} (y_j - \bar{y})^2\right]/(n-1)$ $= \frac{(y_1 - \bar{y})^2 + (y_2 - \bar{y})^2 + \ldots + (y_n - \bar{y})^2}{n-1}$
Standard deviation	$S = \sqrt{\left[\sum_{j=1}^{N} (y_j - \bar{Y})^2\right]/(N-1)}$	$s = \sqrt{\left[\sum_{j=1}^{n} (y_j - \bar{y})^2\right]/(n-1)}$

unbiased (Cochran 1977:22). The sample standard deviation has a small negative bias with respect to the population standard deviation (i.e., s is smaller than S, on average).

Standard Error. The variance and standard error (SE) of $\bar{y}$, the sample mean, are

$$\mathrm{Var}(\bar{y}) = \frac{S^2}{n}(1-f) = \frac{S^2}{n}\left(\frac{N-n}{N}\right) \tag{3.2.1}$$

and

$$\mathrm{SE}(\bar{y}) = \frac{S}{\sqrt{n}}\sqrt{(1-f)}, \tag{3.2.2}$$

f being the fraction of the population sampled. This variance is estimated by replacing the population variance S^2 with the sample variance s^2.

If the sample is drawn from an effectively infinite population or drawn with replacement, the standard error is $S/\sqrt{n}$ (Cochran 1977, Chapter 2). The factor $(1-f) = (N-n)/N$ is called the *finite population correction factor,* and it reflects how much smaller the variance is when sampling is done without replacement. It makes intuitive sense that the variance is less for sampling without replacement because there will always be n distinct units, each providing information. When sampling units are replaced they can be drawn again, yielding no new information. The finite population correction can be ignored if f is small (i.e., $f < 0.1$).

The estimator of the population total is $\hat{Y} = N\bar{y}$ (Table 3.1). The variance and standard error of $\hat{Y}$ are

$$\begin{aligned}\text{Var}(\hat{Y}) &= N^2\text{Var}(\bar{y}) \\ &= N^2 \frac{S^2}{n}(1-f) \qquad (3.3.1)\end{aligned}$$

and

$$\text{SE}(\hat{Y}) = N \frac{S}{\sqrt{n}}\sqrt{(1-f)}. \qquad (3.3.2)$$

Again, these are estimated by substituting s for S.

Confidence Intervals and Sample Size Guidelines. The estimators considered here have approximately normal distributions. Once the standard errors have been obtained, this near normality can be exploited to produce confidence interval estimators (Cochran 1977, Chapter 2). For example, a 95% confidence interval for parameter θ would be $\hat{\theta} \pm 1.96\ \text{SE}(\hat{\theta})$, where $\hat{\theta}$ is an estimator ($\bar{y}$ or $N\bar{y}$ in this case). A 99% confidence interval has the multiplier 2.576 and a 90% confidence interval the multiplier 1.645 instead of 1.96. The confidence interval implies that if repeated samples were drawn from the population, 95% of the intervals computed would include the parameter θ.

Confidence intervals can also be used in planning a desirable sample size. Suppose survey planners specified a 95% confidence interval for $\bar{Y}$, the population mean, with specified half width d. Then the confidence interval is

$$\bar{y} \pm 1.96 \sqrt{\frac{N-n}{N}}\ \frac{S}{\sqrt{n}}$$

and

$$d = 1.96 \sqrt{\frac{N-n}{N}}\ \frac{S}{\sqrt{n}}.$$

If the planners ignore the finite population correction factor ($[N - n]/N$) in the first instance and solve the equation in d above for n, the only unknown,

$$n_o = \frac{(1.96)^2 S^2}{d^2}.$$

More generally,

$$n_o = \frac{z^2 S^2}{d^2} \qquad (3.4)$$

where z is the appropriate value from the normal table ($z = 1.96$ for a 95% confidence interval, 2.576 for a 99% interval, and 1.645 for a 90% interval). If there is a substantial sampling fraction (i.e., $n_o > 0.1N$), the planners cannot ignore the finite population and should use

$$n = \frac{n_o}{1 + \frac{n_o}{N}}. \qquad (3.5)$$

To use these equations the planners need a rough estimate of S^2 from some source such as a prior study or a small pilot study.

Assume the planners intend to survey a small population of $N = 500$ and they have a preliminary estimate of $S^2 = 100$ from a previous study. They want a 95% confidence interval estimator for $\bar{Y}$ with half width $d = 2$ units. They first ignore the finite population correlation and calculate

$$n_o = \frac{(1.96)^2 S^2}{d^2} = \frac{(1.96)^2 \times 100}{2^2} = 96.04,$$

or 97 sampling units (calculated sample sizes are always rounded up to the next whole unit). Because n_o exceeds $0.1N$ (it is $0.19N$), the finite population correction cannot be ignored and the planners should use

$$n = \frac{97}{1 + \frac{97}{500}} = 81.23$$

or 82 sampling units.

3.2.3 Estimation of Population Proportions

In an angler opinion survey based on simple random sampling, it may be important to estimate the proportion of a population that has a certain opinion on a subject, such as whether or not the state agency is doing a good job in managing a particular fishery. Suppose the population of N sampling units includes A units having the particular opinion; further suppose the random sample of n units includes a units having that opinion. Then $P = A/N$ is the population proportion and $p = a/n$ is the random sample proportion:

	Population	Sample
Total size	N	n
Opinion size	A	a
Proportion	$P = A/N$	$p = a/n$

The sample proportion, p, is an unbiased estimate of the population proportion, P. The variance and standard error of p are

$$\text{Var}(p) = \frac{P(1-P)}{n}\left(\frac{N-n}{N-1}\right) \qquad (3.6.1)$$

and

$$\text{SE}(p) = \sqrt{\frac{P(1-P)}{n}\left(\frac{N-n}{N-1}\right)}, \qquad (3.6.2)$$

and they are estimated by substitution of p for P. The equation for Var(p) is very similar to the one for var($\bar{y}$) (equation 3.2); here, $(N - n)/(N - 1)$ is the finite population correction for sampling without replacement. The term $\sqrt{P(1-P)/n}$ is the standard error of a binomial proportion in basic statistical theory.

Suppose some survey planners want a confidence interval for P with specified half width d. The same approach used to derive equations (3.4) and (3.5) can be used again, so

$$n_o = \frac{z^2 P(1-P)}{d^2} \tag{3.7}$$

if the finite population correction factor is ignored. For a substantial sampling fraction the finite population correction cannot be ignored and

$$n = \frac{n_o}{1 + \frac{n_o}{N}} \tag{3.8}$$

should be used. Recall that z is the appropriate value from the normal distribution.

To use these equations, the planners need a rough estimate of P from some source such as a prior study or a small pilot study. When no estimate of P exists, the maximum required sample size can be obtained by using $P = 0.5$, but this default may increase survey costs substantially.

Consider a population of $N = 2{,}000$ anglers and a preliminary estimate of $\hat{P} = 0.3$ and specify a 90% confidence interval for P with half width $d = 0.02$ ($\hat{P} \pm 0.02$). Then

$$n_o = \frac{(1.64)^2 P(1-P)}{d^2} = \frac{(1.64)^2 \times 0.3 \times 0.7}{(0.02)^2} = 1{,}420.66$$

Without a finite population correction, $n_o = 1{,}421$ sampling units (anglers) would be needed. However, a correction is advisable because of the large sampling fraction, so

$$n = \frac{1{,}421}{1 + \frac{1{,}421}{2{,}000}} = 830.75$$

and a sample size of 831 sampling units (anglers) is required. The finite population correction allows a much smaller sample size here because this population is quite small and the sampling fraction is large.

3.2.4 Estimation of a Ratio

Estimation of the population ratio of two different random quantities presents some additional challenges. An example is an angler mail survey in which the surveyors ask for total expenditure on fishing in the last month. In addition to estimates of total expenditure and total trips for the population, an estimate of expenditure per trip would be important. This ratio can be estimated in two ways ($\hat{R}_1$, $\hat{R}_2$) from a simple random sample of the population.

	Population	Sample
Expenditure	$y_1, y_2, \ldots, y_N$	$y_1, y_2, \ldots, y_n$
Trips	$x_1, x_2, \ldots, x_N$	$x_1, x_2, \ldots, x_n$
Expenditure per trip	$R = \dfrac{\bar{Y}}{\bar{X}} = \dfrac{\sum_{j=1}^{N} y_j}{\sum_{j=1}^{N} x_j}$	$\hat{R}_1 = \dfrac{\bar{y}}{\bar{x}} = \dfrac{\sum_{j=1}^{n} y_j}{\sum_{j=1}^{n} x_j}$
		$\hat{R}_2 = \dfrac{\sum_{j=1}^{n} (y_j/x_j)}{n} = \dfrac{\sum_{j=1}^{n} r_j}{n} = \bar{r}$

The traditional ratio estimator (Cochran 1977:30), $\hat{R}_1$, is the ratio of the two sample means. It is the estimator usually favored because it has less bias than $\hat{R}_2$ (the mean of individual ratios), especially if the x_j's can be small. These two estimators will be considered in more detail in later chapters.

Consider a sample of $n = 6$ units with

$$y_1 = \$150, y_2 = \$24, y_3 = \$77, y_4 = \$81, y_5 = \$102, \text{ and } y_6 = \$31;$$
$$x_1 = 4, x_2 = 2, x_3 = 1, x_4 = 2, x_5 = 5, \text{ and } x_6 = 1.$$

Then

$$\bar{y} = \sum_{j=1}^{n} y_j/n = (150 + 24 + 77 + 81 + 102 + 31)/6 = 465/6 = 77.5,$$

$$\bar{x} = \sum_{j=1}^{n} x_j/n = (4 + 2 + 1 + 2 + 5 + 1)/6 = 15/6 = 2.5,$$

and

$$\hat{R}_1 = \bar{y}/\bar{x} = 77.5/2.5 = 31.0.$$

In comparison,

$$r_1 = 150/4, r_2 = 24/2, r_3 = 77/1, r_4 = 81/2, r_5 = 102/5, \text{ and } r_6 = 31/1;$$

thus

$$r_1 = 37.5, r_2 = 12.0, r_3 = 77.0, r_4 = 40.5, r_5 = 20.4, \text{ and } r_6 = 31.0;$$

and therefore

$$\hat{R}_2 = \bar{r} = (37.5 + 12.0 + 77.0 + 40.5 + 20.4 + 30.1)/6 = 218.4/6 = 36.4.$$

In this example the two estimates of expenses/trip, $\hat{R}_1$ = \$31.00 and $\hat{R}_2$ = \$36.40, are not very close.

3.3 STRATIFIED RANDOM SAMPLING

3.3.1 Why Stratify?

Dividing a population into homogeneous strata may reduce the variance of an estimator of a population mean or total. Consider a small example originally given by Barnett (1974), who posed a finite population of $N = 20$ members in which y takes values

$$6, 3, 4, 4, 5, 3, 6, 2, 3, 2, 2, 6, 5, 3, 5, 2, 4, 6, 4, 5.$$

The population mean is $\bar{Y} = 4$; the population variance is $S^2 = 40/19$. A simple random sample of size $n = 5$ gives, via equation (3.2.1), $\text{Var}(\bar{y}) = 6/19$:

$$\text{Var}(\bar{y}) = \frac{S^2}{n}\left(\frac{N-n}{N}\right) = \frac{40}{19} \times \frac{1}{5} \times \frac{20-5}{20} = \frac{40}{19} \times \frac{1}{5} \times \frac{15}{20} = 6/19.$$

Values of $\bar{y}$ could vary from 2.2 to 5.8 among samples. But notice the structure of the population, which could be arranged as

2, 2, 2, 2	3, 3, 3, 3	4, 4, 4, 4	5, 5, 5, 5	6, 6, 6, 6
I	II	III	IV	V

comprising five groups (or strata) in each of which all four y-values are the same. Suppose there is some mechanism by which one member could be chosen at random from each of these strata to constitute the sample of size 5. The sample would always be

$$2, 3, 4, 5, 6,$$

and the sample mean would always be 4. There would be no sampling variation (i.e., $\text{Var}(\bar{y}) = 0$), and the estimate would always equal the population mean, $\bar{Y}$. Such an extremely favorable situation would arise because all variability within the strata has been removed. This oversimplified example illustrates that stratified random sampling can markedly improve the precision of estimators if it is possible to obtain strata that are fairly homogeneous within.

The advantages of stratification can be summarized as follows.

Improved Overall Precision. Creation of strata that are more homogeneous internally than the population as a whole reduces the variance of the population estimates, as just illustrated.

Easier Administration. Stratification may make a survey much easier to administer. For example, in a statewide telephone survey it may be sensible to divide the state into regional strata and have survey teams in each region.

Greater Information Yield. Parameters can be estimated for the strata themselves, which may be very important. For example, in a statewide telephone survey administrators might desire regional means and totals as well as the overall state mean and total.

3.3.2 Mechanism of Stratified Random Sampling

To implement stratified random sampling, the population is divided into L distinct, nonoverlapping strata of *known* size, and a simple random sample without replacement is taken from each stratum independently of all other strata. Our statistical notation, based on that of Cochran (1977), is as follows.

h denotes the stratum being considered ($h = 1, \ldots, L$);

i denotes the unit within the stratum ($i = 1, \ldots, N_h$);

N_h is the population size in stratum h;

n_h is the sample size in stratum h;

$N = \sum_{h=1}^{L} N_h$ is the total population size;

$n = \sum_{h=1}^{L} n_h$ is the total sample size;

$W_h = N_h/N$ is the fraction of the population in stratum h, also called the stratum weight;

$f_h = n_h/N_h$ is the sampling fraction for stratum h;

y_{hi} denotes the value of the ith unit of stratum h;

$\bar{Y}_h = \left[\sum_{i=1}^{N_h} Y_{hi}\right]/N_h$ is the population mean for stratum h;

$\bar{y}_h = \left[\sum_{i=1}^{n_h} y_{hi}\right]/n_h$ is the sample mean for stratum h;

$Y_h = N_h\bar{Y}_h$ is the population total for stratum h;

$\hat{Y}_h = N_h\bar{y}_h$ is the estimated total for stratum h;

$S_h^2 = \left[\sum_{i=1}^{N_h} (Y_{hi} - \bar{Y}_h)^2\right]/(N_h - 1)$ is the population variance for stratum h;

$s_h^2 = \left[\sum_{i=1}^{n_h} (y_{hi} - \bar{y}_h)^2\right]/(n_h - 1)$ is the sample variance for stratum h.

Because simple random sampling is done in each stratum, the sample mean ($\bar{y}_h$) is an unbiased estimate of the population mean ($\bar{Y}_h$) in each stratum, and the sample variance (s_h^2) is an unbiased estimate of the population variance (S_h^2) in each stratum. The estimate of the total in each stratum ($\hat{Y}_h$) is also an unbiased estimate of the population total (Y_h) in each stratum. It is important to know the stratum sizes (N_h) when totals are estimated, and this requires a detailed sampling frame with stratum information for each unit. Such information is not always available in practice.

3.3.3 Estimation of Population Mean and Total

The population mean for a stratified population is

$$\bar{Y} = \left[\sum_{h=1}^{L} \sum_{i=1}^{N_h} y_{hi} \right] / N = \left[\sum_{h=1}^{L} N_h \bar{Y}_h \right] / N = \sum_{h=1}^{L} W_h \bar{Y}_h, \tag{3.9}$$

and the best way to estimate this is by replacing the population mean ($\bar{Y}_h$) in each stratum by the stratum's sample mean ($\bar{y}_h$). The stratified estimator (subscripted st) therefore is

$$\bar{y}_{st} = \sum_{h=1}^{L} W_h \bar{y}_h, \tag{3.10}$$

and it is unbiased with variance

$$\text{Var}(\bar{y}_{st}) = \sum_{h=1}^{L} W_h^2 \frac{S_h^2}{n_h} \left(\frac{N_h - n_h}{N_h} \right). \tag{3.11}$$

An estimate of this variance can be obtained by replacing (in each stratum) the population variance by its corresponding sample variance (i.e., S_h^2 is replaced by s_h^2). Sample variances in each stratum can be calculated only if each stratum has at least two sampling units. Cochran (1977:138) discusses a method of collapsing strata if some of them have only one sampling unit.

The stratified estimator given above (equation 3.10) is almost always more precise (i.e., has a smaller variance) than a sample mean not based on the stratification. Only if sampling is proportional to the size of each stratum (proportional allocation) are the two estimators equivalent. The biggest gains from stratification occur when stratum means are very different and the strata are homogeneous within.

The unbiased stratified estimator of the population total Y is

$$\hat{Y}_{st} = N\bar{y}_{st} = \sum_{h=1}^{L} \hat{Y}_h, \tag{3.12}$$

which is just the sum of the estimated totals in each stratum. The variance of the estimator is

$$\text{Var}(\hat{Y}_{st}) = N^2 \text{Var}(\bar{y}_{st}). \tag{3.13}$$

Consider a special license frame with N = 3,500 anglers for which three regional strata are needed. Stratum populations are N_1 = 2,000 anglers in the first region, N_2 = 1,000 anglers in the second region, and N_3 = 500 anglers in the third region. Stratum weights (W_h) therefore are W_1 = 2,000/3,500 = 0.571, W_2 = 1,000/3,500 = 0.286, and W_3 = 500/3,500 = 0.143. A stratified random sample is drawn with the following results.

Stratum	Sample size	Sample mean	Sample variance
1	n_1 = 400	$\bar{y}_1$ = 120	s_1^2 = 100
2	n_2 = 200	$\bar{y}_2$ = 210	s_2^2 = 400
3	n_3 = 100	$\bar{y}_3$ = 195	s_3^2 = 400

The overall population means ($\bar{y}_{st}$) can be estimated with equation (3.10):

$$\begin{aligned}\bar{y}_{st} &= \sum_{h=1}^{3} W_h \bar{y}_h \\ &= (0.571 \times 120) + (0.286 \times 210) + (0.143 \times 195) \\ &= 68.52 + 60.06 + 27.89 \\ &= 156.47.\end{aligned}$$

The variance of the estimate is calculated with equation (3.11), the population variances (S_h^2) being replaced by the sample estimates (s_h^2):

$$\begin{aligned}\text{Var}(\bar{y}_{st}) &= \sum_{h=1}^{3} W_h^2 \frac{s_h^2}{n_h}\left(\frac{N_h - n_h}{N_h}\right) \\ &= (0.571)^2\left(\frac{100}{400}\right)\left(\frac{2{,}000 - 400}{2{,}000}\right) + (0.286)^2\left(\frac{400}{200}\right)\left(\frac{1{,}000 - 200}{1{,}000}\right) \\ &\quad + (0.143)^2\left(\frac{400}{100}\right)\left(\frac{500 - 100}{500}\right) \\ &= (0.571)^2 \times 0.25 \times 0.8 + (0.286)^2 \times 2 \times 0.8 + (0.143)^2 \times 4 \times 0.8 \\ &= 0.0652 + 0.1309 + 0.0654 \\ &= 0.2615.\end{aligned}$$

The population total (equation 3.12) is

$$\hat{Y}_{st} = N\bar{y}_{st} = 3{,}500 \times 156.47 = 547{,}645,$$

its variance (equation 3.3) is

$$\text{Var}(\hat{Y}_{st}) = N^2\text{var}(\bar{y}_{st}) = (3{,}500)^2 \times 0.2615,$$

and its standard error is

$$\text{SE}(\hat{Y}_{st}) = \sqrt{(3{,}500)^2 \times 0.2615} = 1{,}789.80.$$

3.3.4 Allocation of Sampling to Strata

How much sampling should be done in each stratum? The most straightforward method of allocating sampling effort is called *proportional allocation,* wherein the proportion of the total sample in each stratum equals the proportion of the population size in that stratum:

$$\frac{n_h}{\sum_{h=1}^{L} n_h} = \frac{N_h}{\sum_{h=1}^{L} N_h} \text{ or } \frac{n_h}{n} = \frac{N_h}{N}. \tag{3.14}$$

A large stratum that includes 60% of the total population gets a sample size that is 60% of the total sample size. Proportional allocation has the advantage of simplicity but it does have two disadvantages. First, it does not take into account that the cost of sampling may vary from stratum to stratum. Second, it does not take into account that strata may have different degrees of variability within them.

So-called *optimal allocation* (Cochran 1977:96) is based on minimizing the variance of the stratified estimator for a fixed cost based on the cost function

$$C = c_o + \sum_{h=1}^{L} c_h n_h,$$

or

$$\text{total cost} = \text{overhead cost} + \text{sampling cost},$$

c_o being the overhead cost and c_h the cost of sampling one unit in stratum h. The unit sampling cost (c_h) may differ among strata. The result,

$$n_h = \frac{N_h S_h/\sqrt{c_h}}{\sum_{h=1}^{L} N_h S_h/\sqrt{c_h}} \times n, \quad h = 1, \ldots, L, \tag{3.15}$$

implies the stratum sample sizes are directly proportional to the stratum population sizes (N_h) and to the stratum standard deviations (S_h), and inversely proportional to the square root of the cost of sampling one unit in each stratum ($\sqrt{c_h}$) In other words, big and variable strata need to be sampled more, but strata that are expensive to sample should be sampled less. The total sample size for total cost C is

$$n = \frac{(C - c_o) \sum_{h=1}^{L} N_h S_h/\sqrt{c_h}}{\sum_{h=1}^{L} N_h S_h \sqrt{c_h}}. \tag{3.16}$$

In practice, proportional allocation is used frequently because it is simpler and because optimal allocation requires knowledge of stratum variances that is often not available. Also usually all strata cost equal amounts to sample on a per unit basis.

We continue the example from Section 3.3.3, in which the total population is N = 3,500 anglers; stratum populations are N_1 = 2,000 anglers, N_2 = 1,000 anglers, and N_3 = 500 anglers; and stratum weights are W_1 = 0.571, W_2 = 0.286, and W_3 = 0.143. Suppose a sample of n = 1,094 anglers could be taken in the total population. A simple approach to allocating samples among strata is to assign them in proportion to stratum size:

$$n_1 = 1{,}094 \times 0.571 = 625;$$

$$n_2 = 1{,}094 \times 0.286 = 313;$$

$$n_3 = 1{,}094 \times 0.143 = 166.$$

Now suppose instead that good information about the population is available. In a previous study, stratum standard deviations were about S_1 = 10, S_2 = 20, and

$S_3 = 20$; the approximate costs of sampling one unit in each stratum were $c_1 =$ \$1.00, $c_2 =$ \$1.00, and $c_3 =$ \$0.64; and the overhead cost was $c_o =$ \$200. Further suppose that the total survey budget is $C =$ \$1,200. Now sample allocation among strata can be optimized. First, total sample size, as constrained by budget (equation 3.16) is

$$n = \frac{(1{,}200 - 200)\left[\frac{2{,}000 \times 10}{1} + \frac{1{,}000 \times 20}{1} + \frac{500 \times 20}{0.8}\right]}{[2{,}000 \times 10 \times 1 + 1{,}000 \times 20 \times 1 + 500 \times 20 \times 0.8]}$$

$$= \frac{1{,}000 \times 52{,}500}{48{,}000} = 1{,}093.74, \text{ or } 1{,}094.$$

Then, the total sample is allocated to stratum samples (n_h) by equation (3.15)

$$n_1 = \left[\frac{(2{,}000 \times 10/1)}{(2{,}000 \times 10/1 + (1{,}000 \times 20/1) + (500 \times 20/0.8)}\right] \times 1{,}094 \approx 417;$$

$$n_2 = \left[\frac{(1{,}000 \times 20/1)}{(2{,}000 \times 10/1 + (1{,}000 \times 20/1) + (500 \times 20/0.8)}\right] \times 1{,}094 \approx 417;$$

$$n_3 = \left[\frac{(500 \times 20/0.8)}{(2{,}000 \times 10/1) + (1{,}000 \times 20/1) + (500 \times 20/0.8)}\right] \times 1{,}094 \approx 260.$$

Compared with proportional allocation, optimal allocation gave more sampling units to strata 2 and 3 because of their relatively high variances, and it further favored stratum 3 because that stratum, although small, is least costly to sample.

3.3.5 Estimation of a Population Proportion

Population proportions can be estimated much like population means with stratified random sampling (Cochran 1977:107). In stratum h, P_h is defined as the population proportion with some attribute and p_h as its corresponding sample proportion. Then p_h is an unbiased estimate of P_h and an estimate of the overall population proportion is

$$p_{\text{st}} = \sum_{h=1}^{L} W_h p_h. \tag{3.17}$$

Equation (3.17) is analogous to equation (3.10), and therefore p_{st} is unbiased also. The variance is

$$\text{Var}(p_{\text{st}}) = \sum_{h=1}^{L} W_h^2 \frac{P_h(1 - P_h)}{n_h} \left(\frac{N_h - n_h}{N_h - 1}\right), \tag{3.18}$$

which is analogous to equation (3.11). Stratum sampling effort still follows equation (3.14) for proportional allocation and equations (3.15) and (3.16)—with S_h replaced by $\sqrt{P_h(1 - P_h)}$—for optimal allocation.

3.3.6 Poststratification

Analysis of some variables (e.g., race, income) would benefit from stratification, but the strata to which the units belong cannot be known until after the sample is taken. One common method to solve this problem is to take a simple random sample from the whole population and to poststratify the units. The sample is then treated as though it were proportionally allocated to the strata. This procedure works reasonably well provided the N_h's are known or at least closely approximated and the sample sizes are reasonably large in each stratum ($n_h > 20$).

3.4 SYSTEMATIC RANDOM SAMPLING

Simple random sampling without replacement is an important foundation of sampling theory. Nevertheless, surveyors sometimes may wish to use systematic random sampling for reasons of simplicity or precision.

Simplicity. Occasionally it is easier to draw a systematic sample without making mistakes than it is a fully random sample. For example, a fishing license frame may be a file drawer of license ticket stubs, each with a name and address. It would be easy to make a random start and then systematically sample every kth ticket stub in the file, but it would be very tedious to take a truly random sample.

Precision. In some cases, greater precision may result from systematic random sampling than from simple random sampling.

Cochran (1977:205) discusses systematic random sampling in detail, and only a brief discussion of its properties is given here. The sample mean based on a systematic random sample is always unbiased but its precision varies with the structure of the population. If a population is listed essentially in "random" order, the variance of the sample mean is the same with both systematic and simple random sampling. However, survey investigators never know for sure that a list is in random order. When systematic random sampling is called for, we strongly recommend that investigators take several independent samples, obtain their overall mean, and then estimate the variance directly from the replicate samples.

For K independent systematic random samples with means $\bar{y}_1^*, \bar{y}_2^*, \bar{y}_3^*, \ldots, \bar{y}_K^*$, the overall mean (subscript sy denoting systematic) is

$$\bar{y}_{sy} = \left[\sum_{j=1}^{K} \bar{y}_j^*\right] / K. \tag{3.19}$$

The estimated variance of $\bar{y}_{sy}$ is

$$\hat{\text{Var}}(\bar{y}_{sy}) = s^{*2}/K, \tag{3.20}$$

where

$$s^{*2} = \frac{1}{K-1} \sum_{j=1}^{K} (\bar{y}_j^* - \bar{y}_{sy})^2. \tag{3.21}$$

These results are based on classical statistical methods (Snedecor and Cochran 1980:44).

The estimator of the population total is

$$\hat{Y}_{sy} = N\bar{y}_{sy} \tag{3.22}$$

with variance

$$\mathrm{Var}(\hat{Y}_{sy}) = N^2\mathrm{Var}(\bar{y}_{sy}). \tag{3.23}$$

Consider a license file of $N = 5{,}000$ stubs from which five independent systematic samples, each of size 100, are taken. The aggregate sample of 500 is 10% of the population. For each independent sample, the sampling interval is 50 stubs ($N/n = 5{,}000/100 = 50$), and a random start is made in the first interval. To start the samples, a table of random numbers is used to draw (without replacement) five numbers in the inclusive range 1–50. Suppose these are 16, 47, 34, 50, and 21. Then the samples have units

16, 66, 116, . . ., 4,916, 4,966 in sample 1 (units increase by 50 after the first),
47, 97, 147, . . ., 4,947, 4,997 in sample 2,
34, 84, 134, . . ., 4,934, 4,984 in sample 3,
50, 100, 150, . . ., 4,950, 5,000 in sample 4, and
21, 71, 121, . . ., 4,921, 4,971 in sample 5.

After appropriate sampling has been carried out on the units (such as by a mail survey), five sample means can be calculated: $\bar{y}_1^*$, $\bar{y}_2^*$, $\bar{y}_3^*$, $\bar{y}_4^*$, and $\bar{y}_5^*$. Use of equations (3.19)–(3.23) then will provide the appropriate estimates and their variances. For example,

$$\bar{y}_{sy} = \left[\sum_{j=1}^{5} \bar{y}_j^*\right] / 5$$

(equation 3.19), and this estimate of the population mean will have (equations 3.20 and 3.21)

$$\mathrm{Var}(\bar{y}_{sy}) = \frac{s^{*2}}{5} \text{ with } s^{*2} = \frac{1}{4}\sum_{j=1}^{5}(\bar{y}_j^* - \bar{y}_{sy})^2.$$

3.5 TWO-STAGE (CLUSTER) SAMPLING

3.5.1 Introduction

In some situations, each *primary sampling unit* may be divided into two or more *secondary sampling units*. For example, the whole fishing day might be considered a primary unit, and morning and afternoon might be treated as two secondary units within each day. Once a particular primary unit (day) has been chosen—for an access point survey, say—it may be uneconomical to have a survey agent present during both morning and afternoon. In such a case, an appropriate procedure will be to choose n days at random without replacement and then to randomly choose morning or afternoon within each sample day for data collection.

Here we present just the simplest case in which a population has N primary units and each primary unit is divided into M secondary units (Cochran 1977, Chapter 10). A simple random sample of n primary units is taken without replacement, and then a simple random sample of m secondary units is drawn without replacement from each of the sampled primary units.

The following notation is needed.

N is the number of primary units in the population;

M is the number of secondary units in each primary unit;

n is the number of primary units in the sample;

m is the number of secondary units sampled from each primary unit;

y_{ij} is the value for the jth secondary unit in the ith primary unit;

$\bar{y}_i = \left[\sum_{j=1}^{m} y_{ij}\right]/m$ is the sample mean per secondary unit in the ith primary unit;

$\bar{Y}_i = \left[\sum_{j=1}^{M} Y_{ij}\right]/M$ is the population mean per secondary unit in the ith primary unit;

$\bar{\bar{y}} = \left[\sum_{i=1}^{n} \bar{y}_i\right]/n$ is the overall sample mean per secondary unit;

$\bar{\bar{Y}} = \left[\sum_{i=1}^{N} \bar{y}_i\right]/N$ is the overall population mean per secondary unit;

$s_1^2 = \left[\sum_{i=1}^{n} (\bar{y}_i - \bar{\bar{y}})^2\right]/(n-1)$ is the sample variance among primary unit means;

$S_1^2 = \left[\sum_{i=1}^{N} (\bar{Y}_i - \bar{\bar{Y}})^2\right]/(N-1)$ is the population variance among primary unit means;

$s_2^2 = \left[\sum_{i=1}^{n} \sum_{j=1}^{m} (y_{ij} - \bar{y}_i)^2\right]/n(m-1)$ is the sample variance among secondary units within primary units;

$S_2^2 = \left[\sum_{i=1}^{N} \sum_{j=1}^{M} (y_{ij} - \bar{Y}_i)^2\right]/N(M-1)$ is the population variance among secondary units within primary units.

3.5.2 Estimation of Population Mean

If the n units and the m units are chosen randomly, then $\bar{\bar{y}}$, the sample mean, is an unbiased estimate of $\bar{\bar{Y}}$, the population mean. The variance is

$$\text{Var}(\bar{\bar{y}}) = \left(\frac{N-n}{N}\right)\frac{S_1^2}{n} + \left(\frac{M-m}{M}\right)\frac{S_2^2}{nm} = (1-f_1)\frac{S_1^2}{n} + (1-f_2)\frac{S_2^2}{nm}, \qquad (3.24)$$

for which f_1 is the sampling fraction of primary units and f_2 is the sampling fraction of secondary units. This variance extends the results for simple random sampling (equation 3.3 in Section 3.2.2). If all the secondary units are sampled ($m = M$), the variances are the same.

The corresponding estimated variance is

$$\hat{\text{Var}}(\bar{\bar{y}}) = (1-f_1)\frac{s_1^2}{n} + f_1(1-f_2)\frac{s_2^2}{nm}. \qquad (3.25)$$

Calculation of the variance estimate is not as straightforward as just replacing S_1^2 by s_1^2 and S_2^2 by s_2^2. The expected value of s_1^2 includes S_2^2 and this must be taken into account, which modifies the equation. When f_1 is negligible (i.e., n/N is very small), the estimated variance reduces to

$$\hat{\text{Var}}(\bar{\bar{y}}) \approx \frac{s_1^2}{n}; \qquad (3.26)$$

in this form, it can be computed from a knowledge of primary unit means only. This result is helpful when the secondary sampling is systematic, because in such cases an unbiased estimate of S_2^2 cannot be obtained. In general, equation (3.26) can be used as a conservative estimate of variance (i.e., it is somewhat too large) irrespective of how the secondary sampling is done. In some complex designs with systematic secondary or nonuniform probability sampling, it may be the only reasonable option. (See also Section 3.8.)

The estimator of the population total is

$$\hat{Y} = NM\bar{\bar{y}} \qquad (3.27)$$

with variance

$$\text{Var}(\hat{Y}) = (NM)^2\text{Var}(\bar{\bar{y}}). \qquad (3.28)$$

Proportions can be estimated from two-stage sampling in a similar way (Cochran 1977:279). The optimal choice of primary and secondary sample sizes depends heavily on the cost ratio of the two types of units. More complex designs can include three-stage sampling (Cochran 1977:285), stratification (Cochran 1977:288), and variable numbers of secondary units per primary unit (Cochran 1977, Chapter 11). Also, secondary sampling units may be chosen systematically or with nonuniform probability.

Two-stage sampling is illustrated in detail in Chapter 15 on catch and effort estimation. In Section 3.7, some simple examples show the value of two-stage sampling over simple random sampling or stratified random sampling with only one stage.

3.6 NONUNIFORM PROBABILITY SAMPLING

Sometimes it makes sense to sample units with unequal probabilities. For example, sampling fishing access points based on their expected use (estimated from expert opinion or from an earlier survey) could be more informative than sampling them with equal probability. Nonuniform probability sampling is quite complex statistically, so we just present the basic theory without much detail. In later chapters, examples will clarify its application.

We use the Horvitz–Thompson (HT) estimator (Cochran 1977:259) for nonuniform probability sampling. A sample of n units is taken without replacement. The probability of the ith unit being in the sample is denoted by π_i, and the probability of the ith and jth units both being in the sample is denoted by π_{ij}. The estimate of the population total is

$$\hat{Y}_{HT} = \sum_{i=1}^{n} (y_i/\pi_i). \tag{3.29}$$

If all elements have equal probability, $\pi_i = n/N$ (because $\Sigma_{i=1}^{N} \pi_i$ always equals n) and $\hat{Y}_{HT}$ reduces to $N\bar{y}$, the result for simple random sampling without replacement (Section 3.2.2). If $\pi_i = n_h/N_h$, meaning all units within each stratum have equal probability of selection, $\hat{Y}_{HT}$ reduces to $\Sigma_{h=1}^{L} \hat{Y}_h$, which is the estimator given in equation (3.12) for stratified random sampling. Also, if $\pi_i = nm/NM$, $\hat{Y}_{HT}$ reduces to $NM\bar{\bar{y}}$, which is the estimator given in equation (3.27) for two-stage sampling.

The variance of the Horvitz–Thompson estimator is

$$\mathrm{Var}(\hat{Y}_{HT}) = \sum_{i=1}^{N} \frac{(1-\pi_i)}{\pi_i} y_i^2 + 2\sum_{i=1}^{N} \sum_{j>i}^{N} \left[\frac{(\pi_{ij} - \pi_i\pi_j)}{\pi_i\pi_j}\right] y_i y_j. \tag{3.30}$$

Sampling without replacement with nonuniform probability is not easy in practice (Cochran 1977:261).

We illustrate nonuniform probability sampling in detail in Chapter 15 on catch and effort estimation. In Section 3.7, some simple examples show the value of using nonuniform probability sampling.

3.7 SOME SIMPLE EXAMPLES

To illustrate some of the different sampling designs, consider some simple examples like those presented by Malvestuto (1983). An angler survey is carried out on a small lake that is easily sampled by one survey agent. Sampling occurs in February of a leap year.

Figure 3.2 shows a possible simple random sample of $n = 10$ days out of the $N = 29$ possible days in February. "Random," meaning that each day has an equal chance of being drawn, does not necessarily translate into an even distribution of sampled days over a week or month, and this is confirmed in Figure 3.2. By chance, the days chosen are concentrated towards the later part of the month.

Fishing pressure often is greater on weekend days than on weekdays. This can be accommodated with a stratified design, whereby the 29 days are divided into two strata with $N_1 = 20$ weekdays in stratum 1 and $N_2 = 9$ weekend days in

FEBRUARY

M	T	W	T	F	S	S
					1	2*
3	4	5*	6*	7	8	9
10	11	12	13	14	15*	16*
17	18	19	20	21*	22	23*
24	25	26*	27	28*	29	

Figure 3.2 Simple random sampling design for an angler survey on a small lake. The sample (asterisks) is $n = 10$ days out of the $N = 29$ days in February.

stratum 2. Figure 3.3 shows a possible stratified random sample of five days from each stratum ($n_1 = 5$, $n_2 = 5$).

Sometimes weekly differences in fishing pressure are expected, and a more even spread of sampling is desired than might arise from simple random sampling. A systematic random sample could be taken with or without stratification; the example in Figure 3.4 incorporates weekday–weekend stratification. The sampling interval in stratum 1 (weekdays) is 4 days ($N_1/n_1 = 20/5$). One of the 20 weekdays is drawn at random from the first interval; it turns out to be day 4. Every fourth day is drawn thereafter, up to a total of 5 days, giving the sample day 4 (February 6), day 8 (February 12), day 12 (February 18), day 16 (February 24), and day 20 (February 28). For the weekend stratum, the sampling interval is 2 days ($N_2/n_2 = 9/5$, rounded up to the nearest day). Day 2 is drawn at random from the first interval, and every second day is drawn thereafter, giving the sample day 2 (February 2), day 4 (February 9), day 6 (February 16), day 8 (February 23), and

FEBRUARY

Stratum 1					Stratum 2	
M	T	W	T	F	S	S
					1*	2*
3	4	5	6	7*	8	9*
10	11	12	13	14	15	16
17*	18	19*	20	21	22*	23*
24	25	26*	27*	28	29	

Weekdays $N_1 = 20$ Weekend days $N_2 = 9$
Weekdays $n_1 = 5$ Weekend days $n_2 = 5$

Figure 3.3 Stratified random sampling design for an angler survey on a small lake. The samples (asterisks) are $n_1 = 5$ days of the $N_1 = 20$ weekdays in stratum 1 and $n_2 = 5$ of the $N_2 = 9$ weekend days in stratum 2. The weekends are sampled at a relatively higher rate because fishing pressure is likely to be higher then.

FEBRUARY

Stratum 1					Stratum 2	
M	T	W	T	F	S	S
					1*	2*
3	4	5	6*	7	8	9*
10	11	12*	13	14	15	16*
17	18*	19	20	21	22	23*
24*	25	26	27	28*	29	

Weekdays $N_1 = 20$ Weekend days $N_2 = 9$
Weekdays $n_1 = 5$ Weekend days $n_2 = 5$

Figure 3.4 Systematic stratified random sampling design for an angler survey on a small lake. The samples (asterisks) are $n_1 = 5$ days of the $N_1 = 20$ weekdays in stratum 1 and $n_2 = 5$ days of the $N_2 = 9$ weekend days in stratum 2. A random start in each stratum is used so that there is still an element of randomness in these designs.

day 1 (February 1). In this case, it is necessary to wrap around and go back to day 1 to obtain the last day in the sample. Also in this case, all except one of the sampled days is a Sunday, which might be a problem if fishing differs between Saturday and Sunday. Nevertheless, the sample is nicely spread out over the weeks of the month.

Suppose that the survey agent can only work for part of the day. In Figures 3.5 and 3.6, stratified two-stage sampling designs are presented to deal with this difficulty. The primary sampling units (days) are stratified into weekday and weekend strata and 5 days are sampled from each stratum, as in Figure 3.3. Now the workday is divided up into three work periods of equal length (early, middle, and late). In Figure 3.5 one of these secondary sampling units is sampled randomly within each primary unit (day) sampled. In Figure 3.6 these secondary sampling units are sampled with unequal sampling probabilities (early probability = 0.2, middle probability = 0.3, and late probability = 0.5). The expected number of each secondary unit (time of day) does not result exactly. In the weekend stratum, for example, expected numbers would be 1 early, 1.5 middle, and 2.5 late, whereas the actual numbers are 2 early, 3 middle, and none late.

3.8 VARIANCE ESTIMATION

In angler surveys the primary purpose is usually to estimate population characteristics (means, totals, proportions, etc.) as precisely as possible by using sound sampling design principles. These design principles often push the researcher beyond simple random sampling or stratified random sampling. Many practical angler survey designs are complex and involve systematic allocation, subsampling, and nonuniform probability sampling on primary or secondary sampling units. With these designs, the estimators usually can be calculated without much difficulty, but the same cannot be said for variances of the estimators—especially because sampling typically has to be done without replace-

	WEEKDAY STRATUM			WEEKEND STRATUM	
	Sample Days	Sample Times		Sample Days	Sample Times
1.	7 February	Middle	1.	1 February	Middle
2.	17 February	Late	2.	2 February	Late
3.	19 February	Middle	3.	9 February	Early
4.	26 February	Middle	4.	22 February	Early
5.	27 February	Middle	5.	23 February	Middle

Figure 3.5 Stratified two-stage sampling design for an angler survey on a small lake with equal subsampling probabilities. The primary sampling units (days) are stratified into weekday and weekend strata, and $n = 5$ days are sampled randomly in each stratum. Secondarily, the workday is divided into three work periods of equal length, denoted early, middle, and late. These secondary sampling units are sampled randomly within each primary unit sampled.

ment for practical reasons. (Particular times occur only once, and it does not make sense to interview the same angler twice.)

One simple approach that sometimes can be used to estimate variances is to find an *approximate, conservative* (too large) *variance estimator.* Many sampling designs are based on multistage sampling (Section 3.5). If the sampling at the first (or primary) stage is simple random or stratified random sampling, this approach can be employed irrespective of the sampling design at the secondary or tertiary stages. We presented an example of this with equation (3.26). The approach basically is to take a simple sample variance between the estimated primary unit values. This can be done in each stratum if a stratified random sampling design is employed. Of course, there must be adequate replication of primary units in each stratum so that a variance estimate can be calculated. The disadvantage of a conservative variance estimator based only on primary units is that some of the

	WEEKDAY STRATUM			WEEKEND STRATUM	
	Sample Days	Sample Times		Sample Days	Sample Times
1.	7 February	Late	1.	1 February	Early
2.	17 February	Late	2.	2 February	Middle
3.	19 February	Middle	3.	9 February	Early
4.	26 February	Late	4.	22 February	Middle
5.	27 February	Middle	5.	23 February	Middle

Figure 3.6 Stratified two-stage sampling design for an angler survey on a small lake with unequal subsampling probabilities. The primary sampling units (days) are stratified into weekday and weekend strata, and $n = 5$ days are sampled randomly in each stratum. Secondarily, the workday is divided into three work periods of equal length, denoted early, middle, and late. These secondary sampling units are sampled with nonuniform probabilities (early $\pi = 0.2$, middle $\pi = 0.3$, and late $\pi = 0.5$).

gain in precision provided by an efficient design has been lost because the approximate variance is larger than the true variance of the estimator.

If the design for primary sampling units is more complex than simple random or stratified random sampling, even an approximate conservative variance estimator may be difficult to obtain. Wolter (1985) gave an excellent (if rather mathematical) treatment of some more general methods of variance estimation. A detailed discussion of variance estimation for estimators is too complex to be considered here, but we offer some general comments based on Wolter's book.

The first group of methods described might be called *pseudoreplication* methods. The entire sample is divided into random subgroups that are viewed as independent replicate samples even though they are not usually independent because sampling has been without replacement. (If they were to be truly independent samples an element or sampling unit could be drawn twice.) Simple averages and variances can be obtained over the subgroups. This approach was illustrated in Section 3.4, where it was explained how to calculate the variance of a mean obtained from systematic random sampling. Extensions of the random groups approach lead to the *balanced half samples* approach and the *jackknife* approach, which will not be described here.

Wolter (1985) also presented an intriguing approach based on *generalized variance functions*. The idea is that in large surveys for which many variables are to be analyzed, one can determine the relationship between the mean and variance of an estimate based on one characteristic and then apply this relationship to all the variables. It sacrifices generality and exactness for ease of calculation. We do not think it should be used in most angler surveys.

Problems of variance estimation will arise again in subsequent chapters. We suspect that in the future, simulation modeling will be used more frequently to estimate variances (Jones et al. 1990).

Chapter 4

Questionnaire Construction

4.1 INTRODUCTION

The design of questions for a questionnaire or interview form is just as important as any other aspect of survey planning and execution. Regardless of how efficient or sophisticated the sampling design may be or how thoroughly and properly the data are analyzed, wrong conclusions will be drawn from the survey results if the right questions are not asked or if they are asked in ways that elicit inconsistent and inaccurate responses.

For any angler survey instrument, whether it is administered on-site or off-site, face to face, by telephone, or by mail, the questions both individually and collectively should have the following general properties:

- they should make a contribution to answering a survey objective or subobjective;
- they should be clearly and unambiguously worded; and
- they should evoke the most accurate answer the respondent can give.

Responses to survey questions are subject to several types of errors. Those that occur quite consistently are referred to as biases. Some, such as memory recall biases, occur regardless of the type of survey instrument. It may be impractical to eliminate all such biases, but some steps can be taken in the overall survey design and in the design of questions to minimize these biases.

This chapter concentrates on how to devise unambiguous questions that offer a full range of response options and that minimize response biases. (Response biases are also addressed in Chapter 5, Section 5.5, in the context of survey errors.) The importance of defining and planning the study can not be overstated and will be covered first.

4.2 RELATION OF QUESTIONS TO STUDY OBJECTIVES

It is human nature to begin drafting questions as soon as it is apparent that some type of survey is needed. A second temptation is to get a copy of a questionnaire someone else used in a somewhat similar situation and to use that questionnaire with a minimum of changes. Both temptations should be resisted.

To reemphasize a point covered in Chapter 2, it is incumbent upon the researcher to ascertain what management decisions need to be made and what information, from what angler (or other) population over what time period, is needed to make those decisions. Once agreement is reached at the management level, study objectives can be written. The objectives must be sufficiently detailed to guide construction of the questionnaire. For example, if data on angler effort, expenditures, or fishing preferences are to be evaluated by residential or other demographic categories, the types of data and analytical categories should be stated at least generally in a study objective.

As a particular example, urban fisheries programs are sometimes developed to serve (among others) residents who lack the mobility to get to rural fisheries. Residents with low mobility could generally include youngsters and the elderly as well as people of limited financial means. Two related objectives of an urban fishery study might be "to determine incidence and frequency of participation in the fishery by residence, age, ethnic group, and socioeconomic status," and "to determine the extent to which fishing by participating anglers is restricted to urban areas, by age, ethnic group, and socioeconomic status." In the questionnaire developed for such a study, the participation questions would be asked together near the front of the questionnaire. The socioeconomic and demographic questions, being more sensitive and hence less likely to be answered by respondents, would be located at the end where they would be least likely to deter people from completing the questionnaire.

Once the study objectives are formulated and agreed to, development of the questionnaire can proceed. A sequence of questions usually is needed to achieve a particular study objective. Although the questions should initially be drafted by study objective, the final ordering of the questions may change. Questions should be presented in an order that will be most straightforward and least confusing to the respondent and most likely to hold the respondent's interest. Because the order of questions may differ from the order of study objectives, and because some questions may serve more than one objective, we recommend creating a matrix of questions by objectives (Table 4.1). Such a matrix has several advantages. For survey designers, it confirms that each question is relevant to the study objectives. For external reviewers of a draft questionnaire, it links questions explicitly to objectives, allowing evaluation of the extent to which each question and the collective questions contribute to each study objective. For later analysts and reporters of survey data, it provides a quick reminder of how questions and objectives were related. A question-by-objective matrix thus has value throughout a survey.

4.2.1 Type of Survey Instrument

Whether the questions or the type of survey (mail, telephone, face-to-face interview, or some combination) should be determined first depends upon the experience and expertise of the researcher and the facilities available. If one can envision the approximate length of the survey instrument, complexity of the questions, and amount of memory recall required of respondents, it is more efficient to determine the type of instrument first and then to develop questions in the format appropriate for that instrument. Sometimes the staff and facilities are more geared toward one type of instrument than another, which strongly influences the type of instrument selected, at least initially. However, the draft instrument should be checked closely to determine whether it actually is suitable for the type of implementation planned.

Strengths, limitations, and general use of mail, telephone, and face-to-face surveys are treated in Chapters 6–8.

Table 4.1 Example of a question-by-objective matrix for designing a survey questionnaire.

Question number	Objective number				
	1	2A	2B	3	4
1	×				
2	×				
3	×				
4	×				
5		×			
6		×			
7		×			
8		×			
9			×		
10			×		
11				×	
12				×	
13				×	
14				×	
15				×	
16			×		
17			×		
18	×				×
19	×				×
20	×				×
21	×				×
22	×				×

4.3 MINIMIZING INACCURATE RESPONSES TO QUESTIONS

A wealth of survey research in the social sciences, some of it applied to fisheries, provides insights into the situations and types of questions that are likely to elicit inaccurate responses. An understanding of these problems is an essential prerequisite for constructing questions that consistently elicit correct responses. A good general text on this topic is Sudman and Bradburn (1983).

Regardless of the form of the survey, most incorrect answers are given for one of the following reasons:

- the question lacked clarity, was vague in some respect (such as the time period asked about), or was misunderstood;
- if categorized response options were listed, categories representing all possible alternatives were not provided, or it was not made clear what to do if multiple categories or no categories were applicable;
- the respondent's memory was imperfect regarding the information sought; or
- the respondent was deliberately inaccurate or untruthful.

We first examine the biases represented by poor memory recall and deliberate misrepresentation. We then cover the construction of questions, which addresses clarity and categorization.

4.3.1 Memory Recall

Most people attempt to answer most or all questions in an angler survey as accurately as possible if the questionnaire is not so long that respondents lose their concentration and begin to provide superficial answers. Nevertheless, many responses relating to specific events—such as number of days fished, fish caught, and dollars spent—differ to some degree from what actually transpired because memory is imperfect. The longer the interval to which the questions apply (e.g., fishing during a 3-month, 6-month, or calendar year interval), and the longer the time between this interval and the survey, the less accurate the responses will be.

People remember unique, unusual, and (to them) important events longer than they do commonplace events (Westat, Inc. 1989). Fishing trips typically fall somewhere between unique and commonplace events, but the principle is the same: an unusual fishing trip will be remembered longer than an ordinary one. Factors that make a trip unusual include new types of fishing, novel fishing sites, large catches, catches of unusual species or trophy fish, and nonfishing events such as a vehicle breakdown or a very good time with friends.

A common type of memory recall error is known as telescoping—assigning events to the wrong time period. When anglers are asked how often they fished at a particular site in the past x months, they frequently overreport the number of trips, with no intent of being inaccurate, because they misassign some earlier trips to the time period being asked about. If it has been more than a few weeks since the end of the time period of interest, anglers may also misassign trips into the subsequent time period. A second type of error, called recall decay, is the inability of respondents to recall all of the relevant events asked about in the survey. Recall decay is especially prevalent for local fishing trips of short duration, which tend not to be noteworthy.

Memory recall of specific details of a fishing trip begins to fade almost as soon as the trip ends. Research done for the U.S. National Marine Fisheries Service indicated that 2-month recall data produced underestimates compared with data collected every 2 weeks (Gems et al. 1982). The experience of several states that have conducted angler surveys is that annual recall data produce large overestimates of fishing effort and catch when compared with on-site creel survey data. Annual and even much shorter recall surveys produce underestimates of expenditures unless a highly specific list of expenditure categories is used. Anglers remember major expenses such as meals, lodging, and bait and tackle, but they forget about the variety of incidental expenditures they make while on a fishing trip.

The extent to which inaccurate memory recall affects anglers' estimates is just beginning to be realized. Agencies have begun to take steps to reduce the period of memory recall. In 1991, the U.S. Fish and Wildlife Service's quinquiennial National Survey of Fishing, Hunting and Wildlife-Associated Recreation changed from obtaining annual data to obtaining data every 4 months (budget constraints prohibited a shorter period).

For some biological measures that must be highly accurate, it may be necessary to conduct a creel survey or to choose an off-site survey design that requires no more than a 1-week recall. On the other hand, if attitudes toward proposed changes in fishing regulations are to be cross-tabulated by frequency of fishing along various waterways, only modest precision is required of the frequency data and a longer recall period may be satisfactory. State fisheries leaders in New York chose annual recall data over more frequent but more expensive data because previous annual data were available and the interest was in major changes in fishing for different species or in different types of waters (Brown 1991). The leaders assumed that biases in annual memory recall were fairly consistent between surveys.

4.3.2 Deliberate Misreporting of Data

Although solid data are not available, social scientists strongly believe that attempts to deliberately mislead researchers in a random way or for no particular reason are rare. Nevertheless, mail questionnaire results should be examined for unusual response patterns, such as a check in the first category of every question, that cause logical inconsistencies among responses to related questions. Similarly, telephone and face-to-face interviewers should note any reason why responses provided are of questionable veracity. The researcher who feels strongly enough that false data were provided has little option but to discard the data from that particular response.

A common bias that involves exaggeration of data, if not outright falsification, is prestige bias or social desirability bias. Many respondents will not provide (admit) data or information that might make them appear to be inept, incapable, ignorant, stingy, etc.; rather, they provide "socially desirable" answers. For example, many states have a checkoff option on their income tax forms whereby taxpayers may choose to make a voluntary contribution to nongame wildlife programs. A small proportion of households (typically 5% or fewer) contribute this way in any given year, yet in surveys of the general public conducted in New Jersey and New York, roughly three times the proportion of taxpaying households that actually contributed to these programs claimed to have done so (Applegate 1984; Brown et al. 1986). Part of the discrepancy may have resulted from recall error for the year involved and part from respondents' tax preparers who did not consult them or do as instructed. However, most of the error probably occurred because some respondents did not want to admit that they did not make even a very modest contribution to a worthy cause.

In angler surveys, some anglers provide an overestimate of the number of fish they caught because they want to appear skillful. Social desirability bias also can arise with questions concerning knowledge or awareness, such as questions that ask whether respondents are familiar with a particular regulation or with a health advisory on eating fish.

The general strategy for minimizing social desirability bias is first to recognize the types of questions likely to evoke the bias and then to find appropriate wording that permits the respondent to give an unexaggerated answer without embarrassment. One frequent solution is to use the phrase "if any" in questions that deal with topics such as fish caught or expenditures made. That is, rather than "How many fish did you catch?," the question put is "How many fish did you catch, if any?" The "if any" conveys the suggestion that it is normal that some anglers will not catch fish.

With regard to testing awareness of a regulation or health advisory warning, questions should be worded so they do not imply that respondents are ignorant if they are unaware. For example, the question

> Were you aware that it is illegal to keep brown trout under 10 inches in length in this state?

could be reworded as follows.

> We are interested in how well the state Department of Natural Resources is communicating information on fishing regulations to anglers. For example, were you aware that it is illegal to keep brown trout under 10 inches in length in this state?

A "no" answer to the first question will cause some anglers to feel they are admitting ignorance of something they should be familiar with, and they will respond affirmatively even if they were not previously aware of the regulation. They will find it much easier to answer negatively to the second question because it suggests that the state agency may share some of the fault for poor communications.

4.4 QUESTION STRUCTURE

Dillman (1978) categorized the question structures available to the researcher into four general types.

Open-End Questions. A totally open-end question lists no categories for the respondent to choose from, but provides a certain amount of space (often with blank lines) for the respondent to write an answer. Open-end questions include specific short-answer questions such as "In what year were you born?," for which the dimension of the answer sought (e.g., year of birth) is obvious. We usually think of open-end questions being much more general and attitudinal than factual: for example, "What do you feel should be the agency's first priority in improving fishing at Jones Reservoir?" In this case, no particular response dimension is suggested; one respondent might want more fish stocked, another might want a predator's numbers diminished, and a third might want better boat access facilities. Results of such questions would be more usable if the question were constrained to asking for the first stocking priority or the first access-related priority.

Closed-End Questions with Ordered Response Choices. Closed-end questions provide several answer categories. If it is not obvious from the question, instructions for the question should indicate whether only one or more than one category can be checked. Closed-end questions with ordered response choices provide nonoverlapping categories in a sequential order. The categories should provide for the full range of possible answers. Example:

> What is your age?
>
> ___ Under 25 years old
>
> ___ 25 to 44 years old
>
> ___ 45 to 64 years old
>
> ___ 65 years or older.

Narrower categories can be used, depending on data needs. Many attitude questions also use this general structure. Example:

Please react to the following statement: The harvest and sale of bait fish from public waters should be regulated by the State Department of Natural Resources.

___ Strongly agree

___ Agree

___ Neither agree nor disagree; unsure

___ Disagree

___ Strongly disagree.

Closed-End Questions with Unordered Response Choices. Closed-end questions with unordered response choices are like those just described except that the categories have no numeric or ordinal ordering. Example:

What is the primary reason why you enjoy fishing Jones Reservoir for bass? (Check one):

___ Rest and relaxation

___ The challenge of catching a trophy sized bass

___ To do something with family or friends

___ To see how many fish I can catch

___ To test my fishing skills

___ Just to be in the outdoors.

Unordered response choices may be accompanied by a variety of instructions. One might be asked to rank the categories as to their importance (1 = highest priority, etc.) or to rank the highest two or three categories. One might be asked to check all categories that apply, and then perhaps to circle the most important category.

Partially Closed-End Questions. Partially closed-end questions typically list the most obvious categories, but then allow the respondent to write in other answers. For example, for the question above, "What is the primary reason why you enjoy fishing Jones Reservoir for bass?," the response categories might end with:

___ Other: please specify: ________________________________

4.4.1 Advantages and Disadvantages of the Question Structures

Open-end questions are the most uncontrolled and allow respondents maximum flexibility to state their answers in their own words. In exploratory research when the most common answers are not known, open-end questions are both useful and necessary. However, it is very laborious to summarize and difficult to interpret the results from open-end questions. For example, suppose the question "Why don't you fish for salmon?" is posed to a sample of anglers. Answers could be expected from many dimensions. Some people may not know how to fish for salmon, others may not like eating the fish; still others may lack the gear, the access, or the time for catching salmon. Suppose 15% of respondents indicate they do not have appropriate gear. Can it be assumed that the other 85% have appropriate gear? No, because some anglers may simply not have thought of that answer; perhaps they had stronger reasons for not fishing for salmon and therefore did not write in that choice.

Closed-end questions are the most controlled because answers are limited to a small number of allowable responses that are easy to summarize quantitatively. They tend to provide results that are easier to interpret because each respondent is presented the same set of categories in conjunction with instructions to the question (e.g., check the most important, check all that apply, rank the top three categories). Response options to closed-end questions must be selected carefully however. If the choices are not mutually exclusive, if not all logical choices are listed, or if the question is too complex, respondents may become frustrated and eventually alienated because they do not know or are not sure which response to choose. Partially closed-end questions provide a good compromise between the two extremes of open-end and closed-end questions.

4.5 THE SURVEY INSTRUMENT AS A TOTALITY

A questionnaire or an interview form is not just a collection of individual questions any more than a single question is just a collection of words. Labaw (1980) presented this idea well in describing the survey instrument as a totality or gestalt that has four layers: words, questions, format, and hypotheses. Problems can arise at each level if the instrument is not designed and written carefully.

Starting at the greatest level of detail, individual questions can have such wording problems as ambiguity, use of words with multiple meanings, and use of words or concepts unfamiliar to typical respondents. The second layer, questions, may be problematic in terms of reliability and validity or in eliciting biased responses. Some examples of "bad" questions are those that are not understandable to most respondents because of the words or concepts used, questions that are not answerable because respondents do not have the information at hand or because the true answer is not one of the answer options listed, and questions that lead the respondent toward answering in a particular way. For example, a question worded "Do you agree with state fishery managers that the minimum size limit for smallmouth bass should be increased?" is leading because it "begs" an affirmative answer that is in accord with that of the fishery manager.

The third layer, format, refers to the numerous sections of a questionnaire and the order of both the sections and the individual questions within each section. Questions should be put in an order that avoids "position bias." Position bias occurs whenever respondents answer inaccurately or untruthfully because of information, stated or implied, that is presented earlier in the survey instrument. For example, one would not ask a series of questions about health advisories and *then* ask if respondents are aware that health advisories exist. A second consideration is that of providing a smooth, logical flow and using brief transition statements throughout the questionnaire. A third is clearly specifying any skipping or branching that may be used in the questionnaire. Finally, if a mail questionnaire has a built-in coding or data entry format, it should be designed in a way that does not confuse respondents (see Section 4.5.1).

The final and broadest of Labaw's questionnaire layers—hypotheses—might better be termed "objectives," because studies often are exploratory or designed to monitor trends, not designed around specific research hypotheses. Although it is not visible in the questionnaire itself, this layer is a reminder that angler surveys should have stated objectives. The information needed to adequately address the study objectives should be itemized first. Then questions can be written and the questionnaire can be designed to gather that information.

Once a questionnaire is written to serve specific, preestablished objectives Labaw and other survey research experts advise surveyors to stop writing questions and spare often busy respondents of giving other "nice to know" or "curiosity" information. This advice comes not only out of consideration for respondents. The results of "add-on" questions often raise more questions than they answer because the broader topic to which such questions apply has not been fully developed. A potential add-on question should itself be subjected to the following queries.

- Does this question have such a close relationship to other covered topics that it can be logically covered in this questionnaire?
- Is the topic of the question sufficiently important that it should be elevated to a research objective or subobjective? (If so, the objectives should be revised.)
- Can this question and the topic to which it applies be sufficiently explored to provide meaningful information and still stay within the length limitation needed to achieve a good response rate to the survey?

Only if one can answer all three queries affirmatively should additional questions on other topics be added to a questionnaire.

4.5.1 Machine-Readable Surveys

Machine-readable surveys can be processed very quickly and they theoretically eliminate the need for manual data entry. However, such surveys have two disadvantages. First, they require all questions to be in closed-end format. This eliminates the possibility of "other" categories with write-in options. Hybrid surveys that are largely machine readable are possible, but the need to use data entry for the open-end questions removes a great deal of the time savings of a machine-readable format.

The second disadvantage of a machine-readable format is that it significantly reduces response rates to mail surveys. Although comprehensive research on this topic is lacking, we are aware of several machine-readable surveys that have evoked low response rates, and we attribute this to several reasons. First, some machine-readable format instruments look a lot like a standardized examination form. The first impression they make on a recipient is neither pleasing nor inviting. Second, the print and background colors of these instruments often require respondents to exert additional reading concentration. Third, the format used in these instruments to signify variables and variable numbers is confusing to some respondents.

We recommend that machine-readable instruments not be used for mail surveys. For face-to-face or telephone surveys that require no open-end questions, a machine-readable format can save both time and money.

4.6 WORDING QUESTIONS

The wording of questions in a straightforward, concise, and unambiguous way is an art. Although one gets better at the wording of questions with experience, even those for whom questionnaire design is a career benefit from the input of others. As is true of writing generally, one is more likely to find flaws or ambiguities in someone else's survey questions and overlook those in one's own.

Payne (1951), in the last chapter of his classic book, listed "a concise check list of 100 considerations" in wording questionnaires. A synopsis of the considerations most applicable to fisheries surveys follows.

The Issue. Fisheries surveys often involve an issue such as a proposed change in regulations. First, one should develop a clear understanding of the issue and its ramifications for various publics, then try to evaluate how meaningful the issue is to these publics. The type of question to be asked (e.g., open-end, multiple choice) must be based in part on the public's understanding of the issue. If the public has little recognition of the issue, it may be necessary to ask some probing, open-end questions. The issue should be stated as precisely as possible.

The Free-Answer (Open-End) Question. An open-end question should be used only if necessary. If it is, it should be given as much direction, or as much a frame of reference, as possible. One should indicate how many ideas are wanted in response or add a question that probes further (e.g., "Why" or "Why not?").

Two-Way (Dichotomous) Questions. Both options of a dichotomous question should be stated clearly. If a question embraces both a positive and a negative option—"Should the state continue to stock lake trout, or not?"—it may be necessary to spell out the "or not" portion so that the implications are understood. All reasonable alternatives usually should be included, such as "don't know" or "no opinion." Qualified options may also be needed.

Multiple-Choice Questions. All alternatives of a multiple-choice question should be given. The choices should be mutually exclusive. Whether respondents will be allowed only one or more than one choice must be decided.

Treatment of Respondents. Respondents must not be talked down to. Good grammar and sentence structure must be used, and slang should be avoided. Words with multiple and therefore unclear meanings should be replaced with synonyms that do not pose this problem. Double negatives should be avoided, as should questions worded such that a "yes" means "no," and vice versa.

The Words Themselves. As few words as necessary should be used. Simple, familiar, frequently used words are preferable to polysyllabic words whenever simple words can adequately express the idea.

Loading. Issues the public are least interested in or familiar with are most easily distorted by "loaded" language. Stating the status quo (in the body of the question) and identifying it as such has a strong impact on how the question is answered. Many people with low involvement in a particular issue, who are not dissatisfied with current policy, choose a "no change" option even though they would be just as satisfied with a different option.

Readability. Words of emphasis should be underscored. The units (e.g., percentages, dollars, miles) that apply to each response should be indicated.

In addition to the above considerations from Payne, the following principles of wording good questions should be remembered. They are most critical in mail questionnaires, where no opportunity for further explanation exists. However, failure to follow them can lead to biases or incorrect data in any type of survey.

Time Frame. The time frame covered by the survey should be clear. Even if this is stated in the cover letter or the instructions at the beginning of the questionnaire, it is helpful to work the time frame into a few questions throughout the survey to reinforce it to the respondent.

Dimensions. The question should be worded so that the dimension of the answer is indicated and defined, if it is not obvious. Then, where appropriate, an answer heading can reinforce the dimension or unit of the answer. This type of consistency will help ensure that questions are correctly interpreted. Example:

Between June 1 and August 31, 1993, on how many different days did you fish the following bodies of water?

Water Body	Different Days Fished 6/1–8/31/93
Jones Pond	________
Red River	________
Lake Oswego	________

This example illustrates one of the most basic types of questions asked in an angler survey, and it may seem very straightforward. Yet, most anglers have never answered a fishing survey and are not familiar with the concept of an angler-day. Thus, the challenge is to briefly word the question in a way that elicits the desired information. In this example, "on how many different days" is incorporated into the question. It is then reinforced by an answer heading that incorporates both "different days fished" and the time frame. Other acceptable formats and wordings undoubtedly exist, but they should be consistent with the above principle.

Clarity of Issues. Only one concept or issue should be included per question. Worded another way, the issue must not be confounded. Bad example:

Do you favor or oppose stocking of coho, chinook, and Atlantic salmon in the large lakes and tributaries of this state? (Followed by "favor," "oppose," and "no opinion" categories.)

Respondents do not necessarily have the same opinion about all three species. If the question is phrased this way, an answer choice should be given for each species.

Similarly, preference or attitude questions that contain two independent clauses separated by conjunctions such as "and" and "because" can be troublesome, as the following examples show.

> Do you feel the state should stop stocking Pacific salmon and start stocking Atlantic salmon in *xyz* waters?
>
> Do you feel that foulhooking of salmon should be outlawed because the practice promotes disrespect for the resource and leads to unruly angler behavior? (Both questions would have "Yes," "No," and "No opinion" categories.)

The first of these bad examples contains two issues: whether or not to stop stocking Pacific salmon and whether or not to start stocking Atlantic salmon. How does the angler respond who wants both species groups, or neither, stocked? The second bad example confounds three issues: (a) that foulhooking of salmon should be outlawed (for whatever reason), (b) that the practice promotes disrespect for the resource, and (c) that the practice leads to unruly behavior. It might be argued that this wording is not necessarily a problem; anglers who believe (a) and (b) and (c) should answer "Yes"; all others with an opinion should answer "No." But many respondents are not acquainted with this logic. Some who do not agree with a portion of the question will select "No opinion"; others will write out their view or become frustrated and not answer at all. Furthermore, the analyst will be in a poor position to interpret the "No" responses. There are too many possible combinations of reasons that could lead one to answer "No."

Clarity of Questions. Draft questions should be reviewed carefully for any ambiguities. Often a question written in perfectly good English will have ambiguities, requiring a revision of the question. Example:

> What were your expenditures in association with fishing at the above sites? (To be followed with a listing of expenditure categories.)

The word "you" ("your") can be singular or plural. If not specified, the respondent does not know whether to give only his personal expenditures, those she personally paid for, those of her family, or those of her entire party. Depending on the composition of the fishing party, there could be four answers to this question. Which interpretation should be used is related to the sampling design. If the design specifies each licensed angler as a sampling unit, any expenditures made by the responding angler might be important, and the question should be revised accordingly.

> Please indicate below any expenditures you personally made in conjunction with fishing trips to the above sites, whether on your own behalf or that of others in your fishing party.

Other expenditure considerations, such as the time frame and where the expenditures were made (e.g., near the residence, in transit, or near the fishing site), also need to be incorporated into the question.

Help in wording questionnaires can be obtained on at least three occasions during the design stage. After one or two drafts, a supervisor or a university human dimensions researcher can be asked to review the instrument. After further revisions, it can be given to colleagues for their opinions. In all cases, reviewers should be told the survey objectives, not just shown the draft questions. This will help them judge not only the clarity of the questions, but also the degree to which the answers will provide the needed information.

Finally, after the questionnaire has been revised in light of reviewers' comments, it should be pretested. Several people for whom the survey is relevant (usually part of the survey population) can be asked to complete the questionnaire and then to discuss it with its author. Even though test respondents may provide answers, this does not necessarily mean that they interpreted the questions correctly, so it is important to discuss with them their thought process as they answered each question. Any problem they had with the interpretation of any question, even if they guessed correctly, should be identified. If only one person in a small test group has a problem with a question, that problem is very likely to recur in the larger sample group, so the test responses should be taken seriously and the instrument improved accordingly.

4.7 ORDER OF QUESTIONS

The questionnaire should be arranged in major topic areas, and it should cover each topic in a logical order before it proceeds to the next topic. However, topic sections of the questionnaire should be arranged for the convenience of the respondent, not necessarily of the analyst. For example, suppose part of the survey deals with anglers' fishing trips to a particular waterway over the past season, and includes questions about effort, expenditures, and access and other fishing-related services. Other portions of the survey might ask some general questions about amount of fishing done in total and access improvements needed for other waterways in the state. The analyst might wish to examine all the effort questions in one block and all the access questions in another. The analyst is perfectly free to arrange the reporting of the data in any order. But it will be much easier for the angler to focus on trips made during the season to the specified waterway, and to answer all of the items pertaining to that waterway, before his or her thoughts are shifted to a broader subject. When such a shift occurs, it should be introduced with a transition statement such as "Next, we would value your opinion about the following statewide fishing topics."

Beyond the principle of arranging the questionnaire to "flow" in a manner that is convenient and logical to the respondent, there are some general rules for maintaining as much interest in the process as possible and thereby enhancing the likelihood that the questionnaire or interview will be completed. The first few questions should be of general interest to the entire survey population, should not be difficult to answer, and should not require lengthy answers. Particularly in a mail questionnaire, one should sparingly ask open-end questions in the first couple of pages—if at all. Any sensitive questions should be placed toward the end of the survey. Among these are demographic questions, which may include age, income, and education. They also include questions that might be considered threatening by some, such as whether or not an angler had an applicable fishing license, was familiar with a particular regulation, or had ever violated a regulation.

The potential respondent will usually scan a mail survey before deciding whether to answer it or not. In doing so, more attention will be paid to the cover letter and the first portion of the survey than to the last portion. Telephone respondents may initially agree to answer a survey, but they may terminate the interview if the first portion is difficult or otherwise not to their liking. Regardless of the type of survey, the more of it that the respondent completes, the more committed he or she typically becomes to completing it. Thus, much of the challenge lies in getting the potential respondent to begin. Among respondents who get to an income question at the end of the survey and feel that they do not want to answer it, some will answer it anyway because of their previous effort in answering the questionnaire or interview up to that point. Perhaps 10%–15% will leave it blank or refuse to answer it. However comparisons of response rates for questionnaires with and without a sensitive question (such as one about income) at the end suggest that sensitive questions alone deter relatively few people from completing and returning the rest of a mail survey.

Part III

ANGLER CONTACT METHODS

Chapter 5

Overview of Contact Methods

5.1 INTRODUCTION

Seven basic survey methods are used to estimate angler characteristics and activities: *mail; telephone; door-to-door; fishing logbooks, diaries,* and *catch cards; access point; roving;* and *aerial surveys.* The first four are *off-site* methods; the last three are *on-site* methods. Each of them has strengths and weaknesses that will be discussed in Chapters 6-12 and compared in Chapter 13. Actual angler surveys may be combinations of these basic designs, and such *complemented surveys* are treated in Chapter 14. An example of a complemented survey is a mail survey to elicit angler opinion followed by a small telephone survey of non-respondents. Another example is a telephone survey to estimate fishing effort combined with an access point survey to estimate catch rate.

The structure of Part III of this book is shown diagrammatically in Figure 5.1.

5.2 OFF-SITE VERSUS ON-SITE SURVEYS

Off-site surveys, meaning surveys conducted away from fishing sites, are usually based on sampling from a list of anglers (license file) and interviewing people by mail, by telephone, or door to door. Sometimes data are gathered from fishing diaries, catch cards, or logbooks; we classify this as an off-site method because the data are self-reported (by anglers) as they are in other off-site surveys, although the data sometimes may be received on site. Traditionally, off-site mail, telephone, and door-to-door surveys have been used primarily to sample angler opinion; diaries, catch cards, and logbooks are used to estimate catch and effort. Costs and complexity increase as one moves from diaries to mail to telephone to direct contact with anglers in their homes via a door-to-door household survey.

On-site methods are based on sampling from a list of fishing places and times. Anglers are counted and often interviewed while in the act of fishing or just as they come off the water. On-site methods are often used to estimate fishing effort and catch. Access point and roving surveys can be used to estimate both effort and catch. Aerial surveys of boats on the water can be used to estimate only effort.

On-site methods allow more information to be verified by the survey agent. For example, during access or roving surveys, the catch can be inspected by trained agents who are less likely than anglers to make mistakes in identification of species or in measurement of fish size. Off-site methods depend on self-reported data and suffer from the vagaries of the anglers' memory, knowledge, and truthfulness.

ANGLER CONTACT METHODS

5. Overview of Contact Methods
- Single-Time versus Repeated Surveys
- Frame Types, Construction, Incompleteness
- Survey Errors

Categorization of Methods

Off-Site Methods	On-Site Methods
6. Mail Surveys	10. Access Point Surveys
7. Telephone Surveys	11. Roving Surveys
8. Door-to-Door Surveys	12. Aerial Surveys
9. Logbooks, Diaries and Catch Cards	

13. Comparison of Survey Types
- Strengths and Weaknesses
- Practical Considerations
- Statistical Considerations

14. Complemented Surveys
- Surveys with Two or More Contact Methods
- Examples
    - Mail with Telephone Follow-Up
    - Telephone for Effort; Access for Catch

Figure 5.1 An overview of the structure of Part III. Numbered entries are in chapters.

5.3 SINGLE-TIME VERSUS REPEATED SURVEYS

Some surveys are conducted only once, but many are repeated over time. The repeated surveys may or may not use the same sampling units (anglers, places, or times). If the sampling units are repeatedly used, complex longitudinal information about the individual units may be obtained. Surveys that repeat contact with some or all of the sampling units are now commonly called panel surveys (Kasprzyk et al. 1989).

Bailar (1989) listed the following types of surveys.

Single-Time Surveys. One-time surveys produce estimates for a single point in time. Such surveys are used to obtain data that will facilitate a contemporary management decision, to provide an independent check on management conclusions drawn from other information, or simply to learn something about a fishery that cannot be monitored regularly.

Repeated Surveys with No Designed Reuse of Sampling Units. Repeated surveys with independently drawn sampling units are often called periodic or recurring surveys. An agency may wish to track overall annual trends in an important fishery but does not need to resolve the trends to the level of individual sampling units. The design would probably be similar every year but randomized sampling units would be drawn anew each time. A particular sampling unit would be redrawn only by chance.

Repeated Surveys with Partial Overlap of Sampling Units. Some surveys scheduled at regular intervals include rotating panels of sampling units. Units are introduced into the survey for a while and then rotated out of it. The main purpose of the overlap is to reduce the variance of estimates. This type of survey provides some longitudinal data from the units that are sampled on several occasions, but it allows a steady influx of information from new units entering the panel.

Longitudinal Surveys with No Rotation of Sampling Units. The classical longitudinal survey follows a particular group of sampling units over time to create a longitudinal record for each. For example, the same group of anglers could be surveyed continually to learn how their fishing activities and attitudes change over a period of years.

Longitudinal Surveys with Rotation of Sampling Units. Some surveys with the same objectives as a classical longitudinal survey are given the flexibility for introducing new sampling units. In a long-term survey of angler attitudes, for example, sampling units would be introduced, interviewed for a period of years, and rotated out. New anglers would be rotated in to keep the sample size approximately constant. The progression from repeated survey with partial overlap of sampling units to longitudinal survey with rotation of units to longitudinal survey with no rotation of units is marked by the (somewhat arbitrary) length of time that individual units are tracked.

The types of estimates that can be produced by various types of surveys are summarized in Table 5.1. The more complex longitudinal surveys allow more information on changes over time to be gathered for individual sampling units. Panel surveys were discussed in detail by Kasprzyk et al. (1989). Severe nonresponse problems may arise if the same anglers are interviewed over several years. This problem must be addressed and minimized at the design stage. Some kind of inducement or reward may be necessary to keep anglers in the survey for the required period.

5.4 SAMPLING FRAMES

In this section we consider some special issues in the choice of frame, properties of the frames, and how to deal with incomplete frames (Figure 5.2).

5.4.1 Types of Frame

Off-site surveys typically use *list* frames such as lists of anglers who have purchased fishing licenses, stamps, or permits. Some other lists that may be used in fisheries-related surveys are lists of registered boat owners, lists of angling clubs, and lists of angling club members. Sometimes the completeness of such lists may be questioned. For example, an angler license file may be incomplete for reasons such as age exemptions or failure of an angler to comply with the law to

Table 5.1 Kinds of estimates that can be produced by various types of surveys. (Modified from Bailar 1989.)

	Type of survey				
Kind of estimate	Single time	Repeated, no sampling unit overlap	Repeated, partial unit overlap	Longitudinal, no unit rotation	Longitudinal, unit rotation
Point in time	×	×	×	×	×
Duration, transition, frequency of occurrence	×	×	×	×	×
Relationships among characteristics	×	×	×	×	×
Net change		×	×	×	×
Trends		×	×	×	×
Rare events—cumulated data		×	×		×
Gross change			×	×	×
Characteristics for longer time periods based on cumulated data				×	×

purchase a license. Incomplete lists have statistical implications, as outlined in Section 5.4.2.

Area, time, and *area* × *time* frames typically are used in on-site angler surveys, and they are constructed by the investigators. With an area frame, a region, lake, estuary, or stream is divided into sections, and samples are drawn from the array of sections. Such surveys may be single-time, repeated, or longitudinal (Section 5.3). With a time frame, a year, a fishing season, or other defined period is divided into smaller time units (half days, days, weekdays and weekend days, etc.) and samples are drawn from these units. The most common on-site frame combines space and time and is termed an area × time frame. For example, an investigator may establish a complete list of access points for a fishery and combine this with a list of possible times (days or part days) for sampling. Suppose a small lake has only three access points, and that sampling will be done there for 30 days. Then the matrix of possible samples consists of 3 × 30 space–time combinations, from which an appropriate number of samples would be drawn:

SAMPLING FRAME ISSUES

- Types of Frames Available
  - List Frames
  - Area × Time Frames
- Frame Construction
- Incomplete Frames
  - Biases Induced
  - Estimation of Frame Size

Figure 5.2 A summary of sampling frame issues.

	Access point		
Day	1	2	3
1	×	×	×
2	×	×	×
⋮	⋮	⋮	⋮
30	×	×	×

Area, time, and area × time frames are fairly complete because the investigator knows or defines them. If all the areas of possible contact (such as access points) are not known, an alternative contact method (roving or aerial, perhaps) will have to be used.

5.4.2 Complete and Incomplete Frames

In classical sampling theory, a complete frame is assumed; that is, all population units are presumed to be known so that a probability sample can be drawn from the population. Estimators of population parameters, such as a mean or a total, then have known properties and are easily studied theoretically or numerically. Books on sampling theory (e.g., Cochran 1977) concentrate on this situation and give properties of estimators for common sampling designs such as simple random sampling, stratified random sampling, and multistage (cluster) sampling. Chapter 3 is based on this premise.

Area, time, and area × time frames usually are complete because the investigator makes them so. List frames, however, may be incomplete, as indicated previously, and an investigator may have only one or more incomplete list frames. The usual approach in this situation is to merge all the incomplete lists and ignore any remaining incompleteness. To the extent that the list remains incomplete, estimates of population means may be biased negative or positive and population totals will usually have a severe negative bias.

An investigator who knows or suspects a frame is incomplete may alternatively attempt to estimate the true frame size with capture–recapture sampling (or dual record sampling) with the multiple lists (Fraidenburg and Bargmann 1982; Pollock et al. 1993). However, this technique has not been widely used as yet, and we do not consider the topic further.

5.5 TYPES OF SURVEY ERROR

All contact methods are subject to different types of survey error. These can be broadly grouped as sampling errors, response errors, and nonresponse errors (Figure 5.3). Many of these have been described in previous chapters.

5.5.1 Sampling Errors

Improper Sample Selection. Improper sample selection results when the sample is not drawn according to the probability sampling techniques described in Chapter 3. If an inexperienced surveyor decides to cut costs by sampling only the easiest-to-reach access points, for example, the survey results will be statistically indefensible.

TYPES OF SURVEY ERROR
SAMPLING ERRORS
•Improper Sample Selection
•Incomplete Frame (Undercoverage)
•Duplications (Overcoverage)
•Avidity Bias
•Length-of-Stay Bias
RESPONSE ERRORS
•Recall Bias
•Prestige Bias
•Rounding (Digit) Bias
•Lies (Intentional Deception)
•Question Misinterpretation
•Species Misidentification
•Incorrect Measurements (Lengths, Weights)
NONRESPONSE ERRORS
•Refusal to Answer
•Unable to Answer

Figure 5.3 Types of survey error grouped in the three general categories of sampling, response, and nonresponse errors. (Adapted from Essig and Holliday 1991.)

Incomplete Frame. When important components of the population are unavailable to be sampled, the frame will be incomplete. For example, a license file frame may be incomplete with respect to anglers under a certain age who are not required to buy a fishing license. This problem is also referred to as undercoverage of the population.

Duplications in the Frame. If some names occur on a list more than once and the survey agent has not been able to remove them, the duplication can distort the sample. Duplications often occur in telephone surveys that rely on a telephone directory frame, but they arise in other types of surveys as well.

Avidity Bias. Some anglers are more avid than others (i.e., they fish more often). During on-site access point and roving surveys, anglers are sampled in proportion to their avidity, whereas anglers drawn from a license file are sampled with equal probability. In opinion surveys, anglers should be sampled with equal probability. In catch surveys, however, it is appropriate to sample avid anglers more and the frame is the body of water at a particular time (area × time frame). Thompson (1991) discussed avidity bias at length and showed how to weight estimates made from on-site opinion surveys to remove the avidity bias. Avidity bias is discussed further in Chapters 10 and 13.

Length-of-Stay Bias. Like avidity bias, length-of-stay bias is a special type of "size-biased" sampling. It arises in roving surveys, in which anglers are interviewed with a probability that depends on how long they have been fishing. If anglers who fish longer (or shorter) than average differ with respect to measured characteristics (e.g., catch rate), this can cause a bias. Length-of-stay bias is discussed at length in Chapter 11.

5.5.2 Response Errors

Respondents may give incorrect information to a survey agent for a variety of reasons. These are listed below; most of them are discussed in detail in Chapter 4.

Recall Bias. Anglers may have difficulty recalling past events. Events may be forgotten or they may be placed in the wrong time interval.

Prestige Bias. Anglers may exaggerate their catch rate and the size of the fish they caught in self-reported surveys.

Rounding or *Digit Bias.* Anglers may round their catch (often upwards) to numbers with a 0 or 5 as the last digit (14 fish are rounded to 15, 18 to 20).

Lies or *Intentional Deception.* Anglers may deceive an agent if they are angry with the fisheries agency conducting the survey, if they know they have broken a fishing regulation, if they think they can influence fishery rules to their benefit, or for other reasons.

Question Misinterpretation. Long, complex, or convoluted questions on a questionnaire or in a personal interview may be misunderstood by respondents.

Species Misidentification. Anglers who report their own catches or those of other members in their party may not be able to identify species accurately, especially if several closely related species could have been caught.

Incorrect Measurements. Lengths and weights of fish caught may be reported erroneously, especially when the survey data are self-reported by the angler.

5.5.3 Nonresponse Errors

Nonresponse may be a serious problem, especially in mail surveys (Chapter 6). The problem is not nonresponse per se; it is that the anglers who do respond are often different from the anglers who do not respond, causing *nonresponse bias* (Essig and Holliday 1991). Nonresponse may be due to refusal or to being unable to answer.

Refusals. Potential respondents may decide, for whatever reason, not to take part in a survey. Refusals are highest in mail surveys and lowest in personal interview surveys. Short and user-friendly questionnaires help to reduce refusal rates.

Unable to Answer. Potential respondents may be unable to answer because they are not available (e.g., they are not at home for a telephone survey) or because they do not understand how to answer due to language difficulties or illiteracy.

Nonresponse errors and resulting biases are treated more fully in Chapter 6.

Chapter 6

Mail Surveys

6.1 INTRODUCTION

Mail surveys have been the preferred off-site survey method for many fisheries agencies because they are relatively simple and cost-effective. Mail surveys can be conducted over any defined geographic area to sample opinions about fishing issues and to develop sociological and economic profiles of anglers or of communities affected by fisheries (Lowry 1978; Harris and Bergersen 1985; Williams et al. 1986; Brown 1991). Mail surveys may also be carried out as supplements to on-site creel surveys (Brown 1976, 1977, 1991), and with proper precautions, they may sometimes be used to obtain catch and effort information (Brown 1991; Essig and Holliday 1991; Section 15.4.2).

In this chapter we describe the types of mail surveys and their sampling frames, outline practical approaches to survey design, discuss the special problems of nonresponse bias in mail surveys and the use of telephone follow-up surveys of nonrespondents, and summarize the strengths and weaknesses of mail surveys. Appendices 6.1 and 6.2 provide two detailed examples of mail questionnaires.

6.2 TYPES OF MAIL SURVEYS

Mail surveys of anglers fall into two basic types: *license file surveys* and *add-on surveys* (Brown 1991).

License file mail surveys, as the name implies, draw upon files of fishing licenses as the sampling frames. License files are maintained by state or provincial agencies, and license holders usually are retrievable by county of sale. Consequently, license file surveys typically are geared to political units up to the size of states and provinces. They are used most often for socioeconomic assessments. Some license files are computerized, and drawing simple random or stratified random samples from such files is straightforward. Some agencies, however, are unable to computerize their license files because of the cost. These noncomputerized files are often sampled by systematic random sampling (perhaps within strata), because it is very difficult to obtain simple random or stratified random samples of box files of license cards that may not even be numbered consecutively. It is much easier to sample every *n*th license after a random start. Systematic random sampling is discussed in more detail in Section 3.4.

Add-on mail surveys are those that follow an on-site survey for the purpose of gathering more, or more detailed, information than could be obtained during a direct field contact. A frequent and very important purpose of add-on mail surveys is to learn from anglers the economics of their recently completed fishing trips. Typically an area × time sampling frame is used in the on-site survey. Anglers will be selected in proportion to how often they fish, and in such cases the mail sample will be subject to avidity bias (Thompson 1991). If the on-site survey is a roving survey (Chapter 11), the mail sample also will be subject to length-of-stay bias. Both avidity and length-of-stay biases are discussed further in Chapter 13. An add-on mail survey is part of a complemented survey, which we discuss in Chapter 14.

6.3 DESIGN CONSIDERATIONS

The structure of a typical mail survey is outlined in Table 6.1. It is based on material in Dillman (1978), but we have added the use of rewards and telephone follow-up surveys to evaluate the characteristics of nonrespondents; such evaluations allow nonresponse bias to be estimated. Dillman (1978) showed that nonresponse can be minimized only by a concerted overall effort, and he stressed the importance of professionalism, personalization, honesty, directness, and attention to detail in survey work. He termed his approach the "total design method."

6.3.1 First Mailing

The first mailing consists of a cover letter, a numbered questionnaire, a postage-paid return envelope, and perhaps an inducement to participate in the survey. These materials are sent by first-class mail with a clearly indicated return address (to allow assessment of nondeliverables) to all of the members of the proposed sample (Table 6.1, Appendix 6.1).

The cover letter should be written on official letterhead and signed by the leader of the survey team. The date printed on the letter should be the actual mailing date. The letter should begin with a brief but clear explanation of the survey's purpose and social usefulness. The importance of the respondent's reply should be established as well as who the respondent should be (because sometimes someone else opens the mail). The letter should promise confidentiality and explain that the questionnaire has an identification number only so the researcher may check the respondent's name off the mailing list when the questionnaire is

Table 6.1 Practical design procedures for carrying out a mail survey. Nonresponse is explicitly addressed with second and third mailings and a telephone follow-up sample of nonrespondents.

Item	First mailing[a]	Second mailing	Third mailing (certified)	Telephone follow-up
Personalized cover letter	×	× (new)	× (new)	Script
Questionnaire	×	×	×	Script
Postage-paid return envelope	×	×	×	
Inducement or reward	×[b]			
Weeks since first mailing[c]	0	3	7	10
Sent to	All in sample	All nonrespondents	All remaining nonrespondents	Subsample of remaining nonrespondents

[a]Sometimes a postcard is also sent 1 week after the first mailing.
[b]Inducements are not always used but are likely to be widely used in the future.
[c]Times are rough approximations because circumstances vary from study to study.

returned. (The respondent's name will never be placed on the questionnaire.) The letter should conclude by reiterating the importance of a response, mentioning a reward or other inducement if one is offered, and giving a telephone number respondents may call if they have questions. Rewards might be small amounts of money, premiums like caps or T-shirts, or entry into a lottery of respondents for drawing after completion of the survey analysis.

Questionnaire design is as important for mail surveys as for any other type of survey. The development of questionnaires was treated in Chapter 4, and two examples of actual mail survey questionnaires are presented in Appendices 6.1 and 6.2. Each questionnaire should have an identification number on the top of the first page. We believe that respondents' potential concerns about confidentiality are best addressed forthrightly in the cover letter, including a justification for the number on the questionnaire. Mail survey questionnaires should have brief, clearly stated questions, and the questions should be as few as possible. Open-end questions should be used sparingly, because they are hard to analyze and interpret when there is no opportunity for follow-up questions to clarify confusing answers.

The final element of the mailing is the preaddressed, postage-paid return envelope. Business reply envelopes are probably best because stamps take time to apply and also because the postage only has to be paid on the business reply envelopes actually returned. Based on his research, however, Dillman (1978) stated that stamped return envelopes produce a slightly higher response than business reply envelopes.

The cover letter, questionnaire, and return envelope should be folded and stuffed together in the mailing envelope. Separate folding of elements suggests a less personal approach. When the respondent receives the envelope, the overall effect should be as pleasing as a personal business letter sent to an acquaintance.

Dillman (1978) suggested that a postcard be sent to everyone in the sample 1 week after the first mailing; the message thanks those who have already responded and reminds those who have not yet responded about the survey. The same postcard should be sent to everyone in the sample. One week after the first mailing, many questionnaires will be in the return mail, and it will be impossible to know exactly who has responded already.

6.3.2 Second Mailing

A second mailing to all nonrespondents typically is done 3 weeks after the first mailing. It has many similar elements to the first mailing (Table 6.1). However, it is important to use a new personalized cover letter (Appendix 6.1) that emphasizes that no response has been received to the first mailing and explains again the importance of the survey. It is extremely important to send a second questionnaire and a second return envelope, because the original mailing may have been thrown out or misplaced.

6.3.3 Third Mailing

A third mailing is made to all remaining nonrespondents about 4 weeks after the second. We recommend that certified mail be used despite its cost. Dillman (1978) stated a third mailing raised the overall response rate from 59% to 72% on average in some of his studies, an increase of more than 13 percentage points. The third mailing should have yet another personalized cover letter (Appendix 6.1) but the contents are otherwise similar to those in the second mailing (Table 6.1).

The ability to carry out an efficient and timely mail campaign also depends on a well-organized recording system. Many computerized data systems are now available for this purpose. Larson and Jester (1991) discussed software requirements for administering large statewide mail surveys of anglers.

6.3.4 Telephone Follow-Up Survey

The use of several mailings plus a possible reward is all directed at increasing the response rate. For some mail surveys, that may be sufficient to obtain a valid survey. Sometimes, however, a concern about bias induced by the remaining nonrespondents will be so great as to require a follow-up survey by a different contact method. The follow-up interview usually will be by telephone rather than face-to-face, which is much more expensive.

The purpose of the follow-up telephone survey is not just to increase the response rate. Its primary purpose is to estimate how the mail nonrespondents differ from the mail respondents. Therefore it is not essential to sample all of the mail nonrespondents but only to take a probability sample of them, which helps keep the costs down. If the mail survey had been a stratified random sample, a simple random sample of the nonrespondents in each stratum would be contacted. Once the telephone follow-up is complete, the mail survey estimates can be adjusted to remove the nonresponse bias.

A follow-up telephone survey presents some problems. Sometimes the mail survey frame will not provide telephone numbers, in which case those of nonrespondents will have to be found. This takes time and hence adds to the cost of a survey. Further, some people may not have a phone, or they may have moved, or they may have an unlisted number. This approach does not deal with the hard-core nonrespondent who refuses to cooperate with any survey. Usually, however, the hard core forms a very small percentage of a sample, and almost all mail survey nonrespondents respond favorably to a telephone follow-up interview, especially if it is courteously and professionally conducted.

6.4 NONRESPONSE BIAS IN MAIL SURVEYS

6.4.1 Description

Nonresponse to mail surveys is not a problem in itself; the problem is that nonresponse induces a *nonresponse bias* in the estimates. This happens because nonrespondents usually differ in important characteristics from respondents. Anglers who are very serious about their sport are more likely to respond to an angler survey than casual anglers. The two groups are likely to answer survey questions very differently, and wrong conclusions may be drawn if respondents are viewed as representative of the whole angling population. Nonresponse bias in mail surveys can be a major problem because nonresponse can be substantial. Even when a survey and its instrument have been well designed and three mailings have been made, the response rate may only reach 50–75%. We now discuss nonresponse bias in a more rigorous fashion.

Analysis of nonresponses can be thought of as dividing the population into two strata: the response stratum with population fraction $W_1 = N_1/N$ and mean $\bar{Y}_1$, and the nonresponse stratum with population fraction $W_2 = N_2/N$, and mean $\bar{Y}_2$. If $\bar{Y}_1 = \bar{Y}_2$, there is no nonresponse bias because the simple random sample from stratum 1 is still a simple random sample from the whole population. If, however, $\bar{Y}_1$ and $\bar{Y}_2$ differ, the nonresponse bias is

$$B = W_2(\bar{Y}_1 - \bar{Y}_2). \tag{6.1}$$

This equation shows that the bias gets worse both as the proportion of nonrespondents (W_2) increases and as the nonrespondents differ more from the respondents ($\bar{Y}_1 - \bar{Y}_2$). First consider the influence of W_2. If 20% of the sample are nonrespondents, $W_2 = 0.2$, but if 50% are nonrespondents, $W_2 = 0.5$ and the bias will be much larger for any constant value of ($\bar{Y}_1 - \bar{Y}_2$). Next consider the influence of ($\bar{Y}_1 - \bar{Y}_2$) when W_2 is constant. If $\bar{Y}_1 = 8$ units and $\bar{Y}_2 = 10$ units, the bias is $W_2(8 - 10) = -2W_2$; if $\bar{Y}_1 = 8$ units and $\bar{Y}_2 = 20$ units, the bias is much larger: $W_2(8 - 20) = -12W_2$.

If population proportions are to be estimated, bounds can be placed on the nonresponse bias. Cochran (1977:361) gave a simple example, which we adapt here. Suppose $n = 1{,}000$ anglers who are mailed questionnaires and 800 actually respond. The proportion of anglers strongly in favor of a restrictive regulation change is to be estimated; assume that 80 in the sample of 800 responses strongly favor the change. The best estimate of the population proportion strongly favoring the change in the population is 10%:

$$\hat{p} = 80/800 = 0.1.$$

A lower bound (l) on the proportion arises if *none* of the 200 nonrespondents strongly favor the regulation change:

$$\hat{p}_l = (80 + 0)/(800 + 200) = 80/1{,}000 = 0.08.$$

An upper bound (u) on the proportion arises if *all* of the 200 nonrespondents strongly favor the regulation change:

$$\hat{p}_u = (80 + 200)/(800 + 200) = 280/1{,}000 = 0.28.$$

The bounds on the proportion ($\hat{p}_l, \hat{p}_u$) are often very wide, as in this case.

We now consider the two ways of dealing with nonresponse bias (Figure 6.1): reducing nonresponse by good survey design (multiple mailings, use of rewards), and estimating the remaining bias with a follow-up telephone survey. The two approaches are not mutually exclusive and both can be used in the same survey.

APPROACHES TO NONRESPONSE BIAS

INCREASE RESPONSE RATE
- Good Design Principles
- Multiple Mailings
- Rewards or Inducements

ESTIMATE NONRESPONSE BIAS
- Telephone Follow-Up Survey

Figure 6.1 An overview of the approaches to dealing with nonresponse bias.

6.4.2 Ways to Reduce Nonresponse

Total Design Method. One way to reduce the nonresponse rate and hence the nonresponse bias is to use Dillman's (1978) total design method, which includes follow-up mailings (Section 6.3; Table 6.1). Cochran (1977:360) showed by real example that the respondents in different mailings may give quite different responses. In fisheries mail surveys, serious anglers are likely to respond first because they are most interested in their sport. The less serious anglers are more likely to be picked up in the second and third mailing responses or not at all.

Inducements or Rewards. Another way to reduce the nonresponse rate in a mail survey is to include some kind of incentive in the first mailing. This might be a monetary reward, a premium (such as a cap), or some kind of lottery for those who respond. General research on mail surveys has shown that monetary rewards are more effective than premiums such as key chains or note pads (Goodstadt et al. 1977). An older study showed that a monetary incentive as small as US$0.25 significantly improved the response rate relative to that of a control survey with no incentive (Blumberg et al. 1974). More recently, James and Bolstein (1990) found that incentives of $1.00 to $2.00 produced significantly greater response rates than $0.25, but that $0.25 was still better than nothing. They suggested that incentives have two effects: a psychological effect on respondents who see that researchers value respondents' time; and a monetary effect that increases with the size of the reward used. Of course $1.00 is a very small incentive nowadays. We believe that the use of incentives and rewards must increase as people become more resistant to returning the many mail surveys they receive.

6.4.3 Estimation of Nonresponse Bias

The only sound method of estimating nonresponse bias is to survey a random sample of nonrespondents by some other contact method. Usually contact is by telephone, but it could be by face-to-face interview. Recall that bias (B) in an estimate of a population mean (equation 6.1) is

$$B = W_2(\bar{Y}_1 - \bar{Y}_2),$$

where W_2 is the fraction of nonrespondents, $\bar{Y}_1$ is the population mean for the respondents, and $\bar{Y}_2$ is the population mean for nonrespondents. An estimate of $\bar{Y}_1$ is $\bar{y}_1$, which comes from the mail sample of respondents. An estimate of $\bar{Y}_2$ is $\bar{y}_2$, which comes from the telephone follow-up sample of nonrespondents. Therefore,

$$\hat{B} = W_2(\bar{y}_1 - \bar{y}_2). \tag{6.2}$$

Similar expressions can be derived for other sampling designs that might be used for either the mail survey or the telephone follow-up survey.

6.5 EXAMPLES

6.5.1 Survey of Ohio River Valley Anglers

The 1992 Survey of Ohio River Valley Anglers depicted in Appendix 6.1 was carried out by B. A. Knuth and staff of the Human Dimensions Research Unit, Department of Natural Resources, Cornell University, Ithaca, New York. The survey was sponsored by the U.S. Environmental Protection Agency in cooperation with the Ohio River Valley Water Sanitation Commission. The purpose of this survey was to learn more about freshwater fishing along the Ohio River. The researchers were interested in the activities and opinions of anglers related to fishing in and eating fish from the Ohio River. Some fish from the Ohio River contain elevated levels of polychlorinated biphenyls (PCBs) and chlordane. The researchers particularly wished to know more about how anglers have reacted to various types of health advisories issued about the safety of eating fish from the Ohio River, and they hope to use this information to help states improve the process of advising anglers about the safety of eating such fish.

Because anglers who fished the Ohio River could not be identified without a very expensive on-site creel survey, the sample was drawn from file lists of resident anglers who bought fishing licenses in a county bordering the Ohio River. The sample was divided into six strata, one for each state bordering or straddling the Ohio River. The total sample size was 3,000, and Knuth et al. initially wanted 500 sampling units from each state. However, relatively few anglers were licensed in relevant Pennsylvania and Illinois counties, so 300 were drawn from those states and 600 from each of the other four states. Systematic random sampling was used to draw the sample in West Virginia and Kentucky. Fisheries agencies in the other four states do not receive carbon copies of the licenses back from the license agents, so cluster sampling was used; that is, a sample of license agents was randomly chosen and a systematic random sample was taken from their records.

The total design method of Dillman (1978) was used for this survey, as shown by the documents in Appendix 6.1. An initial cover letter accompanied the first mailing of the questionnaire. The questionnaire itself was designed as a self-mailing piece of business reply mail to facilitate its return by anglers. A follow-up reminder was sent to all anglers in the sample 1 week later. A second full mailing with a new cover letter and questionnaire was sent to nonrespondents 3 weeks after the first, and a third mailing with yet another cover letter and questionnaire was sent 1 week later to the remaining nonrespondents. Even with these multiple mailings, the response rate was low (slightly below 50%), so a follow-up telephone survey was conducted. Knuth et al. anticipated and found a nonresponse bias because of the sampling strategy. Respondents fished the Ohio River more frequently than nonrespondents and were more likely to be aware of the health advisories. Nonresponse bias was estimated as suggested by equation (6.2).

The questionnaire, laid out in an attractive booklet, showed good construction principles (Chapter 4). Questions were worded clearly, appropriate response choices were offered, and the options were coded for easy analysis. Important questions were asked first, and background questions (on age, race, etc.) came at the end of the survey. Type was attractive and boldface highlighting was used to enhance clarity. The survey questionnaire was long but not overly so given the complexity of the information sought.

6.5.2 Texas Survey of Saltwater Anglers

In 1987, the Texas Parks and Wildlife Department conducted a statewide mail survey of saltwater anglers in conjunction with Texas A&M University (Riechers et al. 1991). Its purpose was to obtain social and economic information about respondents' fishing activities during the previous 12 months.

A systematic random sample of 6,371 anglers was manually drawn from the 218,000 holders of 1986 resident saltwater fishing stamps as of July 31, 1986. Persons of all ages who fished saltwater and coastal waters for recreation were required to have such a stamp. The survey instrument was pretested in a pilot study. For the main survey, an attractive 21-question form was used (Appendix 6.2), and Dillman's (1978) total design method was followed (with multiple mailings and personalized cover letters that clearly explained why the survey should be returned). Sixty-six percent of the sampled anglers returned usable questionnaires; 2% of the forms were returned unusable, 5% were not deliverable, and 27% were not returned. This is a reasonable response for a large statewide survey, and the authors decided not to do a telephone follow-up survey, presumably due to expense. The survey results were presented in detail by Ditton et al. (1990) and in summary form by Riechers et al. (1991).

The questionnaire for this important socioeconomic survey was soundly constructed (Chapter 4). It was divided into well ordered sections and had clear, concise questions (Appendix 6.2). The important questions begin immediately and background questions come at the end. The economic information sought often was quite complex (see, for example, question 16), and the authors were careful to specify exactly what was requested.

6.6 STRENGTHS AND WEAKNESSES

Mail surveys will continue to be popular with fisheries agencies, especially for opinion surveys, because of their relatively low cost and simplicity of operation. Many agencies conduct reasonably well-designed mail surveys with their own personnel. Other off-site methods (telephone, door-to-door) are often complex enough that specialized staff or contractors must be hired to do a survey.

One frequent weakness of a mail survey is the frame used. Typically the frame is some kind of license or permit file, which may be incomplete. For example, anglers older than 65 years or younger than 16 years may not require a license. Moreover, illegal anglers (those anglers without a license or permit) will not be included. Incomplete frames cause underestimates of population totals, and if the anglers outside the frame differ from those sampled from inside the frame, population estimates may be positively or negatively biased.

Mail survey questionnaires must be clearer than questionnaires administered by telephone or face to face if misunderstandings are to be avoided and response rates are to be kept high. Voice interviewers can usually clarify confusing questions and may be able to cajole reluctant anglers into answering questions that might be refused in a mailed questionnaire.

A mail survey with several mailings and a telephone follow-up takes a long time, typically 10 weeks or so (Table 6.1). A telephone or a door-to-door survey usually takes less time to get results. Generally it may be said that mail surveys sacrifice time and quality of response to gain lower cost.

One of the major difficulties of using mail surveys is the potential for serious nonresponse bias. The remedies we have suggested—several follow-up mailings, a telephone follow-up, and rewards—all add to the cost of the survey. However, they may be essential for a valid and efficient mail survey.

Mail surveys can be used to estimate effort and catch (see Chapter 15), but as with any survey method, memory or recall bias can be severe if the fishing occurred very far in the past. How far in the past depends on how memorable the fishing experience was; memories of trophy fishing typically remain accurate longer than memories of regular fishing, for example. Catch questions presume that anglers can identify fish species and remember fish lengths and weights with reasonable accuracy, which may not be a reliable presumption. Further, anglers may exaggerate their catches to enhance their images (prestige bias).

Appendix 6.1 Cover Letters and Questionnaire for a 1992 Ohio River Valley Mail Survey

(Courtesy Barbara A. Knuth, Cornell University)

New York State College of Agriculture and Life Sciences
a Statutory College of the State University
Cornell University

Department of Natural Resources
Fernow Hall, Ithaca, N. Y. 14853-3001

Fishery Science
Forest Science
Wildlife Science
Natural Resources
Resource Policy
and Planning
Aquatic Science

September 24, 1992

Dear Angler:

Cornell University is conducting a study to learn more about fishing in the Ohio River Valley. We are working with the Ohio River Valley Water Sanitation Commission and the U.S. Environmental Protection Agency. We are interested in the activities and opinions of anglers related to fishing and eating fish from the Ohio River. With information from you, we hope to help states improve the process of advising anglers about the safety of eating fish caught in the Ohio River.

Your name was selected in a scientific sample of anglers who purchased a license in one of the counties bordering the Ohio River. Very few anglers were chosen for the study, so your help _is_ critical to its success.

Please complete the enclosed questionnaire as soon as possible, seal it, and drop it in the nearest mailbox. Postage has been provided. Your response to the questions will remain confidential and will never be associated with your name.

If you have not fished the Ohio River in the past five years _and_ have not eaten Ohio River fish in the past year, we ask you to fill out just a few questions on the survey then mail it back to us. Even if you haven't fished the Ohio River recently, we would still like to know something about your activities. Returning the questionnaire to us with your brief answers will help ensure we do not bother you with follow-up mailings.

Thank you very much for your help.

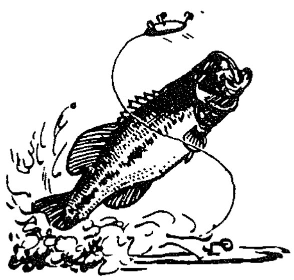
Sincerely,

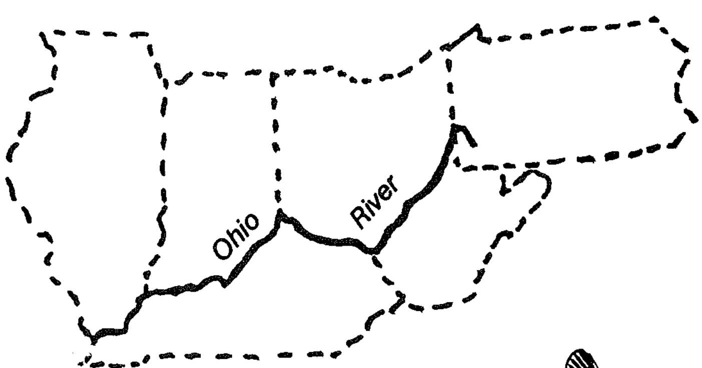
Barbara A Knuth

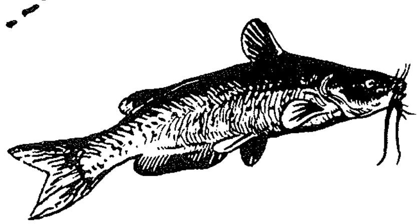
Barbara A. Knuth
Co-leader, Human Dimensions Research Unit
Assistant Professor, Natural Resource
Policy and Management

Appendix 6.1: Continued

A SURVEY OF OHIO RIVER VALLEY ANGLERS







Human Dimensions Research Unit
Department of Natural Resources
College of Agriculture and Life Sciences
Cornell University, Ithaca, NY 14853

Appendix 6.1: Continued

A SURVEY OF

OHIO RIVER VALLEY ANGLERS

Research conducted by the
Human Dimensions Research Unit
in the Department of Natural Resources
College of
Agriculture and Life Sciences
Cornell University

Sponsored by the United States Environmental
Protection Agency

in cooperation with the
Ohio River Valley Water Sanitation Commission (ORSANCO)

The purpose of this survey is to learn more about freshwater fishing along the Ohio River. We are interested in the activities and opinions of anglers related to fishing and eating fish from the Ohio River. Your answers will help improve the process of advising anglers about the safety of eating freshwater fish taken from the Ohio River.

Please complete this questionnaire at your earliest convenience, seal it, and drop it in any mailbox (no envelope is needed); return postage has been provided. Your responses will remain confidential and will never be associated with your name.

THANK YOU FOR YOUR ASSISTANCE!

Printed on recycled paper
(This questionnaire will be recycled again after results are tabulated.)

Appendix 6.1: Continued

1. **Have you gone fishing on the Ohio River within the past 5 years?**

____ Yes **(SKIP TO QUESTION 2A)**

____ No → Why not? *(Check any important reason; you may check more than 1 reason):*

____ I do not have the necessary boat or equipment
____ I believe the Ohio River is too polluted to fish in
____ I would not want to eat the fish due to contaminants
____ I do not think the Ohio River has good fishing opportunities
____ I am not interested in the sizes of fish available to be caught
____ I am not interested in the types of fish available to be caught
____ I prefer to fish other locations
____ Other *(Please list: ____________________)*

If you have not fished the Ohio River in the past 5 years <u>and</u> have not eaten Ohio River fish in the past year, please SKIP TO QUESTION 19.

2a. **How many days did you fish each of the following areas of the Ohio River between October 1, 1991 and September 30, 1992?** *(Count any part of a day as a whole day; Write 0 for those areas you did not fish.)*

I fished pools or river areas between dams about ____ days.

I fished at or near locks and dams about ____ days.

If you did not fish the Ohio River between October 1, 1991 and September 30, 1992, SKIP TO QUESTION 3.

2b. **Which lock and dam on the Ohio River is closest to the location where you did most of your Ohio River fishing between October 1, 1991 and September 30, 1992?** *(Write the name or location of the lock and dam.)*

____

_____ Check here if you don't know

Appendix 6.1: Continued

2c. How many days did you fish from shore or from a boat on the Ohio River between October 1, 1991 and September 30, 1992? *(Count any part of a day as a whole day.)*

I fished from shore (or a pier or dock) about ____ days.
I fished from a boat (or canoe or raft) about ____ days.

3. On the chart below, please list the number of Ohio River fish you personally caught and/or ate this past year (October 1, 1991 to September 30, 1992). In the first column, list how many of each fish you caught. In the second column, list how many fish meals you ate whether you, or someone else caught the fish. *(If you can't remember the number, but know you caught or ate some put a "?" on the appropriate line.)*

	Number Caught	Number of Fish Meals
American eel	____	____
Carp	____	____
Channel catfish	____	____
Flathead catfish	____	____
Freshwater drum	____	____
Largemouth bass	____	____
Paddlefish	____	____
Sauger	____	____
Silver redhorse	____	____
Smallmouth bass	____	____
Smallmouth buffalo	____	____
Spotted bass	____	____
Striped bass	____	____
Striped bass hybrids	____	____
Walleye	____	____
White bass	____	____
White crappie	____	____
Other	____	____

Appendix 6.1: Continued

For the next 2 questions, you will be asked to write down some thoughts. If you find that more than about 20 seconds pass without thinking of anything, go on to the next question. It is okay to leave space blank if you don't think of anything. There are no right or wrong answers. Once you've gone on to another question, please do not go back to these questions even if you think of more. We are interested in what you think about without any further prompting.

4. **On the lines below, please list all information you believe to be true about the safety of eating fish caught in the Ohio River.** Write your ideas down in any order. Some people write a lot of thoughts, some people very few. If more than about 20 seconds pass without thinking of anything, go on to the next question. Please write only one idea on each line. If there are more lines than you need, leave some blank. Once you've gone on to the next question, please do not return to this item even if you think of more.

   ☐ *Check here if you do not have anything to write, and go on to Question 5.*

   ____

   ____

   ____

   ____

   ____

   ____

   ____

   ____

   ____

Appendix 6.1: Continued

5. **On the lines below, please list specific actions you have taken related to the safety of eating fish caught in the Ohio River.** Write them down in any order. Some people write a lot of things, some people very few. If more than about 20 seconds pass without thinking of anything, go on to the next question. Please write only one action on each line. If there are more lines than you need, leave some blank. Once you've gone on to the next question, please do not return to this item even if you think of more.

   ☐ *Check here if you do not have anything to write, and go on to Question 6.*

   ____

   ____

   ____

   ____

   ____

   ____

   ____

   ____

   ____

Remember, please do not turn back to these questions once you have gone on to Question 6.

Appendix 6.1: Continued

6. **How concerned are you personally that eating Ohio River fish could be a health risk to you or members of your immediate family?** *(Circle one number.)*

Very Concerned	Somewhat Concerned	Slightly Concerned	Not at All Concerned	Don't Know
1	2	3	4	5

7. **How often are your household's Ohio River fish meals prepared or cooked in the following ways? Circle one number for each item to best describe how your household prepares or cooks Ohio River fish meals. SKIP TO QUESTION 7 if your household does not eat fish caught in the Ohio River.**

1=No meals; 2=Few meals; 3=Some meals; 4=Most meals; 5=All meals

	No meals				All meals
a. Remove the strip of fat along the back of the fish	1	2	3	4	5
b. Remove belly fat	1	2	3	4	5
c. Remove the skin	1	2	3	4	5
d. Eat whole, gutted fish	1	2	3	4	5
e. Fillet the fish	1	2	3	4	5
f. Pan fry	1	2	3	4	5
g. Deep fry	1	2	3	4	5
h. Make fish soups or chowders	1	2	3	4	5
i. Bake, roast, broil, or grill fish	1	2	3	4	5
j. Microwave fish	1	2	3	4	5
k. Reuse oil or fat from cooking fish	1	2	3	4	5
l. Eat frozen or canned fish caught at an earlier time	1	2	3	4	5

Appendix 6.1: Continued

8. **Some Ohio River states issue fish consumption health advisories. The advisories let people know how to limit their exposure to chemical contaminants by limiting the amount of some types of fish they eat. Only some types of fish and some areas of the River are affected by health advisories.**

   Prior to this survey, were you aware of health advisories issued for fish caught from the Ohio River? *(Check one.)*

   _____ YES, aware of advisories for certain types of fish and/or areas of the River

   _____ YES, generally or vaguely aware

   _____ NO **(SKIP TO QUESTION 13)**

9. **How important have the following information sources been to help you learn about health advisories for Ohio River fish?** *(Circle one number for each information source.)*

   1=Not At All Important 4=Very Important
   2=Somewhat Important 5=Extremely Important
   3=Important

	Not at all Important				Extremely Important
a. Newspaper article or editorial	1	2	3	4	5
b. Magazine article	1	2	3	4	5
c. Fishing regulation booklet distributed with fishing license	1	2	3	4	5
d. Newsletters from fishing clubs	1	2	3	4	5
e. Newsletters from environmental interest groups	1	2	3	4	5
f. Warnings posted at fishing access sites	1	2	3	4	5
g. Health advice brochures available by special request from government agencies	1	2	3	4	5
h. Friends or family	1	2	3	4	5
i. Television or radio	1	2	3	4	5
j. Charterboat operators or guides	1	2	3	4	5
k. My physician	1	2	3	4	5

Appendix 6.1: Continued

10. **Below are some changes you may have made since learning about the Ohio River health advisories. Please indicate how strongly you agree or disagree with each statement.** *(Circle one number for each item.)*

1=Strongly agree 4=Disagree
2=Agree 5=Strongly disagree
3=Neutral 6=Don't know

	Strongly Agree				Strongly Disagree	Don't Know
a. I eat more Ohio River fish now because I feel more confident that I can choose the safer fish.	1	2	3	4	5	6
b. I have changed the ways I clean Ohio River fish before eating them.	1	2	3	4	5	6
c. I have changed the ways I cook Ohio River fish before eating them.	1	2	3	4	5	6
d. I have changed fishing locations because of the advisories.	1	2	3	4	5	6
e. I have changed the types of fish I fish for to try to catch safer fish	1	2	3	4	5	6
f. I take fewer Ohio River fishing trips since learning about the advisories.	1	2	3	4	5	6
g. I take more Ohio River fishing trips now because I can choose areas with less serious contaminant problems.	1	2	3	4	5	6
h. I have changed the sizes of Ohio River fish I eat because of the advisories.	1	2	3	4	5	6

Appendix 6.1: Continued

11. **For each type of fish, please circle the number that best describes the change you made in the amount of Ohio River fish you eat <u>because of the advisories</u>. Circle 5 if you have never eaten that type of Ohio River fish.**

	Stopped Eating	Decreased Amount	No Change	Increased Amount	Never Ate
American eel	1	2	3	4	5
Carp	1	2	3	4	5
Channel catfish	1	2	3	4	5
Flathead catfish	1	2	3	4	5
Freshwater drum	1	2	3	4	5
Largemouth bass	1	2	3	4	5
Paddlefish	1	2	3	4	5
Sauger	1	2	3	4	5
Silver redhorse	1	2	3	4	5
Smallmouth bass	1	2	3	4	5
Smallmouth buffalo	1	2	3	4	5
Spotted bass	1	2	3	4	5
Striped bass	1	2	3	4	5
Striped bass hybrids	1	2	3	4	5
Walleye	1	2	3	4	5
White bass	1	2	3	4	5
White crappie	1	2	3	4	5

12. **Below are some reasons that may have made it difficult for you to follow the recommendations in the Ohio River health advisories. Please indicate how strongly you agree or disagree with each statement.** *(Circle one number for each item.)*

1=Strongly agree 4=Disagree
2=Agree 5=Strongly disagree
3=Neutral 6=Don't know

	Strongly Agree				Strongly Disagree	Don't Know
a. I have never eaten very many Ohio River fish.	1	2	3	4	5	6
b. I don't believe Ohio River fish pose a health risk for me.	1	2	3	4	5	6
c. I couldn't tell from the advisories which locations would have safer fish in them.	1	2	3	4	5	6

Appendix 6.1: Continued

	Strongly Agree				Strongly Disagree	Don't Know
d. I couldn't tell from the advisories which types of fish have less chemicals in them.	1	2	3	4	5	6
e. I don't know how to catch the types of fish that have less chemicals in them.	1	2	3	4	5	6
f. I couldn't tell from the advisories what sizes of fish have less chemicals in them.	1	2	3	4	5	6
g. I couldn't tell from the advisories how to clean my fish in a way that reduces chemicals in them.	1	2	3	4	5	6
h. I couldn't tell from the advisories how to cook my fish in a way that reduces chemicals in them.	1	2	3	4	5	6
i. I'm concerned about what other people might say or think about me if I followed the advisories.	1	2	3	4	5	6
j. I don't think it is important to follow the advisories.	1	2	3	4	5	6
k. Following the advisories would limit my enjoyment of Ohio River fishing.	1	2	3	4	5	6
l. Following the advisories would limit the amount of fish I eat.	1	2	3	4	5	6

13. How well informed are you about the safety of eating fish caught in the Ohio River? *(Circle one number.)*

Very well Informed	Informed	Somewhat Informed	Slightly Informed	Not At All Informed
1	2	3	4	5

14. How easy is it for you to follow the recommendations in Ohio River health advisories? *(Circle one number.)*

Very Easy						Very Difficult
1	2	3	4	5	6	7

Appendix 6.1: Continued

15. In the last month, how often have you: *(Circle one number for each item.)*

	Very Often	Often	Somewhat Often	Seldom	Never
a. Thought about the safety of eating fish caught in the Ohio River?	1	2	3	4	5
b. Had positive feelings about the safety of eating fish caught in the Ohio River?	1	2	3	4	5
c. Had negative feelings about the safety of eating fish caught in the Ohio River?	1	2	3	4	5

16. Before receiving this questionnaire, when was the last time you did each of the following? *(Check the most recent box for each item.)*

When was the last time you ↓	In the past two days	In the past week	In the past month	In the past 3 months	More than 3 months ago	Never
Went fishing in the Ohio River?						
Went fishing somewhere other than the Ohio River?						
Made plans to fish in the Ohio River?						
Shopped for fishing gear for the Ohio River?						
Ate fish from the Ohio River?						
Read or heard about the safety of eating fish caught in the Ohio River?						
Talked with others about the safety of eating fish caught in the Ohio River?						

Appendix 6.1: Continued

17. Think of the type of fishing trip you enjoy the most. (It does not have to be a trip on the Ohio River.) How important are the following factors to making the trip a really satisfying experience for you?
(Circle one number for each item.)

0 = Of no concern at all
1 = Not very important
2 = Somewhat important
3 = Important but not essential
4 = Essential for a really satisfying trip

	No Concern				Essential
a. Catching several fish	0	1	2	3	4
b. Catching a large fish	0	1	2	3	4
c. Catching at least one fish	0	1	2	3	4
d. Catching a particular type of fish	0	1	2	3	4
e. Being with friends or family	0	1	2	3	4
f. Being where the scenery is pleasant	0	1	2	3	4
g. Fishing in areas where I know the fish are safe to eat	0	1	2	3	4
h. Trying out new fishing gear	0	1	2	3	4
i. Mastering fishing skills	0	1	2	3	4
j. Catching the most fish of anyone in my group	0	1	2	3	4
k. Catching fish to eat	0	1	2	3	4
l. Fishing where there are few other people	0	1	2	3	4
m. Exploring new fishing areas	0	1	2	3	4

Appendix 6.1: Continued

18. Please indicate how strongly you agree or disagree with the following statements. *(Circle one number for each item.)*

1=Strongly agree 4=Disagree
2=Agree 5=Strongly Disagree
3=Neutral 6=Don't Know

	Strongly Agree				Strongly Disagree	Don't Know
a. The Ohio River health advisories provide me with enough information to decide whether or not to eat certain fish.	1	2	3	4	5	6
b. If the Ohio River advisories said that only larger fish were unsafe to eat, I would catch and eat the smaller fish.	1	2	3	4	5	6
c. The Ohio River health advisories have increased my interest in water pollution control and cleanup efforts.	1	2	3	4	5	6
d. Eating <u>some</u> types of fish caught in the Ohio River is safe.	1	2	3	4	5	6
e. Eating <u>any</u> fish caught in the Ohio River is safe.	1	2	3	4	5	6
f. The health benefits of eating Ohio River fish are greater than the health risks.	1	2	3	4	5	6
g. Eating contaminated fish over many years increases my health risks.	1	2	3	4	5	6
h. The health risk from eating contaminated Ohio River fish is minor when compared with other risks I'm exposed to.	1	2	3	4	5	6
i. I would eat more Ohio River fish if health risks from chemical contaminants did not exist.	1	2	3	4	5	6
j. I follow the advice in the Ohio River health advisories.	1	2	3	4	5	6
k. Most people who are important to me think eating fish from the Ohio River is safe.	1	2	3	4	5	6
l. I don't think government agencies really know how much chemical contaminants are in fish.	1	2	3	4	5	6
m. Most people who are important to me think I should follow the health advisory recommendations about eating fish caught in the Ohio River.	1	2	3	4	5	6

19. In what year were you born? 19 ____

20. Are you male or female? _____ Male _____ Female

Appendix 6.1: Continued

21. Which of the following best describes the area where you currently live? *(Check one.)*

____ Rural, town, or village (under 5,000 population)
____ Small city of 5,000 to 24,999 population
____ City of 25,000 to 99,999 population
____ Large city of 100,000 population or over

22. How many years of school did you complete, counting 12 years for high school graduation, and 1 year for each additional year of college, technical, or vocational training?

____ years

23. Please circle your approximate 1991 TOTAL HOUSEHOLD INCOME before taxes, in thousands of dollars:

5 6 7 8 9 10 11 12 13 14 15 16 17 18 19

20 22 24 26 28 30 32 34 36 38 40 45 50 55

60 65 70 75 80 More than 80

24. What is your race?

____ White, not of Hispanic origin
____ White, of Hispanic origin
____ Black or African-American
____ Asian or Pacific Islander
____ Native American Indian
____ Other

Please use the space below for any additional comments you may wish to make.

Thank You For Your Time and Effort!

To return this questionnaire, simply seal it (postage has been provided) and drop it in the nearest mailbox.

№ 5498

NO POSTAGE
NECESSARY
IF MAILED
IN THE
UNITED STATES

BUSINESS REPLY MAIL
FIRST CLASS MAIL PERMIT NO. 878 ITHACA, NY
POSTAGE WILL BE PAID BY ADDRESSEE

CORNELL UNIVERSITY
DEPARTMENT OF
NATURAL RESOURCES, B. KNUTH
PO BOX DH
ITHACA NY 14851-9978

Appendix 6.1: Continued

New York State College of Agriculture and Life Sciences
a Statutory College of the State University
Cornell University

Department of Natural Resources
Fernow Hall, Ithaca, N. Y. 14853-0188

Fishery Science
Forest Science
Wildlife Science
Natural Resources
Resource Policy
and Planning
Aquatic Science

October 2, 1992

Dear Angler:

Last week we sent you a questionnaire asking about your fishing activities and your opinions related to fishing and eating fish from the Ohio River.

If you have already completed and returned the questionnaire, please accept our sincere thanks for your help. If you have not yet completed it, please do so today. Your name was selected in a scientific sample of anglers who purchased a license in one of the counties bordering the Ohio River. Your assistance in this survey is critical to its success and important to future fisheries management and information programs about the safety of eating fish.

Thanks again for your cooperation.

Sincerely,

Barbara A. Knuth
Co-leader, Human Dimensions Research Unit
Assistant Professor, Natural Resource
Policy and Management

Appendix 6.1: Continued

New York State College of Agriculture and Life Sciences
a Statutory College of the State University
Cornell University

Department of Natural Resources
Fernow Hall, Ithaca, N. Y. 14853-3001

Fishery Science
Forest Science
Wildlife Science
Natural Resources
Resource Policy
and Planning
Aquatic Science

October 16, 1992

Dear Angler:

About 3 weeks ago we sent you a questionnaire that sought your opinions about fishing and eating fish from the Ohio River. If you have already completed and returned it to us please accept our sincere thanks. If you have not yet done so, please take the time to complete it today.

Cornell University is conducting this study to learn more about fishing along the Ohio River. We are working with the Ohio River Valley Water Sanitation Commission and the U.S. Environmental Protection Agency. With information from you, we hope to help states improve the process of advising anglers about the safety of eating fish caught in the Ohio River.

If you have not fished the Ohio River in the past five years and have not eaten Ohio River fish in the past year, we ask you to fill out just a few questions on the survey then mail it back to us. Even if you haven't fished the Ohio River recently, we would still like to know something about your activities. Returning the questionnaire to us with your brief answers will help ensure we do not bother you with follow-up mailings.

Your cooperation in completing the questionnaire will be appreciated. Your response will remain confidential and will never be associated with your name. In the event your questionnaire has been misplaced, a replacement is enclosed. Postage has been provided. Simply seal it and drop it into any mailbox.

Thank you for your help.

Sincerely,

Barbara A Knuth

Barbara A. Knuth
Co-leader, Human Dimensions Research Unit
Assistant Professor, Natural Resource
Policy and Management

Appendix 6.1: Continued

New York State College of Agriculture and Life Sciences
a Statutory College of the State University
Cornell University

Department of Natural Resources
Fernow Hall, Ithaca, N. Y. 14853-0188

Fishery Science
Forest Science
Wildlife Science
Natural Resources
Resource Policy
and Planning
Aquatic Science

October 23, 1992

Dear Angler:

I am writing to you about our study of fishing activities along the Ohio River and opinions of anglers regarding eating fish from the Ohio River. We'd like to know about <u>your</u> fishing activities along the Ohio River, and what opinions <u>you</u> have regarding the safety of eating fish from the Ohio River. Even if you have not fished recently or don't know very much about the safety of eating fish, your opinions are very important to us.

Although we have received a large number of completed questionnaires from other people, we haven't heard from you. Our past experience tells us that those who have not yet sent in their questionnaires may hold quite different opinions than those who returned their questionnaires earlier. To be able to describe opinions of anglers accurately, we need to hear from you and others who have not yet responded.

I am writing to you again because of the significance each and every questionnaire has to the usefulness of this study. Very few anglers were chosen for the study, so your help <u>is</u> critical to its success.

Your contribution to the success of this study will be greatly appreciated.

Sincerely,

Barbara A Knuth

Barbara A. Knuth
Co-leader, Human Dimensions Research Unit
Assistant Professor, Natural Resource
Policy and Management

Appendix 6.2 Questionnaire for a 1987 Texas Mail Survey of Saltwater Anglers

(Reproduced from Riechers et al. 1991)

Questionnaire #

IN THE FOLLOWING QUESTIONS, PLEASE TELL US ABOUT YOUR FISHING ACTIVITY AND EXPERIENCE.

1. How many years have you been fishing in saltwater?

 _________ YEARS

2. Since this time last year, how many days did you go fishing?

 NUMBER OF DAYS FISHED: (if none, please enter 0)

 _________ IN FRESHWATER
 _________ IN SALTWATER BAYS FROM A BOAT
 _________ IN SALTWATER BAYS FROM SHORE OR PIERS
 _________ IN SALTWATER GULF FROM A BOAT
 _________ IN SALTWATER GULF FROM SHORE OR PIERS

3. How do you compare your fishing ability to that of other fishermen in general?

 1 LESS SKILLED 2 EQUALLY SKILLED 3 MORE SKILLED

4. **BELOW IS A LIST OF REASONS WHY PEOPLE FISH IN SALTWATER. PLEASE CIRCLE THE NUMBER THAT INDICATES HOW IMPORTANT EACH ITEM IS TO YOU AS A REASON FOR FISHING.**

	Degree of importance				
Reasons	Not at all	Slightly	Moderately	Very	Extremely
a) To be outdoors	1	2	3	4	5
b) For family recreation	1	2	3	4	5
c) To experience new and different things	1	2	3	4	5
d) For relaxation	1	2	3	4	5
e) To be close to the sea	1	2	3	4	5
f) To obtain fish for eating	1	2	3	4	5
g) To get away from the demands of other people	1	2	3	4	5
h) For the experience of the catch	1	2	3	4	5
i) To test my equipment	1	2	3	4	5
j) To be with friends	1	2	3	4	5
k) To experience natural surroundings	1	2	3	4	5
l) To win a trophy	1	2	3	4	5
m) To develop my skills	1	2	3	4	5
n) To get away from the regular routine	1	2	3	4	5
o) To obtain a "trophy" fish	1	2	3	4	5
p) For the challenge or sport	1	2	3	4	5

Appendix 6.2: Continued

5. Name the kinds of fish you most prefer to catch in salwater in Texas.

_____________ FIRST CHOICE _____________ SECOND CHOICE

_____________ THIRD CHOICE

6. Do you or someone in your household own a power boat?

1 YES 2 NO

If YES, what length is the longest one? _________ FEET

7. **PLEASE INDICATE THE EXTENT TO WHICH YOU AGREE OR DISAGREE WITH EACH OF THE FOLLOWING STATEMENTS ABOUT SPORT FISHING IN SALTWATER.**

	Strongly disagree	Disagree	Neutral	Agree	Strongly agree
a) The more fish I catch, the happier I am	1	2	3	4	5
b) A fishing trip can be successful even if no fish are caught	1	2	3	4	5
c) When I go fishing, I'm just as happy if I don't catch a fish	1	2	3	4	5
d) I usually eat the fish I catch	1	2	3	4	5
e) A successful fishing trip is one in which many fish are caught	1	2	3	4	5
f) I would rather catch one or two	1	2	3	4	5
g) It doesn't matter to me what type of fish I catch	1	2	3	4	5
h) The bigger the fish I catch, the better the fishing trip	1	2	3	4	5
i) I'm just as happy if I don't keep the fish I catch	1	2	3	4	5
j) I like to fish where there are several kinds of fish to catch	1	2	3	4	5
k) I want to keep all the fish I catch	1	2	3	4	5
l) I catch fish for sport and pleasure rather than for food	1	2	3	4	5
m) I'm just as happy if I release the fish I catch	1	2	3	4	5
n) I usually give away the fish I catch	1	2	3	4	5

8. Do you participate in saltwater fishing tournaments?

1 YES 2 NO

If YES, how many tournaments do you participate in each year?

________ SALTWATER TOURNAMENTS EACH YEAR

9. What type of group do you fish with most often? (mark only one answer please)

1 BY YOURSELF
2 FRIENDS
3 FAMILY
4 FAMILY & FRIENDS TOGETHER
5 CLUB

Appendix 6.2: Continued

10. Have you gone fishing outside the state of Texas in the previous 12 months (where fishing was the primary motivation for the trip)?

1 YES 2 NO

If YES, what states did you fish in (other than Texas)?

State	Days there	Species sought	Total expenditures

11. **TO WHAT EXTENT DO YOU MAKE USE OF THE FOLLOWING FOR SALTWATER FISHING INFORMATION?**

	No use	Little use	Some use	Lots of use	Great deal of use
a) Comments and opinions of other anglers	1	2	3	4	5
b) Texas Parks and Wildlife Magazine	1	2	3	4	5
c) Other information provided by Texas Parks and Wildlife Department (brochures, etc.) ...	1	2	3	4	5
d) Newspaper articles	1	2	3	4	5
e) Magzine articles	1	2	3	4	5
f) Bait and tackle shops	1	2	3	4	5
g) Fishing clubs	1	2	3	4	5
h) Radio shows	1	2	3	4	5
i) Television shows	1	2	3	4	5

12. If you caught a tagged fish, would you report the tag?

1 YES 2 NO

13. Briefly describe your most memorable saltwater fishing trip.

14. **THE FOLLOWING IS A LIST OF TOOLS USED BY THE TEXAS PARKS AND WILDLIFE DEPARTMENT FOR MANAGING RECREATIONAL SALTWATER FISHERIES.**

Please indicate below whether you support or oppose these tools.

	Strongly oppose	Oppose	Neutral	Support	Strongly support
a) Releasing fish below a certain length (minimum size limit)	1	2	3	4	5
b) Releasing fish above a certain length (maximum size limit)	1	2	3	4	5
c) Releasing fish within a certain length range, but keeping the fish below and above this range (slot limit)	1	2	3	4	5

Appendix 6.2: Continued

	Strongly oppose	Oppose	Neutral	Support	Strongly support
d) Being able to keep only a certain number of fish you catch in a day (daily bag limit)	1	2	3	4	5
e) Not being able to fish in certain restricted areas	1	2	3	4	5
f) Having certain fishing areas closed during part of the year (closed season)	1	2	3	4	5
g) Prohibiting the use of certain types of sport fishing gear	1	2	3	4	5
h) Prohibiting the use of certain types of bait ...	1	2	3	4	5
i) Not being able to retain certain species in certain areas	1	2	3	4	5
j) Stocking fish in saltwater	1	2	3	4	5

15. Are you currently living in Texas, even if you are not a resident of Texas?

1 YES 2 NO

If YES, how long have you continuously lived in Texas?

More than 1 year? 1 YES 2 NO

If YES, how many years? ________ YEARS

16. **THE FOLLOWING QUESTION PROVIDES VALUABLE INFORMATION FOR ESTIMATING THE IMPORTANCE OF SALTWATER FISHING TO YOU AND TO THE STATE OF TEXAS. PLEASE HELP US BY BEING ESPECIALLY CAREFUL WITH THIS QUESTION.**

Please record your expenditures for the following items if purchased since this time last year. Use numbered lines to list individual purchases. To see how to complete percents for the last column, please refer to the following example:

EXAMPLE: Assume you purchased a boat and use it a total of 100 hours per year. Of this 100 hours, 25 hours were for saltwater fishing in Texas. In this case, 25% should be allocated to saltwater fishing.

		Did you purchase any of the following items since this time last year? (please circle answer)		Purchase price	Was the item, or most of the items purchased in Texas? (please circle answer)		Percent of time item was used for saltwater fishing
TACKLE:							
a) Rod(s)	(1)	YES	NO	$ ________	YES	NO	________
	(2)	YES	NO	$ ________	YES	NO	________
	(3)	YES	NO	$ ________	YES	NO	________
b) Reel(s)	(1)	YES	NO	$ ________	YES	NO	________
	(2)	YES	NO	$ ________	YES	NO	________
	(3)	YES	NO	$ ________	YES	NO	________
c) Lures, tackle boxes, landing nets		YES	NO	$ ________	YES	NO	________
d) Live bait equip		YES	NO	$ ________	YES	NO	________
e) Fish attracting lights		YES	NO	$ ________	YES	NO	________
f) Lure color selector		YES	NO	$ ________	YES	NO	________

Appendix 6.2: Continued

		Did you purchase any of the following items since this time last year? (please circle answer)		Purchase price	Was the item, or most of the items purchased in Texas? (please circle answer)		Percent of time item was used for saltwater fishing
CAMPING EQUIPMENT:							
a) Trailer or pickup camper insert		YES	NO	$ ________	YES	NO	________
b) Tents, sleeping bags, lanterns, stoves, ice chests, etc.		YES	NO	$ ________	YES	NO	________
BOATING:							
a) Electronic equipment—radios, depth finder, loran, radar, etc.		YES	NO	$ ________	YES	NO	________
b) Boat accessories—anchors, safety equipment, etc.		YES	NO	$ ________	YES	NO	________
c) Boat trailer(s)	(1)	YES	NO	$ ________	YES	NO	________
	(2)	YES	NO	$ ________	YES	NO	________
d) Boat motor(s)	(1)	YES	NO	$ ________	YES	NO	________
	(2)	YES	NO	$ ________	YES	NO	________
e) Boat(s) (except for items listed above)	(1)	YES	NO	$ ________	YES	NO	________
	(2)	YES	NO	$ ________	YES	NO	________
VEHICLES: Auto, van, pickup, recreational vehicle, all terrain vehicles (specify type)							
a) ______________	(1)	YES	NO	$ ________	YES	NO	________
b) ______________	(2)	YES	NO	$ ________	YES	NO	________
OTHER EQUIPMENT: Expenditures not listed above (specify)							
a) ______________	(1)	YES	NO	$ ________	YES	NO	________
b) ______________	(2)	YES	NO	$ ________	YES	NO	________

THE FOLLOWING QUESTIONS WILL HELP US TO KNOW MORE ABOUT FISHERMEN. THE INFORMATION YOU PROVIDE WILL REMAIN STRICTLY CONFIDENTIAL, AND YOU WILL NOT BE IDENTIFIED WITH YOUR ANSWERS.

17. What is your age? ______ YEARS

18. Are you: 1 MALE 2 FEMALE

19. What is your approximate annual HOUSEHOLD income before taxes? (circle only one)

1	UNDER $10,000	7	$60,000 to $69,999
2	$10,000 to $19,999	8	$70,000 to $79,999
3	$20,000 to $29,999	9	$80,000 to $89,999
4	$30,000 to $39,999	10	$90,000 to $99,999
5	$40,000 to $49,999	11	$100,000 AND ABOVE
6	$50,000 to $59,999		

Appendix 6.2: Continued

20. What is the ZIP code of your current home residence? ____________

IS THERE ANYTHING ELSE YOU WOULD LIKE TO SHARE WITH US?

YOUR CONTRIBUTION TO THIS EFFORT IS GREATLY APPRECIATED. PLEASE RETURN YOUR COMPLETED QUESTIONNAIRE IN THE STAMPED RETURN ENVELOPE AS SOON AS POSSIBLE.

TEXAS A&M UNIVERSITY
DEPARTMENT OF RECREATION AND PARKS
COLLEGE STATION, TEXAS 77843

Chapter 7

Telephone Surveys

7.1 INTRODUCTION

Telephone surveys have come a long way since Literary Digest magazine used one to erroneously predict the defeat of Democratic President Franklin Roosevelt by Republican challenger Alf Landon in the 1937 U.S. Presidential election. In the latter 1930s, only 35% of U.S. households had a telephone. Those households were among the wealthiest in the nation and often voted Republican. The error in this election prediction stigmatized telephone surveys for many years (Groves et al. 1988; Massey 1988). Today, over 90% of households in the United States and Canada have a telephone, and the chance of excluding major portions of the population from a telephone survey frame has become much smaller in these countries. (In many other countries, telephone coverage is still modest or low.)

Telephone surveys have not been widely used in fisheries but we believe they will become more common. Weithman (1991) described a comprehensive angler telephone survey for Missouri that has been used since 1983 to estimate statewide catch and effort. Essig and Holliday (1991) outlined the Marine Recreational Fishery Statistics Survey of recreational marine fishing (for effort and catch) around the coast of the United States, which involves both a household telephone survey and an on-site access point survey. Both of these surveys will be described later in this chapter. Although mail questionnaires have been widely used in statewide angler opinion surveys (Brown 1991), we have found little evidence that telephone surveys have been used for this purpose. Telephone surveys are more costly than mail surveys, which may account for the disparity.

In this chapter we present telephone survey methods (random-digit dialing, directories, special frames), discuss practical design considerations, give some examples of telephone surveys used in fisheries, and conclude with a discussion of the strengths and weaknesses of telephone surveys.

7.2 TYPES OF FRAMES

The conduct of a telephone survey depends on the sampling frame that is used. Commonly used methods are based on *random-digit dialing, directory frames,* and *special registration lists* (Figure 7.1). Random-digit-dialing methods include all possible telephone numbers, listed and unlisted, for both angling and non-angling households. Directory frames are telephone subscriber lists; they include both angling and nonangling households but do not include unlisted numbers. Subscriber lists also contain the subscriber's name and address. Special registration lists include fishing license lists, boat registrations, and angling club mem-

TYPES OF TELEPHONE SURVEYS

RANDOM-DIGIT DIALING	LIST FRAMES
•Basic Methods	•Directory Frames
•Multistage Methods	Regular
Mitofsky–Waksberg	Commercial
	•Special Lists

Figure 7.1 An overview of the types of telephone surveys classified by frame type.

bership lists. These special lists contain only anglers or a high percentage of anglers. For any of these designs, the completeness of the sampling frame is important. A frame should include all anglers in the target population.

7.2.1 Random-Digit Dialing

Telephone numbers in the United States and Canada are made up of 10 numbers: 3 for the area code, 3 for the prefix, and 4 for the suffix (e.g., 919-821-1647). The sampling frame for random-digit dialing actually contains all possible telephone numbers in the chosen area code and prefix; however, it is usually limited to all working residential numbers.

Basic Methods. In the basic random-digit-dialing method, the first six numbers—the area code and prefix—are selected in a predetermined manner; then the final four numbers are chosen randomly. When the investigator wants to limit the survey to a geographical area, only the particular area codes of interest and their prefixes are included at the initial selection. However, it may be difficult to match area codes and prefixes to precise geographical boundaries (Lepkowski 1988). The set of sampled telephone numbers is usually developed by randomly choosing four-digit suffixes, which are then combined with a defined set of area code–prefix combinations. The National Marine Fisheries Service uses random-digit dialing for its Marine Recreational Fisheries Statistics Survey of anglers (Essig and Holliday 1991). The survey area comprises coastal counties, and area codes and prefixes are limited to those within a 20–50-mile radius of the coast, the distance varying among states.

Simple random-digit dialing is costly and inefficient because many telephone calls must be made to eliminate nonworking and nonresidential numbers. Frey (1983) noted that as many as five numbers may have to be contacted to obtain one working residential number. In a fisheries survey, moreover, few of the households contacted will be angling households.

Multistage Methods. One common way to minimize problems of ineligible telephone numbers is to carry out random-digit dialing with a multistage or cluster sampling plan. (See Section 3.5 for a statistical discussion of cluster sampling.) One of several approaches is the Mitofsky–Waksberg design (Waksberg 1978). This two-stage cluster sampling method treats the sampling frame of telephone numbers as a set of banks of 100 telephone numbers each. A bank is defined by an area code, a prefix, and the first two digits of the suffix; for example,

919-821-16*XX*.

The last two digits define 100 possible telephone numbers within a bank ($XX = 00$ to 99). The banks are used as primary sampling units in the two-stage cluster design. Within a bank, the 100 secondary sampling units are sampled at random with equal probability and one number is chosen at random. If the telephone number is not a residential number, the entire bank is rejected. If the telephone number *is* a residential number, an interview is attempted and additional random numbers are selected within the bank until a specified number of households have been drawn. In this design, then, banks are primary sampling units sampled with probability proportional to the number of residential numbers in them, and residential numbers within a bank are the secondary sampling units sampled with equal probability. The motivation for this approach is that banks usually have either no or many residential numbers; therefore, rejection of a bank if the first number is nonresidential saves a lot of resources.

The use of the two-stage cluster designs generally leads to less precise estimates than a single-stage (simple random or stratified random) sample of the same size (Waksberg 1978). However, the designs are justified because they produce a much larger proportion of useable residential telephone numbers for a fixed amount of effort. Kalton (1983) stated that about two of three numbers selected within a nonrejected cluster (bank) were residential numbers, a much better ratio than the one-in-five success rate with basic random-digit dialing.

Panel Option. Random-digit-dialing frames cover all telephone numbers, so noncoverage of unlisted numbers is not a problem. However, such frames are very inefficient when the population of interest (e.g., households with anglers) is a small proportion of the total frame. One way to make random-digit dialing more efficient is to retain the telephone numbers of some previously identified angling households from year to year in a panel survey (Section 5.2). It might be possible, for example, to retain a proportion of known angling households for 2 or 3 years. Refusal rates might increase to unacceptable levels because anglers are bothered several times, but households may not be burdened if interviews are not long and rewards or other inducements are offered. This refinement could add substantially to the precision of estimates from random-digit-dialing designs.

7.2.2 Directory Frames

7.2.2.1 Regular Directory Frames

Directory frames may be used instead of random-digit-dialing frames. Directories consist of the names, addresses, and telephone numbers of telephone company subscribers. Telephone directories are confined to specific geographic areas, which is helpful when survey areas are smaller than random-digit-dialing regions. Directories do not include unlisted numbers, however, and surveys based on directory frames are susceptible to undercoverage errors—especially in urban areas, where the percentage of numbers that are listed tends to be lower than elsewhere. If unlisted and listed households differ markedly in their attitudes and experiences, as well they might, survey estimates are likely to be biased.

Certain households, particularly those of professional people, have multiple listings (Dillman 1978). Because of the selection method in directory designs, these households will have a disproportionately high probability of selection. These households are also more likely to differ from the average household in the sampling frame. Interviewed households can be asked how many phones they have and their responses can be weighted accordingly. For example, a household with two phones would have an interview response weighted (down) by 1/2 or 0.5, and a household with three phones would have an interview response weighted by 1/3 or 0.33.

Directory frames quickly go out of date. Telephone directories usually are only updated annually and, because people move a lot, listings become progressively less reliable as the year progresses. Therefore directory frames are best used shortly after directories are published. As directories age, more numbers become ineligible and the proportion of new, eligible, but noncovered households increases.

Relatively straightforward sampling methods are used with telephone directory sampling. These include simple random sampling, stratified random sampling, systematic random sampling, and add-a-digit sampling.

Simple Random Sampling. If a computer listing of the directory is available, a simple random sample is easy to obtain. If a computer listing is not available but the telephone directory is fairly small, a simple random sample can be drawn by sequentially numbering each residential entry and using a table of random numbers for the selection. A slightly more efficient way to draw a simple random sample is to follow the two-stage procedure of selecting a page at random and then a name from that page at random (Frey 1983). This will not produce a truly random sample (each listing having an equal chance of selection) unless each page has exactly the same number of listings. However, for practical purposes it can still be considered random.

Stratified Random Sampling. Stratified random sampling of directories can be done in principle but many directories do not give enough information for strata to be constructed in advance. For example, although individuals may be listed by name, it is not always clear whether they are male or female, so stratification by gender would be problematic. Poststratification of sampled individuals (Section 3.3.6) may be the only way to gain the analytical advantages of such groupings.

Systematic Random Sampling. Simple random sampling often is not convenient for telephone directory frames, and systematic random sampling is used. Systematic random sampling from a directory involves drawing every kth listing until the desired sample size has been drawn. The starting point is determined by randomly selecting a name within the first k listings. The sampling interval, k, is established by dividing the population size by the sample size required. For example, if the directory has 20,000 names and a sample of 500 names is desired, the interval would be $k = 20{,}000/500 = 40$. Systematic random sampling can be dangerous if a cyclical pattern in the frame (Cochran 1977:217) coincides with the sampling interval. Frey (1983) stated that cyclical patterns are not a danger with alphabetical telephone lists, and we agree with this assessment.

Add-a-Digit Sampling. Sometimes plus-one or add-a-digit sampling is used, whereby a number 1 to 9 is added to the last digit of a selected telephone number.

The digit to be added can be constant or chosen randomly each time. For example, suppose 2 is always added to the number drawn. Then if 821-1647 were drawn, 821-1649 would be called. Because the last digit is modified, people with unlisted numbers and numbers put in service after the directory was published are included in the frame. This is advantageous but it brings some statistical problems, because the probability structure is complex. We do not discuss add-a-digit sampling further here; see Frey (1983) for a more detailed presentation.

7.2.2.2 Commercial Directory Frames

Enhanced directory frames are maintained by commercial firms. These directories combine names, addresses, and telephone numbers with other household information obtained from sources such as the Census Bureau. Lists in addition to telephone company subscriber lists are used to increase the coverage. Most of these lists are updated frequently, so they stay more current than telephone subscriber directories. These lists are available in many countries of North America and Europe and can be subselected for specific geographic areas. Commercial directory frames can be expensive for fisheries agencies to purchase or lease. Sometimes a company will provide an agency with a list of the individuals to be sampled from their overall list plus information on the size and other important characteristics of the population.

Commercial directory frames may have advantages over regular telephone directories, but they still suffer from undercoverage because they do not include unlisted numbers. Inevitably, they also contain some ineligible names and numbers because of the frequency with which people move in modern society.

7.2.3 Special Frames

Special frames are more difficult to obtain than directory frames but they may be restricted to anglers, which can improve the precision of estimators. Such frames include boat registration lists, angling club membership lists, and fishing license files. These frames are either exclusively angling households or contain much higher percentages of anglers than do normal directories. These membership lists have the same difficulties as regular telephone subscriber lists: they can become outdated, contain ineligible listings, and suffer from undercoverage. Some anglers fish without obtaining a license, and many anglers do not join angling clubs. Samples obtained from these frames can be biased when the responses of noncovered angling households differ from those covered by the frame. This can easily happen when, for example, only the most avid and interested anglers join clubs. Such frames must be used with a great deal of care to correct for undercoverage. License lists must also contain telephone numbers to be usable. Household selection procedures will depend on the form of the list. Simple random sampling, stratified random sampling, and systematic random sampling might be used.

7.3 PRACTICAL CONSIDERATIONS

Whatever sampling design and frame have been chosen, telephone survey questionnaires have special demands. The investigator must design the questionnaire to be understood verbally. The respondent must be able to comprehend the question and follow its logic. Therefore, telephone surveys work best with simple, straightforward questions. Complex questions with many alternatives are handled better by a face-to-face than by a telephone survey. Questionnaire design was considered in detail in Chapter 4.

It is important to develop a script for the interviewers (Appendix 7.1). The script should be very detailed and include introductory remarks, the questions, and final remarks. Skip patterns—shortened or alternative question sequences triggered by certain answers—should be clearly marked. Interviewers should be told how many callbacks to make and at what times of day before they abandon a sample unit. Generally, telephone methods are least efficient during holidays and summer, when people are away from home and more redialing is necessary to obtain an interview.

To obtain reliable data, interviewers need to be well trained and then well supervised. Once they are trained, a pilot study to find and remove any difficulties in the questionnaire is advisable. The supervisors, who should monitor complete pilot interviews closely, will simultaneously learn which interviewers need further training to refine their skills.

A standard system of data recording must be used, a particularly important consideration when many interviewers are required to complete a telephone survey in reasonable time. Many modern telephone surveys incorporate a computer-assisted telephone interviewing (CATI) system (for an introduction, see Nicholls 1988). With a CATI system, all interviewing is done at a computer terminal, where the interviewer keys responses directly into the system. This eliminates the numerous sheets of paper that otherwise have to be kept organized, as well as the error-prone transfer of data from paper to computer. In effect, the respondent talks directly to the computer via the interviewer. The CATI system directs the flow of the interview by providing one question at a time on the screen. The system is programmed with editing instructions to ensure that only valid responses that are consistent with the question may be entered. If, for example, the interviewer tries to key in "yes" when "a" through "d" are the appropriate choices, an error message appears on the screen. Once a correct response has been entered, the computer automatically produces the next appropriate question on the screen. The computer automatically follows complex skip patterns according to the answers received, which reduces both confusion during the interview and training time for the interviewers. A CATI system facilitates smooth, steady interviews as well as systematic callbacks. The only drawback of such a system is its cost.

After data are logged in, they must be checked, analyzed, and reported as for other kinds of surveys. A big advantage of telephone surveys is the speed with which results can be obtained compared with mail and other contact modes.

7.4 EXAMPLES

We have not found many published examples of telephone surveys for recreational fisheries management. Two were noteworthy and we present brief descriptions of them here.

7.4.1 Missouri Statewide Angler Survey

Weithman (1991) described a comprehensive statewide angler survey in Missouri that began in 1983. The study was divided into three 2-year segments, each involving different cooperating anglers. Sample sizes were 2,500 in 1983 and 5,000 in 1985 and 1987. Anglers were contacted between January and April in each of these years and asked a set of introductory questions in a screening questionnaire (Appendix 7.1). If these people agreed to be cooperators, they were sent a letter of confirmation along with a list of instructions, data records, forms, maps of some key fishing areas (reservoirs), and a reminder that telephone contact would follow. The mail follow-up contact was designed to reinforce the legitimacy of the survey and to encourage responding anglers to keep accurate records. Respondents were telephoned periodically (every 1–3 months) during the following 2 years and asked specific questions about where they went fishing and what they caught (Appendix 7.2). This follow-up survey could be viewed as a longitudinal survey with no rotation of sampling units (Section 5.3), because sampled anglers were contacted repeatedly over 2 years.

A list frame of various license files was used. After the desired sample size had been calculated, names and addresses were selected randomly from the list frame. Telephone numbers were matched with anglers by looking up numbers in telephone books or by calling long-distance information. Anglers with unlisted numbers or no telephones had to be excluded. Of the licensed anglers originally contacted, 92% agreed to be cooperators. Of those, about 90% actually cooperated for 1 year and 80% for 2 years. These response rates are impressive for a large statewide survey.

Because these surveys were primarily directed at estimating statewide catch and effort data for various species, the most important concern is the reliability of the self-reported data. Weithman (1991) and Weithman and Haverland (1991) stated that telephone surveys are an excellent, cost-effective method for obtaining this kind of information. They believed the quality of the Missouri data was comparable to that of data from on-site roving surveys, although other authors have obtained less optimistic results (Essig and Holliday 1991). All surveys require compromises and trade-offs. We believe the Missouri surveys were well designed and that the information obtained was of good quality within the constraints of the telephone design. There may be some tendency to overestimate catch and effort because nonrespondents typically fish less and catch less than respondents do (Essig and Holliday 1991). We discuss catch and effort estimation in more detail in Chapter 15.

7.4.2 Marine Recreational Fishery Statistics Survey (MRFSS)

Essig and Holliday (1991) described the MRFSS carried out by the National Marine Fisheries Service to assess recreational marine fishing around the coast of the United States. This survey has two parts: one is a telephone survey to assess fishing effort, and the other is an on-site access survey to estimate catch rates. This is an example of a complemented survey (Chapter 14). We also discuss this survey further in the chapter on catch and effort information (Section 15.4.1).

In contrast to the Missouri telephone survey just described, which has a list (license file) frame, the MRFSS telephone survey is based on random-digit dialing and all households with telephones (listed and unlisted) in coastal regions are in the frame. The MRFSS cannot use a list frame because most states do not require marine fishing licenses. The advantage of using random-digit dialing is a much broader coverage of the angler population (including nonlicensed anglers). The disadvantage is a low "hit" rate of households with anglers (especially in urban counties), which makes the survey very inefficient.

The telephone survey did not attempt to obtain catch rate information because of concerns about biases in self-reported data (prestige, recall, and digit biases; lies; misidentification of species; inaccurate lengths and weights). This is an extremely important and complex survey, and we recommend that readers consult Essig and Holliday (1991) and the references given therein for more detailed study.

7.5 STRENGTHS AND WEAKNESSES

Telephone surveys may be the preferred off-site contact method when survey results are desired quickly. The questionnaire can be put together in less time than a mail or face-to-face questionnaire. The investigator need not be concerned with how well the questionnaire looks, as long as it can be reliably read by the survey interviewer. Once the selected household is contacted, the interview can begin immediately; hence turn-around time is minimal. Data may be entered directly into a computer or immediately thereafter.

The response rate from telephone surveys can be very high, especially when a letter is sent in advance of the call (Dillman 1978) but an advance letter can be sent only when directory frames are used and the directory gives addresses along with telephone numbers. Advance letters cannot be used with random-digit-dialing methods. (A separate but related issue is that telephone follow-up often provides the best solution to nonresponse problems in mail surveys: Section 6.3.4.)

Telephone surveys can be used instead of on-site methods when the safety of the survey agent is of concern. For example, night fishing effort may be important to the fishery, yet stationing an agent on site at night in urban or remote areas may be too dangerous. Reasonably reliable estimates of legal night fishing activities may be obtained from telephone interviews. (Catch estimates may not be so reliable, as we discuss in Chapter 15.) The telephone method is also valuable in estimating how important non-access point and private access are to a fishery before these are surveyed with expensive on-site contact methods.

Telephone surveys often compare well with other off-site methods in terms of cost. They may be a little more expensive than mail surveys but are much less expensive than face-to-face surveys at residences (door-to-door surveys). They are also usually less expensive than the on-site survey methods (access and roving). If calls are made in the evening after regular working hours, labor costs may be a bit higher than in other surveys. Long-distance telephone charges and the purchase of commercial directory samples can be quite expensive. The questionnaire and responses can be computerized (CATI), which speeds quality control and data analysis but also demands well-trained interviewers who are comfortable with a computer.

Because telephone surveys occur after fishing trips have been completed, they provide reliable data for experiences that can be easily remembered. Hence, telephone surveys provide good information on current attitudes and good demographic and sociological data. The ability to remember events falls off after

2 months and recall bias can occur, although information often is good from anglers who fish infrequently and catch a few, easily identified fish. Recall bias also is less likely when events are memorable, such as fishing for trophy-sized salmon or bonefishing on a Caribbean vacation. However, selective recall of only the more memorable events or only trips on which fish were caught also is a source of bias. Avid anglers who may not remember all of their many trips or be able to enumerate their catch accurately create bias as well. Anglers may not always be able to identify the fish they catch, and they may not admit that they have not caught fish (prestige bias). Telephone surveys work better for trophy fisheries, in which the chance of catching a fish is low and the probability of remembering a catch accurately is high.

Random-digit dialing and especially directory frames suffer from undercoverage; directory frames include only members of the population who have listed telephone numbers. The distribution of unlisted numbers is not uniform, being higher in urban areas (Groves et al. 1988). Although random-digit dialing overcomes the problem of unlisted numbers, it still does not cover households without telephones. Noncoverage is not geographically uniform; in the United States, 90% of southeastern households have telephones versus 93% of households nationally (Thornberry and Massey 1988). Noncoverage is also related to race in the United States: 20% of African-American households do not have phones. In less-developed countries, telephone coverage may be so poor as to make telephone surveys useless.

Appendix 7.1 Screening Telephone Questionnaire Used in the Missouri Statewide Angler Survey

(Reproduced from Weithman 1991)

Hello. May I speak to (Mr. or Mrs.) __________ please? My name is __________, and I am working for the Missouri Department of Conservation. Would you have time to answer a few questions? (If yes, continue; if no, thank them for their time and hang up.)

1. Please estimate the number of days you went fishing in Missouri last year (1982, 1984, or 1986) __________. (In 1987, a subsample of anglers was asked to recall fishing from the prior year between the following time periods: 1 January to Memorial Day, Memorial Day to Labor Day, and Labor Day to 31 December.)

2. Did you (or do you plan to) buy a Missouri fishing license or a combination hunting/fishing license this year?

		Go to Question
(1)	already bought or received a license as a gift	#4
(2)	plan to buy a license	#4
(3)	do not know if I will buy a license	#3
(4)	do not plan to buy a license	#3

3. Why are you considering not buying a Missouri fishing license this year (1983, 1985, or 1987)?

(1)	no longer need a license (≥65 years, handicapped)	
(2)	do not plan to visit Missouri this year	
(3)	no interest or time for fishing this year	
(4)	not able to fish this year (no one to go with, poor health)	
(5)	fish only on own property	
(6)	poor fishing	
(7)	other	Go to Question #7

4. How would you rank the following factors that affect the quality of your fishing, on a five-point scale where 1 is extremely important, 3 is moderately important, and 5 is unimportant?

a.	(1–5)	How important is it for you to catch fish when you go fishing?
b.	(1–5)	How important is it for you to catch a particular kind of fish when you go fishing?
c.	(1–5)	How important is the size of fish you catch on a trip?
d.	(1–5)	How important is the number of fish you catch on a trip?

Appendix 7.1: Continued

e. ___(1–5)___ How desirable is it for you to catch more than one kind of fish on a trip?

f. ___(1–5)___ How enjoyable is it for you to catch and release fish?

g. ___(1–5)___ How important is it for you to be able to keep fish to eat?

5. Please select the fishing conditions you prefer from the following choices.

a. If you could fish for only one kind of fish, what species would you prefer?

___(1)___ largemouth bass
___(2)___ smallmouth bass
___(3)___ white bass
___(4)___ catfish
___(5)___ crappie
___(6)___ trout
___(7)___ other
___(8)___ no preference

b. Would you prefer to catch

___(1)___ large fish at slow rates of catch; or
___(2)___ small fish at fast rates of catch?
___(3)___ no preference

c. Would you prefer to catch

___(1)___ large fish occasionally, and release the small or medium-sized fish you catch; or
___(2)___ mostly small fish, and keep everything you catch?
___(3)___ no preference

d. If you could fish at only one location, what place would you prefer?

___(1)___ reservoir (>400 hectares)
___(2)___ lake (2–400 hectares)
___(3)___ pond (<2 hectares)
___(4)___ large rivers (Mississippi, Missouri, Osage, Gasconade, Meramec, etc.)
___(5)___ small streams
___(6)___ trout parks
___(7)___ no preference

6. Would you be willing to participate in our study for the next 2 years? It would involve answering questions every 1 to 3 months about where you fish, what you catch, and the expense of your trips. (If they are willing to participate, promise to send them a listing of the information we want them to record for each fishing trip.)

___(1)___ yes
___(2)___ no

Appendix 7.1: Continued

7. Personal information

a. ________ sex, where male = 1 and female = 2.

b. ________ age group, where 16 to 20 = 1, 21 to 30 = 2, 31 to 40 = 3, 41 to 50 = 4, 51 to 64 = 5, and ≥65 = 6.

c. ________ ethnic origin, where White = 1, Black = 2, Hispanic = 3, Asian = 4, Indian = 5, and other = 6.

d. ________ education completed, where grade school = 1, high school = 2, college = 3, and graduate school = 4.

e. ________ county of residence, or state if a nonresident.

f. ________ counting yourself, total number of people in household.

g. ________ occupation, where skilled tradesman = 1, government = 2, professional = 3, manager, official, executive = 4, hourly laborer = 5, student = 6, retired = 7, clerical = 8, technical = 9, farmer = 10, salesman = 11, homemaker = 12, disabled or unemployed = 13, and other = 14.

h. ________ total annual family income group, where <10,000 = 1; 10,000 to 15,000 = 2; 15,001 to 20,000 = 3; 20,001 to 30,000 = 4; 30,001 to 50,000 = 5; and >50,000 = 6.

i. ________ personal income, where <10,000 = 1; 10,000 to 15,000 = 2; 15,001 to 20,000 = 3; 20,001 to 30,000 = 4; 30,001 to 50,000 = 5; and >50,000 = 6.

Appendix 7.2 Follow-Up Telephone Questionnaire Used in the Missouri Statewide Angler Survey

(Reproduced from Weithman 1991)

Hello. May I speak to (Mr. or Mrs.) ________ please? My name is ________, and I am working for the Missouri Department of Conservation. I am calling to follow up on the survey in which you agreed to participate.

1. Have you gone fishing in Missouri since ________ (fill in the last month that the angler was checked)?

   If yes: Go to question #2

   If no: Complete. Thank you. When should we contact you again about your fishing? ____________________.

2. Where did you go fishing in Missouri since ________ (fill in last month that the angler was checked)?

We want to collect as much information as possible about each fishing trip. Record the information from each trip separately, where a trip, which can last from one to several days, is defined as a period during which an angler does not return home. However, each different body of water, different fishing method, or different species sought represents a separate trip even if the angler did not return home. For a trip that an angler cannot recall the fish that were caught, go to the bottom of the Catch Survey Information form under the heading No Catch Information.

Trip Information Required—Catch information available

a. Date the trip began: month; day; year.
b. Water #: place fished, expressed as a number. Water types are as follows: reservoir, >400 hectares; lake, 2–400 hectares; pond, <2 hectares; rivers and streams; and trout parks. Assigned numbers can be obtained from our reference notebook.
c. Method or type of fishing: where 1 = hook-and-line; 2 = trot line; 3 = snagging; 4 = gigging; 5 = bow fishing; 6 = netting; 7 = frogging; and 8 = other.
d. Species: primary kind of fish sought.
e. # Anglers: average number of people who fished with you each day including yourself.
f. No license: average number of people who fished with you, but did not need a fishing license.
g. Days: number of days fished at the place identified on this trip.
h. Hours: average number of hours fished per person per day to the nearest half of an hour.
i. Quality: rate the quality of fishing success for your group on a 10-point scale where 10 = excellent, 5 to 6 = average, and 1 = poor.
j. Fish #1–#16: information is recorded separately for each species and size group caught. The following information is recorded about fish caught by all fishermen in the group: species, number, size (length range in inches), and whether or not they were kept or released.

Appendix 7.2: Continued

Trip Information Required—No catch data available

a. Date the trip began: month; day; year.
b. Water #: see explanation above.
c. Species: primary kind of fish sought.
d. Days: number of days fished at the place identified on this trip.

Thank you. When should we contact you again about your fishing?

___

Chapter 8

Door-to-Door Surveys

8.1 INTRODUCTION

Door-to-door surveys of households permit interviews of greater depth and flexibility than any other method discussed in this book. Being a face-to-face technique, door-to-door interviewing allows more immediacy, spontaneity, and complexity than other off-site surveys (mail, telephone). Being itself an off-site technique, it avoids the required brevity of face-to-face interviews on site (access point, roving). Door-to-door interviews have been used in the U.S. National Surveys of Fishing, Hunting, and Wildlife-Associated Recreation, which have been conducted every 5 years since 1955 by the U. S. Fish and Wildlife Service (Grambsch and Fisher 1991). Household visits may be the only practical off-site survey method in developing countries (Bayley and Petrere 1989; Malvestuto and Meredith 1989).

Door-to-door surveys are labor-intensive, however, and for this reason they are usually too costly for a fisheries management agency to conduct by itself. Sometimes, though, an agency can negotiate or pay to attach a few fisheries questions to another household survey with a broader purpose—to a general public opinion survey on environmental issues, for example.

In this chapter we briefly describe the various types of household surveys, give some examples of them, and review the strengths and weaknesses of door-to-door interviewing. The approach is not used often in fisheries management, and greater detail about this class of survey methods can be obtained from the references cited.

8.2 TYPES OF DOOR-TO-DOOR SURVEYS

Both nonprobability (quota) and probability sampling have been used in household surveys (Figure 8.1).

8.2.1 Quota Sampling

Face-to-face surveys in peoples' homes are extremely expensive and complex to administer. Some researchers have tried to reduce costs by using a nonprobability-based approach called quota sampling (Stephan and McCarthy 1958). Based on existing information about the target population, groups of people deemed important to reach are defined. Quotas are set for each group based on that group's relative size in the population (perhaps determined from census figures). Interviewers are then given a lot of latitude in choosing who to interview within each group, and they usually do not have to use any element of randomness

TYPES OF DOOR-TO-DOOR SURVEYS

NONPROBABILITY SAMPLE

Quota Sampling
- Inexpensive
- Unknown biases
- Unknown precision

PROBABILITY SAMPLE

List Frame	Area Frame
•Target group (i.e., anglers) reached directly	•Target group not reached directly
•Higher travel costs	•Lower travel costs
	•All households must be enumerated in each area

Figure 8.1 Types of door-to-door household surveys with some of their important characteristics.

in filling their quotas. Although quota sampling is similar to stratified sampling, the absence of random sample selection can result in badly biased estimates if the hard-to-contact anglers differ from the easy-to-contact anglers. Further, the precision of estimates cannot be calculated. Quota sampling is often used in market research (Stephan and McCarthy 1958) because probability sampling is so much more expensive.

8.2.2 Probability Sampling

Probability samples for household surveys may be drawn from either *list frames* or *area frames* (Cox and Cohen 1985:25). List frames, such as license files with names and addresses, can be restricted to particular target groups, and this is their advantage over area frames. Often, however, households on such a list are widely scattered, making travel costs unacceptably high.

An area frame is a complete list of residential areas in the geographic region of interest. Two-stage or cluster sampling typically is used (Section 3.5). First, some kind of probability sample of subareas is chosen. Then in each subarea selected, every residence is enumerated and a probability sample of these dwellings is taken. Thus, subareas are the primary sampling units and households are the secondary units. Area frames are used more often than list frames because travel costs are much less, but they require the expense of enumerating all the households in an area. Moreover, such frames include many nontarget residents (e.g., nonanglers), so samples may have to be larger than those drawn from list frames. The cost advantage still lies with area frames, but neither approach to probability sampling is inexpensive.

8.3 EXAMPLES

8.3.1 National Survey of Fishing, Hunting, and Wildlife-Associated Recreation

The National Survey of Fishing, Hunting, and Wildlife-Associated Recreation in the United States began in 1955 and has been conducted by the U.S. Fish and Wildlife Service at 5-year intervals since. It provides national and regional estimates of how many people fish and hunt, how often they do so, how much money they spend in the process, and other socioeconomic characteristics. Recent surveys have embraced nonconsumptive uses of fish and wildlife, such as scuba diving and photography.

In the 1985 survey, over 115,000 households were screened (by telephone and in-person visits) for residents germane to the study. The screening produced samples of some 25,000 anglers and hunters and another 25,000 nonconsumptive users; reports for each state had to be generated, so sample sizes had to be large. Detailed questionnaires were administered door to door by the Bureau of the Census, which obtained excellent response rates of around 90%.

Grambsch and Fisher (1991) outlined the planning and execution of the 1985 National Survey, giving particular attention to the problems that arose and to the ways in which future surveys can be improved. Their paper is a good introduction to this extremely large and complex door-to-door survey.

8.3.2 Niger River Socioeconomic Survey

Malvestuto and Meredith (1989) described a socioeconomic household survey carried out in the Niger River fishery in Niger, West Africa, from April 1984 to December 1985. The survey comprised 513 of approximately 1,200 households then engaged in fishing along the river. For efficiency, households were sampled with the same randomized sampling scheme used for a catch assessment survey. First fishery landings were randomly chosen from a list of all landings; then households were randomly selected within the village associated with each chosen landing—a two-stage (cluster) sampling design (Section 3.5).

The questionnaire had been pretested the year before the survey. To interview household members, the survey team went to a randomly selected landing the afternoon before household interviews. Team members met with the village chief to explain the survey's objectives and to gain permission to interview village members. The chief usually called the village together to explain what was going to happen, and the survey team could get a list of all village households at that time. Three households then were chosen at random to receive the interview.

The purpose of the questions was to evaluate the relative benefits of fishing and other activities, and to characterize fishing in economic terms at the household level. Information sought was monetary return from the sale of fish, capital investment in fishing, and expenditures for food in weekly markets. From 1983 to 1985, fishing effort, fishing harvest, and market value of the harvest declined by 50%, which Malvestuto and Meredith (1989) attributed to the Sahelian drought and high fishing pressure. These authors found the socioeconomic household survey to be a very valuable and practical tool for their overall assessment of the Niger River fishery. Bayley and Petrere (1989) also endorsed the value of household surveys for rural fisheries management in South America.

8.4 STRENGTHS AND WEAKNESSES

The big advantage of door-to-door household surveys is that complex questions can be asked, because the interviewer can clarify and explain as needed and ask follow-up questions as appropriate. Such depth and flexibility are less feasible in telephone surveys and impossible in mail surveys. The big disadvantage of door-to-door surveys is their cost and logistic complexity.

Door-to-door surveys also suffer from various kinds of errors (Essig and Holliday 1991). Nonprobability (quota) sampling, if elected, is very vulnerable to sampling error; estimators have unknown properties with potentially high bias. We do not recommend quota sampling despite its lower cost. With probability sampling, undercoverage errors (from incomplete frames) are most likely with list frames; area frames usually have quite good coverage if the enumeration of households in the sampled areas is thorough. Probability sampling of households is not subject to avidity bias, because avid and nonavid anglers are sampled with equal probability (Thompson 1991).

Door-to-door surveys elicit self-reported data, which may suffer from such response errors as recall, prestige, and digit biases, species misidentifications, and incorrect fish lengths and weights. Nonresponse errors usually are less a problem than they are with mail and telephone surveys because refusals are less likely, although some scheduling problems are likely and some respondents will be unavailable for interviews. Literacy and language problems are less important in door-to-door than in mail or telephone surveys (Essig and Holliday 1991). Rewards may improve response rates if the interview takes a substantial amount of time.

Specialized door-to-door surveys of small populations may be warranted if costs can be contained. For example, it might be feasible and relatively inexpensive to probability-sample a group of marina owners based on an area frame or a small population of trophy anglers based on a list frame. Large household surveys may be practical if several government agencies can share the cost. Door-to-door techniques may be the only survey option available in some developing countries.

Chapter 9

Logbooks, Diaries, and Catch Cards

9.1 INTRODUCTION

We classify logbooks, diaries, and catch cards as off-site methods because they contain angler-reported data, and survey agents do not have to be present at a fishery to distribute or recover them. Nevertheless, these instruments sometimes are administered on site. They are used to obtain information on catch, effort, and perhaps other socioeconomic variables. They have most of the characteristics of other off-site instruments (mail questionnaire, telephone and door-to-door interviews)—in particular the biases associated with self-reported data.

These simple methods are used for various purposes. When mandatory reporting by anglers can be required, attempts may be made to obtain absolute population values of total catch, total effort, and other quantities. Usually, however, reporting is voluntary, and the data may be used only to obtain population indices thought to be useful for monitoring trends over time. For example, catch per unit angling effort may be followed for several years to discern changes in the quality of a fishery.

Logbooks, diaries, and catch cards are the cheapest ways to collect fishery information of all the off-site and on-site methods. Low monetary expense brings the cost of potentially high biases in estimators, however.

9.2 DESCRIPTION OF THE METHODS

Uses of preprinted logbooks, diaries, and catch cards are quite diverse, varying with the nature of the fisheries under study. Their applications divide into two general groups, however.

Multitrip Records. Logbooks and diaries normally are used when information about more than one trip is needed. Anglers whose activities are to be monitored for more than one day at a fishing site, or at more than one site over a defined period of time, are likely to be issued diaries. Diaries tend to be compact booklets that anglers can carry easily; they guide anglers in the type of information desired, but they also encourage anglers to report anecdotal information (on weather, changes of gear, etc.) that may facilitate interpretation of the record. Charter boat captains, tournament directors, and others overseeing fishing by many anglers may be asked to maintain logbooks, in which standard but unannotated data are entered by angler, excursion, or event. Logbooks and diaries are returned to a survey agent (usually by mail) at the end of the study period.

Single-Trip Records. Pocket-size catch cards, printed on one or both sides, may be issued to individual anglers to record their catch and effort during a single day trip. They are handed out to anglers as they begin their fishing trips and either collected at the end of those trips or mailed in later.

This classification is not absolute. Diaries can be used to record single day trips, for example. Sometimes tournament and charter boat data are submitted in a summarized form that looks much like a catch card; such a summary is shown later in this chapter. Legibility of the records will be improved if the forms are printed on waterproof paper, although survey expense will be increased. Diaries and logbooks may be returned to anglers, captains, or directors after the data have been transcribed. People often appreciate having this information and the gesture can build good will for the program. If a return policy is stated on the form (with a place for the respondent's name and address), cooperation with the project may be enhanced.

9.2.1 Logbooks and Diaries

9.2.1.1 Angler Diaries

Anderson and Thompson (1991) described a 2-year angler diary program on remote Great Bear Lake in Canada's Northwest Territories. The intent was to monitor fishing effort and harvest, and angler participation was voluntary. Access to the lake was almost entirely restricted to five fishing lodges, and lodge management and staff administered the diary program. The diary (Appendix 9.1) was refined in content and appearance following a pilot study, and lodge managements were coaxed to a high degree of cooperation; both elements (as well as anglers' concerns for fish conservation) were essential for good angler participation in the program. The authors concluded that the diary program produced estimates of total fishing effort and harvest as accurate as could be gained from creel surveys, at 20% of the cost of creel surveys. At best, however, voluntary participation by anglers reached only 70%. The authors recognized that differences in fishing characteristics between participants and nonparticipants was a potential source of bias, and they suggested that a small telephone survey of nonrespondents could be used to assess this error. Such a follow-up would be feasible for the Great Bear Lake program, because nonrespondents could be learned (by difference) from lodge registration records. Nonresponse bias would be more difficult or impossible to measure in some other diary programs.

Sztramko et al. (1991) recounted an angler diary program tested on Lake Erie for use with fisheries that are too dispersed for conventional roving creel surveys. Volunteer participation in the diary program was built up by soliciting fishing clubs, offering incentives, and returning data to anglers. No attempt was made to estimate population parameters such as total catch, given the small size and nonprobability nature of the sample. In one bay, catch per unit effort could be compared for 5 years between diarists and general anglers represented by roving creel surveys. Diarists always had markedly higher average catch rates than general anglers, which could be due either to prestige bias, which the authors discounted, or to response (or nonresponse) bias, which was not examined. Year-to-year trends in catch rate did not agree well between diarists and general anglers for one species, but diarists' trends for another species followed those of commercial catches well. The authors concluded that diary data show promise for indices of relative abundance.

TOURNAMENT CREEL CENSUS

GBCF Number ____________

CLUB NAME ____________	LAKE FISHED ____________
DATE(s) FISHED ____________	TOTAL HOURS FISHED ________
NUMBER OF FISHERMEN ________	DAY ______ NIGHT ______
NO. OF BASS WEIGHED IN ________	NO. RELEASED ALIVE ________
TOTAL WEIGHT ____ lbs. ____ oz.	NO. OF 10 FISH LIMITS ________
WINNING WEIGHT ______ lbs. ______ oz.	NO. OF FISHERMEN W/NO FISH ____

LARGEST BASS: WEIGHT ____ lbs. ____ oz.

NUMBER OF LARGEMOUTH ____ SPOTTED BASS ____ OTHER BASS ____

Submitted by: ____________________ Phone: ____________

THANK YOU Georgia B.A.S.S. Chapter Federation, Inc.

Figure 9.1 Report form used to report bass club tournament data in Georgia. (Reproduced from Quertermus 1991.)

9.2.1.2 Charter Boat Logbooks

Calhoun (1949), Baxter and Young (1953), Chadwick (1962), and Jensen (1964), among others, have found that the use of logbooks on licensed charter boats is an inexpensive way to obtain information on catch and effort. Most programs reported have been voluntary and subject to questions of nonresponse bias, but they have provided reliable indices of fish population change at low cost.

9.2.1.3 Fishing Tournament Diaries

Since 1982, all bass clubs affiliated with the Georgia B.A.S.S. Chapter Federation have been required (by the Federation) to report the results of their monthly fishing tournaments (Quertermus 1991). The program is popular among the clubs, in part because of a continual educational program and in part because the clubs enjoy "competing" with one another. Data from over 900 tournaments have been submitted each year. The data are distilled from diary or logbook records and summarized on a convenient form analogous to a catch card (Figure 9.1). This information is inexpensive for the Federation to obtain (and to share with the state fisheries agency), and it appears to be reliable; when tournament catch rates have been compared with those from roving or access point surveys of the same lakes, agreement usually has been good (Quertermus 1991).

9.2.2 Catch Cards

If on-site sampling is difficult because of low fishing pressure and remote location, use of catch cards may be advantageous (Essig and Holliday 1991). Larson et al. (1986) used catch cards in conjunction with daily permits and a check station to monitor catches of trout in a national park stream. Anglers obtained permits by surrendering their fishing licenses; to reclaim their licenses, they had to present their completed catch cards. Thus, the program was effectively mandatory. Most catch card programs are voluntary, however, and then catch card data typically are biased toward successful anglers. Fraidenburg and Bargmann (1982) reported that successful salmon anglers in Washington readily sent in their catch cards, whereas unsuccessful anglers kept theirs until reminded to return them.

9.3 STRENGTHS AND WEAKNESSES

We suspect that the use of diaries, logbooks, or catch cards to estimate population parameters is uncommon, though it was attempted with reasonable results on Great Bear Lake, as described in Section 9.2.1 (Anderson and Thompson 1991). More often these methods are used for comparative purposes, such as to examine time trends and differences between areas. Their main advantage is that they are very inexpensive and simple to administer compared with any of the other survey methods. This is why fisheries managers come back to them despite their many weaknesses. Under favorable circumstances, which usually include continual public education and the cooperative good will of many parties, these methods can produce trustworthy information, as we described in Section 9.2.

Large biases are likely for these instruments because the data are self-reported. Anglers may exaggerate their catches (prestige bias), misidentify fish species, misreport lengths and weights of fish, and misunderstand questions (Essig and Holliday 1991). High nonresponse rates are likely if reporting is voluntary. Few of the voluntary programs are (or can be) built on probability sampling schemes, either because complete list frames are unavailable or because the cost of such sampling is too high. Participating anglers often are self-selected or avid members of fishing clubs.

Diaries can be used in conjunction with other contact methods, such as mail or telephone surveys. For example, if a diary frame is complete, a small telephone survey could be used to sample nonrespondents, as suggested by Anderson and Thompson (1991). In Section 7.4, we recounted the Missouri statewide survey in which anglers were interviewed periodically by telephone for 2 years (Weithman 1991). In effect, these anglers were asked for a diary of their fishing trips over the survey period, although not all of them kept actual records.

Appendix 9.1 Great Bear Lake Angler Diary

(Reproduced from Anderson and Thompson 1991)

Shown are promotional and instructional pages, one of several daily record pages, and the last page with fishing regulations, instructions for fish release, and summer calendars.

HELP US PROTECT OUR HERITAGE

Trout, grayling, pike, charr and the waters they inhabit are a precious natural resource to be used wisely and guarded carefully. In ensuring the conservation of this heritage, Canada Fisheries and Oceans and the Government of Northwest Territories need realistic assessments of fish populations. You can provide us with the kind of reliable information we need. Please take a few minutes each day and complete this fishing diary. Be sure to turn it in at the lodge before you leave and it will be returned to you as soon as possible.

USING YOUR DIARY

An example of how to use your diary is given below, but please take note of these important points:

- **HOURS SHOULD INCLUDE ACTUAL FISHING ONLY** (exclude travel and lunch time)
- **FILL OUT A PAGE FOR EVERY DAY, EVEN IF YOU DID NOT FISH** (including arrival and departure days)
- **INCLUDE ALL YOUR FISHING ACTIVITY**

EXAMPLE:

This angler fished on July 12, 1987, his fourth day at the lodge. He spent an hour before breakfast casting for grayling from shore near the lodge (area 1). He enjoyed the morning, but caught nothing.

Later, with his guide, he went out to area 3 and fished 2 and 1/2 hours for trout. He caught 6 trout and 1 grayling. He released 4 trout, used 1 small trout and the grayling for shore lunch, and kept a larger trout to take home. Afterwards, he fished 1/2 hour for pike and caught and released 5 of them.

After lunch he returned to the lodge and was flown by float plane to Lac du Bois for some pike fishing. He fished for 3 and 1/2 hours and caught 20 pike. He released 18 and kept 2.

Record for Day 4

Did you fish today? ☒ yes ☐ no

Did you catch any fish? ☒ yes ☐ no

AREA FISHED (SEE MAP)	FISH SPECIES	HOURS FISHED FOR EACH SPECIES	NUMBER OF FISH: CAUGHT =	RELEASED +	SHORE LUNCH +	KEPT to take home
AREA 1	LAKE TROUT					
	ARCTIC GRAYLING	1	0			
	NORTHERN PIKE					
	OTHER specify:					
AREA 2	LAKE TROUT					
	ARCTIC GRAYLING					
	NORTHERN PIKE					
	OTHER specify:					
AREA 3	LAKE TROUT	2½	6	4	1	1
	ARCTIC GRAYLING		1		1	
	NORTHERN PIKE	½	5	5		
	OTHER specify:					
AREA 4	LAKE TROUT					
	ARCTIC GRAYLING					
	NORTHERN PIKE					
	OTHER specify:					
OTHER AREA (specify below)	LAKE TROUT					
	ARCTIC GRAYLING					
	NORTHERN PIKE	3½	20	18		2
	ARCTIC CHARR					
	OTHER specify:					

OTHER AREA FISHED Lac du Bois

Date 12 / 7 / 87 (day / month / year)

Circle one: M T W Th F Sat (Sun)

Notes for the day

Caught 2016 trout on 5 of Diamonds

Clear + calm

Appendix 9.1: Continued

Catch, Size and Possession Limits:

For Great Bear Lake

Species	Maximum Daily Limit	Maximum Possession Limit
Lake Trout*	2	3
Arctic Grayling	5	10
Northern Pike	5	10
Walleye	5	10

* You may take home from your fishing trip 2 Lake Trout, of which only one can be over 28 inches fork length.

The possession limit of 3 is to provide for your consumption.

For all other species please refer to the current Northwest Territories Sport Fishing Guide.

GO BARBLESS

The mortality of released fish often occurs as a result of excessive bleeding when barbed hooks are removed or from improper handling. Many fish hooked in the gullet or gills are released only to swim away and die. Safe removal of barbed hooks is often impossible, so we recommend that the barbs on your hooks be filed off or pinched down with pliers. Careful handling with wet hands and holding the fish gently under the gill cover without touching the gills helps ensure the fish will survive to fight again.

1987

JUNE

	1	2	3	4	5	6
7	8	9	10	11	12	13
14	15	16	17	18	19	20
21	22	23	24	25	26	27
28	29	30				

JULY

			1	2	3	4
5	6	7	8	9	10	11
12	13	14	15	16	17	18
19	20	21	22	23	24	25
26	27	28	29	30	31	

AUGUST

						1
2	3	4	5	6	7	8
9	10	11	12	13	14	15
16	17	18	19	20	21	22
23	24	25	26	27	28	29
30	31					

SEPTEMBER

		1	2	3	4	5
6	7	8	9	10	11	12
13	14	15	16	17	18	19
20	21	22	23	24	25	26
27	28	29	30			

In compliance with the Privacy Act: Completion of this survey is entirely voluntary. Your name and address have been collected so that your diary can be returned to you and will only be kept on file until mailing labels are printed.

1988

JUNE

			1	2	3	4
5	6	7	8	9	10	11
12	13	14	15	16	17	18
19	20	21	22	23	24	25
26	27	28	29	30		

JULY

					1	2
3	4	5	6	7	8	9
10	11	12	13	14	15	16
17	18	19	20	21	22	23
24	25	26	27	28	29	30
31						

AUGUST

	1	2	3	4	5	6
7	8	9	10	11	12	13
14	15	16	17	18	19	20
21	22	23	24	25	26	27
28	29	30	31			

SEPTEMBER

				1	2	3
4	5	6	7	8	9	10
11	12	13	14	15	16	17
18	19	20	21	22	23	24
25	26	27	28	29	30	

Appendix 9.1: Continued

Record for Day of Arrival

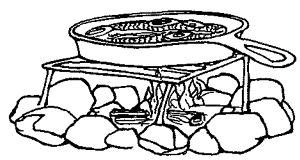
Did you fish today?
☐ yes ☐ no
Did you catch any fish?
☐ yes ☐ no

AREA FISHED SEE MAP	FISH SPECIES	HOURS FISHED FOR EACH SPECIES	NUMBER OF FISH: CAUGHT	= RELEASED	+ SHORE LUNCH	+ KEPT to take home
AREA 1	LAKE TROUT					
	ARCTIC GRAYLING					
	NORTHERN PIKE					
	OTHER specify:					
AREA 2	LAKE TROUT					
	ARCTIC GRAYLING					
	NORTHERN PIKE					
	OTHER specify:					
AREA 3	LAKE TROUT					
	ARCTIC GRAYLING					
	NORTHERN PIKE					
	OTHER specify:					
AREA 4	LAKE TROUT					
	ARCTIC GRAYLING					
	NORTHERN PIKE					
	OTHER specify:					
OTHER AREA (specify below)	LAKE TROUT					
	ARCTIC GRAYLING					
	NORTHERN PIKE					
	ARCTIC CHARR					
	OTHER specify:					

OTHER AREA FISHED ____________________

Date _____ / _____ / _____
day month year

Circle one: M T W Th F Sat Sun



Notes for the day

____
____
____
____
____
____
____
____
____

Chapter 10

Access Point Surveys

10.1 INTRODUCTION

The access point survey method is an *on-site, intercept* design. The method is defined by these adjectives because the access points that agents visit during a survey are chosen from a list of all such sites, and anglers are intercepted immediately after they complete their fishing trips. This method is used when information is needed on catch and effort for specific water bodies; it is used only secondarily to obtain angler-specific information such as economic values and attitudes. Hayne (1991) summarized the features of the access point sampling design; Robson (1960) provided the method's first statistical formulation. Principal characteristics of access surveys are summarized in Figure 10.1.

A requisite of the access point method is that anglers use defined access sites to enter the fishery. ''Defined access'' sites include government-constructed boat trailer ramps, marinas, public piers, small dirt parking lots near popular fishing spots—in short, any place to park that is used routinely by anglers. This design works best where the great majority of anglers use defined public sites to reach the water and few, if any, use private docks or piers or walk to the water from ad hoc parking spots along a road.

Many examples of the access design can be found. Two illustrations come from Missouri, where the Department of Conservation has used access point surveys to study the recreational usage of the lower Osage River (Haverland 1990) and the Missouri River (Fleener 1989). The lower Osage River survey spanned 16 months, and unequal probability sampling (discussed later) was used to select access sites and daily time periods. The Missouri River survey covered 890 kilometers of the river and spanned 4 years. Sixty-seven sites were visited and 61,890 interviews were conducted.

Traditionally the access method has been used to estimate fishing effort (also called fishing pressure in freshwater fisheries), catch (the total number of fish caught, whether kept or released), and harvest (the number of fish kept). Biological sampling of fish to obtain data such as length, weight, age, reproductive state, and condition can be done more easily at access sites than on the water during roving surveys, because measuring boards and weighing scales do not have to be carried around and fishing is not interrupted. (The best way to get biological data, however, is by research sampling.) At fishing access sites, creel clerks (as on-site survey agents are commonly called) directly count anglers coming off the water and record the total harvest, preferably by direct examination. Agents also may question anglers about economic or social concerns (Chapters 16, 17). Access site data are immediately retrospective because they are based upon just-completed trips. In the roving method (Chapter 11), by contrast, effort and

CHARACTERISTICS OF AN ACCESS SURVEY

- The survey takes place on site, physically on shore
- The fishery has a countable number of access sites
- Anglers using these sites are representative of all anglers using the fishery
- Anglers are interviewed as they leave the fishery just as they complete their trip
- Visits to the sites by the creel clerk are chosen randomly with known probability from a list of all sites and from all days of the fishing season
- Information gathered on effort and harvest is unbiased, and can gather information on unlicensed anglers and illegal harvest
- Harvest is examined by the creel clerk

Figure 10.1 Summary of the major characteristics of access point surveys.

catch data are taken while anglers are still fishing and estimates therefore are based upon incomplete trips.

10.2 SPATIOTEMPORAL FRAME

A spatiotemporal sampling frame is used for the access point method. The frame consists of *all* of the times (days, part days, etc.) available for fishing during a defined period and *all* the points of access to the fishery. The time period can be limited to part of a season, but it also can embrace an entire season, several seasons, or several years. Sampling times and places are randomly selected from the frame, usually by multistage sampling (discussed below). Creel clerks are assigned for specified times to specified access sites, where they intercept anglers leaving the water. Spatiotemporal frames thus are applied quite differently from the list frames commonly used in off-site methods.

10.2.1 Choosing Dates and Times

Within the temporal frame, sampling days commonly are stratified by type (weekdays versus weekend days). When the fishing day is longer than a clerk's workday, work shifts are chosen randomly from within the day. This is a combination of stratification and two-stage sampling. Because sampling dates are chosen first, they are called primary sampling units (PSUs), in Malvestuto's (1983) terminology, and work shifts within the day are the secondary sampling units (SSUs). Two-stage sampling is described in Section 3.5 of this book and by Cochran 1977:274.

When nothing is known about temporal patterns of angling in a fishery, sampling days (PSUs) may be chosen with equal probability from all the days available. This selection is done without replacement; once a date is selected it is not placed back into the sampling pool to be selected again. If the fishing effort is expected to be considerably greater during some times than others, however, stratification of sampling effort is advisable. Fishing effort typically is much heavier on weekends and public holidays than it is on weekdays, for example, and effort is heavier during the opening week of a fishing season than it is later. These patterns were recognized early in the development of creel survey designs (Best and Boles 1956). More frequent sampling when angling is heavy will result in more precise estimates of catch and effort. For example, if an angler survey is to be conducted for 4 days a week and 50% of trips to the fishery historically have taken place on weekends, the estimates of effort often have the smallest variance if half the sampling is allocated to the weekends. The survey could be done on two weekdays and on two weekend days each week, giving 40% coverage of weekdays (2/5) and 100% coverage of weekend days (2/2). Of the total fishing effort, 50% will be sampled on weekend days (100% coverage of 50% of the fishing effort) and 20% on weekdays (40% coverage of 50% of the fishing effort). Hence, 70% of the total effort will be covered with this design. This combination allows the highest coverage of effort for the sampling time available, and it permits the calculation of variance within a week for each stratum. (See Section 11.2.1 for further discussion of allocating sampling days to strata.) Similarly, more sampling days can be assigned to the first week of a fishing season if heavier angling is expected during that period.

When a fishing day is longer than a creel clerk's workday, it cannot be sampled completely and must be subsampled. Most commonly, the fishing day is partitioned into morning (AM) and afternoon (PM) periods, and only one of these periods is chosen from each sampling date. The period within the day is the secondary sampling unit (SSU), and it is chosen with known statistical probability. When angling differs between periods, more samples can be allocated to the period with the greater expected fishing effort (unequal or nonuniform probability sampling), and the precision of the effort estimates will be improved (Hayne 1991). For example, in boat-based fisheries typified by long fishing trips, more trips are completed in the afternoon than in the morning, and survey precision is greatest when the afternoon period is sampled more heavily.

When two-stage sampling is used, only one period normally is chosen per survey day, and only one survey clerk or crew is needed that day. If the SSUs are short enough, two or more can be sampled per day, which may give a more precise estimate for the day. Malvestuto (1983) gave examples of unequal probability sampling in two-stage sampling programs. Other demonstrations of this sampling design were given by Haverland (1990), Fabrizio et al. (1991), Osburn and Osborn (1991), and Palsson (1991), and we present yet another in Section 10.2.1.1.

An alternative method for sampling within a survey day is to stratify all days into periods (e.g., AM versus PM) and to sample the strata with known probability. There is a subtle difference between such a stratification scheme and the two-stage sampling discussed above. With true stratification, the periods are chosen independently from among all days. Both the morning and afternoon periods of a particular day might be selected, and if the work periods are long—6 hours, say—two survey crews may have to be hired to maintain compliance with labor laws. Staffing costs are the reason why two-stage sampling is used much more frequently than true stratification of within-day periods. An example of this design is shown in Section 10.2.1.1.

When the day is divided into periods, the periods need not be mutually exclusive, such as morning and afternoon. Staff time and costs often can be optimized if two or more overlapping work periods are established. If a fishing day is 12 hours long (6 AM to 6 PM), for example, and a clerk's 8-hour workday

M	T	W	T	F	S	S
1	2*	3*	4*	5	6	7
8*	9	10*	11	12	13*	14*
15*	16*	17*	18*	19	20	21
22*	23*	24*	25	26*	27*	28

Figure 10.2 Simple random sampling, without replacement, of days within a month for an access point survey. The sample consists of $n = 16$ days (asterisks) of the $N = 28$ days available for sampling. The 16 days are the primary sampling units (PSUs). By chance, 6 days were selected consecutively (13–18).

includes an hour for travel, scheduling two nonoverlapping 6-hour periods (6 AM to noon and noon to 6 PM) will use only 7 hours (travel included) of the workday. In contrast, designating two overlapping 7-hour periods that can be chosen for sampling (6 AM to 1 PM and 11 AM to 6 PM, only one of which will actually be worked) will make full use of the clerk's workday. If the survey covers several seasons, the amount of overlap changes as the length of the fishing day expands or contracts. Overlapping the work periods alters the sampling probabilities, however. With two daily periods, the overlap period is sampled with 100% probability each survey day, whereas the nonoverlapping parts are sampled with smaller probabilities; when sampling is divided equally between mornings and afternoons, these probabilities are both 50%. Hence sampling probabilities must be adjusted to reflect the overlap (see Section 3.6).

Although allocation of sampling effort is most often based on anticipated fishing effort, other key variables such as catch or catch per unit effort may be more important to fishery managers. In these cases, more precision will be obtained when the variable of principal interest, rather than effort, is used as the basis of stratification.

10.2.1.1 Examples Of Sample Selection

We illustrate here five methods of selecting sampling days: (1) simple random sampling, (2) stratification by day type, (3) stratification by week, (4) two-stage sampling of morning and afternoon strata, and (5) stratified sampling of morning and afternoon work shifts. For each example, 16 days were selected without replacement from the 28 consecutive days available for sampling in a normal February. Random numbers were generated with the RANDOM (uniform option) function in MINITAB (Minitab Inc., 3081 Enterprise Drive, State College, Pennsylvania 16801). Other software packages with these functions are available, and we could have used printed tables of random numbers.

Example 1: Simple Random Sampling. In simple random sampling, days are chosen without regard to day type (weekday or weekend day) or to position of a week within a month. The 16 random numbers generated for this example were 2, 3, 4, 8, 10, 13, 14, 15, 16, 17, 18, 22, 23, 24, 26, and 27, resulting in the sampling schedule shown in Figure 10.2. By chance, 6 days in a row were selected for surveying, and only 3 weekend days were drawn for the month as a whole.

Stratum 1					Stratum 2	
M	T	W	T	F	S	S
1	2	3*	4*	5*	6*	7*
8	9*	10*	11*	12	13*	14*
15	16	17*	18	19	20*	21*
22	23*	24	25	26	27*	28*
Weekdays $N_1 = 20$ Weekdays $n_1 = 8$					Weekend days $N_2 = 8$ Weekend days $n_2 = 8$	

Figure 10.3 Stratified random sampling, without replacement, of days in a month after stratification by day type (weekday versus weekend day). The sampling days (asterisks) are $n_1 = 8$ days of the $N_1 = 20$ weekdays in stratum 1 and $n_2 = 8$ days of the $N_2 = 8$ weekend days in stratum 2. These days are the primary sampling units (PSUs).

Simple random sampling is easy to do, and it is useful when there are neither systematic differences in effort and catch among days of the week nor trends in fishing over time. However, because effort and catch are usually far greater on weekends than on weekdays, and because simple random sampling can generate survey schedules that are awkward with respect to labor laws, this technique is rarely used for access point surveys.

Example 2: Stratification by Day Type. When fishing effort and catch are heavier on weekends and holidays than on weekdays, stratification by these day types will insure smaller variances in survey estimates. Weekends can be sampled more heavily by allocating more sampling to this stratum. For this example, we assumed that half the effort and catch occur on weekends, so we allocated 8 of the 16 days to weekends. Because this accounted for all the weekend days available in February, there was no need to select weekend days by random draw. From the 20 available weekdays, 8 were chosen by random draw: 3, 4, 5, 9, 10, 11, 17, and 23 (Figure 10.3).

Like simple random sampling, stratified random sampling can leave uneven coverage of the month—in this case of weekdays. If extended temporal trends in fishing activity are a concern, this sampling problem can be alleviated with stratification by week, either with or without stratification by day type.

Example 3: Stratification by Week. Stratification by week can reduce the variance of catch and effort statistics for fisheries that have disproportionately heavy use early or late during a fishing period—during opening week, for example. Sometimes a temporal trend is suspected but cannot be demonstrated until the first survey is taken. Then sampling can be stratified by week to gain anticipated statistical advantages and, in the absence of concrete information, the sample can be allocated equally among strata.

Continuing with our basic example of 16 sampling days in February, 4 days were chosen by random draw from the 7 days available each week, giving stratum samples of (1, 2, 4, 7), (8, 9, 10, 11), (15, 17, 18, 19), and (22, 23, 24, 26) and the sampling schedule shown in Figure 10.4. This draw had some unsettling results:

	M	T	W	T	F	S	S
Stratum a	1*	2*	3	4*	5	6	7*
Stratum b	8*	9*	10*	11*	12	13	14
Stratum c	15*	16	17*	18*	19*	20	21
Stratum d	22*	23*	24*	25	26*	27	28

Stratum a $N_a = 7$ $n_a = 4$
Stratum b $N_b = 7$ $n_b = 4$
Stratum c $N_c = 7$ $n_c = 4$
Stratum d $N_d = 7$ $n_d = 4$

Figure 10.4 Stratified random sampling, without replacement, of days in a month after stratification by week (strata a–d). The sampling days (asterisks) are $n_X = 4$ days of the $N_X = 7$ days in each stratum (X denotes stratum a, b, c, or d). These days are the primary sampling units (PSUs).

only 1 weekend day was chosen, and 5 days (7–11) were scheduled in a row. (Up to 8 consecutive sampling days could have been scheduled: Thursday–Sunday of one week and Monday–Thursday of the next.)

If trends in effort and catch were detected by this sampling design, the next February survey of the fishery could be altered to allocate more sampling days to the weeks with greater fishing activity. However, because simple stratification by week sometimes brings problematical distributions of sampling effort, as just noted, and because reasonable assumptions usually can be made about the distribution of fishing effort between weekdays and weekend days, stratification by week is used most commonly in conjunction with stratification by day type.

Such a dual stratification is illustrated in Figure 10.5. As before (Figure 10.3), the month was stratified into weekdays and weekend days, and half the total sample of 16 days was allocated to weekends. Then the month was stratified by week as in Figure 10.4, but because weekend days were fully covered, this stratification applied only to weekdays. The 8 sampling days allocated to weekdays were distributed evenly among the 4 weeks, 2 days from each week of 5 days. The resulting schedule is shown in Figure 10.5.

Example 4: Two-Stage Sampling. Often the fishing day is longer than the creel clerk's workday and a work shift must be chosen from within each day. First the day is chosen (primary sampling unit, PSU), as in Figure 10.5, then the part day (secondary sampling unit, SSU). The simplest division of the sampling day is into morning (AM) and afternoon (PM) shifts. Printed random number tables can be used to select (with replacement) the SSUs with equal probability; random numbers 0–49 can designate AM sampling and numbers 50–99 can designate PM sampling. We drew 16 random numbers—88 (PM), 3 (AM), 17, 12, 21, 22, 51, 60, 8, 4, 11, 35, 70, 84, 91, and 36—and matched them in order with the 16 sampling days; the results are shown in Figure 10.6.

In fisheries where most anglers finish their trips in the PM period, shifts can be selected with unequal probabilities (see Section 3.7) to insure more sampling when the chance of obtaining interviews is greater. The SSUs, for example, could have been selected to give twice as much sampling in the PM than in the AM shift (AM

	Stratum 1					Stratum 2	
	M	T	W	T	F	S	S
Stratum a	1*	2	3*	4	5	6*	7*
Stratum b	8	9	10*	11	12*	13*	14*
Stratum c	15	16	17	18*	19*	20*	21*
Stratum d	22	23*	24*	25	26	27*	28*
		Weekdays $N_1 = 20$ Weekdays $n_1 = 8$				Weekend days $N_2 = 8$ Weekend days $n_2 = 8$	

Figure 10.5 Stratified random sampling, without replacement, of days in a month after stratification by day type (weekday, weekend day) and week. Stratum sizes and sample allocations are as in Figures 10.3 and 10.4, except only stratum 1 (weekdays) was substratified by week, and $n_X = 2$ sampling days were selected from the $N_X = 5$ days available each week (X denotes week stratum a, b, c, or d). The days selected (asterisks) are the primary sampling units (PSUs).

probability, $P = 0.33$; PM, $P = 0.67$). Then random numbers 0–32 would designate AM sampling and 33–99 would denote PM sampling.

Example 5: Stratification of Work Shifts. In the previous example, work shifts were chosen with two-stage sampling; first the day was chosen and then the work shift within the day. In true work shift stratification, the shifts form the strata and the sampling days are selected independently within each shift stratum. Days to be sampled in the AM stratum are randomly selected from all available days, and likewise for days to be sampled in the PM stratum. For this example, in

	Stratum 1					Stratum 2	
	M	T	W	T	F	S	S
Stratum a	1* PM	2	3* AM	4	5	6* AM	7* AM
Stratum b	8	9	10* AM	11	12* AM	13* PM	14* PM
Stratum c	15	16	17	18* AM	19* AM	20* AM	21* AM
Stratum d	22	23* PM	24* PM	25	26	27* PM	28* AM
		Weekdays $N_1 = 20$ Weekdays $n_1 = 8$				Weekend days $N_2 = 8$ Weekend days $n_2 = 8$	

Figure 10.6 Two-stage stratified random sampling of days in a month and work shifts within a sampling day. After sampling days were selected (primary sampling units, PSUs: asterisks) by the scheme shown in Figure 10.5, they were divided into morning (AM) and afternoon (PM) work shifts (secondary sampling units, SSUs). The PSUs were sampled without replacement, the SSUs with replacement.

	M	T	W	T	F	S	S
	1	2	3	4	5	6	7
Stratum I-AM				*	*		
Stratum II-PM		*			*		
	8	9	10	11	12	13	14
Stratum I-AM							
Stratum II-PM	*				*	*	
	15	16	17	18	19	20	21
Stratum I-AM			*	*	*	*	
Stratum II-PM	*		*	*			
	22	23	24	25	26	27	28
Stratum I-AM			*	*			
Stratum II-PM							

Mornings (AM) $N_1 = 28$ $n_1 = 8$
Afternoons (PM) $N_2 = 28$ $n_2 = 8$

Figure 10.7 Stratified random sampling, without replacement, of days in a month after stratification by work shift (morning, AM, versus afternoon, PM). Sampling days were selected within these strata independently. The sampling days (asterisks) are $n_{\mathrm{I}} = 8$ mornings of the $N_{\mathrm{I}} = 28$ mornings in stratum I and $n_{\mathrm{II}} = 8$ afternoons of the $N_{\mathrm{II}} = 28$ afternoons in stratum II. Days 5, 17, and 18 were randomly drawn in both shift strata.

which sampling was to be equally allocated between mornings and afternoons, 8 sampling days were chosen from the 28 days available in the AM stratum and 8 from the 28 days available in the PM stratum. The random draw for the AM stratum was 4, 5, 17, 18, 19, 20, 24, and 25; for the PM stratum it was 2, 5, 8, 12, 13 15, 17, and 18. The resultant schedule is shown in Figure 10.7.

Because sampling days are chosen independently between strata, the same day can be drawn for both AM and PM periods, as happened here for days 5, 17, and 18. This sampling method can result in overly long workdays for a single clerk and more than one clerk may be needed with this type of design.

10.2.2 Choosing Access Sites

Access sites must also be chosen in a statistically sound manner. If the fishery has several access sites, one or more of them will be chosen probabilistically from a *current* and *complete* list of all available access sites. The accuracy of access site estimates of catch and effort depend on a complete and correct site list (Hayne 1991). If important sites are left off the list, fishing effort will be underestimated. If outdated and unused access sites are included, sampling effort will be wasted and the survey will be inefficient. Survey estimates will be more precise when the sampling effort can be allocated among sites in at least rough proportion to the fishing effort associated with those sites. (The same allocation principle applies to time units, as already noted.)

Access sites can be chosen either with or without replacement. Sampling without replacement insures that all sites are visited before one of them is revisited; sampling with replacement does not. However, the variance calculations for sampling without replacement are slightly more complex. Section 10.2.2.1 gives examples of sampling access sites with and without replacement.

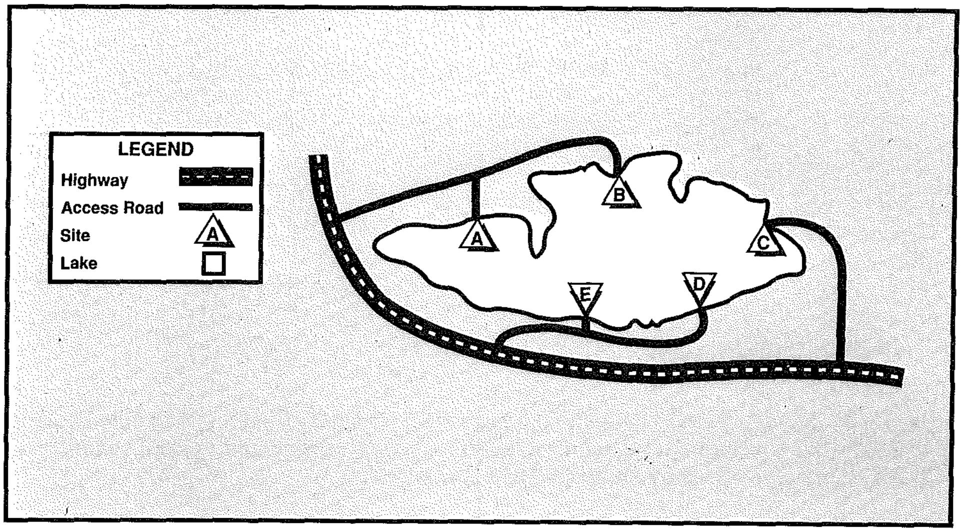


Figure 10.8 Map of a fishery with five well-defined access sites, A–E.

10.2.2.1 Examples Of Access Site Selection

Consider a month-long creel survey on a lake with five equally used sites, A, B, C, D, and E, depicted in Figure 10.8. The month has been stratified by day type (weekdays, weekend days). Sampling days have been selected as described in Section 10.2.1.1 and illustrated by Figure 10.5. Sites have been assigned equal probability of selection and two designs are to be evaluated: sampling of sites with replacement and sampling without replacement.

Example 1: Sampling Sites with Replacement. In sampling with replacement, after a site is selected it is returned to the sampling pool and has an equal chance of being selected again. In this example, access sites were selected with equal probability for each of the 16 sampling days with replacement. The five sites were coded by the following numbers: 1 = A, 2 = B, . . ., 5 = E. The random draw from numbers 1–5 (4, 4, 2, 2, 5, 4, 4, 4, 4, 3, 1, 2, 3, 3, 3, 2) resulted in the sampling schedule shown in Figure 10.9. By chance, site D was sampled six times but site A only once.

This selection process can be used when effort and catch are similar across all sites and when there is no trend in fishing effort over the month. Sampling with replacement results in larger variances in estimates than sampling without replacement (Deming 1960:385; Kish 1965).

Example 2: Sampling Sites without Replacement. In sampling without replacement, after a site is selected it is not returned to the sampling pool and cannot be selected again until all sites have been chosen. Then all numbers are returned to the pool and selection begins anew. To demonstrate this approach, access sites were selected for each of the 16 sampling days in a series of draws done without replacement. Again, the five sites were coded by numbers 1–5, and the random draw of 16 sites without replacement yielded the site sequences (4, 2, 5, 3, 1), (2, 3, 4, 1, 5), (4, 3, 2, 1, 5), and (4). The sampling schedule shown in Figure 10.10 resulted.

Stratum 1					Stratum 2	
M	T	W	T	F	S	S
1* D	2	3* D	4	5	6* B	7* B
8	9	10* E	11	12* D	13* D	14* D
15	16	17	18* D	19* C	20* A	21* B
22	23* C	24* C	25	26	27* C	28* B

Figure 10.9 Sampling schedule for which days were selected as in Figure 10.5 and access sites A–E were assigned with equal probability and with replacement. When sampling is with replacement, same site can be sampled consecutively.

This selection process is used when there is a possibility that fishing effort and catch differ among sites, vary systematically through a month, or both. It has the advantage that all sites are sampled more evenly throughout the month. Therefore, variances are less than they are for sampling with replacement, but the formulas for variance are more complex.

10.3 SAMPLING OF ACCESS SITES

The "traditional" one-site-per-day approach to sampling access points is the method of choice when sites are few—perhaps five or fewer for each survey clerk or team. It is straightforward to implement (see Section 10.3.1 for an example), and procedures to estimate catch and effort are simple. Sampling times and sites are chosen probabilistically, as discussed in Section 10.2. At the end of the survey, the daily effort (or catch or other variable) is calculated for each site. If a

Stratum 1					Stratum 2	
M	T	W	T	F	S	S
1* D	2	3* B	4	5	6* E	7* C
8	9	10* A	11	12* B	13* C	14* D
15	16	17	18* A	19* E	20* D	21* C
22	23* B	24* A	25	26	27* E	28* D

Figure 10.10 Sampling schedule like that of Figure 10.9, except access sites A–E were assigned without replacement. When sampling is without replacement, the same site cannot be sampled consecutively until all sites have been drawn (the last site of one draw and the first site of the next draw may be the same by chance).

site has been visited more than once, the daily totals for that site are averaged, and the average (or the single value if only one visit was made) is expanded to estimate angling over the entire sampling period. The expanded site totals then are pooled to represent the entire fishery. (Chapter 15 gives methods of calculation.)

In the traditional access point survey, a creel clerk visits only one site per day or per part day. If sites are numerous and, as often happens, survey budgets allow only one creel clerk, some sites may not have been sampled by the end of the survey. Effort, catch, and catch rate data from the excluded sites will not be available for expansion, and these omissions cause several problems. For one, such a survey is not advisable for effort estimation because it does not give good coverage of access and because the precision is likely to be poor. Total effort (angler-hours) should be estimated by another survey method and combined with the catch rate from the access survey (fish per angler-hour) to estimate total catch (hours times fish per hour). (These issues are discussed in more detail in Chapter 15.) Secondly, rare occurrences at monitored sites—a one-time influx of a large angler group or a limnological anomaly that briefly concentrates fish, for example—may unduly influence the catch analysis if coverage of sites is incomplete. Finally, if the sites that are sampled are not truly representative of unsampled sites in the fishery, biased catch estimates can result.

When numerous access sites must be sampled, several ways to conduct the survey are available.

- Add more staff, with or without stratification of sites.
- Shorten site visitation times so two (or more) sites can be visited per day (Section 10.2.2 laid the groundwork for this.)
- Use a bus route design.

Because a staff shortage leading to undercoverage of sites is usually caused by budget constraints in the first place, it may seem ingenuous to suggest staff additions. Nevertheless, if the survey is important and the statistical consequences of undersampling are severe, the survey sponsor may have no choice but to reallocate funds or personnel from some other program. Such trade-offs should be considered during the survey's planning phase.

Sometimes efficiencies can be gained by stratifying the sites. If the fishery is large, geographical stratification may allow survey clerks or teams to operate from dispersed base points, saving travel times and costs. If it is known that some sites are used less than others, they can be placed in a stratum that is sampled with lower probability. The point behind these and other suggestions for stratification is that good survey design can minimize the conflict between statistical and budgetary demands.

Visiting two or more sites per day is advantageous when a one-site-per-day program would cause severe statistical problems because of undercoverage. However, the logistics can be tricky. Travel time between sites must be randomized so it is not the same each day, and this means that the time spent at sites will vary from day to day and site to site. If travel between sites always occurs from noon to 1 PM, for example, nothing will be known about this period and biased estimates can easily result. The period would be excluded from the sampling frame, and inferences about it would have to be drawn from outside the frame—always a questionable and statistically invalid practice. It is difficult to plan these studies properly. Pitfalls include scheduling of variable travel times and lack of independence between sites chosen on the same day. Help from a survey statistician in planning a two-site-per-day survey is advised.

The bus route method is a modified access survey developed for fisheries with numerous access sites spread over broad geographic regions (Robson and Jones 1989; Jones et al. 1990). To use the method, numerous access sites are treated as a group and all of them are sampled during 1 or 2 days. The survey route, or trajectory, is analogous to a bus route with stops at designated places (access sites) on a predetermined time schedule. The route is a loop with an arbitrarily defined origin. On a particular sampling day, the starting position around the route (in units of route time, as exemplified in Section 10.3.2) is chosen at random. The clerk proceeds around the route, surveying at each access site on a precise schedule and waiting for a predetermined period (which could be directly proportional to expected use). While at the access site, the clerk interviews any departing anglers and conducts effort counts. The clerk then departs on schedule and proceeds to the next site. The route is usually set up so that it can be completed by a clerk traveling at a reasonable speed in one workday, although the route need not be completed in a day. The number of sites that can be included in a route depends on the travel time between routes and a minimum waiting time at each site. Usually a route includes 5–12 sites. The logistics of route scheduling are described in Section 10.3.2.

The bus route method differs from the traditional approach in how estimates are expanded along the route and to the entire fishery. Daily totals are calculated for the entire route; site values are not estimated separately. Route totals are averaged and the averages are expanded by the number of days in the survey period to obtain survey totals. The calculations are detailed in Chapter 15.

The advantage of the bus route design is that it provides a practical way to sample large regional fisheries with lots of access sites. Clerks are less bored than they would be if they were stuck in one little-used place all day, so the quality of their work is likely to be higher. The principal disadvantages of the method are its complexity, which requires clerks to be especially well trained, and an often large proportion of time spent in travel between sites, which lowers the number of interviews that can be conducted. Another inefficiency occurs when a site that is not used during certain times of day is scheduled during these periods. Such a site is best excluded from the bus route and handled as a separate traditional access site.

Jones and Robson (1991) showed that the bus route often (but not always) provides more precise estimates of effort than the traditional access design when effort is low. The precision is enhanced by the use of a method of counting anglers' cars rather than the anglers themselves (the "time (or car) interval method," discussed in Sections 10.4.1 and 15.5.3.2).

10.3.1 Example: Scheduling a Traditional Access Survey

The example in this section is based on the exposition by Malvestuto (1983). Suppose a state fisheries agency has stocked a lake with trout in the hope of establishing a naturally reproducing population. However, fishing mortality became so high that the population could not be sustained without continued stocking. Consequently, the agency imposed creel limits on the trout harvest last year, and now it wants to learn if fishing mortality has been reduced to target levels. An access point survey of the lake's anglers is indicated.

Sampling takes place in May, and 16 days are to be sampled that month. The

Stratum 1					Stratum 2	
M	T	W	T	F	S	S
1	2*	3*	4*	5	6*	7*
8	9	10	11	12	13*	14*
15*	16	17	18*	19	20*	21*
22*	23	24	25*	26*	27*	28*
29	30	31				

Weekdays $N_1 = 23$
Weekdays $n_1 = 8$

Weekend days $N_2 = 8$
Weekend days $n_2 = 8$

Figure 10.11 Schedule of sampling days used for a traditional access survey of a fishery in May. The month was stratified by day type (weekdays, weekend days). The sampled days (asterisks) are $n_1 = 8$ days of the $N_1 = 23$ weekdays in stratum 1 and $n_2 = 8$ days of the $N_2 = 8$ weekend days in stratum 2. Selection was done with equal probability and without replacement. These days are the primary sampling units (PSUs).

single creel clerk's 8-hour workday includes 1 hour of travel to and from the work sites, leaving an effective workday of 7 hours. The 14-hour fishing day, 6 AM to 8 PM, is divided for survey purposes into two 7-hour work shifts changing at 1 PM. The agency knows that fishing at this lake is about twice as heavy in afternoons as in mornings and much heavier on weekends than on weekdays. The lake has five access sites, which anglers use to differing degrees.

Selecting Primary Sampling Units. The sampling frame of days is stratified into 8 weekend days and 23 weekdays. Eight of the 16 sampling days are allocated to each stratum. Thus, weekend days receive 100% coverage and weekdays 34.8% coverage. Because weekend days are completely enumerated, no random draw is necessary for them. Weekdays for sampling are selected with equal probability and without replacement from all available weekdays. Weekdays are numbered from 1 to 23, and eight random numbers are drawn: 19 (May 25), 16 (May 22), 2, 20, 3, 4, 11, and 14. The resulting survey schedule is shown in Figure 10.11.

Selecting Secondary Sampling Units. The agency has some qualitative information about the distribution of fishing activity, which varies both spatially and temporally at the lake. On this basis, the following unequal probabilities of site and work shift selection are assigned:

Site probability	Shift probability
A: 0.250	AM: 0.33
B: 0.125	PM: 0.67
C: 0.250	
D: 0.250	
E: 0.125	

All potential combinations of sites and shifts are listed and their inclusion probabilities are specified. From the cumulative distribution of these probabilities,

		Stratum 1				Stratum 2
M	T	W	T	F	S	S
1	2* C-PM	3* D-PM	4* C-AM	5	6* D-PM	7* A-PM
8	9	10	11	12	13* E-AM	14* C-AM
15* C-PM	16	17	18* A-AM	19	20* D-AM	21* C-PM
22* D-PM	23	24	25* E-PM	26* B-PM	27* B-PM	28* D-AM
29	30	31				

Weekdays $N_1 = 23$
Weekdays $n_1 = 8$

Weekend days $N_2 = 8$
Weekend days $n_2 = 8$

Figure 10.12 Complete schedule for a traditional access survey, showing the assignment of sites (A–E) and work shifts (AM, PM) to the schedule of sample days illustrated in Figure 10.11. Sites and shifts were selected with unequal (nonuniform) probability and with replacement. These site-shift combinations are the secondary sampling units (SSUs).

a corresponding distribution of three-digit random numbers is created for selection of secondary sampling units (SSUs).

SSU	Unit probability	Cumulative probability	Random number range
Site A–AM	(0.250 × 0.33) = 0.08250	0.08250	000–082
Site A–PM	(0.250 × 0.67) = 0.16750	0.25000	083–249
Site B–AM	(0.125 × 0.33) = 0.04125	0.29125	250–291
Site B–PM	(0.125 × 0.67) = 0.08375	0.37500	292–375
Site C–AM	(0.250 × 0.33) = 0.08250	0.45750	376–457
Site C–PM	(0.250 × 0.67) = 0.16750	0.62500	458–624
Site D–AM	(0.250 × 0.33) = 0.08250	0.70750	625–707
Site D–PM	(0.250 × 0.67) = 0.16750	0.87500	708–874
Site E–AM	(0.125 × 0.33) = 0.04125	0.91625	875–915
Site E–PM	(0.125 × 0.67) = 0.08375	1.00000	916–999

The 16 random numbers drawn are: 623 (C–PM), 843 (D–PM), 438, 809, 143, 914, 378, 504, 61, 627, 486, 759, 930, 370, 333, and 626. They are assigned to the primary sampling units in the sequence drawn (May 2, C–PM; May 3, D–PM; etc.). The completed sampling schedule is shown in Figure 10.12.

Data Recording. The day's survey data are collected and summarized as shown in Figure 10.13.

10.3.2 Example: Scheduling a Bus Route Access Survey

Suppose an agency is considering angling restrictions for a short-season coastal fishery and needs data on catch and effort to support its management recommendations. The boat-based fishery is accessible at eight marina sites. This is judged

Interviewer Mary Smith Site C Month May Day 4 Year 1994

Angler Number	Time of Finish	Time of Start	Species Sought (code)	Species Caught (code)	Number Kept	Number Released	Length or Weight	State of Residence
1	10:34	6:30	33	0	0	0	---	xx
2	11:27	7:00	33	33	2	1	xxx	xx
							xxx	xx
3	12:03	9:30	33	33	0	1	---	xx
14	12:47	7:30	33	15	3	0	---	xx

Summary
Total anglers 14 Total angler-hours 60 h 26 min

Species 33 Total directed angler-hours 50 h 5 min
Total kept 8 Total released 9 Total caught 17

Species other Total directed angler-hours 10 h 11 min
Total kept 3 Total released 0 Total caught 3

Figure 10.13 Example of a data collection sheet for a traditional access survey (Section 10.3.1). For brevity, anglers 4–13 are not shown. Species 33 denotes the trout species stocked in the lake (all the state's fish species have standardized species codes). Because the survey was concerned only with the harvest of species 33, all other species were lumped as "other." Fish lengths and weights were measured by the clerk as anglers left the water.

to be too many access points to be covered by a traditional survey, so a bus route program is planned. The creel clerk has a 7-hour workday, exclusive of travel time to and from the survey area, so the 14-hour fishing day (6 AM–8 PM) is partitioned into two 7-hour work shifts. Weekend days and afternoons historically have had the heaviest fishing, and they are assigned twice the sampling probabilities allocated to weekdays and mornings. Some of the marina sites receive much more use than others.

Selecting Primary and Secondary Sampling Units. Primary sampling units (days) and secondary sampling units (shifts) are selected as described in Section 10.3.1 with (for purposes of this example) the same results (Figure 10.14). Because all eight sites are to be visited during each shift, the schedule in Figure 10.14 has only the designation "Route 1" for each sampling day. (Fisheries that are spatially extensive or that have many access sites may require two or more bus routes.)

Determining Travel and Waiting Times. Travel times between the eight sites—including time from site 8 to site 1—have to be measured in order to determine how much time will be left to wait at access sites. The survey team visits the sites, measures the travel times, and estimates expected use for each site based on the physical features of the sites and prior knowledge of site use. This preliminary information is used to apportion site-specific waiting times.

Travel times are measured for moderate driving speeds. Including time for loading and unloading equipment, they are as follows.

	Stratum 1				Stratum 2	
M	T	W	T	F	S	S
1	2* PM Rte 1	3* PM Rte 1	4* AM Rte 1	5	6* PM Rte 1	7* PM Rte 1
8	9	10	11	12	13* AM Rte 1	14* AM Rte 1
15* PM Rte 1	16	17	18* AM Rte 1	19	20* AM Rte 1	21* PM Rte 1
22* PM Rte 1	23	24	25* PM Rte 1	26* PM Rte 1	27* PM Rte 1	28* AM Rte 1
29	30	31				

Weekdays $N_1 = 23$ Weekend days $N_2 = 8$
Weekdays $n_1 = 8$ Weekend days $n_2 = 8$

Figure 10.14 Complete schedule for a bus route access survey showing the assignment of work shifts and route (rte) to the schedule of sample days illustrated in Figure 10.11. Work shifts were selected with unequal (nonuniform) probability and with replacement. In this example, all sites were combined into a single route and this route was assigned to each sampling day.

Site to site	Travel time (min)
1 to 2	5
2 to 3	10
3 to 4	5
4 to 5	2
5 to 6	20
6 to 7	5
7 to 8	5
8 to 1	2
Total	54

From the clerk's workday of 420 minutes (7 hours), then, 54 minutes of travel time are deducted, leaving 366 minutes for waiting at access sites.

Site use is numerically ranked (1–8) in increasing order of relative importance as a rough means of proportionally allocating the 366 minutes of wait time.

Site	Relative importance	Wait time (min)
1	8	81
2	4	41
3	6	61
4	2	20

Site	Relative importance	Wait time (min)
5	1	11
6	3	30
7	5	51
8	7	71
Total	36	366

Time allocations are calculated in the following way:

site 1: (8/36) × 366 min = 81 min;

site 2: (4/36) × 366 min = 41 min;

and so on.

Building the Prototype Route Schedule. With travel and wait times established, the prototype bus route schedule can be developed.

	Location	Cumulative times (min)
Arbitrary start	Site 1	0–81
	Travel, 1 to 2	82–86
	Site 2	87–127
	Travel, 2 to 3	128–137
	Site 3	138–198
	Travel, 3 to 4	199–203
	Site 4	204–223
	Travel, 4 to 5	224–225
	Site 5	226–236
	Travel, 5 to 6	237–256
	Site 6	257–286
	Travel, 6 to 7	287–291
	Site 7	292–342
	Travel, 7 to 8	343–347
	Site 8	348–418
	Travel, 8 to 1	419–420

This prototype schedule cannot be used directly, because it would mean starting at the same access site each survey day. Therefore, an actual daily schedule with a randomly selected starting location is developed for each sampling day. This approach ensures that a given access site will be sampled at various times of day during the course of the multiday survey.

Building the Actual Daily Schedule. Each daily schedule is produced by picking a starting point along the cumulative route time at random and beginning the run at that point. Because the route is built in units of time, not of distance, the starting point selected must be translated from time to a physical starting access site. A random number is chosen between 0 and the maximum route time (420 minutes in this example), and this number establishes the starting point in route-minutes. Movement around the route from there can be either clockwise or counterclockwise (chosen randomly). Two daily example schedules follow for the

AM shift, one with the random starting route time falling in a travel period and one with the random start falling within a waiting period. Calculations for the PM shift are made in the same manner.

One random starting number, 289, falls 2 minutes into the 5 minutes of travel time between sites 6 and 7. Hence, the survey period begins at site 7 at 6:03 AM, 3 minutes (the remaining travel time) after the start of the workday, and the rest of the schedule is as follows.

Site	Wait period
7	6:03 AM–6:54 AM
8	6:59 AM–8:10 AM
1	8:12 AM–9:33 AM
2	9:38 AM–10:19 AM
3	10:29 AM–11:30 AM
4	11:35 AM–11:55 AM
5	11:57 AM–12:08 PM
6	12:28 PM–12:58 PM

Another random starting number, 270, falls 14 minutes into the waiting period at site 6, with 16 minutes of wait time remaining. Hence, the survey period begins at site 6 at 6:00 AM, and the clerk leaves site 6 after 16 minutes. At the end of the route, the clerk returns to site 6 and completes the final 14 minutes of the 30 minutes allocated to this site.

Site	Wait period
6	6:00 AM–6:16 AM
7	6:21 AM–7:12 AM
8	7:17 AM–8:28 AM
1	8:30 AM–9:51 AM
2	9:56 AM–10:37 AM
3	10:47 AM–11:48 AM
4	11:53 AM–12:11 PM
5	12:15 PM–12:26 PM
6	12:46 PM–1:00 PM

Data Recording. The day's survey data are collected and summarized as shown in Figure 10.15.

10.4 OBTAINING EFFORT AND CATCH DATA

10.4.1 Effort

Effort data in the traditional access point design are obtained directly from angler interviews. Site effort is usually calculated by summing the total trips or trip durations for all anglers encountered at access sites. When it is too busy at a site for the creel clerk to talk with all anglers, all anglers still must be counted. If the survey objective is to produce estimates of angling trips, then these count data are sufficient. However, if the survey objective is to produce estimates of angler-hours, the clerk must also obtain an estimate of the average number of hours fished from the anglers who can be interviewed.

In fisheries where low effort results in few interviews, alternative approaches can be used to estimate fishing effort. Fishing effort data can be obtained during

Interviewer Mary Smith Route 1 Month May Day 4 Year 1994

Site	Angler Number	Time of Finish	Time of Start	Species Sought (code)	Species Caught (code)	Number Kept	Number Released	Length or Weight	State of Residence
6	1		6:05	33	---	---	---	---	xx
7	2		7:15	12	---	---	---	---	xx
8	3	8:15	6:15	99	0	0	0	---	xx
	4	8:21	6:00	33	33	0	2	---	xx
1	5	8:31	6:00	99	0	0	0	---	xx
	6	9:45	6:30	33	33	2	0	xxx	xx
								xxx	
2	7	10:23	7:00	99	0	0	0	---	xx
3	8	11:16	7:15	33	33	4	0	xxx	xx
								xxx	
								xxx	
								xxx	
	9	11:20	7:00	33	33	1	3	xxx	xx
	10	11:35	9:00	12	0	0	0	---	xx
4	none								
5	11	12:17	9:00	33	33	1	2	xxx	xx
	12	12:22	9:30	99	33	1	0	xxx	xx
6	13	12:48	10:00	33	33	3	0	xxx	xx
								xxx	
								xxx	
	14	12:55	10:00	33	0	0	0	---	xx

Summary
Total anglers 14 Total angler-hours 37 h 18 min

Species 33 Total directed angler-hours 21 h 57 min
Total kept 11 Total released 7 Total caught 18
and 1 caught non-directed

Species other Total directed angler-hours 13 h 21 min
Total kept 1 Total released 0 Total caught 1

Figure 10.15 Example of a data collection sheet for a bus route access survey (Section 10.3.2). The numbers under "species sought" are the standardized codes used for fish species by the agency. All fish lengths and weights were measured by the clerk as anglers left the water. All sites were visited by the clerk during the sampling day. Anglers 1 and 2 were interviewed as they began their trips to get information on species sought; these anglers would not enter calculations of catch or effort.

bus route surveys by recording the number of cars parked at a fishing site or the amount of time they are parked there. This is a useful method not only when fishing effort is low, but also when fishing trips are long and the clerk cannot be at a site throughout the day. This approach was used to estimate tributary angling effort during the 1984 New York Great Lakes creel survey. Entry to the fishing area was restricted to defined access sites, and parking lots at these sites were used almost exclusively by anglers. Total effort was estimated by recording either the number of cars or the amount of time that a car remained parked during the waiting period, an approach called a time (car) interval count (Robson and Jones 1989).

Occasionally a lake or embayment has a constriction or observation point from which the comings and goings of all boats can be seen. When the clerk is stationed at such a site counting boats, rather than at the typical access site, the resulting estimate of effort is the *exit count*. All boats equipped for angling that pass the observation point going in the *same* direction (any direction can be chosen) are counted (Fabrizio et al. 1991). Interviews can then be conducted at various marinas to determine the proportion of boats making excursions that actually were involved in fishing that day. This approach gives an estimate of fishing trips and also of angler-hours if mean trip duration is obtained in the interviews. A survey form that can be used for exit counts is shown in Figure 10.16. Technological advances for taking counts include the use of time-lapse cameras that provide a series of instantaneous counts without the cost of a clerk on site.

As noted, fishing effort can be recorded as trips or angler-hours. The number of trips is recorded directly from observation, whereas numbers of angler-hours are volunteered by anglers during interviews. Because trip numbers are obtained without necessarily having to conduct interviews, trip estimates can be made from instantaneous and exit counts. However, estimates of effort expressed as angler-hours have more information content for comparisons of fisheries that target several species and have different trip durations.

Methods for calculating estimated fishing effort are covered in detail in Chapter 15.

10.4.2 Catch

Catch can be calculated directly from interviews of anglers or indirectly by obtaining catch rate estimates and expanding them to total catch with an independent measure of effort. When effort does not come directly from interviews but from time interval counts, exit counts, instantaneous counts, or off-site methods, total catch must be calculated from catch rate expansions. Methods for calculating estimated catch are covered in Chapter 15.

Harvest—the fish caught that are also kept—can be measured in an on-site survey with little or no recall bias (inaccurate memory) or prestige bias (exaggeration of fish size or number: Section 5.5.1). The creel clerk can handle, identify, and measure the fish that are brought to shore. Estimates of catch—fish both kept and released—are more subject to bias than harvest estimates. The clerk must rely on angler recall and truthfulness about the number, size, and species of fish that have been released. Recall bias could be substantial if many nonmemorable fish have been released. In catch-and-release fisheries, the reliability of catch and catch-per-unit-effort data is lower than it is when a trained clerk sees and handles the harvest. However, because interviews take place immediately after a fishing trip is completed, access point surveys elicit fresher memories of released fish than do later off-site interviews. Anglers are also less likely to inflate the number of fish released when asked face-to-face by the creel clerk. Hence both recall and prestige biases are lower with access and other on-site interviews than with off-site surveys.

EXIT COUNT FORM

Date______________ Sampling Period______________

Weather______________ Location__________________

Craft Code: (1) <20 ft open deck (2) <20 ft cabin (3) >20 ft

Activity: (1) outriggers rigged (2) no outriggers, fishing
(3) commercial (4) sailboat
(5) can't tell

Observation	Time	Craft	Activity
1			
2			
3			
4			
5			
6			
7			
8			
9			
10			
11			
12			

Figure 10.16 Example of a survey instrument that can be used to record exit counts. This form includes a craft code to identify boat size and an activity code to visually categorize vessel activity. Access site interviews would be necessary to confirm the activities that were actually engaged in. For example, a boat may not have its outriggers set up, but may indeed have been fishing.

10.4.3 Beyond Catch and Effort

Information other than catch and effort data can be collected during an access survey. Interviews can include questions on angler attitudes, demographics of the angling population, and economic aspects of fishing. When the prime objective of a survey is to obtain economic and human dimensions data, off-site surveys are most often used (Chapters 16, 17), because questionnaires for these purposes tend to be long and anglers continue to accumulate fishing-related expenses until they reach home. However, when the questionnaire is relatively short, an access survey can provide some unbiased socioeconomic estimates. Access interviews also can provide a list of angler names, addresses, and telephone numbers for subsequent in-depth mail questionnaires and telephone interviews (Chapters 6, 7, 14).

10.5 QUALITY CONTROL

Quality control procedures (Chapter 2) should be applied to all aspects of the design, implementation, and analysis of access point surveys. Particular attention should be paid to the presence of clerks at work sites and the accuracy with which clerks identify species and measure fish. Survey clerks must be at their work sites for entire sampling periods even if fishing is very light. Supervisors should conduct random spot checks to insure that clerks are on site and not falsifying data. Clerks have been known to fill out interview forms at home, and this problem seems to be greater when clerks are recruited from the general public, not from agency staff. Falsifying data should be cause for dismissal, a condition that should be stated explicitly during training. The importance of proper training, including species identification for clerks unfamiliar with local fishes, cannot be overemphasized. Good training gives clerks ability, confidence, and enthusiasm for the project, all of which are fundamental elements of quality assurance. If clerks have been well trained, supervisory site visits to check a clerk's technique and to answer the clerk's questions provide positive reinforcement of good work habits and help sustain the morale of isolated field staff.

When data are recorded on field data forms and transferred to computer files, the survey team must check that data were both recorded accurately in the field and transcribed correctly to the computer files. Insuring that clerks and supervisory staff check field sheets soon after the day's sampling is completed will reduce recording errors; with the day's activities fresh in a clerk's mind, many mistakes can be corrected. Transcription errors can be minimized by thorough proofreading of computer files against field sheets. Some agencies use double-entry keying to eliminate these errors: the data are keyed twice into the computer (by the same or different people), and the two files are compared for differences.

10.6 PROCEDURES ON SITE

Once the times and sites for the survey have been selected, the creel clerks are given their work schedules. During a clerk's visit to a site, he or she counts the number of individual anglers or parties leaving the water at the completion of fishing. The clerk requests interviews of the anglers and asks questions about fishing effort (trip duration), fish released and kept, and perhaps other subjects germane to survey objectives (species preference, residency, attitudes, etc.). Usually the clerk examines the harvested fish as well. Two types of forms—scripted questionnaires (Appendix 10.1) and data matrices (Appendix 10.2)—are commonly used. (See Chapter 4 for a discussion of questionnaire designs.) The length of the interview depends on the number of questions asked. The more questions asked, the greater the likelihood that some anglers will be missed during periods of heavy activity. Furthermore, longer questionnaires are more likely to be resisted by anglers. Often just names, addresses, and telephone numbers are obtained after the harvest is examined, and a more extensive questionnaire is administered later off site. Thus, access surveys provide sampling frames for telephone and mail surveys.

The interview can be directed to the entire party or to each angler separately. If the purpose of the survey is to record differences and similarities among anglers with respect to attitudes, residency, or fishing success, individuals must be interviewed. If the survey's main purpose is to estimate overall catch and effort for the fishery, the party can be treated as a unit and one spokesperson (put forward by the group or selected by the clerk) can be interviewed. Interviewing one spokesperson has several advantages over contacting all anglers separately. The number of interviews and, therefore, the opportunity for recording error are reduced. Members of the group who caught few or no fish, more fish than the daily limit, or illegal fish will not feel stigmatized and hostile. If harvested fish already have been pooled in a common cooler, the annoying and time-consuming process of sorting fish by angler is avoided.

Sometimes it is not practical to examine every angler's harvest in detail and the clerk may have to rely on each angler's report. If many anglers leave the fishery in a short period of time, the clerk may have to subsample the anglers for interviewing purposes. A common strategy is to systematically subsample every *k*th angler (e.g., every third angler, or whatever interval is demanded by the flow of people). It would be statistically valid to simply pick the next available angler or party if all anglers at the site were equally available (Rubin and Robson 1990). However, successful anglers are more likely to present themselves for interviews than are unsuccessful anglers, so the clerk must guard against picking the next angler who comes up to talk. Even when anglers have to be subsampled for interviews, the noninterviewed anglers still must be counted so that the appropriate expansion can be made to estimate total catch and effort. Chapter 15 gives these calculations.

Avid anglers can be encountered more than once—by the same clerk or different clerks—during the course of a survey that lasts for weeks or months. These anglers may resist repeated interviews. The clerk should explain, in a friendly manner, the importance of each day's new information, refrain from asking for answers that remain the same from one day to the next (such as age or residence), and keep the interview as short as possible.

10.7 PRACTICAL CONSIDERATIONS

Survey planners must be familiar with physical features of the fishery because a complete list of the access sites in the fishery is essential for access surveys. Site lists can be developed from existing maps (specialized fishing maps are often available but sometimes outdated), from the knowledge of regional conservation officers and field biologists, and from information provided by local angling clubs. If access sites are missed, the estimates of total effort and catch will be too small. Familiarity with the fishery also allows survey planners to estimate relative effort at sites and thereby permit the allocation of unequal (nonuniform) sampling probabilities for each site.

A pilot study and test of the access survey design and questionnaire can help insure that the survey will meet its objectives. A pilot study on site can test assumptions about site usage, for example, and the sampling plan can be adjusted if the assumptions are found wanting. The questionnaire (which is called an interview schedule when administered orally) can also be tested and, if necessary, revised. During the testing period, survey clerks can become familiar with access sites and interviewing procedures. For a bus route, the travel time should be tested and revised as needed before the actual survey.

Because clerks are stationary at access sites, whether all day or at intervals

Table 10.1 Field supply checklist for access point survey clerks.

Necessary for all surveys
Clipboard (with cover for inclement weather)
Survey forms (preferably on waterproof paper)
Pens, pencils
Clerk's handbook of procedures
Watch (timer is also handy)
Survey identification signs (for car or site)
Rain gear and seasonal clothing
Fish identification handbook
First aid kit
Food and drinking water
Mechanical counter
Necessary for biological measurements
Measuring boards
Weighing scale
Fish scale envelopes
Cooler and ice
Necessary for physicochemical measurements[a]
Thermometer
Dissolved oxygen meter
Ph meter
Current meter
Secchi disk or turbidity meter
Water sampler and jars
Optional (depending on agency regulations)
Clerk uniform (patch, hat, identification card, etc.)
Incentive gifts for angler cooperation (pencils, hats, lures, etc.)
Agency mileage log forms, gas credit card, etc.

[a]The physicochemical measurements implied are those limited to nearshore areas and most relevant for small streams.

along a bus route, they can take more equipment and do more biological and limnological analyses than roving clerks can. For this reason, access surveys are preferred when such ancillary data are an objective of the survey. Supplies and equipment (Table 10.1) should be sturdy enough to withstand inclement weather. Waterproof paper and pens can be conveniences at any time but especially in rainy weather. Clerks at stream access sites can measure temperature, turbidity, particulate load, and current speed, among other variables. Physicochemical measures of large rivers, lakes, and embayments would be better obtained from a boat during a roving survey of the fishery. In the future, direct recording of data into portable field computers will be more common, eliminating transcription errors. Computers can be programmed to check data against realistic ranges as the data are entered.

In locations with several neighboring access sites, clerks should keep alert for anglers who seem to avoid sites just because the clerks are there. This behavior can cause an underestimate of catch and effort. The problem should not be serious if the agency and clerks have built a good relationship with anglers using the fishery and if they provide benefits such as informational brochures. However, when there are restrictions on the fishery, such as size and creel limits, law violators can be expected to avoid exposure by using an exit site without a clerk or by refusing an interview.

The personal safety of clerks is a paramount concern in on-site survey designs. Access site clerks do not face the dangers of boating or of hiking rough shoreline terrain that roving clerks encounter, but they are vulnerable to physical abuse at sites that are remote and isolated or near violence-prone neighborhoods. Access surveys are the most practical means to measure night fisheries (sometimes roving night surveys can be conducted, though they usually are too dangerous), but survey planners must be sure that the access sites selected are well lighted and otherwise safe. Dangerous sites should be omitted from a survey and the consequent undersampling acknowledged.

A postsurvey evaluation of site choice, probability weightings, on-site procedures, and effectiveness of stratification will enhance the quality of subsequent surveys. Previously unrecorded access sites may have been discovered during the survey, and it may be important to include them in future surveys. Conversely, some sites thought to be important might actually have received little use and could be deleted from the frame. Recent housing and other developments may change the relative importance of sites. Allocation of sampling effort should be compared with the actual number of interviews obtained, and sampling effort should be reallocated when over- or undersampling is evident (e.g., if too much time has been spent at a low-use site). Thus future sampling will be optimized.

10.8 STRENGTHS AND WEAKNESSES

The important features of access surveys are that they are conducted on site and they provide information on catch as soon as anglers come off the water. If sampling is done probabilistically, the statistical methods to obtain total effort and catch are straightforward (Chapters 3, 15).

One strength of the access method, shared with other on-site surveys, is that angler harvests are examined by trained clerks. This insures that species identifications are made properly and that any biological data such as length, weight, or sex are recorded accurately. Because harvests generally are not self-reported (as they are with off-site methods), access clerks can detect illegal harvests when they occur—if unlawful anglers do not actively avoid sites occupied by survey staff and thereby bias catch and effort estimates.

On-site clerks still must rely on anglers to report the number and species of fish that were released and the duration and places of fishing. Because memory is fallible, especially when many fish have been caught, access and other on-site surveys may be less accurate for catch-and-release fisheries than for fisheries in which harvest is allowed. Misremembered starting times can distort estimates of fishing effort obtained by any survey method. However, the proximity of on-site interviews to the fishing experience means that the data are less likely to be influenced by memory recall problems than data obtained by off-site interviews.

Access and other on-site surveys disproportionately sample avid anglers because these people are encountered more frequently. When the objective of the study is to characterize the population of anglers in terms of demographics, economics, or attitudes, the access method will bias the study toward avid anglers (Thompson 1991); off-site methods may give less bias. With respect to other distortions, access surveys suffer less from prestige bias than off-site surveys, and they are not vulnerable to length-of-stay bias, which is a problem with roving surveys.

Another advantage of access over roving surveys is that information is obtained from anglers after, not during, their fishing trips. Interpretation of completed trip data requires fewer assumptions than interpretation of data from incomplete trips. Catch and harvest can be calculated directly from completed trip data but not from uncompleted trip data.

Access surveys also are safer than roving surveys and usually more practical at night. This consideration may influence survey design in hot climates, where night fishing is common.

The value of the access survey design can be weakened if coverage of the fishery is incomplete because too many sites have been overlooked or because all access points cannot be surveyed. Sometimes it is not possible to list even all the well-defined access sites. Furthermore, a fishery may have many small, infrequently used access sites or it may have a substantial amount of diffuse access that cannot be monitored by stationary or bus route clerks. When these problems arise, roving surveys are the better approach. Private docks and piers that are closed to survey clerks lead to undercoverage by the access method.

Access surveys, and on-site surveys in general, are more costly than off-site surveys. Far fewer interviews are obtained for each working hour compared with off-site surveys, which makes each interview relatively expensive. The costs of conducting an access survey depend, in part, on the resources of the group that will implement the field design. Certain costs can be anticipated. Transportation to access sites must be provided, bringing expenses associated with vehicle depreciation and maintenance, insurance, and fuel or with mileage allowances paid to clerks who use their own vehicles. Field equipment must be bought, serviced, and periodically replaced. Training in safe vehicle use, biology, face-to-face interviewing, and personal relations with anglers is more extensive than required for mail and telephone surveys. Quality control is more expensive when staff have to go into the field to exert it.

Access surveys cannot provide complete economic data for a fishing trip in its broad sense (home to home). Anglers may incur expenses later in the day after their interviews, they may stay at the fishery for additional days, and they will spend more money just to get home (if only for fuel). However, access (and roving) clerks can obtain names, addresses, and telephone numbers that establish the frame for follow-up economic surveys by mail or telephone.

On balance, when an angler survey is geared to effort, catch, harvest, and biological data, and when the fishery of interest can be reached via relatively few, well-defined public sites, the access point survey is the method of choice.

Appendix 10.1 Scripted Questionnaire for an Access Point Survey

Interview #:________________ Interviewer Code:__________

Month_____ Day_____ Hour (24-hour Clock)_____ Minutes_____

County__________________ Site Code_____

Approximate Age 1 Senior 2 Adult 3 Juvenile

Gender 1 Male 2 Female

Q1 Hello, my name is ________ and I am representing the [name of organization] in a study of the fishery in this area. This study concerns the type of fish you have caught and some information about you as an angler. May I ask you some questions?

1 No I hope you enjoy your fishing trip. END OF INTERVIEW
2 Yes Thank you. CONTINUE

Q2 What city and state or province do you live in? City_________________State/Province_________________

I'D LIKE TO ASK YOU A FEW QUESTIONS ABOUT YOUR FISHING TRIP TODAY

Q3 When did you start your fishing trip today? Hour (24-hour clock)_____ Minutes_____

Q4 Sometimes people combine fishing trips with other activities. Was this trip primarily for fishing?

1 Yes 2 No

Q5 What types of fish were you especially trying to catch at this site?

1 Trout 2 Whitefish 3 Any Fish

Q6 I'd like to inspect your catch and discuss the fish you caught and released on this trip.

How many Trout______, Whitefish_____, Other ______ (specify each) did you **release** today?

How many Trout______, Whitefish_____, Other_______ (specify each) did you **keep** today? (Confirm number and identification if possible)

(Continued on next page)

Appendix 10.1: Continued

Q7 May I measure your catch? 1 Yes 2 No

Species____________________

Number													
Length													

Species____________________

Number													
Length													

Species____________________

Number													
Length													

Species____________________

Number													
Length													

NOW I'D LIKE TO ASK YOU QUESTIONS ABOUT YOUR ANGLING EXPERIENCES IN GENERAL AND SOME INFORMATION ABOUT YOU

Q8 When was your last fishing trip this year? Month______ Day______ (show calendar)

Q9 How many times did you fish last month?______

Q10 Employment Status

1 Employed full time 2 Employed part time
3 Employed in the home 4 Unemployed 5 Retired

To better manage this fishery we will be seeking information throughout the season from a random sample of anglers whom we have interviewed here. If you agree to participate, you may be contacted by phone or by mail at a later date. Would you please provide your name, address, and phone number? [If yes, Continue].

1 Yes Thank you very much.

Name ____________________
Address ____________________
City ____________________ State/Province____________ Postal/Zip Code__________
Phone (____)______-________

2 No

Comments:

Appendix 10.2 Unscripted Data-Recording Questionnaire for an Access Point Survey

(See next page for codes)

101(10/90)

Old Dominion University
1990 Striped Bass
Recreational Angler Study

ROUTE				CLERK		DATE DA		MO		YR		DT
1	2	3	4	5	6	7	8	9	10	11	12	13

PARTY RECORD — CATCH RECORD

PARTY CODE							# IN PARTY		# OF ANGLERS		# OF LINES		FT	TIME STARTED				TIME FINISHED				SITE		CT	SPECIES SOUGHT			SPECIES CAUGHT			# KEPT		# RELEASED		LENGTH				STATE		COUNTY					PERMIT	LAST FISHED			
14	15	16	17	18	19	20	21	22	23	24	25	26	27	28	29	30	31	32	33	34	35	36	37	38	39	40	41	42	43	44	45	46	47	48	49	50	51	52	53	54	55	56	57	58	59	60	61	62	63	64

Appendix 10.2: Continued

EXPLANATION FOR ANGLER INTERVIEW FORM

Column	Variable Name	Coding Information
1-4	Route	Identifies survey route (e.g., SB1)
5-6	Clerk code	Identification number of survey agent
7-12	Date	Day/month/year (e.g., 11 05 90: 11 May 1990)
13	Day type (DT)	1) Weekday; 2) weekend; 3) holiday; 4) fishing tournament or derby
14-20	Party code	Sequential number assigned by clerk; the first party interviewed each day receives code 1
21-22	Number in party	Total number of persons in the fishing party
23-24	Number of anglers	Number of active anglers in the fishing party
25-26	Number of lines	Number of lines used may exceed number of anglers
27	Fishing type (FT)	1) Boat angling; 2) shore angling; 3) pier angling
28-31	Time started	Time party began fishing at first site; use 24-hour clock and mark with an asterisk if fishing started last night
32-35	Time finished	Time party stopped fishing (=time of this interview)
36-37	Site	Code number of interview site
38	Trip (CT)	1) Complete; 2) incomplete; 3) arriving angler
39-41	Species sought	Species code for target species; if more than one, record additional codes on consecutive lines; enter 999 for "anything that bites."
		For Each Species Caught
42-44	Species caught	Species code
45-46	Number kept	Number of fish kept
47-48	Number released	Number of fish released
		For Each Fish by Species (if time permits)
49-52	Length	Total length to nearest quarter-inch; an 18½" fish is recorded 18-2 (18 2/4")
53-54	State	2-letter abbreviation for state of residency
55-59	County or city	First 5 letters of residence county or city
60	Permit	1) Anglers have permit for striped bass; 2) anglers do not have permit
61-64	Last fished	Record date on which anglers last fished for striped bass in 1990

Note: Use 1 line per fish. For the same party, record additional fish of the same species in columns 49-52 only; for the next species, record data in columns 42-52 for the first fish and in columns 49-52 for additional fish.

Chapter 11

Roving Creel Surveys

11.1 INTRODUCTION

The roving survey, like the access survey, is an *on-site, intercept* design that is extensively used to sample recreational fisheries (Malvestuto et al. 1978, 1983; Dent 1986; Beisser 1989: Roth and Delaney 1989; Bayley et al. 1990, 1991). This method is used when information is needed on catch and effort for specific water bodies. Although socioeconomic data can be obtained with roving surveys, such data are better obtained with off-site surveys. Roving surveys are conducted by boat to contact boating anglers and by foot to contact shoreline and streamside anglers. Robson (1991) summarized the statistical features of the roving survey design.

The roving method is used to estimate fishing effort, catch rate, and other parameters when access to a fishery occurs at too many points to accommodate in a traditional access point design. Where anglers can simply walk to the water at many points along a lake, streambank, estuary, or seashore, a roving design may be the only way to sample the fishery. Even in fisheries where well-developed public access sites exist, the roving survey may be useful if a substantial amount of boat angling originates from private docks, piers, or other landings from which access site clerks may be barred. The roving design allows anglers—at least those fishing from boats—to be counted and interviewed regardless of where they began their fishing trips.

Roving surveys produce estimates of catch or harvest rate and fishing effort. (Catch refers to all fish caught, whether kept or released, and harvest refers to fish kept; fishing effort sometimes is called fishing pressure.) Catch rate (fish/hour) is derived from interviews, during which anglers are asked what time they started fishing and how many fish they have caught up to the time of the interview. Effort (angler-hours) in a fishing area is based on counts of anglers extrapolated to the number of hours in a fishing day. Total catch or harvest is not estimated directly, because interviews document only part of the catch; rather, it is calculated as the product of effort and catch rate (angler-hours $\times$ fish/hour).

The roving survey has unique properties because interview data are taken from anglers who have not completed their fishing trips. These properties include the unequal probability with which anglers are encountered and the assumptions that underlie catch estimation (Section 11.3.2). In access surveys, all anglers who leave the fishery at an access site during defined monitoring periods have equal probability of being counted, regardless of how long they fished. In roving surveys, by contrast, anglers are intercepted during the act of fishing and the probability of intercepting them is proportional to the duration of their fishing (Robson 1961, 1991; Lucas 1963; Brown 1971). Anglers who fish longer are more likely to be intercepted and interviewed in roving surveys. This principle has been

CHARACTERISTICS OF A ROVING SURVEY

- The survey takes place on site, physically on the shore or on the water
- Specific locations on water bodies can be targeted
- The fishery has numerous access sites, walk-on access, or access that can not be surveyed by stationary creel clerks
- Sampling events are chosen randomly with known probability from a list of water body segments and from all days of the fishing season
- Roving clerks are mobile, interviewing and counting anglers in the process of fishing
- Anglers fishing longer are sampled disproportionately more than short-term anglers
- Harvest can be examined by the creel clerk

Figure 11.1 Summary of the major characteristics of roving surveys.

demonstrated: mean trip length is higher for anglers interviewed by roving clerks than for all anglers using the fishery (Malvestuto 1983; Wade et al. 1991). Sampling thus is subject to a "length-of-stay" bias. If catch rate (fish/hour) is related to the length of the fishing trip, a length-of-stay bias will be introduced in estimates of total catch and harvest (Robson 1961; Brown 1971; Wade et al. 1991; see Section 11.4).

In addition to obtaining data on fishing effort and catch and harvest rates, roving creel clerks (survey agents) may examine and measure the fish that anglers have harvested up to the time of the interview, and they may ask questions about social, attitudinal, or economic issues. They often obtain anglers' names, addresses, and telephone numbers for use in follow-up surveys of biological, economic, or sociological interest. The roving survey is an excellent method for obtaining location-specific information on effort, species composition, and size of the harvest because the creel clerk counts and interviews anglers at their fishing spots, avoiding recall and prestige biases associated with off-site surveys. (Recall bias arises when anglers fail to recollect events accurately, and prestige bias arises when anglers inflate the numbers or sizes of fish they have caught and released to increase their status.) The characteristics of the roving survey are summarized in Figure 11.1.

11.2 SPATIOTEMPORAL FRAME

The spatiotemporal sampling frame for a roving survey is very similar to that of an access survey. Unlike the list frames used for off-site surveys, the frame for the roving design consists of all of the times (days, part days, etc.) available for fishing during a survey period and the physical locations (river segments, lake sections, etc.) of the fishery. The usual selection of sampling times and places is sequential: date of sampling is chosen first, then the time period within the date (e.g., morning or afternoon), then an area of the fishery.

11.2.1 Choosing Dates and Times

Several methods can be used to schedule sampling dates and times (Chapter 3, Section 10.2, and examples in Chapter 10), but the most commonly used method is cluster or two-stage sampling (Cochran 1977; Krebs 1989). Choices must be made about the number of days to be sampled during the fishing season and whether to stratify by month and day type (weekday versus weekend day) within the fishing season (Best and Boles 1956; Lambou and Stern 1958; Pfeiffer 1966; Malvestuto et al. 1978; Sztramko 1985; Malvestuto 1991). In this regard, both the roving and access methods are identical. See Krebs (1989:223) for rules on constructing strata.

Sampling of dates can be done with simple or stratified random selection as described for access surveys (see examples in Section 11.2.3 and Section 10.2.1). Typically, fishing effort changes month-by-month and heavier fishing effort occurs on weekend days than on weekdays. Surveys usually will be most efficient (have least variance) when the distribution of sampling effort coincides with the distribution of fishing effort (Best and Boles 1956) or with the distribution of catch, if catch is the variable of interest. The easiest way to match up sampling and fishing effort is to stratify the fishing season or year into months and day types and to draw separate samples from each stratum (see Malvestuto 1983, 1991 for additional discussion). For example, if 30% of the annual fishing occurs during July and August, 30% of the annual sampling should be allocated to these months.

Sampling can be allocated to day types either in proportion to the number of days of each type or in proportion to the fishing effort associated with each type. The second alternative is preferable when information about the distribution of fishing effort is available. Consider a fishery in which 50% of the fishing historically has taken place on weekends. The fishery is to be surveyed, and 4 sampling days are to be allocated each week. A week of 7 days is 71% weekdays and 29% weekend days, divided between weekday and weekend strata. If the 4 sampling days were allocated in proportion to the number of stratum days, 3 weekdays (75% of the sampling effort) and 1 weekend day (25%) would be sampled each week, the closest proportional match possible. This schedule would cover 60% of weekdays and 50% of weekend days. However, this approach will not result in the minimum variance of estimated effort; weekday sampling would cover 30% of the fishing effort (60% coverage of 50% of the fishing effort) and weekend sampling would cover 25% (50% coverage of 50% of the fishing effort), which sums to only 55% of the total fishing effort. The minimum variance will usually be obtained when sampling effort is allocated in proportion to fishing effort. Thus, 50% of sampling would be allocated to weekdays and 50% to weekends—2 weekdays and 2 weekend days per week. With this schedule, weekday sampling would cover 20% of total fishing effort (40% coverage of 50% of the effort) and weekend sampling would cover 50% (100% coverage of 50% of the effort), for an overall coverage of 70%. There is a trade-off between minimizing the variance and having adequate sampling in all strata. Sometimes it is necessary (and appropriate) to accept a lower precision, particularly in exploratory surveys.

As shown above, prior knowledge of a fishery is necessary for optimal stratification. Angler usage is the variable most likely to be known, which is why fishing effort is the usual basis of sampling stratification. However, stratification can be based on patterns of catch and harvest, if these are known. If catch were the focus of a study and the temporal distribution of catch were known to differ from that of effort, catch would be the preferred basis of stratification. Likewise, spatial distributions of effort or catch can be used to establish a spatial stratification of sampling effort.

The selection of sampling dates and times within a day can be made either randomly with replacement or randomly without replacement (see the example in Section 11.2.3). Traditionally, the sampling date (primary sampling unit, PSU) is chosen without replacement and the time period (secondary sampling unit, SSU) is chosen with replacement. The sampling area can be chosen with or without replacement, as discussed in Section 11.2.2.

The fishing day is usually longer than the creel clerk's working day and therefore the day must be subsampled in a legitimate statistical manner. The usual approach is to partition the fishing day into two or more work periods, usually mornings (AM) and afternoons (PM), and to choose randomly one of these periods to sample on each survey day. When the date (PSU) is chosen first and a time period within the day (SSU) is chosen next, the selection method is termed two-stage or cluster sampling (Cochran 1977, also see Section 3.5). These work periods can be chosen either with equal or unequal (nonuniform) selection probabilities. When there is no prior knowledge about the pattern of fishing within an average day, equal sampling probabilities usually are assigned to AM and PM periods. However, when prior knowledge of fishing patterns exists, survey efficiency and precision of estimates can be enhanced by sampling the more heavily fished period in proportion to the effort or catch occurring then. Malvestuto (1983) gave examples of unequal probability sampling in two-stage sampling procedures and examples of this sampling design demonstrate its value (Malvestuto et al. 1978).

By an alternative selection method, within-day periods are stratified (usually into AM and PM periods) and become the PSUs; then dates (SSUs) are selected independently within each time period. This method allows the same date to be drawn in both the AM and PM strata, which may force a clerk to work overtime or more than 40 hours a week—or force the hiring of another clerk—and true stratification of within-day periods is usually avoided for this reason.

11.2.2 Choosing the Location

The roving survey differs from the access method in the spatial aspect of the sampling frame. In the access method, a complete list of discrete access sites is the spatial frame. In the roving method, there are no discrete sites; rather, a complete set of subareas covering the full geographic extent of the fishery is the spatial frame. Roving survey frames include river- and streambanks as well as open channels, and shorelines as well as open lakes, estuaries, or oceans.

Whenever a water body is too large to survey entirely within a clerk's workday, a statistically valid method of sampling must be used. The fishery is divided into subareas, each of which can be completely surveyed in the clerk's work shift, and one of these area sampling units is selected at random with equal or unequal selection probabilities and with or without replacement. Some areas of the water body may be more heavily fished than others and sampling usually should be concentrated in those areas (see example in Section 11.2.3).

The decision to choose an area with or without replacement will depend on the particular circumstances of the fishery. Choosing an area with replacement is the more common approach, but it can result in choosing the same area on consecutive sampling days. When effort or catch differs markedly between areas

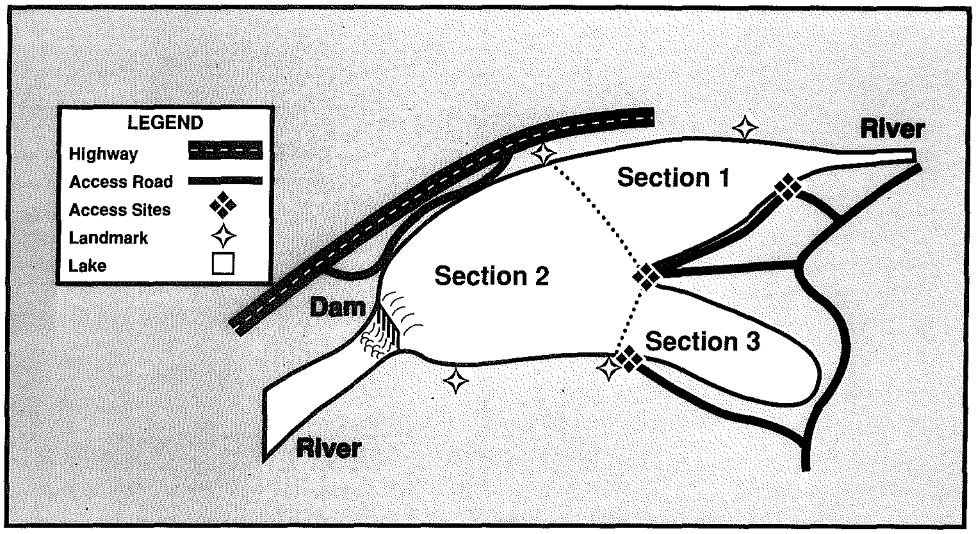


Figure 11.2 Map of a hypothetical reservoir whose fishery is to be sampled with a roving creel survey. The reservoir was divided into three sections, each of which can be covered by a creel clerk in one workday. Access roads parallel the shoreline along much of the reservoir, permitting anglers to reach the water at many points.

or when major fishing pressure shifts among areas over the course of a season, choosing areas without replacement may give more representative estimates of effort and catch. This approach spreads the sampling over the entire fishery. When an area is chosen without replacement, it is no longer available for selection until all areas have been chosen, and then it again becomes available for selection in the next round.

11.2.3 Example: Selecting Dates and Locations

The following extended example is based on concepts illustrated by Pfeiffer (1966) and Malvestuto (1983).

11.2.3.1 Background

A natural resources agency wants to determine whether bag limits imposed 2 years ago on a reservoir fishery have resulted in better survival of bass and therefore in better catches of bass. An on-site angler survey is planned to determine catch rates, which will be compared with data from a previous survey. The reservoir has many access sites and much diffuse access, so a roving survey design is selected. For budgetary reasons, only one creel clerk can be assigned to the survey. The reservoir is too big for one person to cover completely in one workday, so it is divided into three sections (Figure 11.2), each of which can be surveyed within a work period.

Sampling is to take place in June. The creel clerk lives nearby and needs only 15 minutes for travel to or from the reservoir. Thus the effective workday is 7.5 hours long. The fishing day lasts from 5 AM to 8 PM, 15 hours, and it is divided into two nonoverlapping periods: 5:00 AM-12:30 PM and 12:31 PM-8:00 PM. Based on previous surveys, the agency also knows that fishing is heavier on

JUNE

		Stratum 1				Stratum 2
M	T	W	T	F	S	S
1	2*	3*	4	5	6	7*
8	9	10	11	12	13*	14
15*	16	17*	18	19	20*	21
22	23	24	25	26	27*	28
29	30					

Weekdays $N_1 = 22$ Weekend days $N_2 = 8$
Weekdays $n_1 = 4$ Weekend days $n_2 = 4$

Figure 11.3 Stratified sampling design for a roving survey. The samples (asterisks) are $n_1 = 4$ days of the $N_1 = 22$ weekdays in stratum 1 and $n_2 = 4$ days of the $N_2 = 8$ weekend days in stratum 2. These represent the primary sampling units (PSUs).

weekends than on weekdays, that effort is three times heavier in the afternoon than the morning, and that section 1 of the reservoir receives twice as much use as either of the other two sections.

11.2.3.2 Choosing Primary Sampling Units

The primary sampling units are days, and these are selected first. Because of the disparity in fishing between weekdays and weekends, the agency has chosen a stratified random sampling design based on day type, rather than simple random sampling. (See Section 10.2 for examples of alternative designs.) The agency can afford to sample for 8 days in June, a 27% coverage of all fishing days that month. Four sampling days are allocated to weekends (50% coverage) and 4 to weekdays (18% coverage).

For the weekend stratum, 4 days are chosen from the 8 weekend days by selecting 4 random numbers without replacement from the range 1-8; the draw is 5 (June 20), 3 (June 13), 2 (June 7), and 7 (June 27) (Figure 11.3). For the weekday stratum, 4 days are chosen without replacement from the 22 available days (random number range, 1-22): 11 (June 15), 2 (June 2), 13 (June 17), and 3 (June 3).

11.2.3.3 Choosing Secondary Sampling Units

With primary sampling units chosen, the agency next allocates sampling effort to within-day work shifts and reservoir sections. Four ways of selecting these secondary sampling units (SSUs) are available: equal probability sampling with or without replacement, and unequal probability sampling with or without replacement.

Equal Probability Sampling with Replacement. The simplest way to choose SSUs is to assign equal probabilities to both work shifts and reservoir sections. These probabilities are assigned to work shifts and reservoir sections as follows:

Shift selection	
Shift	Probability
AM	1/2 = 0.50
PM	1/2 = 0.50
	1.00

Section selection	
Section	Probability
1	1/3 = 0.33
2	1/3 = 0.33
3	1/3 = 0.33
	0.99

Work shifts and lake sections can be assigned sequentially—first shift, then section—or simultaneously after their probabilities are combined. For the latter, all potential combinations of shifts and sections are listed and their component probabilities are multiplied to obtain the combination probabilities. The combinations then are selected by random draw. In the present example, shifts have the same probabilities of selection and so do sections, and the product of their probabilities also are constant. The combinations (SSUs) thus can be drawn with a simple suite of random numbers:

SSU	Probability	Random number
AM-section 1	(0.5 × 0.33) = 0.165	0
PM-section 1	(0.5 × 0.33) = 0.165	1
AM-section 2	(0.5 × 0.33) = 0.165	2
PM-section 2	(0.5 × 0.33) = 0.165	3
AM-section 3	(0.5 × 0.33) = 0.165	4
PM-section 3	(0.5 × 0.33) = 0.165	5
	0.990	

Eight random numbers from 0 to 5 are chosen: 5 (PM-section 3), 2 (AM-section 2), 1 (PM-section 1), 4 (AM-section 3), 1 (PM-section 1), 2 (AM-section 2), 1 (PM-section 1), and 3 (PM-section 2). The completed schedule is shown in Figure 11.4.

Equal Probability Sampling without Replacement. Sampling with equal probability and without replacement is one of the most common approaches to sampling; Cochran (1977:18) referred to it as simple random sampling. The procedure for selecting SSUs is the same as shown in the previous example *except* that once an SSU is drawn, it cannot be drawn again until all SSUs have been chosen once; once they have, the draw begins anew.

In the previous example where sampling was done with replacement, only eight numbers were drawn to assign the combinations of work shift lake section. Sampling without replacement usually requires a larger draw because duplicate selections are discarded until all combinations are picked. For the random number assignments shown just above, the first draw to get all six combinations once (discarded numbers are in parentheses) is 5, 4, 2, 3, (5), 1, (3, 3, 4, 1, 5), 0. The second draw to fill the remaining two sampling days is 2, (2), 0. The survey schedule for equal probability sampling without replacement is illustrated in Figure 11.5.

Equal probability sampling is easy to do, and it is appropriate to use (with or without replacement) when nothing is known about a fishery or when the distribution of fishing has no temporal or spatial trend. However, it does not allow efficient sampling of predictably heterogeneous fisheries. The agency knows, in

JUNE

Stratum 1					Stratum 2	
M	T	W	T	F	S	S
1	2* PM Sec. 3	3* AM Sec. 2	4	5	6	7* PM Sec. 1
8	9	10	11	12	13* AM Sec. 3	14
15* PM Sec. 1	16	17* AM Sec. 2	18	19	20* PM Sec. 1	21
22	23	24	25	26	27* PM Sec. 2	28
29	30					

Figure 11.4 Stratified sampling design with secondary sampling units (SSUs)—work shift and reservoir section—added to Figure 11.3. The sampling of SSUs shown here was done with equal inclusion probabilities and with replacement.

this example, that fishing effort differs between mornings and afternoons and among sections of the reservoir. Although equal probability sampling without replacement results in smaller variances than equal probability sampling with replacement, even smaller variances can be obtained with unequal probability sampling where sampling effort is proportioned to fishing effort. When fishing

JUNE

Stratum 1					Stratum 2	
M	T	W	T	F	S	S
1	2* PM Sec.3	3* AM Sec.3	4	5	6	7* AM Sec.2
8	9	10	11	12	13* PM Sec.2	14
15* PM Sec.1	16	17* AM Sec.1	18	19	20* AM Sec.2	21
22	23	24	25	26	27* AM Sec.1	28
29		30				

Figure 11.5 Stratified sampling design with secondary sampling units (SSUs)—work shift and reservoir section—added to Figure 11.3. The sampling of SSUs shown here was done with equal inclusion probabilities and without replacement.

effort and catch are correlated this will also yield smaller variances for catch. For this reason, the agency discards equal probability sampling in favor of unequal (nonuniform) probabilities.

Unequal Probability Sampling with Replacement. Unequal probability sampling is done by assigning selection probabilities proportional to effort (or other variable of interest). Because fishing is expected to be three times heavier in afternoons than in mornings in this example, the PM work shift is given three times as much chance of selection as the AM shift. Effort is expected to be twice as heavy in section 1 of the reservoir as elsewhere, and it is assigned twice as much chance of being chosen as the other sections. Probabilities are assigned to work shifts and reservoir sections as follows:

Shift selection	
Shift	Probability
AM	0.25
PM	0.75
	1.00

Section selection		
Section	Weighting	Probability
1	2	2/4 = 0.50
2	1	1/4 = 0.25
3	1	1/4 = 0.25
	4	1.00

All potential combinations of shifts and sections are listed and their inclusion probabilities are specified. The probabilities are accumulated, and these cumulative probabilities are used to specify the range of random numbers that result in the selection of each SSU.

SSU	Probability	Random number range
AM-section 1	(0.50 × 0.25) = 0.1250	0–124
PM-section 1	(0.50 × 0.75) = 0.3750	125–499
AM-section 2	(0.25 × 0.25) = 0.0625	500–561
PM-section 2	(0.25 × 0.75) = 0.1875	562–749
AM-section 3	(0.25 × 0.25) = 0.0625	750–811
PM-section 3	(0.25 × 0.75) = 0.1875	812–999
	1.0000	

Eight random numbers are chosen from the interval 0–999: 513 (AM-section 2), 122 (AM-section 1), 559 (AM-section 2), 897 (PM-section 3), 734 (PM-section 2), 725 (PM-section 2), 571 (PM-section 2), and 500 (AM-section 2). The completed schedule is shown in Figure 11.6.

In this draw, section 2 is chosen six times out of eight by random chance; section 1, with a selection probability of 50%, represents only 13% of the draw. This is an unusual sample, but a statistically valid one. The AM-section 3 combination, with a low selection probability (6%), was not drawn in this sample. Hence, the agency would have to infer fishery-wide catch rates without ever having sampled this unit. With nonuniform probability sampling, estimates for SSUs are expanded up to PSU estimates, and usually it is only a minor problem if certain SSUs are not sampled. Nevertheless, random sampling with replacement works best when the ratio of primary to secondary sampling units is high, giving a high likelihood that all SSUs will be represented during a survey.

JUNE

Stratum 1					Stratum 2	
M	T	W	T	F	S	S
1	2* AM Sec.2	3* AM Sec.1	4	5	6	7* AM Sec.2
8	9	10	11	12	13* PM Sec.3	14
15* PM Sec.2	16	17* PM Sec.2	18	19	20* PM Sec.2	21
22	23	24	25	26	27* AM Sec.2	28
29		30				

Figure 11.6 Stratified sampling design with secondary sampling units (SSUs)—work shift and reservoir section—added to Figure 11.3. The sampling of SSUs shown here was done with unequal inclusion probabilities and with replacement.

Unequal Probability Sampling without Replacement. Again, sampling without replacement means that after a unit is selected, it is not returned to the sampling pool and can not be selected again until all units have been chosen. Unequal probability sampling without replacement is useful when spatial differences or strong seasonal trends in fishing are likely. It has the advantage that all units are sampled more evenly throughout the survey. However, sampling without replacement in this context can bring statistical complexities (Cochran 1977:258) and it is best done with the assistance of a statistician.

Because it wants some data from all SSUs and has statistical expertise on staff, the agency chooses unequal probability sampling without replacement for its reservoir survey.

11.3 OBTAINING SURVEY DATA

11.3.1 Effort

In roving surveys, counts of anglers to estimate effort usually are separated conceptually and often actually from the interviews that yield data on catch and angler attributes. In many programs, the survey team randomly schedules counting events, and interviews are conducted in those times between counts.

In the roving creel design, effort data cannot be obtained directly from angler interviews as in the access point design, because roving clerks intercept anglers before they complete their fishing trips. Anglers sometimes are asked when they plan to stop fishing, but their estimates are unreliable; many factors such as weather and fishing success can influence trip duration. Furthermore, anglers fishing longer are disproportionately sampled. A different approach to effort estimation is necessary.

Effort during a roving survey can be measured in two ways (Figure 11.7): with

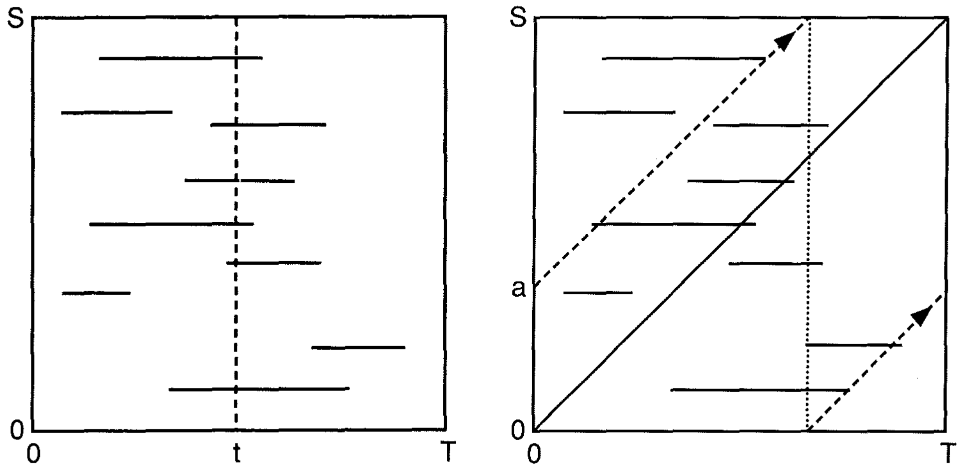


Figure 11.7 Representation of instantaneous (left) and progressive (right) angler counts in time (T) and space (S). Nine angler trips are shown as horizontal lines. An instantaneous count at time t would include six anglers. A progressive count starting at time 0 and location 0 and ending at location S (solid diagonal) would include two anglers. An alternative progressive count beginning at location a at the same time (dashed lines) would include four anglers. Locations 0 and S are the same if the count progresses along a circuit of the fishery, as it might on a lake. (Reproduced from Hoenig et al. 1993.)

an *instantaneous* or a *progressive* count (Neuhold and Lu 1957; Sigler and Sigler 1990; Hoenig et al. 1993). The instantaneous count is made quickly from an airplane, from a vantage point such as a bridge, dam, hilltop, or counting tower, or from a fast-moving boat or automobile such that there is little or no change in the position and numbers of anglers during the count. Not all sampling areas can be viewed from vantage points or traversed quickly, and a count that takes place over an extended period is termed a progressive count. The time division between instantaneous and progressive counts is not absolute. A count that requires 15 minutes or less can safely be considered instantaneous; one that takes an hour or more is undoubtedly progressive. What counts taking 15–60 minutes should be called is a matter of judgment. When the fishery covers a large area and the count takes substantial time to complete, the count can be called progressive. During progressive counts, there is change in both angler numbers and location. In the progressive count, nevertheless, each small area is viewed instantaneously (angler number does not change) even though the clerk may take several hours to cover the entire sampling area.

Averaged instantaneous counts and single and averaged progressive counts do not measure angler numbers directly (Neuhold and Lu 1957). A single instantaneous count gives the number of anglers in the fishery at a given moment, but an angler can move during a single extended progressive count and be counted twice, and multiple counts of either type in the same day can enumerate the same angler several times. Therefore, all counts—instantaneous or progressive, single or averaged, are multiplied by the number of hours in the fishing day to estimate the angler-hours of fishing effort that day. To estimate the number of anglers, the estimate of angler-hours is divided by the mean trip length. However, an unbiased estimate of mean trip length cannot be obtained from a roving survey (Wade et al. 1991) and must be achieved by other means.

11.3.1.1 Instantaneous Counts

The more instantaneous counts that are taken each day, the more accurate will be the overall estimate of fishing effort (Neuhold and Lu 1957; see also Malvestuto 1983). However, the time spent counting is time not spent interviewing, and this trade-off must be evaluated for each survey. Lester et al. (1991) provided a detailed analysis of sample size and precision for roving surveys.

Instantaneous counts are unbiased if no anglers are missed due to visibility problems and if anglers can be distinguished from nonanglers. Vegetation can easily obscure shoreline anglers and even boats, and survey teams must determine the magnitude of this problem. If anglers are missed because they are not visible or because they look like nonanglers, fishing effort will be underestimated. If nonanglers are mistakenly counted as anglers, effort will be overestimated. Not only must the clerk properly identify anglers, she or he also must decide which activities constitute fishing. Phippen and Bergersen (1991) concluded that the most liberal definition of angling (which included changing tackle and moving to sites as well as actual time fishing) gave the least bias in their fishery, but the most appropriate definition should be established independently for each fishery.

11.3.1.2 Progressive Counts

Progressive angler counts can last up to an entire survey day (see Amesbury et al. 1991 for an example). Often only one progressive count is made per day because of the time required to traverse the fishery, but that means that within-day variability cannot be estimated, only between-day variability. To obtain within-day variability, the survey team must schedule two or more progressive counts each survey day. This usually requires that clerks be given smaller areas to survey, however. The consequences are either a dilution of spatial sampling effort for the same budget, which has (other) statistical implications, or an increase in the number of clerks for the same spatial coverage, which has budgetary implications. The appropriate trade-off must be determined (preferably in advance) for each survey.

If the progressive count is made without interviews and if the starting point and direction of travel are randomized, the count is unbiased just like an instantaneous count (Neuhold and Lu 1957). However, counts often are conducted while anglers are being interviewed (von Geldern and Tomlinson 1973), a process termed "count-while-interviewing," and a biased estimate of fishing effort can result from this practice (Robson 1961). While the clerk is interviewing an angler, he or she is unavailable to count or interview another angler elsewhere (Robson 1991; Wade et al. 1991). The interview, in essence, casts a "shadow" that decreases the probability of counting or interviewing parties that would have been counted with a truly instantaneous count. Wade et al. (1991) showed that this results in a potentially severe underestimate of fishing effort, the magnitude of which depends on the length of the interview time and on the number of anglers in the fishery (which is almost never known when shadowing occurs). The bias exists even when the interview length is as short as 5 minutes.

The "count-while-interviewing" progressive count can be made virtually unbiased with only a small change in procedure that establishes checkpoints along the route and forces the clerk to follow a time schedule (Wade et al. 1991). Without checkpoints, the usual practice is to maximize the number of interviews, slow down movement around the water body during heavy interviewing times, and speed up when few anglers are encountered. This approach produces a maximum of probability shadowing and usually an undercount of anglers. If the clerk is kept on schedule with checkpoints, she or he is forced to skip some interviews and to do only counting at intervals throughout the work period, rather than just at the end, and the estimate of effort will be nearly unbiased. Several checkpoints should be chosen per work period. To schedule themselves correctly, clerks must know their fisheries well and plan ahead to establish landmarks.

11.3.1.3 Scheduling Counts

One is commonly advised to randomly select starting times for counts (Sigler and Sigler 1990). This approach is correct for instantaneous counts that can be made from a vantage point or from a boat that can circuit the fishery in, say, half an hour. Such counts are most often scheduled by simple or systematic random sampling, as demonstrated in Section 11.3.1.4. The choice of sampling method depends both on the duration of the work shift and on the variability of effort within the day. Simple random sampling is useful when the work periods are 3 hours or less, because effort is fairly homogeneous over short periods. When work periods are long and effort varies greatly throughout the day, systematic sampling may yield more reliable estimates.

When the count is progressive, lasting an hour or more, the best practice is to divide the day into nonoverlapping segments equal to the duration of an actual count, starting at the beginning of the fishing day (Hoenig et al. 1993). One or more segments is chosen randomly to produce the day's count schedule. In addition, the survey team should randomly select the starting location and the travel direction (clockwise or counterclockwise, upstream or downstream, etc.). Procedures for scheduling aerial counts are discussed in Chapter 12.

11.3.1.4 Examples: Scheduling Angler Counts for Effort

Various methods for scheduling instantaneous and progressive counts are described in this section.

Instantaneous Count: Simple Random Sampling without Replacement. Three instantaneous counts are to be made during a 7.5-hour workday beginning at 5:00 AM. The workday is 450 minutes long, so three random numbers between 1 and 450 are drawn without replacement:

Random number	122	259	358
Time into the survey	2 h, 2 min	4 h, 19 min	5 h, 58 min
Start time	7:02 AM	9:19 AM	10:58 AM

Even though schedules usually are not strictly adhered to, the counts should begin as close to these times as possible.

Instantaneous Count: Systematic Random Sampling. Simple random sampling of starting times can result, by chance, in counts that are clustered closely together in time or even overlapping. Systematic random sampling of start times avoids this problem and provides more uniform coverage of a workday; it is often preferred for this reason. The only price to be paid for choosing a systematic sample is more complexity of the variance formulas.

In systematic random sampling, the working day is divided into periods that equal the number of counts to be taken. A random time is chosen within the first period of k minutes, followed by counts every k minutes later. If three counts will be taken during a 450-minute sampling day, 450 minutes divided by 3 gives three periods of length $k = 150$ minutes. A random number on the range 1–150 is chosen, t_1. The sequence of three starting times is t_1, $t_2 = k + t_1$, and $t_3 = k + t_2$. Say the random number drawn for t_1 is 76; the subsequent start times will be 76 + 150 = 226 and 226 + 150 = 376. For a survey beginning at 5:00 AM, then:

Starting minute	76	226	376
Time into the survey	1 h, 16 min	3 h, 46 min	6 h, 16 min
Start time	6:16 AM	8:46 AM	11:16 AM

Progressive Count: Simple Random Sampling without Replacement. Because progressive counts take substantially longer to complete than instantaneous counts, the method of selecting counting times differs from those given above. The workday is divided into sequential blocks of time equal to the duration of the progressive count. These blocks are numbered and chosen by drawing a random number. Suppose a progressive count takes 45 minutes. The working day of 450 minutes (7.5 hours) consists of ten 45-minute blocks of time:

Period	Time of day	Period	Time of day
1	05:00–05:44	6	08:45–09:29
2	05:45–06:29	7	09:30–10:14
3	06:30–07:14	8	10:15–10:59
4	07:15–07:59	9	11:00–11:44
5	08:00–08:44	10	11:45–12:29

Three counts are to be done per survey day, so three random numbers in the interval 0–9 are drawn without replacement: 9, 1, and 4. The following schedule results:

Random number	1	4	9
Counting period	5:00–5:44 AM	7:15–7:59 AM	11:00–11:44 AM

This random draw happened to space the counts fairly evenly through the workday. Simple random sampling will cover all parts of the workday over the course of an extended survey, on average, but it cannot assure uniform coverage on a particular day. If a more uniform coverage is desired for any or all survey days, a systematic sample of time blocks can be chosen instead by the method shown previously for instantaneous counts.

11.3.2 Catch, Harvest, and Their Rates

Harvest (and catch), along with fishing effort, is commonly estimated from roving creel surveys. Roving clerks obtain harvest and catch information by interviewing anglers who are still fishing. The two key assumptions underlying harvest and catch rate estimation from incomplete fishing trips are (1) that the catch rate (fish/hour) at time of an interview will equal the rate for entire trip (Malvestuto 1983; Phippen and Bergersen 1991), and (2) that the catch rate of the interviewed anglers is equal to that of noninterviewed anglers. These assumptions are important because total catch is calculated by multiplying the catch rate (fish/hour) by the estimate of angler-hours, which is obtained from instantaneous or progressive counts. The accuracy of catch estimations depends on the bias of both the catch rate estimator and the calculated effort. Some investigators have found that catch rates are similar for completed and uncompleted fishing (Carlander et al. 1958; von Geldern 1972; Malvestuto et al. 1978), but others have found that catch and harvest rates changed toward the end of fishing (MacKenzie 1991). Therefore, it is prudent to check that the catch rate estimation is similar for complete and incomplete fishing before total catch is calculated (Malvestuto 1983). Some random interviews to check this point can be obtained at a defined access site, if any exist, or from follow-up surveys. The importance of the second assumption, that interviewed and noninterviewed anglers' catch rates are similar, is discussed in Section 11.4.

The correct procedure for calculating mean catch rate from incomplete trips is a matter of debate (Van Den Avyle 1986; Crone and Malvestuto 1991). The two common estimators involve ratios of catch (C, c_i) to fishing duration (effort: E, e_i); R_1 is the mean of the ratios and R_2 is the ratio of the means:

$$\hat{R}_1 = \frac{\sum_{i=1}^{n} \frac{c_i}{e_i}}{n}; \quad \hat{R}_2 = \frac{\dfrac{\left(\sum_{i=1}^{n} c_i\right)}{n}}{\dfrac{\left(\sum_{i=1}^{n} e_i\right)}{n}};$$

i denotes an angler, and n is the number of anglers interviewed (in the expression for $\hat{R}_2$, the n's cancel out, simplifying the formula). The ratio of means, R_2, is advantageous because its variance, which is always a finite number, is the more stable. The choice of which estimator to use in a creel survey, however, depends on whether or not the probability of sampling anglers is independent of trip length; R_2 is the correct estimator for calculating catch rate when trip duration does not affect an angler's probability of being selected for an interview. Access and off-site survey methods, which deal with completed trips, all meet this criterion. The roving method does not meet this criterion because the probability of selecting an angler to interview is proportional to the length of that angler's fishing trip. Preliminary studies indicate that the mean of ratios, R_1, correctly estimates catch rates in this case (D. S. Robson, unpublished). However, this is an area where further research is needed. These two estimators, R_1 and R_2, can give very different numbers, and when used in expansions, they can give large differences in calculated total catch. In fisheries that are under strict regulation or in decline, miscalculations can negatively impact the stocks or the anglers. The procedures for calculating catch and harvest are given in Chapter 15.

11.3.3 Other Data

Like access surveys, roving creel surveys can be used to gather information besides catch and effort. Interviews can elicit data on angler attitudes, economic expenditures, demographics, and other topics that can help managers understand their angling clientele and assess or improve their fishery programs. Corollary information on fishing conditions (e.g., weather, temperature) and habitat variables (e.g., streamflow, turbidity) recorded by roving clerks can help managers interpret the variances in survey estimates (Malvestuto et al. 1979).

11.4 LENGTH-OF-STAY BIAS

Length-of-stay bias is intrinsic to the roving creel method, because the probability of intercepting an angler fishing is proportional to the length of the angler's fishing trip (Lucas 1963). Roving clerks interview disproportionately more anglers who have been fishing longer than average; thus the overall mean trip length estimated from incomplete trips (double the overall average fishing time elapsed by the time of interview) is longer than the overall mean trip length determined from completed trips in the same fishery. This is not a problem if anglers fishing for short and long periods have the same characteristics. However, such equality is difficult to determine, and it is easy to imagine situations in which the catch rates differ for trips of different length. For example, successful anglers may linger as long as they can, releasing fish in excess of a daily limit, whereas unsuccessful anglers might quit early in frustration. If anglers fishing longer are predominantly the successful ones, they will be interviewed in greater proportion, and the estimated mean catch rates for the fishery will be biased high. Conversely, if successful anglers have short trips because they quit when they reach a bag limit, the clerk will not encounter many of them and the estimated catch rate will underestimate the true overall catch rate. In each of these instances, the estimate of total catch will also be incorrect. As yet we know of no way to correct these biases. The amount of bias depends on the strength of the relationship between the catch rate and the trip duration.

11.5 QUALITY CONTROL

Quality assurance in roving surveys begins with careful planning and development of the survey and questionnaire designs. When possible, the survey routes and questionnaire should be tested in the field beforehand. This pretesting will ensure that the questionnaire is clear and the water body can be traversed on schedule.

Some quality assurance issues are specific to on-site surveys and are harder to check in the roving method. Specific concerns are that the clerk is present on site on the proper day and time; that counts and interviews are done correctly; that species identifications are correct; and that biological data are collected properly. Roving clerks are more difficult to find for spot checks than access site clerks. When the survey is conducted by boat, the supervisor must either accompany the clerk throughout the survey or follow in a separate boat, both of which are expensive and impractical. Therefore, it is easier to check the clerk's mastery of procedures in a "dry run."

Proper training and good management practices help assure good quality work by the clerks. Clerks who understand the principles of good survey design will be more likely to adhere to scheduling and randomization procedures than those who do not. When the survey designers conduct a thorough training program, they demonstrate to the clerks that the program has their support and is important.

Such a training program should (1) clearly state the objectives of the survey, (2) present particular concerns for the fishery, (3) take the clerks through their survey areas, (4) review species identifications and interviewing procedures, (5) present the survey design, (6) give clerks thorough briefings on their schedules, and (7) review the quality assurance procedures.

11.6 PROCEDURES ON SITE

Procedures on site must be planned before the survey begins. Clerks should be given their schedules and circuit maps ahead of time so they can become familiar with them. Prior to the start of the survey, vehicles and boats, if used, should be serviced.

Even when counts of anglers are scheduled, roving clerks begin most survey days by interviewing anglers. The survey form used in the interviews may be similar to the ones shown in Appendices 11.1 and 11.2 (see appendices to Chapter 10 for other questionnaires). During the interview, the clerk asks when the angler began the trip, solicits the species and numbers of fish that have been caught, released, and kept, and examines the harvest if possible. Other questions about angler attitudes, economic expenditures, and demographics (among others) may be asked if they serve the objectives of the survey. The interview length depends on the number of questions asked. When interviews are short, more anglers will be contacted and those contacted will be less inconvenienced. If the survey includes boat anglers, the procedures for interviewing parties presented in section 10.6 should be followed. An example of a daily summary of interviews is shown in Figure 11.8.

The creel clerk may encounter too many anglers at a location to interview them all. Because interviews are used to estimate catch rates, not effort, the clerk only needs to pick anglers randomly. Commonly the clerk systematically subsamples every *k*th angler (every second or third angler, or whatever interval is demanded by the number of people present). However, it is also valid to just pick the next nearest angler as long as all anglers are equally available (Rubin and Robson 1990)—that is, as long as no angler seems more eager than others to encourage or avoid an interview.

Multiple intercepts of the same angler on the same day are more likely to occur in roving than in access surveys, especially if the clerk does more than one circuit of the fishery each day. Anglers may feel pestered by these interruptions and refuse a second interview. If it is necessary to interview the angler again, the clerk should quickly ask the angler if any new fish were caught or released and proceed along. The clerk can fill in the rest of the information from the first interview and indicate that this is a repeat contact. However, if little can be gained from repeated interviews, skipping them may improve public relations.

Avid anglers may be interviewed repeatedly over a fishing season and come to resent these interruptions. Again, this can lead to an increase in refusals. The best procedure is for the clerk to acknowledge that the angler has been interviewed previously, explain the importance of the new day's information, interview the angler as quickly as possible, and move on. The need for information must be balanced against the right of anglers to enjoy their recreational activity with minimum disturbance.

Interviewer Bruce Smith Section 2 Month June Day 3 Year 1992								
Angler Number	Time of Interview	Time of Trip Start	Species Sought (code)	Species Caught (code)	Number Kept	Number Released	Length or Weight	State of Residence
1	12:34	10:15	33	0	0	0	---	xx
2	12:47	10:00	33	33	2	1	xxx	xx
							xxx	xx
3	13:09	11:45	33	33	0	1	---	xx
4	13:20	13:00	99	0	0	0	---	xx
Instantaneous count taken between 13:30 and 13:45								
5	14:01	12:15	33	33	1	1	xxx	xx
6	14:23	12:00	99	0	0	0	___	xx
7	14:35	12:30	33	0	0	0	---	xx
...								
22	16:47	16:00	33	0	0	0	---	xx
Summary Total anglers 22 Species 33: Total directed angler-hours = 31 h 5 min Total kept= 12; Total release= 14; Total caught= 26 Other species: Total directed angler-hours = 9 h 42 min Total kept= 3; Total release= 5; Total caught= 8								

Figure 11.8 Example of a form summarizing a full day's roving interviews.

In many roving surveys, interviewing is interrupted from time to time by preplanned angler counts. Just before a scheduled instantaneous count, the clerk completes any ongoing interview and proceeds to the specified location where the count will begin or take place. If the count cannot be made from one or a few vantage points, the clerk must perform a complete traverse of the assigned fishery area—by boat or automobile or on foot—starting in a randomly chosen direction (clockwise or counterclockwise, upstream or downstream, east or west, depending in the area's geography). During the traverse, the clerk does not interview anglers but only records the number (and sometimes the location) of anglers along the circuit. Figure 11.9 shows one type of form for recording counts. The count is sometimes recorded on the same form as the interview (see Appendix 11.2). When the count is finished, the clerk returns to interviewing anglers.

In the count-while-interviewing method, the two functions occur simultaneously. The clerk both records interviews and keeps a count of the anglers encountered during each circuit of the fishery. A modification of this method that we strongly encourage is to establish checkpoints and schedules that will ensure good angler counts for effort estimation. The checkpoints are visual reference points around or along the fishery that the clerk must reach by predesignated times. If a checkpoint will not otherwise be reached on schedule, the clerk suspends interviewing and proceeds rapidly along the circuit to the checkpoint, conducting an instantaneous count of anglers en route. Interviewing resumes when the checkpoint is reached.

Count Form - Upper Blue River Drainage Survey

Date______(m/d/y) Sampling Period_______ Weather______
Sheet Number_________
River Segment________
Beginning Time of Count_________(use military time--e.g., 14:10)

Activity code : 1-actively fishing
2-changing tackle
3-fishing, changing location
4-unknown, unable to determine
5-non angler

Number	Time	Group Size	Location	Shore=S or Boat=B	Activity Observed	Comments
1						
2						
3						
4						
5						
6						
7						
8						
9						
10						
11						
12						
13						
14						
15						
16						
17						
18						
19						
20						

Figure 11.9 Example of a survey instrument used to record effort from instantaneous and progressive counts during roving surveys of low-use fisheries.

11.7 PRACTICAL CONSIDERATIONS

The survey team must know the topography of the fishery in considerable detail before it designs a roving survey—in greater detail than is needed for an access survey. Often the team should see the area firsthand rather than relying on a map. The team must know the locations of public access and walk-on areas, because these are likely spots to encounter anglers, and of potential vantage points, because these may influence the type of angler count selected. They also must be familiar with the fishery's physical features. Water depths, bank structure, tides, bars, currents, and other factors can affect a creel clerk's progress by boat; tree blowdowns, ravines, and swamps can impede progress by foot. With knowledge of typical angler locations and use patterns and of local geography, the survey team can stratify the area and design circuits that can be completed within the allotted time thus optimizing survey efficiency.

Safety is an even greater concern with roving surveys than it is with access surveys. Roving clerks, whether boating or walking, are vulnerable to a variety of accidents and to rapid changes in weather. They must be trained to anticipate problems—likely storms, water releases from reservoirs, shifts in tide, abusive anglers—and trained and equipped to deal with problems that arise. Clerks who survey from boats need particular training in boat handling and water safety—a requirement often mandated by law as well as by common sense. Night surveys by boat can be very hazardous; although some successful night surveys have been conducted by well-trained people operating under favorable fishery conditions, we do not recommend their use.

Equipment maintenance is a particular consideration for boat-based surveys. Boats and trailers must be kept in good repair and meet federal, state, or provincial specifications.

Clerks operating in boats may be limited in the amount of information they can obtain. Measuring or weighing fish can be difficult or impossible in rough water or when the survey and fishing boats differ greatly in size. A clerk who damages a fish or drops one overboard during transfer is likely to lose an angler's cooperation.

Roving clerks have to carry all survey equipment with them (Table 11.1), so it should be lightweight and kept to a minimum—especially for foot surveys. Although backup equipment usually is recommended for fieldwork, much less of it can be carried for roving than for access surveys. As much of the equipment as possible should be waterproof, including the paper used for survey forms.

The scheduling checkpoints needed for modified count-while-interviewing surveys further complicate an already exacting design. When the design further depends on randomly chosen start locations—which are essential in a statistically sound design—new checkpoints must be established each survey day. Clerks must be very familiar with physical surroundings and travel timing to establish checkpoints properly. Hence, this design may be most successful with seasoned clerks who are thoroughly familiar with the fishery.

11.8 STRENGTHS AND WEAKNESSES

The important features of roving surveys are that they are conducted on site, they are not limited by the type of angler access, anglers are actively sought out for interviews, and anglers are interviewed before they complete their trips. The principal advantages of the method lie in the first three features; the principal disadvantages lie in incomplete trip data and in survey costs.

Roving surveys share with access surveys several advantages over off-site

Table 11.1 Field supply checklist for roving survey clerks. Equipment should be lightweight and portable, especially if the clerk is on foot.

Necessary for all surveys
Clipboard (with cover for inclement weather)
Survey forms (preferably on waterproof paper)
Pens, pencils
Watch (timer is also handy)
Mechanical counter
Survey identification badge (or sign for a boat)
First aid kit
Rain gear and seasonal clothing
Food and drinking water
Necessary for boat surveys
Boat and motor[a]
Life jackets
Boat bumpers
Trailer
Necessary for biological measurements
Measuring board or tape
Hand-held weighing scale
Fish scale envelopes
Necessary for physicochemical measurements
Thermometer
Dissolved oxygen meter
Ph meter
Current meter (river or stream surveys)
Secchi disk or turbidity meter
Water sampler and jars
Optional (chiefly for new clerks)
Handbook of procedures
Fish identification handbook
Optional (depending on agency regulations)
Clerk uniform (patch, hat, identification card, etc.)
Agency mileage log forms, gas credit card, etc.
Optional (boat surveys)
Incentive gifts for angler cooperation (pencils, hats, lures, etc.)
Dip net, buckets

[a]A trolling motor is desirable for operating near anglers. A more powerful motor may be needed (in addition) for conducting rapid angler counts.

methods (see Section 10.8). Anglers are interviewed at the fishery, reducing recall (memory) bias and providing site-specific information. Harvested fish can be examined by trained clerks, increasing the accuracy of harvest data and reducing prestige bias. Samples for biological analysis can be taken and illegal harvests can be monitored. Ancillary data on weather and water conditions can be recorded for later correlation with fishing trends or variations revealed by the survey.

Roving and access surveys also share disadvantages with respect to off-site methods. On-site survey designs can be complex. The socioeconomic data that can be obtained on site are necessarily sketchy: the complete cost of a fishing experience cannot be estimated until anglers reach home, and anglers fishing or leaving a fishery are unlikely to tolerate lengthy sociological or attitudinal surveys. (Names, addresses, and telephone numbers can be obtained on site for later off-site surveys, however.) Both on-site methods are subject to avidity bias: anglers who fish more frequently have a higher probability of being contacted (greater experience may bring greater fishing success). Both methods are more costly than off-site surveys (except door-to-door efforts); staff need special training, quality control checks must be done in the field, field equipment must be purchased and maintained, and the number of interviews obtained per unit staff time is relatively low. Surveys conducted in the field are inherently less safe than surveys conducted from an office.

Between the on-site angler contact methods, roving surveys have some important advantages over access surveys. They can be used when access to a fishery is very abundant or ill-defined, or when access is from private property unavailable to creel clerks. They typically produce more interviews per unit staff time, because roving clerks seek out anglers—a distinct benefit in low-use fisheries. And they produce more precise estimates of fishing effort when complete instantaneous counts of anglers can be made from a vantage point.

Roving survey designs often are more complex than access designs, however, partly because clerk mobility introduces another variable to be controlled and partly because effort and catch data must be obtained by different methods. Effort must be estimated independently from angler interviews because interviews are conducted before fishing effort has been completed. Incomplete trip data are subject to length-of-stay bias: anglers who fish longer are more likely to be contacted by roving methods and they may differ in important respects from anglers who fish for shorter periods. Estimates of catch rate obtained from midtrip interviews require the assumption that the rates do not change after the interviews. Estimates of effort from angler counts require the assumptions that all anglers have been seen during counts and that anglers have not been confused with nonanglers; violations of these assumptions can bias estimates of both effort and (via catch rate) catch. Roving clerks are exposed to more hazards than stationary access clerks, and they may be less able to examine or sample harvested fish. When boats are used, roving surveys are among the most expensive to implement.

This catalog of problems notwithstanding, the roving creel survey is the method of choice when on-site angler data are needed and fishing access is diffuse or inaccessible to stationary clerks.

Appendix 11.1 Scripted Questionnaire for a Roving Survey
(Modified from Hudgins and Malvestuto 1982.)

Sample Number_______ Date______ Clerk_____________ Reservoir _______
Lake section___________ Number in party__________ Sex: M__ F__
Fishing from: Shore____ John boat_____ Bass boat____ Other____
Age: <16___ 16-20___ 21-30___ 31-40___ 41-50___ 51-65___ >65___

Fishing location: Open water _____ Shoreline _____ Tree shelter _____ Riprap _____
Fishing pier _____ Under bridge _____

Interview time____________

Good morning/afternoon. I'm doing a survey for [name of organization] and I'd like to ask you some questions about your fishing trip.

Q1. What is your county and state of residence?
County_____________ State______________

Q2. When did you start your fishing trip today? Hour (24 clock)_____ Minutes____

Q3. When do you expect to finish your fishing trip today? Hour (24 clock)_____ Minutes____

Q4. What species are you primarily fishing for?
Hybrid striped bass____ Largemouth bass____
Smallmouth bass____ Bluegill____ Other________(specify)

Q5. How many times do you fish this lake per year?________

Q6. How many times do you fish here and other places per year?_______

Now I'd like for you to respond to the next 4 questions on this scale (show scale) of poor, fair, good, and excellent and briefly tell me why you answered the way you did.

Q7. How do you rank the maintenance of public facilities on the lake?______
Why?____

Q8. How would you rank the natural beauty of the lake?_________
Why?____

Q9. How would you rank your fishing success today?_________
Why?____

Q10. How would you rank your total trip quality today?________
Why?____

We would like to know your feelings about some of the management practices on this lake.

Q11. Do you feel there is any need to change the 12" length limit on largemouth bass? Yes___ No___
Why?____

Appendix 11.1: Continued

Q12. Are you aware that [agency name] has stocked hybrid striped bass in this lake? Yes___ No___
(If yes continue)

Q13. Do you feel that stocking this fish has changed the quality of fishing in this lake? Yes___ No___
How____

Q14. Do you feel that a largemouth bass and a hybrid striped bass are of equal quality as a sportfish?
Yes___ No___
Why?____

If you caught any fish today I would like to measure the catch.

Species	Kept	Released		1	2	3	4	5	6	7	8	Total
			len									
			wgt									
			len									
			wgt									
			len									
			wgt									
			len									
			wgt									
			len									
			wgt									

Time interview ended: Hour (24 clock)______ Minutes______

Comments:

Appendix 11.2 Data Sheet for Angler Interview and Count Data

(Courtesy Missouri Department of Conservation.)

MISSOURI ANGLER CONTACT SHEET

Week[3a] ________(2) I.D. number[3b] ________ (3) Double Shift[3c] ____(1) 4/92 CC

Census Days[1]
Sun-1 Mon-2 Tue-3 Wed-4 Thur-5 Fri-6 Sat-7 Major-8
(circle) (1)

Survey Type[2] (circle)
Reservoir-1 Stream-2 Public Use-3 Reed/Busch-4
Roving-1 Probability-2 Other-3

Page ________ of ________ [4]
Lake/Stream[5] ________(2)
Area[6] ________ (2)
Site[7] ________ (2)
Year[8] ________ (2)
Date[9,10] ________ ________ (4)
month day
Survey Period[11] ________ (1)
Clerk[12]:

Secchi[13]: (inches)
________ (3)
Water Temp[14]: (°F)
________ (2)
Water Level[15]:
Fall-1 Rise-2 Stable-3
(circle) (1)

Weather[16]:
Clear-1
Scatter-2
Broken-3
Overcast-4
Rain-5
Snow-6
Ice-7
(circle) (1)

Angler Counts	1	2	3	4
Time[17-20]: (4)				
Boat/Ang[21-24]: (3)				
Bank/Not Cont.[25-28]: (3)				
Pl Boat[29-32]: (3)				

CODING AIDS

*Measurements of all fish are total length except for paddlefish which are body length measurement. Measure to tenth inch.

METHODS[44]		FISHING TYPE[42]	
Still	1	Boat	1
Cast	2	Bank/Dock	2
Troll	3	Heat Dock	3
Drift	4	Canoe	4
Set/Radio	5	Wade/Belly	5
Gig	6	Handicap	6
Trotline	7		
Jug	8		
Snag	9		
Bow	0		

LURE[43]
Art-1 Prepared-4
Fly-2 Comb-5
Nat-3

FISH SPECIES[45,46]

030 Rainbow trout	245 Flathead catfish	**407 BASS sp/pref**	**555 FISHING FOR ANYTHING**
031 TROUT sp/pref	263 Muskie	412 Green sunfish	
032 Brown trout	310 White bass	416 Redear sunfish	
039 BUFFALO sp/pref	311 Striped bass hybrid	**422 SUNFISH sp/pref**	
069 SUCKER sp/pref	313 Striped bass	424 Bluegill	
070 Carp	**314 White/Striped/hybrid**	426 Rock bass	
230 Channel catfish	318 Walleye	430 Black crappie	
232 Blue catfish	400 Spotted bass	431 White crappie	
233 CATFISH sp/pref	402 Smallmouth bass	**432 CRAPPIE sp/pref**	
242 BULLHEAD sp/pref	406 Largemouth bass	439 Hybrid sunfish	

BOAT TYPE[31]
1-Prop 2-Jet 3-Canoe

FISH CAUGHT CATEGORY[47]
L - Harvested *for slot*
R - Release B - Rel Below
S - Sublegal
X - Illegal length harvest
Z - Illegal number harvest

CONTACT INFORMATION													FISH CAUGHT				Stream Creel		Other Optional Questions[32]						Comments
Angler[33] (3)	Time Started[34] (4)	Time Contact[35] (4)	Hours[36] (4) xx.xx	Zipcode[37] (5)	Dest[38] (1)	Satisfy[39] (2)	Trip[40] (1) I - 1 C - 2	Race[41] (1) W - 1 B - 2 O - 3	Type[42] (1) see list	Lure[43] (1) see list	Method[44] (1) see list	Angler Pref[45] (3) see list	Spec[46] (3) see list	Cat.[47] (1) see list	No.[48] (3)	Lt.[49] (3) tenth inch · xx.x	Loc Cat[30] (1) see guide	Boat Type[31] (1) see list	Opt 1 (3)	Opt 2 (3)	Opt 3 (3)	Opt 4 (3)	Opt 5 (3)	Opt 6 (3)	

Chapter 12

Aerial Surveys

12.1 INTRODUCTION

Aerial surveys are unique among angler survey methods in that they can only be used to estimate fishing effort. We consider aerial surveys to be an on-site method because angling parties are counted in the midst of fishing; however, there is no personal contact with the parties.

Aerial surveys are particularly appropriate for counting large numbers of anglers over large areas. A small fixed-wing airplane (Figure 12.1) can cover over 800 km in 4 hours. Dozens of small lakes or several major estuaries can be visited within a day. The only personnel needed are a pilot and one or two observers. Although an airplane is costly to operate, it provides a very cost-effective way to sample in relation to the area it covers.

During aerial surveys of streams or shores where anglers fish on foot, agents count individual anglers. For boat fisheries, aerial agents may instead count fishing parties (boats), because it is often difficult to accurately count people on a boat.

The principle of an aerial survey is that a plane flies over a portion of the fishery's area and observers make instantaneous counts of anglers or boats within successive portions of the area swept; the overall survey count is progressive. If the area swept was chosen randomly from the set of all areas, the ratio of anglers or boats counted to all those present during the survey can be estimated as the ratio of area swept to the entire fishery area, which can be measured on maps.

12.2 SPATIOTEMPORAL FRAME

Aerial surveys, like other on-site surveys, have spatiotemporal sampling frames covering a geographic area of fishing and a defined part (perhaps all) of a fishing season. As with access and roving surveys (Sections 10.2, 11.2), sampling times usually are chosen first, then an area of the fishery.

12.2.1 Choosing Dates and Times

In common with other on-site methods, aerial sampling times must be selected with known probability, typically by two-stage sampling. First, the survey days (primary sampling units) are chosen from among all days in the fishing season (or defined part thereof) by either simple random or stratified random sampling. The fishing season usually is stratified by day type, weekday versus weekend day, because effort is often heavier on weekends. Within these strata, sampling days (primary sampling units) are chosen without replacement and with known probability. Once the survey days have been selected, the flight times (secondary

Figure 12.1 Photograph of a small fixed-wing aircraft used in an aerial survey of anglers. This airplane has its wing set above the fuselage, which permits relatively unobstructed views beneath the plane.

sampling units) are chosen. Selection of primary and secondary sampling units is demonstrated in Sections 3.7, 10.2, and 11.2.

The simplest way to choose a flight time (secondary unit) would be to select a starting moment with equal probability from all moments in the sampling day. This procedure, however, will result in unequal sampling probabilities (Hoenig et al. 1993). Suppose the fishing day extends from 6:00 AM to 6:00 PM and starting times for a 2-hour survey are chosen at random. A flight can start at 6:00 AM, 6:01 AM, 6:02 AM, 6:03 AM, et cetera. Moments 6:00, 6:01, 6:02, and 6:03 will be sampled by a flight beginning at 6:00 AM. A flight beginning at 6:01 AM will sample the moments 6:01, 6:02, and 6:03, but not 6:00 AM. A flight beginning at 6:02 AM will sample 6:02 and 6:03 AM but not 6:00 or 6:01 AM. With such a design, 6:00 AM will have one chance of being sampled, 6:01 AM two chances, 6:02 AM three chances, 6:03 AM four chances, and so on. At the end of the day, the opposite situation exists, because the flight must finish by 6:00 PM. Hence, moment 6:00 PM can be sampled only once, 5:59 PM twice, 5:58 PM thrice, and so forth. Thus, the chance of sampling any given moment is not uniform (Figure 12.2).

The correct way to obtain uniform probabilities of sampling time selection is to divide the day into units of time equivalent to the duration of one flight and to choose those time units at random. For a 2-hour survey, a 12-hour day could be divided into six 2-hour units: 6:00–7:59 AM, 8:00–9:59 AM, . . ., 4:00–5:59 PM. One (or more) of these time periods would be chosen at random, and the sampling probabilities for each moment of time would be equal. The day can also be divided

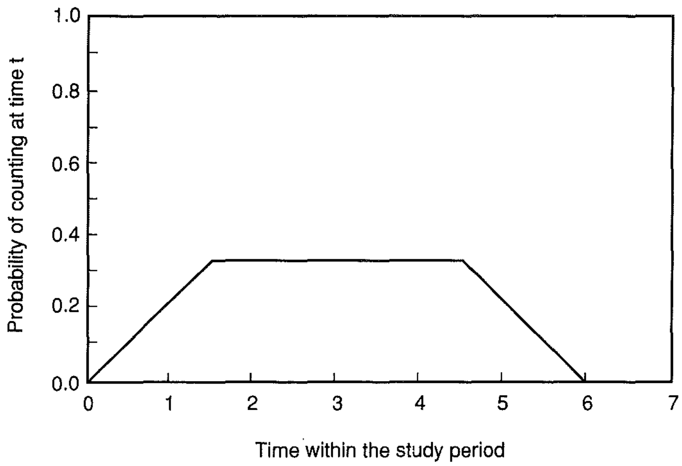


Figure 12.2 When the starting time for a flight is chosen randomly from all available moments, the beginning and ending moments of the day have less chance of being selected than those in the middle of the day. In this example, flight times are 1.5 hours long within a 6-hour sampling day. (Reproduced from Hoenig et al. 1993.)

into strata such as morning (6:00–7:59 AM, 8:00–9:59 AM), midday (10:00–11:59 AM, 12:00–1:59 PM), and afternoon (2:00–3:59 PM, 4:00–5:59 PM). These strata can then be sampled with equal or unequal (but known) probability; within a stratum, each moment will be sampled with known probability.

12.2.1.1 Sampling Flight Periods with Unequal Probability

Consider the schedule established in example 4 of Section 10.2.2.1. The month has been stratified into weekday and weekend strata, and eight sampling days have been chosen randomly without replacement for each stratum (Figure 12.3). Flight periods were chosen with unequal probability and with replacement. Anglers are found in largest numbers during the middle of the day; on this basis the probabilities of flight time selection were more heavily weighted toward midday as follows:

Flight period	Weighting	Unequal probability	Random number
6:00–7:59 AM	1	0.083	1
8:00–9:59 AM	1	0.083	2
10:00–11:59 AM	3	0.250	3–5
12:00–1:59 PM	3	0.250	6–8
2:00–3:59 PM	2	0.167	9–10
4:00–5:59 PM	2	0.167	11–12
	12	1.000	

From a random number table, the following 16 random numbers (one for each sampling day) were drawn from the range 1–12: 5 (10:00–11:59 AM), 1 (6:00–7:59 AM), 8, 4, 3, 10, 4, 9, 8, 10, 6, 3, 12, 6, 1, and 11. They were assigned to the primary sampling units in the sequence drawn (first, 10:00–11:59 AM; second, 6:00–7:59 AM; etc.); the completed sampling schedule is shown in Figure 12.5.

Stratum 1					Stratum 2	
M	T	W	T	F	S	S
1* 10:00-11:59 AM	2	3* 6:00-7:59 AM	4	5	6* 12:00-1:59 PM	7* 10:00-11:59 PM
8	9	10* 10:00-11:59 AM	11	12* 2:00-3:59 PM	13* 10:00-11:59 AM	14* 2:00-3:59 PM
15	16	17	18* 12:00-1:59 PM	19* 2:00-3:59 PM	20* 12:00-1:59 PM	21* 10:00-11:59 AM
22	23* 4:00-5:59 PM	24* 12:00-1:59 PM	25	26	27* 6:00-7:59 PM	28* 4:00-5:59 PM

Weekdays $N_1 = 20$
Weekdays $n_1 = 8$

Weekend days $N_2 = 8$
Weekend days $n_2 = 8$

Figure 12.3 In two-stage sampling, sampling days are chosen first. Days (primary sampling units) were stratified by day type (weekday versus weekend day) and selected without replacement from each stratum (asterisks). Then flight periods (secondary sampling units) were selected with unequal probabilities and with replacement.

12.2.2 Sampling in Space

After the sampling day and flight period have been chosen, the survey area is selected. Ideally, all aspects of spatial sampling are randomized. In practice, compromises with the ideal usually are made to minimize "wasted" flight time (time when no angler counts can be made) or to cope with locally unfavorable flight conditions (fog patches, strong air turbulence, etc.). The statistical implications of such compromises are poorly understood for aerial fisheries surveys, however, and research on these problems is needed. Beyond emphasizing once again that any deviation from probability sampling increases an estimator's risk of bias, we are unable to assess the seriousness of common deviations from ideal aerial sampling principles.

Fishing areas normally are divided into nonoverlapping spatial sampling units, or segments, that cover the entire fishery area; the segments are selected with known probability for sampling. The shape of segments depends on the fishery. Marine areas, estuaries, and large lakes can be divided into contiguous segments of known width (Figure 12.4); such segments are equivalent to traditional transects. Rivers and lakes narrow enough to be observed from bank to bank can be divided into longitudinal segments of varying width (Figure 12.5). In regional surveys of discrete and dispersed water bodies, segments are distances along the flight path. Sometimes segments are equal in length to the distance that can be covered during a standard flight, but more often they are shorter than that. A water body small enough to permit an instantaneous count of all anglers or boats with one flight pass does not have to be segmented.

Most aerial surveys yield progressive counts (as a sequence of instantaneous counts), so the direction of travel and starting point in the fishery should be randomized each sampling day, just as they are in roving surveys (Section 11.3.1.2). This randomization adds operational cost when it causes an airplane to start and end a survey at remote locations, but it insures that any given point in

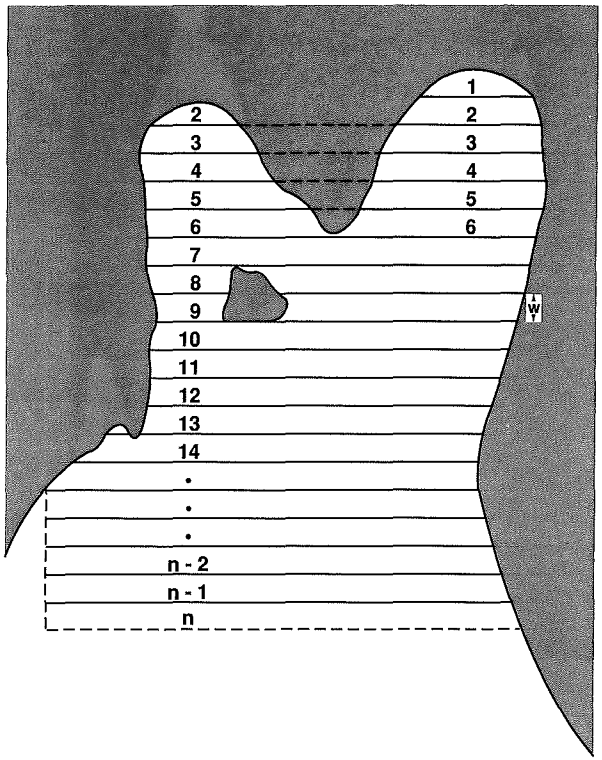


Figure 12.4 Partitioning of an estuary into a series of flight segments (transects).

the fishery is randomly sampled throughout the day. When starting location is not selected at random, estimation biases can result, especially when surveys last a substantial part of each day. For example, if long surveys always start at the same place, various areas of the fishery will always be sampled at one or two times of day, depending on the direction of flight. When the fishing area can be surveyed completely in a flight period, the segments can be linked together in a continuous route and the starting location—a segment or ground distance—and direction of travel can be randomly chosen each day. (If the route is "linear" along a beach, river, or series of open-water transects, the airplane usually will have to double back from one end of the fishery to the other during the route, and this period of waste time must be budgeted.) When the fishing area is too large to be surveyed within a flight period, the area should be partitioned into segments that can be completed in a period. For each sampling day, one of these segments would be chosen with known probability, with or without replacement, to start the survey, and the travel direction would also be chosen randomly. The same principle applies to the selection of a starting *point* along the route (Figure 12.6).

As Caughley (1977b) pointed out, the practical implementation of designated flight paths is subject to trade-offs between concerns over safety, fatigue (of observer as well as pilot), visibility, navigation, and waste time. Expensive flight time is wasted (in terms of counting) during travel between noncontiguous fishery units and during breaks that must be scheduled for the observer, who otherwise

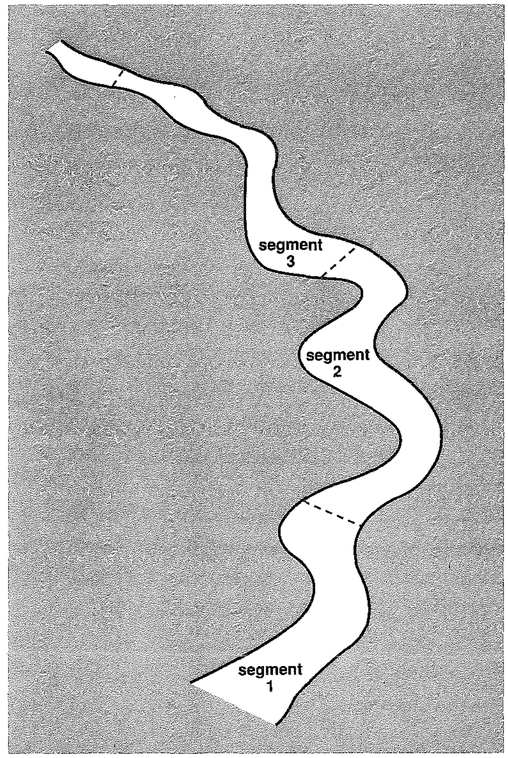


Figure 12.5 Partitioning a large river into a series of flight segments.

would lose concentration after a period of counting (Gunderson 1993). Budgetary pressures may tempt a survey administrator to compromise a proper survey design in order to maximize angler or boat counts per unit flight time. The temptation should be resisted. If funding is a problem, it is usually better to reduce the number or duration of flights than to abandon rigorous probability sampling designs. Other inadvertent or unavoidable problems can distort estimates of fishing effort (poor visibility, inaccurate identification of anglers, etc.), and there is no point in compounding them with bad survey design.

Sometimes it is necessary to truncate flight transects across open water before an opposite shore is reached. The ends of such transects often can be designated by the line of sight between two landmarks, but landmarks may be unavailable on very large lakes and estuaries. Modern satellite navigation systems remove this problem if the airplane is equipped with such a system. Indeed, aerial surveys of large water bodies could be conducted along flight paths designed only from random selection of navigational coordinates and compass bearings. This approach has not yet been used for fisheries surveys, to our knowledge.

12.2.3 Segment Width

On wide rivers, very large lakes, estuaries, and coastal waters, it may be necessary to establish segments (transects) of fixed width according to the formal procedures set forth by Caughley (1977a:34) and Gunderson (1993:186–187). In a

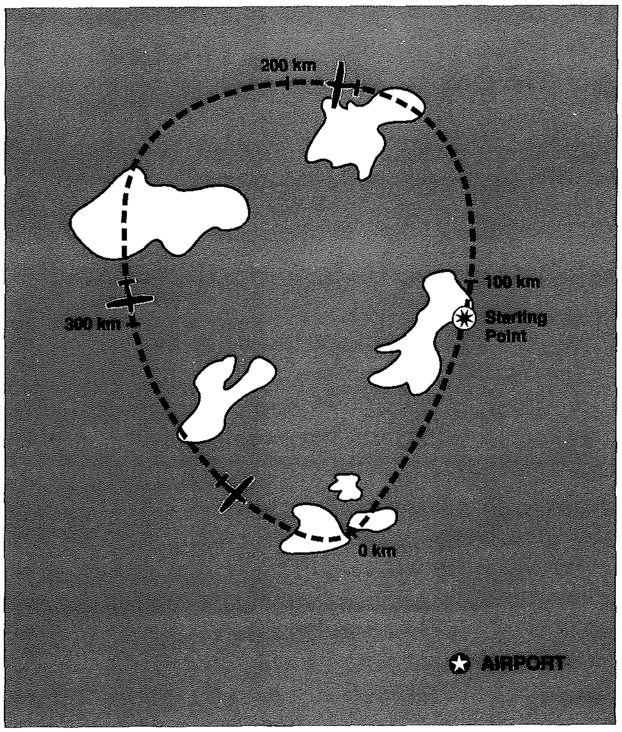


Figure 12.6 Illustration of a randomized starting location and flight direction in an aerial survey. A series of lakes is shown along a circular route of 400 kilometers. A starting point 90 kilometers from a reference location (km 0) and a counterclockwise flight direction (-) were randomly selected.

large and crowded fishery, use of a fixed transect width diminishes the possibility of miscounting boats. The width of the transect is determined by the altitude of the plane, visual obstructions (such as wheels), and window size, and it is demarcated by markers attached to the wing struts. Gunderson (1993) suggested that the calculated transect width be calibrated against a structure of known width on the ground.

12.2.4 Airplane Height and Angler Identification

Survey airplanes must fly low, 150 meters or less above the fishery, and slowly so commercial, pleasure, and recreational fishing boats or anglers can be distinguished (Malvestuto 1983). The kind of boat or person that will be counted should be decided before the flight. Generally, any person or boat showing some kind of fishing gear is counted, whether moving or stationary. However, criteria for inclusion can differ with survey objectives. If fishing effort determined from the air is to be combined with catch determined by another survey, the definition of an angler or angling party must be the same for both surveys.

12.3 EFFORT ESTIMATION

Aerial counts of boats or anglers are used only to estimate fishing effort. Because total flight times often are long (however long the actual counting period) and any flight is expensive, only one count typically is made each sampling day. The count is expanded from a period count to a daily count as shown in Sections 15.6.1 and 15.6.3. The physical locations of boats or anglers can be recorded directly on a map of the fishery. The resulting displays of fishing concentration are useful for establishing area weightings for future aerial or ground survey sampling, and they can be used to quantify sampling errors.

12.4 SAMPLING ERROR AND VISIBILITY BIAS

Incorrect counts of anglers and angling parties are major sources of error in aerial surveys. Some of these errors may be difficult to correct because they are caused by undocumented changes in the size of aerial sampling units or by cover that obscures anglers (Caughley 1977b). Changes in the airplane's altitude can alter the visual width of counting paths that are keyed to structural elements of the airplane such as marks on wing struts. Changes in altitude and ground speed of the airplane can affect the accuracy with which anglers or their boats are identified and counted. Observer fatigue can cause parties to be misidentified or overlooked. If the width of counting paths varies in an unknown way from expected widths and when angler identification is inconsistent, large errors can result when sample counts are expanded to the entire fishery. To some extent, these problems can be corrected if photographs or video recordings have been made along the flight path. These records allow corrections for altitude (from changes in size of boats or landmarks) and attitude (from variations in lateral horizons) and they allow identifications to be checked. Photographic and video records, however, can be unreliable under very turbulent flight conditions, and they are of no help if anglers are visually obscured by vegetation, cliffs, or other landscape features.

Counting errors can be partially corrected by quantifying the visibility bias. Bias can be estimated by coordinating aerial counts of anglers or boats with ground-level counts from a vantage point or with a roving survey. For such a combination survey, transect width for the ground or water survey must be the same as the nominal aerial counting width. All anglers or boats are counted by both survey crews, an example of double sampling (Cochran 1977:327). When ground-level counts are incomplete, both the aerial and ground crews must map all anglers or boats seen and compare maps to identify parties in common. Pollock and Kendall (1987) offered details on how to estimate visibility bias for aerial surveys.

12.5 EXAMPLES

12.5.1 Puget Sound, Washington

As part of an experiment to compare different contact methods, Fraidenburg and Bargmann (1982) carried out an aerial survey of a boat-based marine recreational fishery near Seattle, Washington. Their objective was to estimate directed fishing effort for salmon and for marine species.

The survey occurred in February–April 1978. Sampling was stratified between weekends and weekdays, and five flights were made within each stratum. Random selection of flight days was compromised by fog and other poor flying conditions.

All counts were made between 10:30 AM and 12:30 PM. The observer's judgment of a boat's participation in fishing was somewhat subjective. Aerial counting bias was assessed with a postcard survey of anglers, who were handed cards at access points on flight days. The postcards, which asked for information on trip length, target species, and number of anglers per boat, were returned at a rate of 53%. Aerial counts of boats (which averaged 60 on weekdays and 288 on weekend days) were corrected according to the numbers of boat trips estimated from the postcard survey.

This survey was notable for the effort made to correct certain biases by use of a complemented survey (Chapter 14). It also illustrates several problems that often frustrate aerial surveyors. Most of these were pointed out by Fraidenberg and Bargmann in their critique of the study. Good flying days generally were good boating days, and the inability to adhere to random selection of flight days meant that estimated fishing effort was biased high. The overestimate was compounded by the observer's inability to consistently distinguish fishing boats from nonfishing boats, which led to inclusion of parties that looked like they had been fishing or were going to fish. Furthermore, several biases were associated with the postcard survey (including nonresponse bias associated with the low postcard return rate). To this catalog of problems, we add the nonrandom selection of flight times on survey days. Fraidenburg and Bargmann felt that this aerial survey provided a less satisfactory estimate of fishing effort than a roving survey and other methods that were tested concurrently, but that it provided a useful map of fishing effort that would have future value for survey design.

12.5.2 Lake Vermillion, Minnesota

Hoenig et al. (1989) and Hoenig and Heywood (1991) recounted an aerial survey of fishing effort on Lake Vermillion, a 20,000-hectare lake in Minnesota, during the summer fishing seasons of 1984 and 1985. The sampling design consisted of two-stage sampling within stratified sampling (Chapter 3). Eight temporal strata were established: two day types (weekday and weekend day) and four quarter-day periods within each day type. Sampling units (quarter days) were randomly selected within each of the eight strata; selection probabilities differed among strata. A complete survey of the lake, during which all boats fishing were counted, took about 1 hour, considered an instantaneous count by the authors.

This was a very well-designed aerial survey that gave quite precise results. For example, the estimate of total weekend fishing effort during the 19-week season (68,323 boat-hours) had a proportional standard error of only 17.8%. The authors showed how additional refinements in the surveys—for example, increasing sampling effort early in the season when fishing effort changes most rapidly, and making more detailed notes of weather conditions during flights—could improve the utility of survey data for predicting fishing trends in future years.

12.5.3 Maine Lakes

Ice fishing is an important winter activity on Maine lakes. McNeish and Trial (1991) described surveys of ice fishing effort in south-central Maine from 1980 to 1987. Because the lakes are numerous and dispersed and because winter driving in the region can be difficult, aerial surveys have been the principal means of estimating winter fishing effort for several years. However, the lakes also are visited when possible by ground crews, which have provided useful information for calibrating and improving the aerial surveys.

McNeish and Trial studied the 1980–1987 survey data to find ways of making the aerial surveys more cost-efficient. Survey sampling had been stratified between weekdays and weekend days. Up to three flights per week were planned during January–March; these were randomly selected "as weather permitted." Over the 8 years, the ratio of weekday to weekend day flights was approximately 1.5:1. All flights occurred between 11:00 AM and 2:00 PM.

Thanks to on-ice interviews conducted by ground crews, McNeish and Trial determined that approximately 90% of all anglers fishing during a day were on the ice during the 11:00–2:00 survey window. By constructing probability-of-use curves based on the interviews, the authors could adjust daily aerial counts for time of day with only trivial increases in projected effort variances. They regressed mean adjusted weekday counts against mean adjusted weekend day counts and found a relationship that was consistent among lakes and years; that is, weekday effort was a consistent proportion (17%) of weekend day effort. On the basis of this analysis, they could recommend that weekday flights be discontinued.

This study did not address some basic flaws in the winter survey design. For example, bad weather distorted the strictly random selection of flight days, raising the same specter of biased effort estimates noted above for the Puget Sound surveys (Section 12.5.1). The nonrandom selection of flight times (which occurred in the same 3-hour period each survey day) also is problematical, although angler interview data made this problem more tractable than it otherwise would be. The study did show clearly the value of long-term data for improving the cost-efficiency of aerial surveys, and the importance of angler contact surveys for corroborating and interpreting aerial survey data.

12.6 PRACTICAL CONSIDERATIONS

Estimation of fishing effort with aerial surveys is affected by adverse weather conditions and the need to fly during daylight hours. Inclement weather may ground a scheduled flight, even though anglers may still be fishing. Under such circumstances, an alternative survey of effort should be planned—such as access site sampling for that day. Without such alternative estimates, fishing effort would be inestimable. Inclement days can be recorded as having zero fishing effort only when it is certain that no fishing occurred. Because many anglers are hardy and determined, this certainty is rare; some fishing is likely to occur, and although it will be relatively low, its magnitude will not be known. Consequently, estimates of effort obtained on clear days will overestimate fishing effort for the fishery.

Aerial surveys are almost always limited to daylight hours, making them inadequate for effort estimation in fisheries with substantial amounts of night fishing activity. Sophisticated infrared photography could be used to count anglers, but it is costly and beyond the financial resources of most fisheries agencies. Where night fishing is important, an alternative method of surveying, such as an access site survey, is recommended.

The success of an aerial survey depends on the ability to meet the predetermined flight schedule. When the survey team competes for airplane time with law enforcement or other important agency uses, it is difficult to assure that sampling schedules can be honored. If the survey flight time is preempted more than rarely, sample days can no longer be selected with known probability. The survey becomes ad hoc and effort cannot be reliably estimated.

Logistic problems in maintaining scheduled flight times are common. Airport delays, head winds, and other unexpected problems can make it difficult to bring an airplane to a distant site exactly at the designated moment. The survey team should insure that there are no consistent biases caused by delays in meeting scheduled times.

12.7 STRENGTHS AND WEAKNESSES

Aerial surveys are efficient and cost-effective for estimating effort; they cover large geographic areas with minimum personnel (Hoenig et al. 1989), and they allow total enumeration on spatial scales that other survey methods cannot match (Malvestuto 1983). By flying at speeds of 160–240 kilometers per hour, an airplane can cover 640–960 kilometers in just 4 hours. With recent technical improvements in long-range navigation (loran), transect locations can be pinpointed for aerial surveys of the Great Lakes and marine fisheries (Boer et al. 1989). Accurate navigation permits efficient movement from one transect to another and from one lake to another.

Aerial surveys only provide estimates of effort, and they must be combined with another type of survey to produce other estimates of interest to fisheries managers. The complementary method usually is an access or roving survey, but it can be an off-site survey. When aerial surveys are combined with other surveys, effort from the aerial survey is multiplied by catch rates obtained from the second survey to produce an estimate of total catch (Section 15.6.3).

Aerial surveys provide angler-independent estimates of effort, whereas effort obtained from telephone and mail surveys depends on the angler's interpretation of events and are subject to recall bias. Aerial surveys have been used to provide estimates of commercial crabbing effort in Texas (Hammerschmidt and Benefield 1986) and of recreational salmon fishing effort in Alaska (Hammarstrom 1990).

Aerial surveys are very helpful in determining spatial and temporal patterns in a fishery. This information can be used to establish sampling probabilities for designing on-site surveys (Malvestuto 1983). Aerial surveys can provide inventories of access points, shoreline types, and fishing grounds. When the relative use of fishing areas is determined from an aerial survey, the designs of roving and access surveys will be improved, because survey teams will be able to concentrate sampling frequency and site visits at times and in areas of heaviest fishing. Although the primary purpose of an aerial survey is to estimate fishing effort, a variety of information such as violations of closed areas, pollution discharges, and illegal filling of wetlands may also be obtained.

The expense of aerial surveys can deter their use; flights cost hundreds of dollars per hour. However, if the alternative to an aerial survey is an on-site survey, aerial surveys become cost-effective when large areas are to be surveyed. An aerial survey can be done with one or two agents, whereas an on-site survey of the same spatial scale might need dozens of agents. The cost-effectiveness of aerial surveys is somewhat agency specific; some agencies have small planes available for survey work at reduced cost, and costs depend on many factors including the staffing of the agency.

Although considerable preplanning is needed for an aerial survey, aerial surveys have tended to be less thoroughly planned than other types of surveys. Probability-based selection of sampling day, time of day, flight segment, and starting location should be done before the survey begins.

Effort estimation with an aerial survey is discussed in Section 15.6.3. There we also show how to combine an aerial survey with an access or roving interview survey to estimate catch rate and hence total angler catch.

Chapter 13

Comparison of Angler Contact Methods

13.1 INTRODUCTION

Each of the seven basic angler survey methods described in Chapters 5–12 has strengths in some applications and weaknesses in others. We summarize these in the next section in terms of the errors associated with each method. Then we discuss the criteria by which a method may be selected. Finally we note the advantages of using more than one contact method—complemented surveys—to strengthen an angler survey.

13.2 COMPARISON OF ERRORS

In general, on-site contact methods have lower potentials for sampling, response, and nonresponse errors than off-site methods (Table 13.1); they are also more costly to conduct (Section 13.3). Aerial surveys are subject only to sampling errors because they incorporate no interviews or questionnaire. Although access point and roving surveys include interviews, response errors associated with these usually are low because anglers are contacted during or just after fishing, and trained clerks usually identify and measure the harvested fish. Access and roving surveys also have low sample selection errors; however, both are vulnerable to avidity bias. Access surveys are further subject to possible undercoverage errors because some access points may be overlooked. Roving surveys are subject to length-of-stay bias.

Among off-site methods, diaries, logbooks, and catch cards are least likely to provide accurate and representative data. These methods should be used only when all other sampling techniques are impractical—and even then only with great circumspection. Mail surveys have more tractable problems, but response errors may be high and mail surveys based on list frames often suffer from undercoverage error. Nonresponse errors may be reduced by multiple mailings, rewards, and telephone follow-ups of nonrespondents, but at added cost. Generally, telephone surveys are less error prone than mail contacts but telephone directory frames may suffer from undercoverage. Door-to-door surveys give the most accurate information of the off-site methods but at a very high (often prohibitive) cost.

Table 13.1 Potential for errors in different types of recreational fishing surveys: H = high; M = medium; L = low; 0 = not applicable. (Adapted from Essig and Holliday 1991.)

	Off-site methods					On-site methods		
Error type	Mail	Phone list or directory	Phone, random	Door-to-door	Diary	Access	Roving	Aerial
				Sampling errors				
Improper selection	L	L	L	L	H	L	L	L
Undercoverage	M	M	L	L[a]	H	M[b]	L	M[c]
Avidity bias	M	M	L	L	H	H	H	0
Length-of-stay bias	0	0	0	0	0	0	M	0
				Response errors				
Recall bias	H	M	M	M	L	L	L	0
Prestige bias	H	H	H	H	H	L	L	0
Rounding bias	H	H	H	H	H	L	L	0
Lies	M	M	M	M	M	L	L	0
Question misinterpretation	H	M	M	L	H	L	L	0
Species misidentification	H	H	H	H	H	L	L	0
Incorrect lengths, weights	H	H	H	H	H	L	L	0
				Nonresponse errors				
Refusals	H	M	M	L	H	L	L	0
Unavailables	L	M	M	M	L	L	L	0
Impediments (language, literacy)	M	L	L	L	M	L	L	0

[a]Low for area frames, medium for list frames.
[b]Medium because sometimes access points are missing from the list frame. It depends very much on the fishery.
[c]Anglers or boats may not always be visible from the air even though the area frame is complete.

13.3 SELECTING AN APPROPRIATE CONTACT METHOD

The comparisons presented in Table 13.2 may prove useful in deciding which contact method is most appropriate for a particular study. The errors associated with these methods (Section 13.2) do not lend themselves to dichotomous comparisons because they vary with circumstances, even for the same method. However, the types and amounts of errors that can be tolerated should be among the first considerations in the design of any study; they might preclude the use of certain survey methods from the outset. Once a method has been selected according to criteria suggested by Table 13.2, it should be evaluated against acceptable error tolerances. If that method seems unlikely to meet required standards of accuracy and precision, a different method should be selected. If no method can perform well under the prevailing circumstances, the study should be deferred, modified, or abandoned. It is pointless to commit funds to a study that cannot produce useable information. In the following paragraphs, we elaborate on the comparisons arrayed in Table 13.2.

Type of information, information context, appropriate questionnaire length, and *mode of data collection* are closely related survey attributes that segregate quite clearly between on-site and off-site methods. If a survey's primary purpose is to gain accurate information about fishing effort, harvest, and biological characteristics of landed fish in a particular fishery, an on-site access point or roving survey is the method of choice, because direct observations and measurements by a trained clerk produce the most reliable data of these types. Anglers sometimes cannot identify the fish they catch, especially in species-rich marine waters, and they often misestimate the sizes of fish hooked (Haw and Buckley

Table 13.2 Features of angler surveys that influence the choice of survey method for a particular study. The ×'s denote which of two or more alternatives best characterize each method. Open cells mean no alternative applies. (Adapted and expanded from Essig and Holliday 1991.)

	Off-site methods					On-site methods		
Comparison	Mail	Phone list or directory	Phone, random	Door-to-door	Diary	Access	Roving	Aerial
Type of information								
Fishing data					×	×	×	×
Angler opinions	×	×	×	×				
Information context								
Current					×	×	×	×
Retroactive	×	×	×	×				
Time to conduct retroactive surveys								
Short		×	×	×				
Long	×							
Appropriate questionnaire length								
Short					×	×	×	
Long	×	×	×	×				
Data collection								
Observed by clerk						×	×	×
Reported by angler	×	×	×	×	×			
Sampling frame								
List	×	×	×	×[a]				
Spatiotemporal						×	×	×
Access to fishery								
Defined points						×		
Undefined or diffuse	×	×	×	×	×		×	×
Fishing effort								
Low		×			×		×	
High	×		×	×		×		×
Fishing area								
Small						×[b]	×	
Large	×	×	×	×	×	×[b]		×
Survey cost								
Low	×	×	×		×			
Medium						×	×	×
High				×				

[a]Door-to-door surveys may use an area frame.
[b]Traditional access survey is better for small areas, whereas the bus route access survey is better for large areas with many access points.

1968; Hiett and Ghosh 1977). If additional information is requested of anglers, such as the time they spent fishing or the number of fish they caught and released, on-site clerks can elicit the information with the least memory recall bias. However, on-site questionnaires must be kept short, both to maximize the numbers of anglers interviewed and to minimize resentment from anglers who would rather be fishing or on their way home.

Off-site surveys (other than generally undesirable diaries) are best for learning angler opinions and attitudes, which may require lengthy questionnaires (but not too long, or nonresponse will increase); for obtaining economic data about a trip that may not be complete until an angler reaches home; and for assessing angling patterns and trends over large regions embracing several fisheries. Of the three principal off-site methods, telephone and door-to-door surveys offer the greatest response rate, the best opportunity to clarify questions, and the shortest time to complete the survey; door-to-door and mail surveys give anglers the greatest opportunity to consult personal records; and mail and telephone surveys provide the greatest number of interviews per unit cost.

The *time* required to obtain estimates generally is longer for mail surveys than for other off-site surveys because of the need for multiple mailings and (usually) a telephone follow-up. On-site methods can produce spot estimates quickly, but access and roving surveys often last for much or all of a fishing season.

The list *sampling frames* typically used in off-site surveys (door-to-door surveys may use area frames) are usually based on license files and may be incomplete (see Section 5.4.2). The spatiotemporal frames of on-site surveys are likely to be complete, although minor access points sometimes are overlooked.

Among the on-site methods, roving surveys are preferred when *access* is diffuse, but access point surveys, which usually are cheaper, should be used when access is limited and well defined. For a diffuse-access fishery, a telephone survey may be an off-site alternative to a roving survey of catch and effort if errors in angler-reported data are unlikely to be serious.

It is hard to obtain reliable survey data when *fishing effort* is low. When angling intensity is slight, the best choice of methods is between on-site roving surveys and off-site telephone directory surveys. A diary or logbook approach might be used if only indices of temporal change in catch rate are needed.

Fishing area has constrained catch estimates until recently, because traditional on-site access and roving surveys, which produce the best estimates of catch (and effort), are only efficient for relatively small angling areas. The new bus route design for access point surveys should remove much of this constraint. Aerial surveys remain effective ways to estimate fishing effort (only) over large areas. When direct observations of catch are not needed, all the off-site methods may be used for large-area fisheries, and the choice among them is influenced predominantly by cost.

Except for door-to-door surveys, the *cost* of off-site methods tends to be lower than the cost of on-site methods. Among off-site methods, diary surveys are typically the cheapest, although they cannot often be considered because of their high potential for error. Mail surveys are less expensive than telephone surveys. Door-to-door surveys are often too costly to be practical, but they have made important contributions to large national studies and in developing countries. On-site access and roving surveys have quite high costs for the relatively small size of the fisheries surveyed. Aerial surveys are often quite reasonable in cost for the large areas covered. Relative survey costs vary among fisheries, so our comparisons should be viewed only as general guidelines.

13.4 COMPLEMENTED SURVEYS

After considering the possible errors and decision criteria outlined in this chapter, a survey researcher may still be in doubt about what contact method to use. If the survey objectives are complex and multifaceted, a combination of methods may be considered. For example, a coastal fishery may have a pier component that can be sampled with an access point survey and a surf component that requires a roving survey. Complemented surveys are treated in Chapter 14.

Chapter 14

Complemented Surveys

14.1 INTRODUCTION

A complemented survey is one in which two or more contact methods are used (Malvestuto 1983). Complemented surveys serve several purposes; among them are bias correction (Section 14.2), data augmentation (Section 14.3), expanded fishery coverage (Section 14.4), and estimation of total catch and total effort by different methods (Section 14.5; Chapter 15). These purposes are outlined in Figure 14.1 and discussed in the following sections.

14.2 BIAS CORRECTION

The use of any contact method may impart a serious bias to survey estimates. It may be possible to estimate the bias with a different contact method and then to correct estimates for that bias. The serious problem of nonresponse bias in mail surveys offers an example. Even if the total design principles of Dillman (1978) are used, as well as inducements or rewards, nonresponse rates reach 40% in many mail surveys. When nonrespondents differ in any important way from respondents, estimates are biased. A telephone survey of some (but not all) of the mail nonrespondents can reveal whether or not a difference exists and—if it does—its magnitude. Then the mail survey estimates can be corrected for the nonresponse bias.

In roving surveys, catch rates are obtained from anglers before fishing trips are completed. Because catch rates may change after the interviews and because roving surveys preferentially sample anglers who fish for long periods, these estimates of catch rate may be biased. Two types of complemented survey have been used to test these biases. One is a small access point survey to obtain completed trip information, which then can be compared with the incomplete trip data from the roving survey. The second is a postcard survey of the interviewed anglers to find out their completed trip information. (We do not recommend postcard surveys because they usually have low response rates and thus may introduce serious nonresponse biases.)

As a third example of many possible ones, a small in-person survey might be used to test comprehension of a question intended for a mail questionnaire. Such a pilot survey could reveal bias-inducing flaws in a question before it is used in a large survey.

14.3 ADD-ON SURVEYS

Anglers contacted during an access point or roving survey are known clientele of a fishery. Information about the economics of their fishing trips, their attitudes

COMPLEMENTED SURVEYS

Bias Removal
- Mail Survey with Telephone Follow-up
- Roving Survey with extra Access Point Interviews

Add-On Surveys
- Mail Survey after Access Point or Roving Survey
- Telephone Survey after Access Point or Roving Survey

Large Complex Fisheries
- Boat Access Point Survey with Bank Roving Survey
- River Roving Survey with Stream Access Point Survey
- Night Access Point Survey with Day Roving Survey

Catch-Effort Surveys (Chapter 15)
- Telephone Survey for Effort with Access Point Survey for Catch Rates
- Roving Survey for Effort with Access Point Survey for Catch Rates

Figure 14.1 Some reasons for using more than one contact method in an angler survey, with corresponding examples of complemented surveys.

toward management of the fishery, or their general opinions of a fishing matter might be of value to an agency. Such questions are difficult to ask in the field because they take too long and cause the anglers to be uncooperative. One solution to this problem (Brown 1991) is to obtain the anglers' names, addresses, and possibly telephone numbers during the short catch–effort field interview. Later a sample of these anglers can be sent a complementary mail questionnaire containing the more detailed questions. An add-on telephone survey could be done in the same way.

In surveys added to on-site surveys, anglers are sampled in proportion to how often they fish. Thus, such surveys are subject to avidity bias (Thompson 1991; Sections 6.2, 13.2).

14.4 LARGE COMPLEX SURVEYS

Many fisheries are so large or varied that it is impractical to survey anglers with a single contact method. For example, a lake or reservoir fishery may have both an important boating component that can be sampled effectively at a few launching ramps and a well-developed bank component to which access is diffuse. One way to cover both components is to complement an access point survey of boat anglers with an on-foot roving survey of bank anglers. Similar strategies could be applied to river fisheries with main-channel and headwater components, to coastal fisheries with pier and surf fishing components, and to any other fishery that lends itself to stratification by angling category.

If a fishery has both day and night components, nighttime safety of the survey agents might dictate a complemented survey. Whereas either an access or roving survey might be used during the day, an access survey might be the only safe choice of nighttime on-site methods. In some areas, even access points may not be safe at night, in which case an off-site telephone survey could be used to ask about night fishing activity.

14.5 CATCH–EFFORT SURVEYS

Complemented surveys are used commonly to estimate catch and effort. Catch and catch per unit fishing effort are best estimated on site, where the catch can be inspected, but effort may be estimated off site as well as on site, and an off-site effort survey may be cheaper to conduct. One example is the Marine Recreational Fisheries Statistics Survey, conducted by the National Marine Fisheries Service along U.S. coasts, in which catch rates are estimated from access point surveys and effort is determined from telephone surveys (Essig and Holliday 1991). Sometimes a roving survey is used to estimate effort and an access point survey is used to estimate catch rates from completed trips, thereby avoiding length-of-stay bias. Single and complemented surveys to determine catch and effort are treated further in Chapter 15.

Part IV

APPLICATIONS

Chapter 15

Effort and Catch Estimation

15.1 BACKGROUND

In this chapter we review the available sampling methods for estimation of total fishing effort and total catch or harvest. We discuss the appropriateness of these methods from both statistical and biological viewpoints, building on the general discussion of contact methods presented in Chapters 5–14.

Referring to angler surveys for effort and catch estimation by generic names such as telephone, mail, access, roving, or aerial is inadequate and confusing. Optimal survey designs for estimation of effort and catch may require a different contact method for each parameter: that is, complemented or combination surveys (Chapter 14). We present the possible complements, discuss how to choose the best one, and outline how to estimate effort and catch with each. We present some complements that are not frequently used now and that might be useful in the future, but we give more attention to the commonly used methods. We conclude with a comparative discussion of the strengths and weaknesses of the important design combinations.

15.2 EFFORT AND CATCH VARIABLES

15.2.1 Fishing Effort

Fishing effort or *fishing pressure* is a measure of the use of the resource by anglers. It is typically measured in angler-hours: the sum of all hours fished by all anglers. (Four anglers fishing for 2 hours each and eight anglers fishing for 1 hour generate 16 angler-hours of fishing pressure.) To convert angler-hours to angler trips can be difficult in some survey combinations because some methods, such as aerial and roving surveys, do not yield complete trip information.

In some cases it may only be possible to obtain information on parties (boats), not on individual anglers. Hence sometimes party-hours or boat-hours may be the reported unit of fishing effort. If the average number of anglers on a boat or in a party can be determined, then estimated angler-hours could be obtained by multiplying boat-hours or party-hours by this average. Use of angler-hours makes it easier to compare measures of effort in different fisheries where party size may differ.

It may be desirable to partition effort according to the fish species or group of species sought. This is commonly called *directed fishing effort*. It requires that anglers be asked what they are fishing for in addition to what they caught.

15.2.2 Catch and Harvest

Catch is the number or weight of a particular species of fish caught (kept and released or discarded) in a particular body of water over a particular time period.

Table 15.1 Complemented survey designs for effort and catch estimation: Yes = commonly used design; (Yes) = possible but rarely used design; No = inappropriate design. Designs on the diagonal use the same method for both effort and catch estimation.

Effort estimation	Catch estimation				
	Telephone	Mail	Access	Roving	Aerial
Telephone	(Yes)	No	Yes	(Yes)	No
Mail	No	(Yes)	(Yes)	(Yes)	No
Access	No	No	Yes	No	No
Roving	No	No	Yes	Yes	No
Aerial	No	No	Yes	Yes	No

Harvest refers to all fish kept. Only harvested fish can be measured directly in a creel survey. There may be additional undetected fishing mortality beyond the harvest due to hooking injury to fish that escaped or were released.

Although the distinction between catch and harvest is useful and important, we use catch generically to represent catch or harvest throughout this chapter (and elsewhere in this book), unless an explicit distinction between the two terms is necessary.

15.2.3 Catch Per Unit Effort

Catch per unit effort or *catch rate* is an estimate of *success rate*. In recreational fisheries, catch rate is usually expressed as number or weight of fish caught per angler-hour. Methods of calculating catch rate will be discussed later in this chapter. Sometimes catch rate is estimated from samples and multiplied by total effort to obtain total catch (e.g., roving–roving design), and sometimes catch and effort are first calculated by expansion from samples (e.g., access–access design) and catch rate is obtained afterwards from these totals.

15.3 OVERVIEW OF COMPLEMENTED SURVEY DESIGNS

Both effort and catch can be estimated with off-site methods (telephone, mail surveys) and on-site methods (roving, aerial [effort only], access surveys). One method can be used for both purposes, or the methods can be used in various complements or combinations (Table 15.1). The choice of a combination (broadly construed to include the use of one method for both effort and catch estimation) is influenced by several considerations peculiar to each study, among them the geographic extent of a survey (regional or local), cost, practicality, and the types of estimates desired.

We introduce design combinations and their uses in this section, and then develop these subjects in more detail through the rest of the chapter. Except for passing mention, we do not dwell on three topics of relatively minor importance.

- Off-site door-to-door surveys (Chapter 8) likely will be used very rarely to estimate effort or catch because they are so expensive.
- Angler logbooks, diaries, and catch cards (Chapter 9) sometimes are used to estimate effort and catch but usually only relative estimates are obtained. Diaries also could be used in support of telephone or mail surveys to reduce

recall or memory problems. Diaries can be used in a form of panel survey (Kasprzyk et al. 1989; Section 5.3), in which individuals are asked to record their angling activity and are contacted repeatedly over a period of time. However, keeping an accurate diary requires strong commitment from an angler; less avid anglers tend to drop out of diary surveys, and anglers who are dissatisfied with a fisheries agency are less likely to cooperate. The diary method thus can be used only in specially controlled circumstances.
- In some fisheries, more than one survey combination may be used to estimate total effort and total catch. For example, a diurnal fishery might require a roving (effort)–roving (catch) design during the day but an access–access design at night for safety. Estimation is carried out for each combination separately and then the estimates are added to obtain the overall effort and catch for the whole fishery.

In our notation for combination designs, we give the method for effort estimation first, then (after a dash) the method for catch estimation. For example, "telephone–access" means a telephone survey for effort estimation and an access point survey for catch. The notation does not indicate a sequence in time; catch may be estimated on site before effort is estimated off site.

15.3.1 Design Combinations

15.3.1.1 Off-Site Effort Surveys

Telephone surveys are sometimes used to estimate effort over a large area (e.g., a state or province), but they are rarely used to estimate catch (telephone–telephone) because memory of catch is very fallible except when written records are kept. If catch data are needed, telephone designs are often combined with intercept methods that allow the catch to be examined by a clerk (telephone–access, telephone–roving). Telephone surveys have the advantage of obtaining information easily on night fishing, which may be dangerous to obtain with an on-site survey.

Mail surveys are also infrequently used to estimate effort or catch, because of recall (memory) problems. As better (i.e., more focused) sampling frames become available, however, mail surveys may be used more often for limited-season fisheries or small trophy fisheries for which recall bias is not too severe because the fishing experience was memorable. Designs of possible use are mail–mail, mail–access, and mail–roving. For catch, it is usually better to use an on-site method, so mail–access or mail–roving may be the most useful. Mail–mail, however, has the advantage of simplicity and lowest cost and may be reasonable for memorable, easily identified trophy species. Mail surveys also can be used to estimate night fishing parameters much more safely than on-site surveys.

15.3.1.2 On-Site Effort Surveys

An access survey is often used for both effort and catch (access–access). If access surveys are used for effort, they are almost never combined with another method for catch estimation. For large regional fisheries amenable to access surveys, the bus route access design (Robson and Jones 1989) may be very useful instead of the traditional access design (Chapter 10).

A roving survey is also frequently used for effort. If catch is needed, roving surveys are often combined with roving or access surveys (roving–roving,

roving–access). Aerial surveys are only used to estimate effort, and they must be combined with intercept methods if catch is needed (aerial–access, aerial–roving).

15.3.2 Choice of Methods

The choice of an angler survey design to estimate angler effort and catch is often a daunting prospect. The flow charts in Figures 15.1–15.3 show one set of decision sequences but other sequences are possible, particularly when factors arise that are not considered in those figures. There is never only one way to make a sensible series of decisions.

The first factor to be considered is the size of the fishery. If the fishery or fisheries to be surveyed has a regional scale (e.g., several counties, a whole state or province, a large estuarine system), the design will be very different from that for a small localized fishery on a lake, river, or reservoir. For regional surveys (Figure 15.1) using telephone or mail to estimate effort is very appealing if a good angler list frame is available (perhaps from a special license). Telephone surveys (Section 15.4.1) are quicker but have higher cost than mail surveys (Section 15.4.2). They also tend to have higher response rates. The decision on how to obtain catch information is an important one. If one is prepared to rely on angler-supplied information, a telephone or mail survey is the simplest way to obtain catch data. We only recommend these for fisheries in which the species caught are easy to identify, fishing experiences are likely to be memorable, and management restrictions (bag limits) are not likely to cause anglers to underreport their catches. Repeated telephone or mail contacts over time and the use of diaries may reduce memory problems. If an on-site catch inspection method is to be used, access or roving interviews may be used. Access interviews are based on complete trips, whereas roving interviews are based on incomplete trips. Access interviews are not feasible in some diffuse-access fisheries, leaving roving interviews as the only practiced choice despite their limitations.

If there is no good list frame of anglers for a regional survey, it may be feasible to estimate effort with an aerial survey if visibility of anglers is good and night fishing is minimal (Figure 15.2). Access or roving interviews will be needed to estimate catch. If aerial counts are not feasible, the bus route access–access design (Section 15.5.3) may be useful. If access is too diffuse even for the bus route design, viable choices get very difficult. Roving surveys with a boat are usually not feasible in large regional fisheries. A telephone survey for effort based on random-digit dialing or a large license file might be practical, but costly. The question of whether to estimate catch on site or off site again arises. One could also use a mail survey for effort based on a large license file, but it would be very inefficient and likely would elicit a low response rate. Sometimes the best decision is to postpone a regional survey until an adequate sampling frame can be established.

The design decisions are more straightforward for localized fisheries (Figure 15.3). If visibility from the air is likely to be good and night fishing is minimal, an aerial survey could be used for effort and an access or roving survey for catch. When an aerial survey is not feasible, access or roving counts and catch interviews must be used. If access is restricted and well defined, an access–access survey (Section 15.5) is likely to be useful and is commonly used. The bus route version of the access–access design could be very useful where access points are numerous. If access is not restricted or if a substantial amount of fishing is done

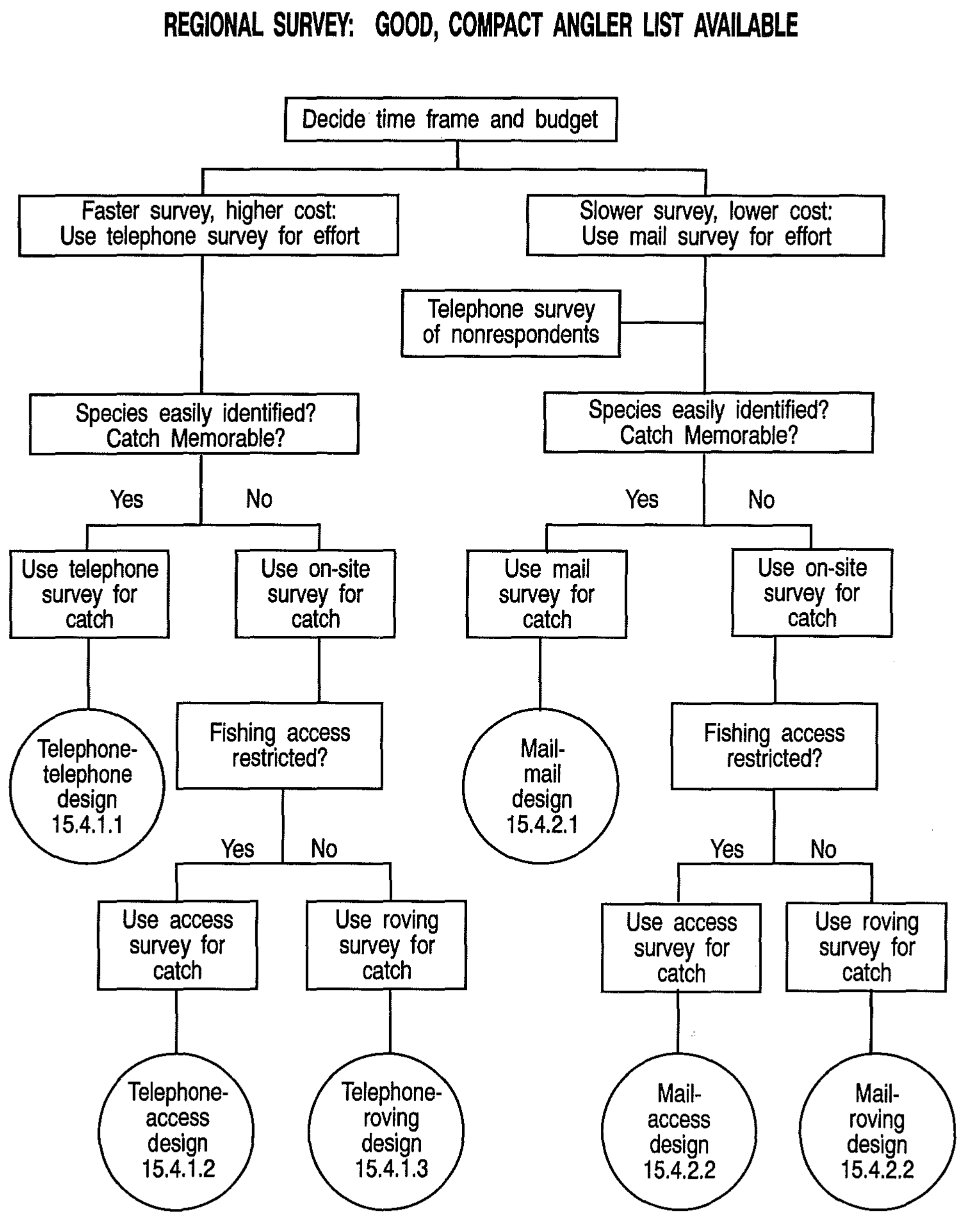


Figure 15.1 Design choices for regional angler effort and catch surveys when a good compact angler license file is available. Numbers within the ovals refer to sections in the book where the designs are described in detail.

via private access points, roving counts and access or roving catch interviews are commonly done (Section 15.6). Access data reflect complete trips, but they may not be representative of all possible interviews. Roving data reflect incomplete trips and may give biased estimates of catch rate (length-of-stay bias), but they may be much more practical to carry out.

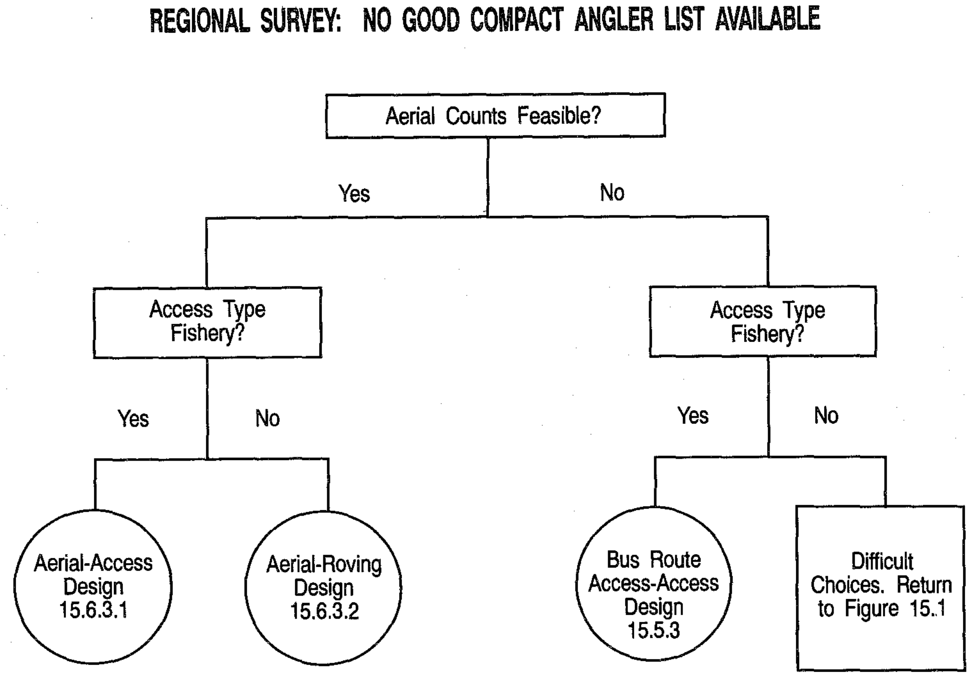


Figure 15.2 Design choices for regional angler effort and catch surveys when a good compact angler license file is not available. Numbers within the ovals refer to sections in the book where the designs are described in detail. The last square box involves difficult choices. Return to Figure 15.1 and consider a random-digit-dialing telephone survey or a larger, inefficient angler license file for a telephone or mail survey.

15.3.3 Estimation Procedures

Estimation procedures aggregate into three general groups. For each, a brief description and then the calculations of effort and catch are presented. Brief discussions of catch rate and variance estimation conclude this section. Some readers may find the examples in Sections 15.4–15.6 more helpful in understanding estimation procedures than this more mathematical treatment.

15.3.3.1 Notation

The following notation is used to develop the general estimation equations for total effort and catch.

- E is the total effort for the population.
- C is the total catch for the population.
- R is the catch rate for the population.
- N is the number of sampling units in the population.
- n is the number of sampling units in the sample.
- e_i is the fishing effort for the ith sampling unit (usually a day or part day).
- c_i is the catch for the ith sampling unit.
- L_i is the length of the fishing trip at the time of interview. In a roving intercept survey L_i represents an incomplete trip; in an access point survey it represents a complete trip.

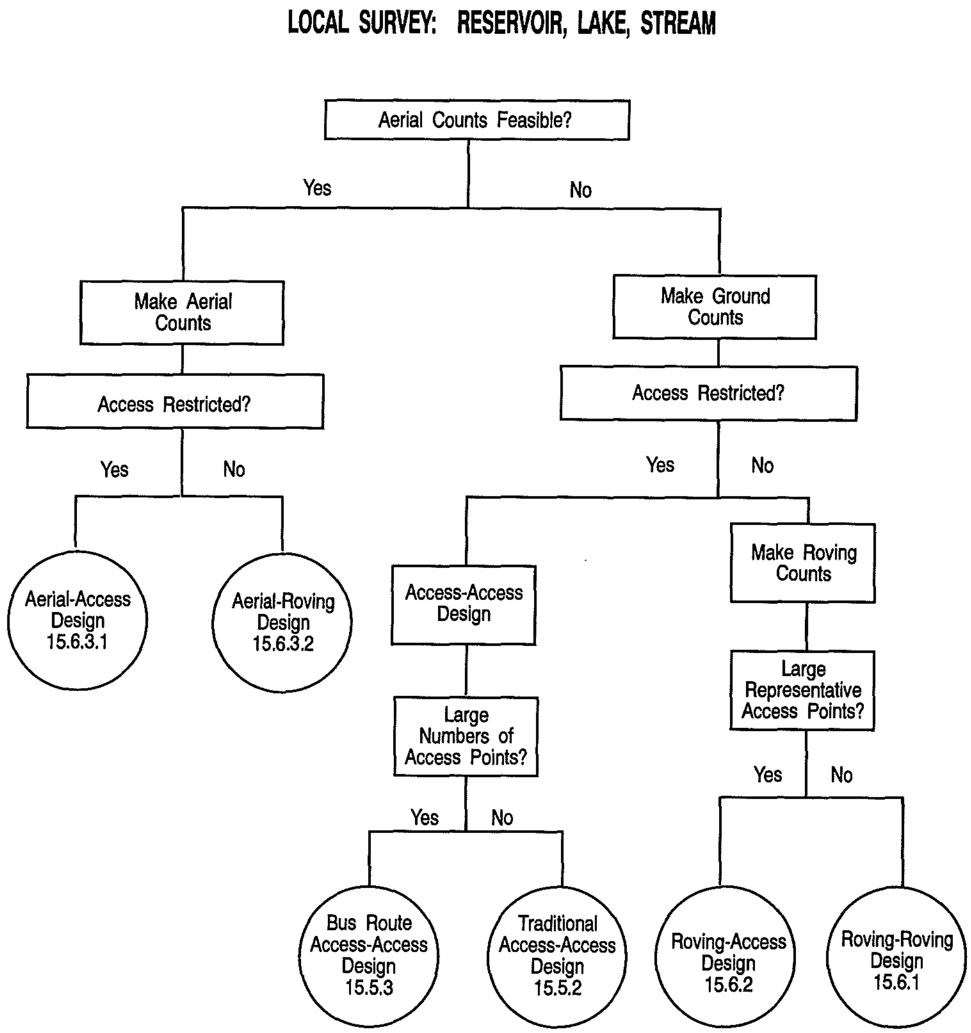


Figure 15.3 Design choices for local angler effort and catch surveys. Numbers in the ovals refer to sections in the book where the designs are described in detail.

- I_i is the instantaneous count of the number of anglers or parties in the ith sampling unit.
- π_i is the total probability that the ith sample unit is included in the sample; it may involve a combination of probabilities over several levels (days, part days, areas, etc.).
- T is the total length of the fishing day.

Here, C is used to indicate "catch" in the generic sense. Catch (kept and released fish) and harvest (kept fish) are estimated in the same way in all designs.

15.3.3.2 Simple Combination Designs

One group of methods—telephone–telephone (Section 15.4.1.1), mail–mail (Section 15.4.2.1), and access–access (Section 15.5)—directly expand both effort and catch from angler interviews to totals by the methods discussed in Chapter 3. These are simple designs in that both effort and catch are estimated with the same method.

The following general equations based on equation (3.29) are used:

$$\text{total effort: } \hat{E} = \sum_{i=1}^{n} (e_i/\pi_i); \tag{15.1}$$

$$\text{total catch: } \hat{C} = \sum_{i=1}^{n} (c_i/\pi_i). \tag{15.2}$$

In some cases, these equations may simplify. For example, in the telephone–telephone design a simple random sample might be used. Then $\pi_i = n/N$ for all the sampling units, so $\hat{E} = N\bar{e}$ by substitution ($\bar{e}$ is mean effort among sampling units). The same result applies to estimation of catch (C). Alternatively, a stratified random sampling design may be used. Then $\pi_i = n_h/N_h$ for all the sampling units in the hth stratum, and $\hat{E}$ is just the sum of the estimated effort totals in each stratum ($\hat{E}_h = N_h\bar{e}_h$; $\hat{E} = \Sigma\hat{E}_h$).

In the access–access design, more complex sampling protocols are often used, including stratification and subsampling variables. However the general equations (15.1) and (15.2) still apply and it is just a matter of seeing that the total probability (π_i) is specified correctly for each sampling unit.

15.3.3.3 Off-Site Interviews for Effort, On-Site Interviews for Catch Rate

A second group of methods obtains effort by direct expansion of off-site interview data but derives total catch indirectly from the product of effort and catch rate. Catch rate is estimated from on-site interviews. The designs used this way are telephone–access (Section 15.4.1.2), mail–access (Section 15.4.2.2), telephone–roving (Section 15.4.1.3), and mail–roving (Section 15.4.2.2). Here the access points are used to obtain an estimate of catch rate and therefore not all access points have to be included in the frame. However, it must be assumed that the catch rates calculated are unbiased with respect to the whole population (i.e., that interviews at access points sampled are representative of interviews at all access points). Also with roving interviews, it must be assumed that catch rates based on incomplete trips are unbiased estimates of true catch rates (i.e., that there is no length-of-stay bias).

Total effort is estimated as in equation (15.1), but total catch is estimated as

$$\hat{C} = \hat{E} \times \hat{R}. \tag{15.3}$$

Different formulations of catch rate $\hat{R}$ are used for complete and incomplete trips (Section 15.3.3.5).

15.3.3.4 Instantaneous Counts for Effort, On-Site Interviews for Catch Rate

A third group of methods estimates total effort by expanding instantaneous angler counts. With a roving or aerial survey, an agent counts anglers fishing at a particular time, and the count is multiplied by the length of the fishing period to obtain total effort in angler- or party-hours. There is no reliance on angler-supplied effort information, which is subject to fallible memory. Designs in this group are roving–access (Section 15.6.2), aerial–access (Section 15.6.3), roving–roving (Section 15.6.1) and aerial–roving (Section 15.6.3).

Effort is estimated by expanding instantaneous counts to the total effort for the ith fishing day,

$$\hat{e}_i = I_i \times T, \tag{15.4}$$

and then expanding to the period of interest:

$$\hat{E} = \sum_{i=1}^{n} (\hat{e}_i/\pi_i). \tag{15.5}$$

Catch is estimated by equation (15.3), $\hat{C} = \hat{E} \times \hat{R}$. Again, $\hat{R}$ differs for complete and incomplete trips (Section 15.3.3.5).

15.3.3.5 Catch Rate Estimation

The proper estimator to use for catch rate is a source of confusion and there is surprisingly little literature on the subject. D. S. Robson and colleagues (unpublished manuscript) have made the following recommendations, which we follow. For complete trips, the ratio of means should be used for catch rate. For incomplete trips, the mean of ratios should be used, but very short incomplete trips (say, less than 0.5 hour) should not be included so the variance of the estimator will not be influenced by extreme catch rates that happen by chance during short trips. Detailed simulation studies are needed to validate these recommendations which are currently based on theoretical considerations.

For complete trips, then, we use the ratio of the means ($\bar{c}$, $\bar{L}$):

$$\hat{R}_1 = \frac{\sum_{i=1}^{n} c_i/n}{\sum_{i=1}^{n} L_i/n} = \bar{c}/\bar{L}. \tag{15.6}$$

For incomplete trips we use the mean of the ratios:

$$\hat{R}_2 = \frac{\sum_{i=1}^{n} c_i/L_i}{n} = \bar{R}. \tag{15.7}$$

15.3.3.6 Variance Estimation

For simple combination surveys that use direct expansion, the variance equations used depend on whether the design is simple random, stratified random, or multistage. The approaches discussed in Chapter 3 can be followed.

For designs that obtain effort off or on site and catch rates on site, the variance estimate of total effort ($\hat{E}$) also depends on the sampling design used and the approaches developed in Chapter 3 again can be followed. Whichever catch rate estimator is used (for complete or incomplete trips), the primary sampling units usually are days. The obvious way to estimate the variance of $\hat{R}$ is to use the day-to-day variation of catch rate within each stratum (type of day, period of the season, etc.) and then add the estimates to get an overall variance.

Because total catch has the equation $\hat{C} = \hat{E} \times \hat{R}$, its variance is the variance of a product:

$$\hat{\mathrm{Var}}(\hat{C}) = \hat{E}^2\hat{\mathrm{Var}}(\hat{R}) + \hat{R}^2\hat{\mathrm{Var}}(\hat{E}) + \hat{\mathrm{Var}}(\hat{E})\hat{\mathrm{Var}}(\hat{R}) \qquad (15.8)$$

(see an example in Seber 1982:9).

15.4 OFF-SITE EFFORT DESIGNS

In this section we consider use of designs in which effort estimates are obtained off site by telephone or mail surveys. These designs are not commonly used now but they are potentially useful for large regional surveys when a good compact angler list frame exists.

15.4.1 Telephone Effort Designs

We consider the telephone–telephone, telephone–access, and telephone–roving designs. Implementation of telephone surveys is treated in Chapter 7.

15.4.1.1 Telephone–Telephone Design

It is uncommon at present to estimate both effort and catch with telephone surveys, for several reasons. Telephone frames usually include too many non-anglers to be efficient for angler surveys, although this may be less of a problem in the future as more special licenses are required. Exaggeration of catch for reasons of self importance (prestige bias) arises in any off-site survey, because the catch cannot be inspected. Telephone surveys for fishing effort may involve substantial memory recall bias unless the fishing queried was recent and memorable. Recall bias is likely to be even more severe for catch because numbers and weights of fish are more difficult to recall than fishing duration (Essig and Holliday 1991). One way to reduce recall bias in this design is to poll the same anglers regularly at short intervals over a whole fishing season (e.g., every week or every month, depending on the fishery). Rewards or other inducements may be necessary to get a good response rate, but the expenditure could be a good investment if data quality is markedly improved. Anglers can also misidentify fish species, a problem avoided in on-site surveys staffed by a trained agent who usually examines the fish (Essig and Holliday 1991). Telephone interviews, however, may be very useful for obtaining catch information about memorable species such as black marlin. We would not usually recommend the telephone–telephone design for abundant, frequently fished for, or easily misidentified species. However, a different opinion arises from a program in Missouri.

Weithman (1991) and Weithman and Haverland (1991) reported on a large, well-designed telephone–telephone survey used by the Missouri Department of Conservation to estimate angler effort and catch. A fishing license frame was used, and telephone numbers were found in directories or obtained from directory information. Anglers chosen for the sample had to be discarded if they had an unlisted number or no telephone at all, and anglers fishing without a license obviously were excluded. In the first telephone interview, a screening questionnaire was administered to obtain personal information and to encourage anglers to take part in an ongoing survey for 2 years. If anglers agreed to cooperate, they were sent a letter of appreciation, instructions, and data-recording forms. They were recontacted every 1–3 months by telephone (during evenings and weekends), according to how frequently they fished, to cut down on recall bias. They were asked to report their fishing effort and catch for all their fishing trips. Ninety-two percent of contacted anglers agreed to cooperate; of those, 90% provided data for 1 year and 80% provided data for the full 2 years. The people who dropped out did so for a variety of reasons, but the main problem was anglers who moved and could not be traced to a new address. Weithman (1991) and Weithman and Haverland (1991) believe this survey is extremely cost efficient and that the data are of good quality. They recommend this approach over others for statewide data. They did not address misidentification of species by anglers, prestige bias, or the possible bias caused by the 8% refusal and 20% drop out rates. Nevertheless, their survey deserves study for the potential of telephone surveys it demonstrates.

The estimation equations for both effort and catch obtained by the telephone–telephone design (Section 15.3.3.2) are based on direct expansion:

$$\hat{E} = \sum_{i=1}^{n} (e_i/\pi_i) \text{ and } \hat{C} = \sum_{i=1}^{n} (c_i/\pi_i).$$

15.4.1.2 Telephone–Access Design

This telephone–access design is the basis of the National Marine Fisheries Service's Marine Recreational Fisheries Statistics Survey of coastal marine fishing areas around the United States (Essig and Holliday 1991). The design consists of two parts: a random-digit-dialing telephone survey to estimate the fishing effort (number of trips) and an access point survey to estimate catch rate (number of fish caught per trip). As discussed in Section 15.3.3.3, total catch is estimated by multiplying effort by catch rate, and hence both parts of the survey are critical to its objectives. The possibility of using the telephone survey to estimate catch rates was considered and discarded primarily because of concern about the quality of angler-reported catch statistics. Many anglers are not good at identifying fish, especially fish in diverse marine faunas, and there may be regional differences in common names of fish that could also cause confusion (Essig and Holliday 1991).

In the random-digit-dialing survey (Section 7.2.1), all listed combinations of prefixes (first three digits) and the first two digits of the suffixes are obtained for the band of coastal counties; for example, 821-16XX. Then numbers are completed by randomly adding the last two digits to the fixed combination. This method reduces the number of nonworking numbers that have to be contacted. Unlisted numbers are also included.

When a number is contacted, the respondent is asked if the household contains any marine anglers. If yes, the respondent is asked the number of anglers in that house, the number of fishing trips taken in the last 2 months, where each trip was taken (ocean or open bay, sound, river, or enclosed bay), and the mode of fishing used for each trip (shore, private or rental boat, charter or party boat). The number of trips by coastal residents is estimated for each mode as the average number of trips per household contacted multiplied by the total number of households in the coastal county band obtained from the U.S. Census Bureau. This follows the typical method of estimating a total described in Chapter 3.

In the access interview, anglers are asked, at the end of their trip, to provide catch and other demographic information, including where they live and whether they have a telephone. Within a given state and for each mode of fishing, access interviewing is allocated to locations with a site register or list. These sites are sampled with probability proportional to expected usage. The interviewer is given the sampling dates and locations of all the sites he or she is expected to visit. The time of day to sample is up to the interviewer, who is encouraged to sample in such a way as to maximize the number of interviews obtained, up to 20 per day. Interviewers are encouraged to stop sampling or to switch modes or locations if they are unlikely to get eight interviews in the day. This is a difficult, complex survey to administer, but a lot of flexibility appears to be given to the interviewers so they can maximize the number of interviews in a limited time. This means that potentially serious biases in catch rates due to time of day are ignored. The primary goal of the access survey is to obtain estimates of catch per trip to combine with the effort information (number of trips) from the telephone survey. For some fisheries with a lot of diffuse access, the people interviewed may not adequately represent the fishing population for that species.

In other surveys, the telephone part of a telephone–access design might be based on a license list frame rather than on random-digit dialing. The advantage of random-digit dialing is that the whole population with telephones (listed and unlisted) is at least theoretically included. The disadvantage is that many households have no anglers. A telephone–roving design (Section 15.4.1.3) may be necessary for some diffuse-access fisheries.

We believe that the telephone–access design is very important, although costly. The alternative of using the telephone survey for catch, although cheaper, probably will not generate reliable enough data. A fully on-site design, such as access–access (Section 15.5), may not be feasible over the large area of a regional fishery.

The estimation equations for the telephone–access design (Section 15.3.3.3) are

$$\hat{E} = \sum_{i=1}^{n} (e_i/\pi_i) \text{ and } \hat{C} = \hat{E} \times \hat{R}_1,$$

with

$$\hat{R}_1 = \sum_{i=1}^{n} c_i / \sum_{i=1}^{n} L_i = \bar{c}/\bar{L}.$$

15.4.1.3 Telephone–Roving Design

The telephone–roving design has not been used very much, but it could be used for any large, diffuse-access fishery or where a lot of private access points, inaccessible to survey agents, exist. An example is the blue crab fishery of Chesapeake Bay, where the fishing is very spread out. This design is vulnerable to length-of-stay bias because catch rate is estimated from anglers in the act of fishing (uncompleted trips). The estimation equations for the telephone–roving design (Section 15.3.3) are

$$\hat{E} = \sum_{i=1}^{n} (e_i/\pi_i) \text{ and } \hat{C} = \hat{E} \times \hat{R}_2$$

with

$$\hat{R}_2 = \sum_{i=1}^{n} (c_i/L_i)/n.$$

15.4.2 Mail Effort Designs

Mail surveys to estimate angler effort deserve more consideration than they have received to date, especially when compact license files are available and the fish species are easy to identify and memorable when caught. In this section we consider the mail–mail, mail–access, and mail–roving designs. Their estimation equations are identical to those of the corresponding telephone designs. Implementation of mail survey designs is discussed in Chapter 6.

15.4.2.1 Mail–Mail Design

The mail–mail design is very uncommonly used at present for estimating effort and catch. We believe it could be a low-cost yet efficient survey method for rare trophy species such as black marlin, especially if a small frame based on a special permit existed and if there were no reason to under- or overreport the catch. Turnaround times will be slower than for the telephone–telephone design, which we also recommended for this type of fishery, longer time being the trade-off for lower cost. As with telephone surveys, memory recall bias is likely to be severe, especially for catch—thus the need for the species to be rare and its catch memorable. Diaries could be used to reduce the recall bias. Lying may be a problem if there is a bag limit or if the angler wants to exaggerate his or her catch to gain prestige with the interviewer. Nonresponse, which can be a big problem in mail surveys, would need to be addressed either by offering rewards for return of the survey or by estimating the nonresponse bias with a small telephone follow-up survey of the nonrespondents. A partly offsetting advantage is that more questions usually can be asked in a mail than in a telephone questionnaire.

15.4.2.2 Mail–Access and Mail–Roving Designs

If time were not critical, mail–access surveys might provide a lower-cost alternative to the telephone–access design (Section 15.4.1.2) for such large-scale projects as the Marine Recreational Fisheries Statistics Survey. The mail–roving survey might be used for a large-scale survey of a diffuse-access fishery or one with many private access points. On-site sampling considerations were discussed in Section 15.3.2.

Table 15.2 Example effort and catch calculations for a striped bass fishery sampled with a stratified random telephone–telephone design. Stratification is by angler residence.

Quantity	Stratum 1: coastal anglers	Stratum 2: noncoastal angler	Combined: all anglers
Population size	$N_1 = 1{,}000$	$N_2 = 4{,}000$	$N = 5{,}000$
Sample size	$n_1 = 200$	$n_2 = 400$	$n = 600$
	Sample data and expansions		
Mean effort[a]	$\bar{e}_1 = 4.152$ trips	$\bar{e}_2 = 2.272$ trips	
Mean catch[b]	$\bar{c}_1 = 2.413$ fish	$\bar{c}_2 = 1.015$ fish	
Total effort	$\hat{E}_1 = N_1\bar{e}_1$	$\hat{E}_2 = N_2\bar{e}_2$	$\hat{E} = \hat{E}_1 + \hat{E}_2$
	$= 1{,}000 \times 4.152$	$= 4{,}000 \times 2.272$	
	$= 4{,}152$ trips	$= 9{,}088$ trips	$= 13{,}240$ trips
Total catch	$\hat{C}_1 = N_1\bar{c}_1$	$\hat{C}_2 = N_2\bar{c}_2$	$\hat{C} = \hat{C}_1 + \hat{C}_2$
	$= 1{,}000 \times 2.413$	$= 4{,}000 \times 1.015$	
	$= 2{,}413$ fish	$= 4{,}060$ fish	$= 6{,}473$ fish
	Effort variances		
	$s_1^2 = 33.2410$	$s_2^2 = 40.8130$	
Var (mean)	$\hat{\text{Var}}(\bar{e}_1) = \left(\frac{N_1 - n_1}{N_1}\right)\frac{s_1^2}{n_1}$	$\hat{\text{Var}}(\bar{e}_2) = \left(\frac{N_2 - n_2}{N_2}\right)\frac{s_2^2}{n_2}$	
	$= \frac{800}{1000} \times \frac{33.2410}{200}$	$= \frac{3600}{4000} \times \frac{40.8130}{400}$	
	$= 0.132964$	$= 0.091829$	
Var (total)	$\text{Var}(\hat{E}_1) = N_1^2\,\hat{\text{Var}}(\bar{e}_1)$	$\text{Var}(\hat{E}_2) = N_2^2\,\hat{\text{Var}}(\bar{e}_2)$	$\text{Var}(\hat{E}) = \hat{\text{Var}}(\hat{E}_1) + \hat{\text{Var}}(\hat{E}_2)$
	$= 1{,}000^2 \times 0.132964$	$= 4{,}000^2 \times 0.091829$	$= 132{,}964 + 1{,}469{,}264$
	$= 132{,}964$	$= 1{,}469{,}264$	$= 1{,}602{,}228$
SE (total)			$\hat{\text{SE}}(\hat{E}) = \sqrt{1{,}602{,}228}$
			$= 1{,}265.79$

Table 15.2 Continued.

Quantity	Stratum 1: coastal anglers	Stratum 2: noncoastal angler	Combined: all anglers
	Catch variances		
	$s_1^2 = 18.1590$	$s_2^2 = 23.3125$	
Var (mean)	$\hat{\text{V}}\text{ar}(\bar{c}_1) = \left(\frac{N_1 - n_1}{N_1}\right)\frac{s_1^2}{n_1}$	$\hat{\text{V}}\text{ar}(\bar{c}_2) = \left(\frac{N_2 - n_2}{N_2}\right)\frac{s_2^2}{n_2}$	
	$= \frac{800}{1{,}000} \times \frac{18.1590}{200}$	$= \frac{3{,}600}{4{,}000} \times \frac{23.3125}{400}$	
	$= 0.072636$	$= 0.052453$	
Var (total)	$\hat{\text{V}}\text{ar}(\hat{C}_1) = N_1^2\hat{\text{V}}\text{ar}(\bar{c}_1)$	$\hat{\text{V}}\text{ar}(\hat{C}_2) = N_2^2\hat{\text{V}}\text{ar}(\bar{c}_2)$	$\hat{\text{V}}\text{ar}(\hat{C}) = \hat{\text{V}}\text{ar}(\hat{C}_1) + \hat{\text{V}}\text{ar}(\hat{C}_2)$
	$= 1{,}000^2 \times 0.072636$	$= 4{,}000^2 \times 0.052453$	$= 72{,}636 + 839{,}250$
	$= 72{,}636$	$= 839{,}250$	$= 911{,}886$
SE (total)			$\hat{\text{S}}\text{E}(\hat{C}) = \sqrt{911{,}886}$
			$= 954.93$

[a]Angler trips per season (angler-hours or party-hours are alternative measures of fishing effort).
[b]Number of striped bass caught per angler during the season.

15.4.3 Examples

15.4.3.1 Example 1: Telephone–Telephone (or Mail–Mail)

A hypothetical striped bass fishery in a state is to be surveyed with a telephone–telephone design. The telephone survey was chosen because striped bass are easy to identify and their catch is memorable. The fishing season is short. A special license file is available from which to draw the telephone sample. Anglers are to be asked to provide their fishing effort (number of trips) and catch (number and weight) for the whole season.

The state is divided into two regional strata. The first stratum of coastal residents has a population size of $N_1 = 1{,}000$ striped bass anglers; 20% of these are sampled randomly, so $n_1 = 200$. The second stratum of noncoastal residents has $N_2 = 4{,}000$ anglers; 10% of these are sampled randomly, so $n_2 = 400$. The second stratum of noncoastal anglers was sampled at a lower rate because less effort and catch were expected in that population. The sampling design is a stratified random sample (without replacement).

Statistics and calculations are summarized in Table 15.2. For simplicity, we have assumed that correct telephone numbers were available for all anglers and that no anglers contacted refused to cooperate. Such sources of bias have to be addressed in most real surveys.

A mail–mail survey could have been used in this example if a longer time to obtain estimates were acceptable. If a mail–mail design were used, nonresponse would probably have to be reduced with rewards or accounted for with a small follow-up telephone survey (Chapter 6).

Many researchers are concerned about estimating catch from a telephone survey (Essig and Holliday 1991; Chapter 7) because of possible prestige bias and misidentification of self-reported catch. In the next example, we reconsider the striped bass fishery but estimate catch from access point interviews instead of telephone interviews.

15.4.3.2 Example 2: Telephone–Access (or Mail–Access)

The striped bass fishery is the one already described (Section 15.4.3.1) and the telephone survey for effort estimation has the same protocol and outcome (Table 15.2). Here we concentrate on an access survey for catch rate.

The 2-week-long fishing season is stratified into weekdays and weekends (Table 15.3). Two interviewers each sample 3 weekdays and both weekend days each week. Each sampling day lasts 8 hours, so each interviewer works 40 hours per week. Weekdays are sampled randomly but with the additional restriction that each day must be sampled at least once by at least one interviewer. The 50 access points on the list are sampled randomly without replacement to go with the sampled days in each stratum. The list of 50 access points is not exhaustive. They are large public access points where many interviews are possible. The assumption has to be made that the catch rate estimated for these access points is an unbiased estimate of the catch rate for all access points. This may or may not be reasonable but it may be impractical to test by sampling intensively at smaller access points because of budget restrictions. The data from the access interviews are shown in Table 15.4.

Catch Rate Estimation. Here catch rate is estimated from completed trips, so the estimate is total catch from interviews divided by total effort from

Table 15.3 Structure of an access point survey of a striped bass fishery conducted over 2 weeks by two interviewers. Weekdays, three per interviewer, were sampled randomly without replacement, with the additional restriction that every day had to be sampled at least once. Access points (numbers in the table) also were selected randomly without replacement from a list of 50 sites.

		Sampling days and sites						
		Weekday stratum					Weekend stratum	
Week	Interviewer	Mon	Tue	Wed	Thu	Fri	Sat	Sun
1	1	14		31	42		45	46
	2		48	03		10	17	09
2	1	36	49	06			32	22
	2		11		38	16	13	07

interviews (Section 15.3.3.5, equation 15.6). The summed catch and effort by interviewed anglers (Table 15.4) are 339 fish and 825 trips, giving $\hat{R}_1 = 339/825 = 0.4109$.

We use the delta or Taylor series approximation (Seber 1982:7) to estimate the variance of $\hat{R}_1$ as

$$\text{Var}(\hat{R}_1) = \frac{1}{L^2}\{n_1 s_{c1}^2 + n_2 s_{c2}^2 + \hat{R}_1^2 n_1 s_{L1}^2 + \hat{R}_1^2 n_2 s_{L2}^2 - 2\hat{R}_1 n_1 s_{cL1} - 2\hat{R}_1 n_2 s_{cL2}\}. \quad (15.9)$$

In our example, $L = 825$ trips is the total effort in both strata; the stratum sample sizes are $n_1 = 12$ and $n_2 = 8$; the catch sample variances are $s_{c1}^2 = 76.4470$ and $s_{c2}^2 = 97.0714$; and the effort sample variances are $s_{L1}^2 = 313.0606$ and $s_{L2}^2 = 406.8393$. The covariances between the interview catch and effort for the two strata are

$$s_{cL1} = \frac{1}{n_1 - 1}\sum_{i=1}^{n_1}(C_{i1} - \bar{c}_1)(L_{i1} - \bar{L}_1) = 143.3030,$$

and

$$s_{cL2} = \frac{1}{n_2 - 1}\sum_{i=1}^{n_2}(C_{i2} - \bar{c}_2)(L_{i2} - \bar{L}_2) = 178.3214.$$

Table 15.4 Access point component of a striped bass fishery survey. Catch rate (number of fish per trip) is estimated on site from access point interviews of anglers who have completed trips. Catches/number of trips are presented for each day–interviewer combination.

		Catch/trip						
		Weekday stratum					Weekend stratum	
Week	Interviewer	Mon	Tue	Wed	Thu	Fri	Sat	Sun
1	1	9/21		21/41	5/11		12/41	26/63
	2		11/31	16/30		21/53	30/52	31/71
2	1	11/27	9/24	2/8			15/47	20/57
	2		25/45		8/36	31/71	31/81	5/15

The variance (equation 15.9) and standard error of the catch rate now can be calculated:

$$\text{Var}(\hat{R}_1) = \frac{1}{825^2}[12 \times 76.4470 + 8 \times 97.0714 + (0.4109)^2 \times 12 \times 313.0606$$
$$+ (0.4109)^2 \times 8 \times 406.8393 - 2 \times 0.4109 \times 12 \times 143.3030$$
$$- 2 \times 0.104 \times 8 \times 178.3214]$$
$$= \frac{1}{825^2}[917.3640 + 776.5712 + 634.2813 + 549.5221$$
$$- 1413.1969 - 1172.3563]$$
$$= \frac{292.1855}{825^2} = 0.00042929;$$
$$\text{SE}(\hat{R}_1) = \sqrt{0.00042929} = 0.0207.$$

Total Catch Estimation. Total catch is total effort × catch rate (equation 15.3), and its variance is the variance of a product (equation 15.8):

$$\hat{C} = \hat{E} \times \hat{R}_1 = 13{,}240 \times 0.410909 = 5{,}440.44 \text{ fish};$$

$\hat{E}$ = 13,240 trips is imported from the telephone survey (Table 15.2). The variance is

$$\text{Var}(\hat{C}) = (\hat{E})^2\hat{\text{Var}}(\hat{R}_1) + (\hat{R}_1)^2\hat{\text{Var}}(\hat{E}) + \hat{\text{Var}}(\hat{E})\hat{\text{Var}}(\hat{R}_1)$$
$$= (13{,}240)^2\ 0.00042929 + (0.410909)^2\ 1{,}602{,}228$$
$$+ 1{,}602{,}228 \times 0.00042929$$
$$= 75{,}253.5067 + 270{,}530.1194 + 687.8204$$
$$= 346{,}471.4465;$$

$\hat{\text{Var}}(\hat{E})$ also was imported from Table 15.2. Then the standard error of total catch is

$$\hat{\text{SE}}(\hat{C}) = \sqrt{\hat{\text{Var}}(\hat{C})} = \sqrt{346{,}471.4465} = 588.62.$$

The total catch estimate is $\hat{C}$ = 5,400.44 fish with $\hat{\text{SE}}(\hat{C})$ = 588.62, which is quite a precise estimate. In the telephone–telephone design example, the total catch was $\hat{C}$ = 6,473 fish and its standard error was $\hat{\text{SE}}(\hat{C})$ = 954.93. These hypothetical examples were contrived this way to emphasize that catch in a telephone survey may be overestimated because prestige bias causes self-reported catches to be too high.

Other Designs. It may be feasible to use a mail–access design for a fishery like this if there is no need for a fast turnaround on estimates and if nonresponse can be controlled. It may sometimes be necessary to use telephone–roving or mail–roving designs if the fishery has no large access points where representative interviews can be obtained, but an important disadvantage of roving surveys for this purpose is potential length-of-stay bias arising from incomplete trip data.

TELEPHONE	MAIL
•Telephone has quicker response time	•Mail is cheaper
•Telephone has higher response rate	•Mail has lower response rate
•Telephone is more expensive	•Mail has longer response time

Telephone-Telephone or Mail-Mail

- •Same method is used for effort and catch
- •Catch is estimated off-site from angler-supplied data, which may be inaccurate

Telephone-Access or Mail-Access

- •Catch is estimated on site and observed catch is inspected by trained interviewers
- •On-site catch estimates are costly
- •Completed trip data are used to estimate catch rate

Telephone-Roving or Mail-Roving

- •Catch is estimated on site and observed catch is inspected by trained interviewers
- •On-site catch estimates are costly
- •Incomplete trip data are used to estimate catch rate (potential length-of-stay bias)

Figure 15.4 Comparison of strengths and weaknesses of the six off-site effort survey designs discussed in Section 15.4.

15.4.4 Comparison of Designs

An overview of the strengths and weaknesses of the six designs discussed in this section are presented in Figure 15.4. We believe these designs will become more widely used in the future, especially when special fishing license and permit files facilitate cost-effective telephone and mail surveys of the anglers of interest.

15.5 ACCESS EFFORT DESIGNS

15.5.1 Access–Access Design

The access point design is one of the most common methods used in angler surveys. It generates effort and catch estimates directly. Thus when access surveys are used for effort, the only relevant complemented design is the access–access combination. Access surveys have two forms, the traditional form in which a few sites are visited each sampling day, and the bus route form in which many sites are visited for part of each sampling day. With both forms, reliance must be placed on the veracity of the angler's responses to questions about starting times and number and species of released fish. However, both forms provide data for completed trips and hence avoid the pitfalls and biases associated with the incomplete trip data provided by roving surveys. We present estimation procedures for both types of access survey, and we illustrate the calculations of effort and catch with simple examples. Access designs are described in Chapter 10.

15.5.2 Traditional Method

The traditional access–access design (Malvestuto 1983; Hayne 1991; Chapter 10) is widely used on lakes and reservoirs, typically on a relatively small body of water with only a few well-defined access sites (e.g., a boat-based reservoir fishery with three concrete boat ramps and no other access). Sampling days, part-days, and sites are selected probabilistically, and the selection probabilities often are set in proportion to the likely use of the site (Hayne 1991); for example, if weekend days, afternoons, and a particular site are used more often than alternatives, they would be assigned higher selection probabilities. Effort and catch estimation are straightforward, being direct expansions of the information obtained by the agent in the interview. We showed these calculations in Section 15.3.3.2.

15.5.2.1 Description of the Method

The traditional access design is simple to implement. One or a few sites are chosen to sample on a selected survey day. Typically an agent (clerk) drives to a site and stays there all day or part of the day. While at the site, the clerk intercepts anglers returning from fishing and records when the anglers arrived (information supplied by the anglers), when they finished fishing (the time of the interview), the number of fish caught but released (angler-supplied data), and the number of fish harvested (usually obtained by inspecting the fish brought back). The clerk calculates the trip duration from the information on starting and ending times. At a busy site, clerks may be unable to interview all anglers and must subsample them (e.g., every second or third one), although all returning anglers, interviewed or not, must be counted.

Stratification and unequal selection probabilities add to the complexity of the access design, although they improve the precision of the estimates. The most common stratification variables are time period (usually month) and day type (weekend day versus weekday). The day-type stratification is important, and weekends are usually sampled at a much higher rate because fishing effort typically is much higher then. The day is usually divided into morning and afternoon subsamples to accommodate an 8-hour workday within a longer fishing day (fishing days may last 16 hours or more in summer). Unequal (nonuniform) sampling probabilities permit heavier sampling of time–space combinations in which the heaviest fishing effort occurs. In a boat-based fishery, for example, more people complete their trips in the afternoon and an agent gets more interviews then. Sampling more frequently when the fishing is heaviest reduces the overall variance and increases the precision of effort and catch estimates. Therefore, the agent may be assigned twice as many afternoons as mornings. In this case, the selection probabilities would be $\pi_{AM} = 1/3$ and $\pi_{PM} = 2/3$. Unequal selection probabilities usually are desirable for sites, because some sites often are more popular than others. When the selection probability of a given time or place is very small, an unusual occurrence will have exaggerated importance. This problem is exemplified by the now-classical story of the arrival of a boy scout troop at a rarely used site while an agent is present. Such an event will lead to an abnormally large estimate of effort with a large variance, because the effort of the party will be divided by a very small selection probability. For this reason, we suggest that a lower limit be established for selection probabilities. Where that limit lies will depend on how well planners know the fishery to be surveyed and the level of risk they are willing to assume. As always, professional judgment must be exerted here.

Table 15.5 Stratified random sampling schedule for a traditional access point survey with one access site. Morning (AM) and afternoon (PM) periods were selected with equal probability.

Week	Mon	Tue	Wed	Thu	Fri	Sat	Sun
1		PM	AM			PM	AM
2	PM			PM		PM	AM
3			AM		AM	AM	PM
4		AM	PM			PM	PM

15.5.2.2 Examples

The estimation procedure for the traditional access design is straightforward; effort and catch (kept and released) are expanded directly from angler interview data. Anglers provide the information on hours fished and the number of fish released, and the agent typically enumerates and identifies the harvest by direct observation. The general equations for direct expansion are presented in Section 15.3.3.2 as equations (15.1) and (15.2). Now some examples are presented to illustrate the estimation procedures.

One Access Site. A hypothetical fishery with one access site was sampled for 4 weeks in a month. The month was stratified into weekdays and weekend days. Both weekend days were sampled, and two weekdays were chosen randomly each week. The sampling schedule is given in Table 15.5. In this fishery, the fishing day is 12 hours long and the agent's effective (on-site) workday is 6 hours. Therefore the day was partitioned into 6-hour morning and afternoon periods, which were selected with equal probability ($\pi_i = 0.5$).

Table 15.6 shows the totals of trip durations in hours reported by all anglers within half-day work periods, and expansions of those data to a full day of fishing (half-day values divided by the sampling probability, 0.5). Similar calculations are done for catch in Table 15.7.

Mean daily fishing effort and catch first are calculated by stratum (Table 15.8). Next the totals are estimated by multiplying the means by the number of days in each stratum. For the weekday stratum, $N_1 = 20$ days, so total effort is $N_1\bar{e}_1 = 20 \times 16.5 = 330.0$. Overall total effort and catch are obtained by summing the stratum totals.

Table 15.6 Summary of daily effort calculations[a] for stratified random sampling in a traditional access point survey of a fishery with one access site. Data were collected for half-days (mornings or afternoons; see Table 15.5).

Week	Mon	Tue	Wed	Thu	Fri	Sat	Sun
			Trip hours recorded (half-day)				
1		5	7			33	10
2	8			9		35	15
3			11		3	36	21
4		7	16			43	18
			Estimated daily fishing effort (angler-hours)[a]				
1		10	14			66	20
2	16			18		70	30
3			22		6	72	42
4		14	32			86	36

[a]The daily effort estimates are recorded trips divided by 0.5, the selection probability for morning or afternoon.

Table 15.7 Summary of daily catch calculations[a] for stratified random sampling in a traditional access point survey with one access site. Data were collected for half-days (mornings or afternoons; see Table 15.5).

Week	Mon	Tue	Wed	Thu	Fri	Sat	Sun
			Number of fish recorded (half-day)				
1		4	2			15	20
2	5			6		18	10
3			7		3	21	15
4		2	11			28	11
			Estimated daily catch[a]				
1		8	4			30	40
2	10			12		36	20
3			14		6	42	30
4		4	22			56	22

[a]The daily catch estimates are obtained from the above figures by dividing by 0.5, the selection probability.

Because this is a stratified random sampling design with weekday and weekend strata and subsampling is based on time of day, it is not possible to estimate the second-stage variance component because only one period has been sampled in each primary sampling unit (day). Therefore, we obtain a conservative variance estimate based on the daily or primary unit estimates as described by equation (3.26) of Section 3.5. Here the equation is applied to each stratum separately. Only effort variances are shown, but catch variances can be calculated with the same equations.

For the weekday stratum,

$$\bar{e}_1 = 16.5,$$

$$n_1 = 8,$$

$$s_1^2 = \frac{1}{n_1 - 1}\sum_{i=1}^{n_1} (e_{1i} - \bar{e}_1)^2$$

$$= \frac{1}{7}[(10 - 16.5)^2 + (14 - 16.5)^2 + \ldots + (32 - 16.5)^2]$$

$$= 62.5714,$$

and therefore (from equation 3.26),

Table 15.8 Estimates of mean and total fishing effort ($\bar{e}$, $\hat{E}$) and catch ($\bar{c}$, $\hat{C}$), by stratum and overall, for stratified random sampling in a traditional access point survey with one access site. Sampling schedules and data are given in Tables 15.5–15.7.

Stratum or total	Sampling days	Sample mean	Total days	Stratum total	SE
Weekday stratum	$n_1 = 8$	$\bar{e}_1 = 16.5$	$N_1 = 20$	$\hat{E}_1 = 20 \times 16.5 = 330.0$	55.93
	$n_1 = 8$	$\bar{c}_1 = 10.0$	$N_1 = 20$	$\hat{C}_1 = 20 \times 10.0 = 200.0$	42.76
Weekend stratum	$n_2 = 8$	$\bar{e}_2 = 52.75$	$N_2 = 8$	$\hat{E}_2 = 8 \times 52.75 = 422.0$	67.07
	$n_2 = 8$	$\bar{c}_2 = 34.50$	$N_2 = 8$	$\hat{C}_2 = 8 \times 34.50 = 276.0$	33.09
Total fishing effort				$\hat{E} = \hat{E}_1 + \hat{E}_2 = 752.0$	87.33[a]
Total fishing catch				$\hat{C} = \hat{C}_1 + \hat{C}_2 = 476.0$	54.07[a]

[a]See text for how these standard errors are calculated.

$$\hat{\text{Var}}(\bar{e}_1) \approx \frac{s_1^2}{n_1}$$

$$\approx \frac{62.5714}{8}$$

$$\approx 7.8214.$$

Also

$$\hat{\text{Var}}(\hat{E}_1) \approx N_1^2 \, \hat{\text{Var}}(\bar{e}_1)$$

$$\approx 20^2 \times 7.8214$$

$$\approx 3{,}128.5600$$

and

$$\hat{\text{SE}}(\hat{E}_1) \approx \sqrt{3{,}128.5600}$$

$$\approx 55.93.$$

For the weekend stratum,

$$\bar{e}_2 = 52.75,$$

$$n_2 = 8,$$

$$s_2^2 = \frac{1}{n_2 - 1} \sum_{i=1}^{n_2} (e_{2i} - \bar{e}_2)^2$$

$$= \frac{1}{7}[(66 - 52.75)^2 + (20 - 52.75)^2 + \ldots + (36 - 52.75)^2]$$

$$= 562.2143,$$

and therefore

$$\hat{\text{Var}}(\bar{e}_2) \approx \frac{s_2^2}{n_2}$$

$$\approx \frac{562.2143}{8}$$

$$\approx 70.2768.$$

Also

$$\hat{\text{Var}}(\hat{E}_2) \approx N_2^2 \, \hat{\text{Var}}(\bar{e}_2)$$

$$\approx 8^2 \times 70.2768$$

$$\approx 4{,}497.7143$$

and

$$\hat{\text{SE}}(\hat{E}_2) \approx \sqrt{4{,}497.7143}$$

$$\approx 67.07.$$

The overall effort estimate and variance are

Table 15.9 Stratified random sampling schedule for a traditional access point survey with two access sites, A and B. Sites were selected with probabilities $\pi_A = 0.75$ and $\pi_B = 0.25$. Mornings and afternoons were selected with probabilities $\pi_{AM} = 0.4$ and $\pi_{PM} = 0.6$.

Week	Mon	Tue	Wed	Thu	Fri	Sat	Sun
1		A–PM	A–AM			A–PM	B–PM
2			A–AM	A–PM	A–PM	A–PM	A–AM
3				A–PM		A–PM	A–AM
4	B–AM		A–AM			B–PM	A–AM

$$\hat{E} = \hat{E}_1 + \hat{E}_2$$
$$= 330.0 + 422.0$$
$$= 752.0$$

and

$$\hat{\text{Var}}(\hat{E}) = \hat{\text{Var}}(\hat{E}_1) + \hat{\text{Var}}(\hat{E}_2)$$
$$\approx 3{,}128.5600 + 4{,}497.7143$$
$$\approx 7{,}626.2743,$$

with standard error

$$\hat{\text{SE}}(\hat{E}) \approx \sqrt{7{,}626.2743}$$
$$\approx 87.33.$$

Two Access Sites. Now we illustrate the calculation of effort (or catch) for a traditional access survey of a fishery with two access sites sampled for 1 month. Fishing effort was thought to be far heavier on weekend days than weekdays, and the month was stratified accordingly. Both weekend days and two weekdays were chosen randomly each week. Site A had three times more activity than site B, so site selection probabilities were set at $\pi_A = 0.75$ and $\pi_B = 0.25$. The fishing day is 12 hours long and the agent's workday is 6 hours. Each day was therefore partitioned into morning and afternoon periods of 6 hours. The afternoon had half again as much effort as the morning, and work periods were therefore selected with probabilities $\pi_{AM} = 0.4$ and $\pi_{PM} = 0.6$. (Selection probabilities also could have been based on expected catch or other variables of importance.) The sampling schedule is shown in Table 15.9, and the totals of the trip durations reported in work periods are given in Table 15.10. Successive adjustments of the raw data for the site selection and period probabilities yield estimates of daily fishing effort for the days sampled (Table 15.10). These data are then used to obtain total effort in each stratum and then overall effort for the month (Table 15.11) by the approach used in the previous example for one access site.

In the present example, selection probabilities for site (A, B) and work period (AM, PM) were handled separately to illustrate the calculations more clearly. However, the same results are achieved when both correction factors are applied simultaneously. For example, consider Tuesday of week 1. The site adjustment is $15/\pi_A = 15/0.75 = 20$, and the following period adjustment is $20/\pi_{PM} = 20/0.6 = 33.3$. Alternatively, $15/(\pi_A \times \pi_{PM}) = 15/(0.75 \times 0.6) = 15/0.45 = 33.3$, which is the same answer.

Table 15.10 Summary of daily fishing effort for stratified random sampling in a traditional access point survey with two access sites. Summed half-day trip durations (hours) were adjusted for site and then for work period probabilities (Table 15.9) to estimate daily effort.

Week	Site	Mon	Tue	Wed	Thu	Fri	Sat	Sun
				Measured half-day fishing effort (hours)				
1	A		15	7			37	
	B							15
2	A			9	16	30	59	22
	B							
3	A				19		75	33
	B							
4	A			10				27
	B	5					16	
				Half-day effort adjusted for site probabilities				
1	Combined		20.00	9.33			49.33	60.00
2	Combined			12.00	21.33	40.00	78.67	29.33
3	Combined				25.33		100.00	44.00
4	Combined	20.00		13.33			64.00	36.00
				Daily effort (adjusted for site and work period probabilities)				
1	Combined		33.33	23.33			82.22	100.00
2	Combined			30.00	35.55	66.67	131.12	73.33
3	Combined				42.22		166.67	110.00
4	Combined	50.00		33.33			106.67	90.00

15.5.3 Bus Route Method

The traditional access design can be too limited even when access is well defined. Some fisheries cover broad geographic areas and have lots of access sites, too many to be easily covered in a traditional access survey. If a month-long traditional survey encompasses 16 sampling days and a single creel clerk is responsible for 12 sites, some sites will be visited only once if sites have equal selection probabilities; with unequal selection probabilities, some sites may not be sampled at all. Any seasonal trend in effort and catch could be incorrectly attributed to site-to-site differences in activity.

The bus route design was developed specifically to be used with fisheries that have many access points and cover a broad geographic area (Robson and Jones 1989; Chapter 10). In the bus route design, an agent visits each site along a route for a portion of every day. Effort can be estimated in two ways: with a time interval count of anglers' cars present or with direct expansion from completed trip interviews. We present both these methods in the next section. See Section 10.3 for a more detailed discussion of the bus route method.

Table 15.11 Estimates of mean ($\bar{e}$) and total ($\hat{E}$) fishing effort (trip hours) for stratified random sampling in a traditional access point survey with two access points. Sampling schedules and data are given in Tables 15.9 and 15.10.

Stratum or total	Sampling days	Sample mean	Total days	Stratum total	SE
Weekday	$n_1 = 8$	$\bar{e}_1 = 39.3$	$N_1 = 20$	$\hat{E}_1 = 20 \times 39.3 = 786.0$	96.4
Weekend	$n_2 = 8$	$\bar{e}_2 = 107.5$	$N_2 = 8$	$\hat{E}_2 = 8 \times 107.5 = 860.0$	84.8
Total fishing effort				$\hat{E} = \hat{E}_1 + \hat{E}_2 = 1{,}646.0$	128.39[a]

[a]Calculated by the method developed for the example of one access point.

15.5.3.1 Estimation Procedures

Time Interval Count Method. Time interval counts (car counts) are used on fisheries where there is little fishing effort, where few interviews can be expected, and where the parked cars can be reasonably attributed to angling parties. For such fisheries, car counts yield more effort information than the traditional access design and consequently more precision for the same amount of sampling. The formula for the time interval method is

$$\hat{E} = T \sum_{i=1}^{n} \frac{1}{w_i} \sum_{j=1}^{m} \frac{e_{ji}}{\pi_j}; \qquad (15.10)$$

E = estimated total party-hours of effort;
T = total time to complete a full circuit of the route, including traveling and waiting;
w_i = waiting time at the ith site ($i = 1, \ldots, n$ sites);
e_{ji} = total time that the jth car is parked at the ith site while the agent is at that site ($j = 1, \ldots, m$ cars parked at site i).

The π_j is used to adjust for the sampling probability. For example, if the day were divided into AM and PM work periods with no overlap, and both were chosen with equal probability, then $\pi_j = 1/2$. However, if the afternoon period is sampled twice as often as the morning period, then $\pi_j = 2/3$ for interviews taken in the PM period, and $\pi_j = 1/3$ for interviews obtained in the AM period. If the interest is in angler-hours of effort, the estimate of party-hours must be multiplied by the mean number of anglers in a party, which is obtained by interviewing parties.

Direct Expansion Method. The second way to estimate effort in the bus route design is to expand effort directly from angler interviews, which is similar to the method used in the traditional access design. This approach is used when the fishery has high activity and interviews are plentiful or when parked cars may not belong exclusively to anglers. The angler-supplied trip duration is used in equation (15.10), in which e_{ji} now stands for the trip duration for the jth angler at the ith site.

The two methods for estimating effort could be combined in some fisheries, but more research is needed on the best way of combining them.

Catch Estimation. Catch is also calculated in two ways because of the differing effort estimators. When effort is obtained from time interval counts, catch is obtained by multiplying effort by the catch rate (catch per party-hour or angler-hour) obtained from the completed trip interviews. Total catch is

$$\hat{C} = \hat{E} \times \hat{R}_1,$$

and catch rate is

$$\hat{R}_1 = \bar{c}/\bar{L}.$$

When effort is estimated directly from interviews, catch is also calculated directly. In this case, C_{ji}, the total catch during the jth angler's trip at the ith site, is substituted for e_{ji} in equation (15.10):

Table 15.12 Bus route schedule for illustrating the time interval method of estimating effort and catch. The bus route covers six access points in a 6-hour (360-minute) survey day that begins at 6 AM. Travel time is the time to the next site; for site 6, it is the time to return to site 1.

Site	Scheduled wait time	Length of wait (min)	Travel time (min)
1	6:00–7:00	60	30
2	7:30–9:00	90	20
3	9:20–10:00	40	15
4	10:15–10:30	15	25
5	10:55–11:25	30	15
6	11:40–11:55	15	5
All		250	110

$$\hat{C} = T\sum_{i=1}^{n} \frac{1}{w_i} \sum_{j=1}^{m} \frac{c_{ji}}{\pi_j}. \tag{15.11}$$

15.5.3.2 Examples

Time Interval Counts. Here the method of estimating effort (and catch) with time interval counts of cars (plus interviews for catch rates) is illustrated. For simplicity, we limit the bus route to six access sites. The total time to complete the route is 6 hours, or half the fishing day. A random choice (i.e., with equal probabilities) is made of morning (first 6 hours) or afternoon (second 6 hours) periods for sampling. Table 15.12 gives the schedule for the bus route on a day when the morning period was chosen. The total route time (T) is 360 minutes (6 hours), of which 250 minutes are spent waiting at sites and 110 minutes are spent traveling between sites.

Table 15.13 shows the car count data obtained. The agent records the time that he or she sees an angler's car at the access site during the scheduled wait period. At site 1 the agent began the wait time at 6:00 AM, at which time two cars were already present. One of these left at 6:45 and the party was interviewed; the other was there at 7:00 when the agent left for the next site. The third car arrived at 6:15 and was still there when the agent left the site. When an agent arrives at a site and cars are already present, the start time is recorded as the time the wait period begins. If cars are still present when the agent departs from the site, the scheduled completion of the wait period is recorded as the finish time. It should now be obvious why the agent must adhere strictly to the predetermined schedule.

The formula for calculating total party effort from the time interval count is

$$\hat{E} = T \sum_{i=1}^{n} \frac{1}{w_i} \sum_{j=1}^{m} \frac{e_{ji}}{\pi_j}.$$

The calculations are set up in Table 15.14 and the components of the formula are expressed as minutes for ease of calculations. All total car count times in the first column are divided by 0.5 because π_j = 1/2, the equal probability of choosing morning or afternoon periods for the route. (Sometimes the π_j's will not be uniform for all interviews in the sampling day.) Next the adjusted car count times

Table 15.13 Hypothetical car count data along a six-site bus route followed according to the schedule in Table 15.12.

Site	Car	Arrival time	Departure time	Total time
1	1	6:15	7:00[a]	45
	2	6:00[b]	7:00	60
	3	6:00	6:45	45
2	1	8:30	9:00	30
3	1	9:20	9:30	10
4	0			0
5	1	10:55	11:25	30
	2	11:10	11:25	15
6	1	11:40	11:55	15

[a]Cars still present when the agent leaves a site are recorded as departing at the agent's departing time.
[b]Cars present when the agent reaches a site are recorded as arriving at the agent's arrival time.

are divided by the wait times to obtain the final column. These numbers are summed and then multiplied by the total route time $T = 360$ minutes to obtain $\hat{E} = 4{,}021.2$ minutes, which (divided by 60 minutes/hour) is 67.02 party-hours.

An estimate of angler-hours is obtained by multiplying the estimate of party-hours by the average number of anglers in a party. The number of anglers in a party can be averaged for each day or over a longer time period, such as the fishing season. The advantage to averaging over the season is that the variance will be minimized. In this example, the average party size for the day was 2.5 anglers. The estimate of angler-hours is therefore $67.02 \times 2.5 = 167.55$.

The calculation of catch relies on obtaining catch rate information from the interviews that occur. No interviews can be obtained for anglers who do not return to their cars before the agent leaves the site. The daily catch rate is multiplied by the estimate of daily total party effort. For this example, the catch rate was 1.2 fish per party-hour, so the estimate of this day's total catch is

$$\hat{C} = \hat{E} \times \hat{R}_1 = 67.02 \times 1.2 = 80.4 \text{ fish.}$$

Direct Expansion. Here we illustrate the direct expansion method for 1 day of data from a bus route survey of five access sites. The total time to complete the

Table 15.14 Calculation of total fishing effort ($\hat{E}$) by the time interval method for a bus route design with six access points.

Site	Σe_{ji} (min)[a]	$\Sigma e_{ji}/\pi_j$ (min)[b]	w_i (min)[c]	$(1/w_i)\Sigma e_{ji}/\pi_j$ (min)	$\hat{E}$ (min)[d]
1	150	300	60	5.00	
2	30	60	90	0.67	
3	10	20	40	0.50	
4	0	0	15	0.00	
5	45	90	30	3.00	
6	15	30	15	2.00	
All				11.17	4,021.20

[a]Car presence time, summed over j cars at the site, from Table 15.3.
[b]Summed car presence time weighted by the probability of sampling morning or afternoon work periods; $\pi_j = 0.5$, by survey design.
[c]Waiting time at site i, from Table 15.12.
[d]Total estimated effort, $T\Sigma(1/w_i)\Sigma e_{ji}/\pi_j = 360 \times 11.17 = 4{,}021.20$; $T = 360$ min is the length of the daily survey period.

Table 15.15 Effort and catch by anglers interviewed along a five-site bus route, to illustrate the direct expansion method for estimating total effort and catch.

Site	Party code	Number of anglers	Start time	Finish time	Trip duration (min)	Catch (number of fish)
1	1	2	6:15	8:30	135	3
	2	1	6:00	8:45	165	2
2	3	2	6:30	9:25	175	0
	4	3	6:00	9:35	215	1
3		0				
4	5	1	8:00	10:30	150	1
	6	2	8:15	10:40	145	2
	7	2	8:00	10:48	168	3
5	8	1	8:00	11:45	225	1

route is 6 hours, or half the fishing day. Morning or afternoon routes were selected randomly and with equal selection probabilities ($\pi_j = 0.5$). These are straightforward calculations and quite similar to those of the traditional access design. The anglers' trip durations are obtained from the anglers themselves during interviews (Table 15.15). For the time interval count method, we used the symbol e_{ji} to indicate the time that the agent sees the car present at the access site. In the direct expansion method, we use e_{ji} to represent trip duration for the *j*th angler. In essence, this is the only difference in how these calculations proceed (Table 15.16). To calculate catch with direct expansions, we use the symbol c_{ji} to represent the catch as counted by the agent or supplied by the *j*th angler, and proceed as above (Table 15.17).

Multiple-Day Estimates and Variances. Once estimates are obtained by either method for multiple days, the methodology for getting overall estimates and variances follows the same approach as for the traditional access design (Section 15.5.2.2; Table 15.8). An example of this will not be repeated here.

15.5.4 Discussion

The access–access design is commonly used when public access is well defined and private access off limits to survey agents is absent or minor. The traditional access design remains one of the easiest designs to use both logistically and in

Table 15.16 Calculation of total fishing effort ($\hat{E}$) by direct expansion for a bus route design with five sites. Notation is as in Table 15.14, except e_{ji} now denotes fishing duration for the *j*th interviewed angler at site *i*.

Site	Σe_{ji} (min)	$\Sigma e_{ji}/\pi_j$	w_i (min)	$(1/w_i)\Sigma e_{ji}/\pi_j$ (min)	$\hat{E}$ (min)[a]
1	300	600	30	20.00	
2	390	780	30	26.00	
3	0	0	15	0.0	
4	463	926	60	15.43	
5	225	450	60	7.50	
All				68.93	24,814.80

[a] $\hat{E} = T\,\Sigma(1/w_i)\Sigma e_{ji}/\pi_j$; $\pi_j = 0.5$ and $T = 360$ minutes, the length of the daily survey period.

Table 15.17 Calculation of total catch ($\hat{C}$, number of fish) by direct expansion for a bus route design with five sites. Notation is as in Table 15.14, except c_{ji} is the catch by the *j*th angler at site *i*.

Site	Σc_{ji}	$\Sigma c_{ji}/\pi_j$	w_i (min)	$(^1/w_i)\ \Sigma c_{ji}/\pi_j$	$\hat{C}$
1	5	10	30	0.333	
2	1	2	30	0.067	
3	0	0	15	0.000	
4	6	12	60	0.200	
5	1	2	60	0.033	
All				0.633	227.88

calculation. It is straightforward in concept and can usually be implemented without error even by people with little statistical training. However, it is not useful when access sites are numerous and have broad geographical range. It also may suffer because the variance estimation is sensitive to rare events at sites that are considered to have low usage and are given small sampling probabilities.

The bus route design provides an alternative to the traditional access design. We encourage its use when there are many access sites in the fishery. It is particularly useful in fisheries with low effort, because time interval car counts provide data when few interviews can be obtained. Agencies that have used the design have commented on its relative ease of implementation, although it does require more preparation (in developing the route schedules) than is required by the traditional access design. The bus route design is less useful when the travel time between sites is great, because that time is wasted for interviews and the outcome is high variances of effort and catch estimates.

Access-based designs have some disadvantages. When access is poorly defined, it is difficult to put together a comprehensive and complete site list, which is necessary for building the sampling frame. In fisheries where anglers can walk anywhere along the water body, access is so ill defined that the access method is worthless in estimating effort. Also, when anglers emanating from private access (docks and piers at private residences or inaccessible marinas) make up a substantial proportion of anglers, their unavailability to access point clerks will cause a marked underestimation of fishing effort. Catch rate estimates can be biased when private-access anglers have different fishing patterns from public-access anglers. In such cases it may be wise to consider another survey design such as the roving survey, which is discussed next.

15.6 ROVING EFFORT DESIGNS

15.6.1 Roving–Roving Design

15.6.1.1 Introduction

The roving–roving angler survey design is commonly applied to estimate effort and catch in diffuse-access or private-access fisheries. A roving creel clerk intercepts anglers while they are fishing and asks for the time spent fishing and the numbers of fish caught and harvested. The number of anglers fishing is obtained from by a count taken at a random time during the work period, and total effort is obtained from the product of count times number of hours in the work period.

Catch rate (catch/hour) is averaged over interviewed anglers to obtain an overall catch rate. Total catch is obtained by multiplying the total effort by the overall catch rate (Section 15.3.3).

This design has two assumptions that are difficult to meet in practice: interviews of anglers to obtain catch information do not disrupt the counting process; and the catch rate estimated for the incomplete trip up to the time of interview is an unbiased estimate of the catch rate for the complete trip, which cannot be determined with this design. Violation of these assumptions may cause serious bias in estimates.

In the following sections, we consider the count methodology (instantaneous or progressive) and discuss the use of incomplete interviews to estimate catch rate. Practical issues and estimation methods precede several detailed examples. We close with a discussion of the strengths and weaknesses of the roving–roving design.

15.6.1.2 Description of the Method

In a roving–roving survey, the survey agent is on site and moves through the fishery counting and interviewing anglers while they are in the act of fishing. Typically the method is used when access to the fishery is physically ill-defined, and a substantial portion of the anglers do not use public-access sites such as piers, boats, ramps, state parks, or marinas. Where anglers can simply walk on to the fishery at many points along a shore or streambank, the roving–roving design may be the only way to sample the fishery. Where the survey agent does not have the right to visit private docks and marinas, the roving–roving design again may be the only alternative; anglers sampled in the act of fishing can be counted and interviewed regardless of where they originated.

Effort from a roving creel survey can be measured in two variations: with an *instantaneous* count or with a *progressive* count (Table 15.18). The instantaneous count method is of the same nature as an aerial count. The count can be made (quickly) from an airplane, helicopter, fast automobile, or fast boat or from a vantage point (hill, tower, bridge, etc.). The only requirement is that the agent moves through the fishery as quickly as possible counting *all* the anglers; no interviews are conducted during the count. The count is converted to effort in angler-hours or party-hours: effort = count of anglers (parties) × number of hours in the fishing period. Sometimes several counts may be taken at random times in the period and averaged to increase precision of the effort estimate. The instantaneous count method is unbiased if no anglers are missed due to visibility problems.

Effort can also be estimated with a progressive count, which may last for a substantial portion of the work period or even for the entire survey day. If the progressive count is made without interviewing anglers (as, for example, in an aerial survey) and if the starting point and direction of travel are randomized, the count is unbiased like an instantaneous count (Neuhold and Lu 1957). Except in aerial surveys, however, the progressive count is typically made concurrently with interviewing (the so-called "count-as-you-go" method), and then it gives a biased estimate of fishing effort. While the agent is interviewing an angler, he or she is unavailable to count or interview another angler who may be fishing elsewhere. The interview, in essence, casts a "shadow" that decreases the probability of counting anglers who are on the fishery and who would have been

Table 15.18 Variations of the roving–roving design for estimation of effort and catch rate.

Parameter estimated	Method variant: Instantaneous count	Progressive count, no interviews[a]	Progressive count, interviews[b]	Progressive count, interviews, checkpoint[c]
Effort	Unbiased	Unbiased	Negatively biased	Minimum bias
Catch	Unbiased[d]	Unbiased[e]	Negatively biased	Minimum bias

[a]Interviews are conducted separately from the count.
[b]Interviews are conducted during the count (count-as-you-go method).
[c]Checkpoints are used to keep the agent on schedule throughout the day.
[d]Catch is unbiased if other assumptions such as no length-of-stay bias hold.
[e]If the progressive count is aerial, catch rate and hence catch will depend on ground interviews.

counted with a truly instantaneous count. Wade et al. (1991) showed that this results in a potentially severe underestimate of fishing effort; the magnitude of the underestimate depends on the length of the interview time and on the number of anglers in the fishery (which is almost never known with progressive counts). The bias is measurable even when the interview length is as short as 5 minutes.

We recommend, therefore, that a progressive count *not* be combined with interviewing because of the inherent bias in the method. The best procedure is to obtain interviews between the random-start instantaneous or progressive counts. If doing a progressive count while interviewing is unavoidable, some correction can be made to the estimates to minimize the inherent bias (Wade et al. 1991) if a time schedule is established for the agent with frequent checkpoints along the route; the checkpoints keep the agent on schedule. Agents frequently try to maximize the number of interviews; thus they slow down during busy times and speed up when few anglers are encountered. This approach produces a maximum likelihood of shadowing effects and results in a severe undercount of anglers. If the agent is kept on schedule with checkpoints, this bias is markedly reduced, because the agent is forced to skip interviews and simply count at intervals throughout the whole day rather than just at the end. Fewer interviews are obtained, but the estimate of effort is more nearly unbiased. The checkpoints could be chosen to occur at, say, 2-hour intervals. To do this scheduling correctly, the agents will need to know the fishery well and to plan ahead. Preliminary simulations (Wade et al. 1991) have shown that the bias can be made very small with 2-hour checkpoints in an 8-hour survey period.

With the instantaneous count method, it is easy to select a random time to make the count because the count is efficiently made in what is essentially an instant. With the progressive count method, obtaining a count at random requires more thought (Hoenig et al. 1993). When the count lasts an appreciable time, the best practice is to divide the day into segments (equal to the duration of the actual count) starting at the beginning of the fishing day. The agent randomly selects the segment in which to do the count from those available each day. If more than one count is to be made, the agent will randomly select the next time period from those that remain. The agent also should randomly select the starting location and the travel direction (clockwise or counterclockwise).

The incomplete trip interviews are used to estimate catch rate, as discussed in detail in Section 15.3.3. The assumption that these catch rates are unbiased estimates of catch rate over the entire trip is crucial. On average, the agent will interview anglers halfway through the fishing trip. Total catch is obtained by total effort times catch rate, and it could be substantially biased if catch rate varies markedly and systematically through the fishing trip. Total catch will also be biased in the count-as-you-go approach, because shadowing biases the estimate of total effort, which is used to calculate total catch.

15.6.1.3 Practical Issues

The actual implementation of a roving–roving creel survey is complex and variable due to the wide range of fisheries to be sampled. The counting part of the survey could be carried out by roving through the area in a boat or automobile or on foot. Even better (if practical) is to count all anglers from a vantage point (hill, bridge, etc.). The interviewing part of the survey also can be done by roving boat or automobile or on foot. Boat surveys are commonly used on lakes and larger streams. Automobiles are sometimes used on streams where there is a road parallel to the water. Agents on foot are not commonly used except when there is no alternative. On a trout stream, for example, the only practical method may be to have an agent walk up and down the stream, counting and interviewing in the process. Any shore- or bank-based fishery with diffuse access, such as an ocean beach fishery, might have to be treated similarly. Then, it is very important for the agent to use check points to keep on schedule.

Fisheries that involve both boat and bank anglers create some special problems. A roving agent in a boat could count and interview boat anglers while they are fishing, but may have trouble interviewing the bank anglers or even counting them all if the shoreline vegetation is dense. Sometimes it is necessary to run separate surveys of bank and boat anglers with different methods, even though this is very expensive.

Roving night surveys also may be problematical. Anglers are difficult to count in the dark, and agents walking, driving, or riding in boats and stopping anglers for interviews may encounter very dangerous situations. We noted earlier that telephone and mail surveys could indirectly get at night fishing, and access point surveys may be practical if access points are well lighted and safe from crime. (For other practical considerations see Chapter 11.)

The sampling designs for roving surveys are usually complex. Stratification variables often include date, kind of day (weekend, weekday), and area, and there is often a need to subsample different times of the day and perhaps area. The subsampling may often use nonuniform (unequal) probability (see also Section 15.6.1.5).

15.6.1.4 Estimation

In the roving–roving design, effort (for a fishing period) is estimated by

$$\hat{e}_i = I_i \times T,$$

which is the instantaneous count (I_i) of anglers or parties at time i multiplied by the length of the fishing period (T), which makes the units angler-hours or party-hours. Total effort for a survey period is

$$\hat{E} = \sum_{i=1}^{n} (\hat{e}_i/\pi_i),$$

Table 15.19 Instantaneous angler counts (I) and daily effort (e) calculations for a stratified random sampling, roving–roving angler survey on a small lake. The survey day is 12 hours long.

Date or statistic	Three counts	Average ($\bar{I}_i$)	Daily effort ($\hat{e}_i$)
		Weekdays	
Feb 7	3, 6, 5	4.67	4.667 × 12 = 56
Feb 17	3, 1, 6	3.33	3.333 × 12 = 40
Feb 19	4, 3, 10	5.67	5.667 × 12 = 68
Feb 26	0, 1, 3	1.33	1.333 × 12 = 16
Feb 27	4, 5, 4	4.33	4.333 × 12 = 52
Mean ($\bar{e}_1$)			46.40
		Weekend days	
Feb 1	10, 14, 20	14.67	14.667 × 12 = 176
Feb 2	21, 15, 5	13.67	13.667 × 12 = 164
Feb 9	12, 14, 11	12.33	12.333 × 12 = 148
Feb 22	15, 10, 17	14.00	14.000 × 12 = 168
Feb 23	20, 22, 19	20.33	20.333 × 12 = 244
Mean ($\bar{e}_2$)			180.00

where π_i is the total probability that fishing period i is included in the sample. This probability might include the probabilities of choosing time of day, area, and day. Total probability is obtained by multiplying the individual probabilities.

Catch is estimated by

$$\hat{C} = \hat{E} \times \hat{R}_2,$$

where $\hat{R}_2$ is the catch rate calculated from incomplete trips,

$$\hat{R}_2 = \frac{\sum_{i=1}^{n} (c_i/L_i)}{n},$$

by taking the average of the individual catch rates (c_i/L_i) for each angler. We illustrate these equations further in the examples that follow.

15.6.1.5 Examples

Whole Day Sampled. Consider a hypothetical fishery sampled for 1 month (1 period) in February. The design was used as an illustration in Chapter 3 and is described in Figure 3.3. Stratified random sampling is used in a roving–roving angler survey design for a boat-based fishery on a small lake. The samples are $n_1 = 5$ of the $N_1 = 20$ weekdays and $n_2 = 5$ of the $N_2 = 9$ weekend days. The weekends are sampled at a relatively higher rate because fishing pressure is likely to be higher then. The fishing day is from 6 AM to 6 PM (12 hours) and the agent works all 12 hours on each sampling day. There is a hill summit that gives a clear view of the whole lake so that truly instantaneous counts can be taken. During each sampling day, three randomly timed instantaneous counts are taken. For the remainder of the day the agent roves through the fishery in a boat interviewing anglers to learn their catch and how long they have been fishing during their incomplete trip.

Table 15.19 shows the instantaneous counts by day together with their average. To obtain the estimated daily effort ($\hat{e}_i$), we use equation (15.4) and multiply the average instantaneous count by the length of the fishing day, which in this case is 12 hours. Next we present the calculations for total effort by stratum and then overall:

$$\begin{aligned}\hat{E}_1 &= N_1\bar{e}_1 \\ &= 20 \times \bar{e}_1 \\ &= 20 \times 46.40 \\ &= 928 \text{ angler-hours.}\end{aligned} \qquad \begin{aligned}\hat{E}_2 &= N_2\bar{e}_2 \\ &= 9 \times \bar{e}_2 \\ &= 9 \times 180.00 \\ &= 1{,}620 \text{ angler-hours.}\end{aligned}$$

Therefore, overall total effort is

$$\begin{aligned}\hat{E} &= \hat{E}_1 + \hat{E}_2 \\ &= 928 + 1{,}620 \\ &= 2{,}548 \text{ angler-hours.}\end{aligned}$$

This approach uses the results on stratified random sampling given in Section 3.3.2 and is based on equation (3.12). An alternative approach is to use equation (15.5) directly. Note that $\pi_1 = n_1/N_1 = 5/20$ for all counts in the weekday stratum and $\pi_2 = n_2/N_2 = 5/9$ for all counts in the weekend stratum:

$$\begin{aligned}\hat{E} &= \sum_{i=1}^{n} (\hat{e}_i/\pi_i) \\ &= \frac{(\hat{e}_1 + \hat{e}_2 + \ldots + \hat{e}_5)}{(5/20)} + \frac{(\hat{e}_1 + \hat{e}_2 + \ldots + \hat{e}_5)}{(5/9)} \\ &= 20\,\bar{e}_1 + 9\,\bar{e}_2 \\ &= \hat{E}_1 + \hat{E}_2 \\ &= \hat{E}, \text{ as before.}\end{aligned}$$

The similar calculations for catch are based on daily catch calculations in Table 15.20, which presents catch rates based on incomplete trips:

$$\begin{aligned}\hat{C}_1 &= N_1\bar{c}_1 \\ &= 20 \times \bar{c}_1 \\ &= 20 \times 93.70 \\ &= 1{,}874.00.\end{aligned} \qquad \begin{aligned}\hat{C}_2 &= N_2\bar{c}_2 \\ &= 9 \times \bar{c}_2 \\ &= 9 \times 446.38 \\ &= 4{,}017.42.\end{aligned}$$

Therefore, the overall total catch is

$$\begin{aligned}\hat{C} &= \hat{C}_1 + \hat{C}_2 \\ &= 1{,}874.00 + 4{,}017.42 \\ &= 5{,}891.42 \text{ fish.}\end{aligned}$$

The variance calculations are presented in Table 15.21 for effort and Table 15.22 for catch. The method is similar to that used in Section 15.5.2.2.

Subsampling Parts of a Day. A hypothetical fishery sampled for 1 month (1 period) in February is now considered. The design was used as an illustration in Chapter 3 and is described in Figure 3.6. As in the first example, a stratified random sampling design for a roving–roving angler survey of a boat-based fishery

Table 15.20 Daily catch calculations (c, daily effort × daily catch rate) for a stratified random sampling, roving–roving angler survey on a small lake.

Date or statistic	Daily effort	Daily catch rate[a]	Daily catch ($\hat{c}_i$)
		Weekdays	
Feb 7	56	1.91	56 × 1.91 = 106.96
Feb 17	40	2.32	40 × 2.32 = 92.80
Feb 19	68	1.82	68 × 1.82 = 123.76
Feb 26	16	2.53	16 × 2.53 = 40.48
Feb 27	52	2.01	52 × 2.01 = 104.52
Mean ($\bar{c}_1$)			93.70
		Weekend days	
Feb 1	176	2.15	176 × 2.15 = 378.40
Feb 2	164	2.36	164 × 2.36 = 387.04
Feb 9	148	1.87	148 × 1.87 = 276.76
Feb 22	168	2.71	168 × 2.71 = 455.28
Feb 23	244	3.01	244 × 3.01 = 734.44
Mean ($\bar{c}_2$)			446.38

[a]We use the average of the individual catch rates for each angler for each day, and we ignore all short trips (less than 0.5 hour).

Table 15.21 Effort variances and standard errors for a stratified random sampling, roving–roving angler survey on a small lake.

Weekdays

$n_1 = 5;\ \bar{e}_1 = 46.40$

$$s_1^2 = \frac{1}{n_1 - 1}\sum_{i=1}^{n_1}(e_{i1} - \bar{e}_1)^2$$

$$= \frac{1}{4}[(56 - 46.40)^2 + \ldots + (52 - 46.40)^2]$$

$$= 388.80$$

$$\hat{\text{Var}}(\bar{e}_1) = \frac{s_1^2}{n_1} = \frac{388.8}{5} = 77.76$$

$$\hat{\text{Var}}(\hat{E}_1) = N_1^2\hat{\text{Var}}(\bar{e}_1) = 20^2 \times 77.76 = 31{,}104.00$$

$$\hat{\text{SE}}(\hat{E}_1) = \sqrt{31{,}104} = 176.36$$

Weekend days

$n_2 = 5;\ \bar{e}_2 = 180$

$$s_2^2 = \frac{1}{n_2 - 1}\sum_{i=1}^{n_2}(e_{i2} - \bar{e}_2)^2$$

$$s_2^2 = \frac{1}{4}[(176 - 180)^2 + \ldots + (244 - 180)^2]$$

$$= 1384.00$$

$$\hat{\text{Var}}(\bar{e}_2) = \frac{s_2^2}{n_2} = \frac{1384.0}{5} = 276.80$$

$$\hat{\text{Var}}(\hat{E}_2) = N_2^2\hat{\text{Var}}(\bar{e}_2) = 9^2 \times 276.8 = 22{,}420.80$$

$$\hat{\text{SE}}(\hat{E}_2) = \sqrt{22{,}420.8} = 149.74$$

Total

$$\hat{\text{Var}}(\hat{E}) = \hat{\text{Var}}(\hat{E}_1) + \hat{\text{Var}}(\hat{E}_2) = 31{,}104.00 + 22{,}420.80 = 53{,}524.80$$

$$\hat{\text{SE}}(\hat{E}) = \sqrt{53{,}524.80} = 231.35$$

Table 15.22 Catch variances and standard errors for a stratified random sampling, roving–roving angler survey on a small lake.

Weekdays	Weekend days
$n_1 = 5$	$n_2 = 5$
$\bar{c}_1 = 93.70$	$\bar{c}_2 = 446.38$
$s_1^2 = \frac{1}{n_1 - 1}\sum_{i=1}^{n_1}(c_{i1} - \bar{c}_1)^2$	$s_2^2 = \frac{1}{n_2 - 1}\sum_{i=1}^{n_2}(c_{i2} - \bar{c}_2)^2$
$= 1{,}007.42$	$= 29{,}992.81$
$\hat{\text{Var}}(\bar{c}_1) = \frac{s_1^2}{n_1}$	$\hat{\text{Var}}(\bar{c}_2) = \frac{s_2^2}{n_2}$
$= 201.484$	$= 5{,}998.56$
$\hat{\text{Var}}(\hat{C}_1) = N_1^2\hat{\text{Var}}(\bar{c}_1)$	$\hat{\text{Var}}(\hat{C}_2) = N_2^2\hat{\text{Var}}(\bar{c}_2)$
$= 20^2 \times 201.484$	$= 9^2 \times 5{,}998.56$
$= 80{,}593.600$	$= 485{,}883.36$
$\hat{\text{SE}}(\hat{C}_1) = \sqrt{80{,}593.600}$	$\hat{\text{SE}}(\hat{C}_2) = \sqrt{485{,}883.36}$
$= 283.89$	$= 697.05$

$$\hat{\text{Var}}(\hat{C}) = \hat{\text{Var}}(\hat{C}_1) + \hat{\text{Var}}(\hat{C}_2)$$
$$= 80{,}593.60 + 485{,}883.36$$
$$= 566{,}476.96$$
$$\hat{\text{SE}}(\hat{C}) = \sqrt{566{,}476.96}$$
$$= 752.65$$

on a small lake is considered. The samples are $n_1 = 5$ of the $N_1 = 20$ weekdays and $n_2 = 5$ of the $N_2 = 9$ weekend days. We assume that the fishing day lasts from 6 AM to 6 PM (12 hours), but that the agent can only work 4 hours a day. The workday is divided up into work periods of equal lengths, denoted early (6 AM–10 AM), middle (10 AM–2 PM), and late (2 PM–6 PM). On a particular sampling day, only one of the work periods is sampled. These secondary sampling units are sampled with nonuniform probabilities: early, 0.2; middle, 0.3; and late, 0.5. These probabilities are chosen to roughly reflect the expected relative amounts of fishing pressure in different periods of the total fishing day. We assume again that a hill summit gives a clear view of the whole lake so that truly instantaneous counts can be taken. During each sampled work period (e.g., February 7, 2 PM–6 PM), three randomly timed instantaneous counts are taken. For the remainder of the work period, the agent roves through the fishery in a boat, interviewing anglers to obtain their catches and their fishing times during their incomplete trips.

Table 15.23 presents the average instantaneous counts by period and the daily effort calculations. These are converted to period effort by multiplying average daily counts by the period length (4 hours). We convert these values to daily effort by dividing by the probability that the period is included in the sample (early, 0.2; middle, 0.3; late, 0.5). In Table 15.24, daily catch estimates are based on daily catch = daily effort × daily catch rate. In Table 15.25 we present the results for

Table 15.23 Daily instantaneous counts of anglers (I) and daily effort calculations (e) for a stratified random sampling, roving–roving angler survey on a small lake. The secondary sampling units are sampled with nonuniform probabilities (early, 0.2; middle, 0.3; late, 0.5). The daily sampling period is 4 hours.

Date or statistic	Period	Average count (I_i)[a]	Period effort	Daily effort ($\hat{e}_i$)
		Weekdays		
Feb 7	Late	4.50	4.50 × 4 = 18.00	18.00/0.5 = 36.00
Feb 17	Late	2.33	2.33 × 4 = 9.33	9.33/0.5 = 18.66
Feb 19	Middle	1.67	1.67 × 4 = 6.67	6.67/0.3 = 22.23
Feb 26	Late	1.33	1.33 × 4 = 5.33	5.33/0.5 = 10.67
Feb 27	Middle	2.67	2.67 × 4 = 10.67	10.67/0.3 = 35.57
Mean ($\bar{e}_1$)				24.63
		Weekend days		
Feb 1	Early	7.00	7.00 × 4 = 28.00	28.00/0.2 = 140.00
Feb 2	Middle	13.67	13.67 × 4 = 54.67	54.67/0.3 = 182.23
Feb 9	Early	7.67	7.67 × 4 = 30.67	30.67/0.2 = 153.35
Feb 22	Middle	14.00	14.00 × 4 = 56.00	56.00/0.3 = 186.67
Feb 23	Middle	18.33	18.33 × 4 = 73.33	73.33/0.3 = 244.43
Mean ($\bar{e}_2$)				181.34

[a]Average of three instantaneous counts taken in the particular period.

total effort and total catch by stratum and then overall, as well as standard errors based on the same method used in the previous example.

15.6.1.6 Discussion

The roving–roving design, despite some serious drawbacks, is probably the most widely used on-site creel survey design. Off-site methods rely completely on angler-supplied information. The other on-site approach—access point surveys—has major practical limitations for fisheries with diffuse access or private access points not available to agents.

We encourage fisheries biologists, however, to have less faith in the assumptions of this design. Effort estimates can have severe negative biases when

Table 15.24 Daily catch calculations (daily catch = daily effort × daily catch rate) for a stratified random sampling, roving–roving angler survey on a small lake.

Date or statistic	Daily effort	Daily catch rate[a]	Daily catch
		Weekdays	
Feb 7	36.00	2.92	36.00 × 2.92 = 105.12
Feb 17	18.76	3.33	18.76 × 3.33 = 62.47
Feb 19	22.23	2.83	22.23 × 2.83 = 62.91
Feb 26	10.67	3.54	10.67 × 3.54 = 37.77
Feb 27	35.57	3.02	35.57 × 3.02 = 107.42
Mean ($\bar{c}_1$)			75.14
		Weekend days	
Feb 1	140.00	3.16	140.00 × 3.16 = 442.40
Feb 2	182.23	3.37	182.23 × 3.37 = 614.12
Feb 9	153.35	2.86	153.35 × 2.86 = 438.58
Feb 22	186.67	3.74	186.67 × 3.74 = 698.15
Feb 23	244.43	4.09	244.43 × 4.09 = 999.72
Mean ($\bar{c}_2$)			638.59

[a]We used the average of the individual catch rates for each angler for each day; all short incomplete trips (less than 0.5 hour) were ignored.

Table 15.25 Total effort and total catch estimates for weekday and weekend strata and overall for a stratified sampling, roving–roving angler survey on a small lake. Standard errors are also presented.

Stratum	Variable	Estimate	SE
Weekday	Effort	492.60	98.51
	Catch	1,502.78	270.09
Weekend day	Effort	1,632.06	162.88
	Catch	5,747.33	928.91
Overall	Effort	2,124.66	189.84
	Catch	7,250.11	967.39

interviews are conducted during progressive counts, unless scheduled checkpoints are used. Incomplete trip data are vulnerable to length-of-stay bias, and catch rates estimated with roving surveys should be checked more often against complete trip data for the same fishery (or even the same anglers). Current evidence suggests that length-of-stay bias ranges from negligible to substantial.

15.6.2 Roving–Access Design

15.6.2.1 Description of the Method

At first glance it would seem that a design that uses roving counts for effort would automatically use roving interviews for catch rate estimates. However, as noted in the previous section, use of incomplete trips to obtain catch rate estimates is not without problems. Interviews during progressive counts can distort effort estimates, and length-of-stay bias may influence catch rate estimates themselves. In some cases it makes more sense to obtain instantaneous or progressive counts to estimate effort but to interview for catch rate data at access points: the roving–access design.

Palsson (1991) described surveys of localized fisheries in channels of Puget Sound, Washington. Complete counts of boats in two such fisheries could be made from a bridge and a hill. After counts were made, agents moved to nearby marinas, where they obtained catch and other data from returning parties. Even when counts may require roving through the fishery by boat or automobile or on foot, this design could be useful because it takes advantage of completed trip interviews for catch rate data.

In this design, effort (for a fishing period i) is estimated by

$$\hat{e}_i = I_i \times T,$$

which is the instantaneous count (I_i) of anglers or parties multiplied by the length of the fishing period with units of angler- or party-hours. Total effort for a survey period is obtained by expansion:

$$\hat{E} = \sum_{i=1}^{n} (\hat{e}_i/\pi_i);$$

π_i is the total probability that fishing period i is included in the sample. This probability might include the probabilities of choosing time of day, area, and day. Total probability is obtained by multiplying the individual selection probabilities.

Table 15.26 Sampling schedule for a stratified random sampling, roving–access survey on a river. The survey lasted 4 weeks and two sites were covered with morning and afternoon work periods.

Week	Site	Mon	Tue	Wed	Thu	Fri	Sat	Sun
1	A				PM			
	B							
2	A						AM	
	B			AM				AM
3	A				PM			
	B							PM
4	A		AM					
	B						PM	

Catch is estimated by

$$\hat{C} = \hat{E} \times \hat{R}_1,$$

where $\hat{R}_1$ is the catch rate calculated from complete trips,

$$\hat{R}_1 = \sum_{i=1}^{n} c_i / \sum_{i=1}^{n} L_i,$$

which is the sum of the catches divided by the sum of the trip lengths.

15.6.2.2 Example

This example is for a fishery on a moderate-size river with some private access and two public access sites. A previous mail survey had found that catch rates for users of public and private access points were similar, indicating that interviews obtained at public sites would accurately represent the fishery. This fishery lent itself to use of a roving–access design. The roving design was used to obtain estimates of effort, and completed trip information on catch rates was obtained from the access portion of the survey.

The example covers 1 month of sampling. The month was stratified into weekdays and weekend days, and 4 days were sampled in each stratum. The two public sites (A and B) were sampled for catch rate data with unequal probabilities: $\pi_A = 0.6$ and $\pi_B = 0.4$. The agent worked 6 hours each sampling day in either the morning or the afternoon, for which the unequal sampling probabilities were $\pi_{AM} = 0.3$ and $\pi_{PM} = 0.7$. The sampling schedule for this design is presented in Table 15.26.

A data summary of the roving portion of the survey for effort is presented in Table 15.27. The instantaneous counts were conducted on the entire water body and were independent of the site selected for the intercept portion of the survey. To calculate daily effort, the instantaneous counts are averaged, then expanded to work period effort, and then expanded again to a full day by dividing by the selection probabilities. Daily catch calculations (daily catch = daily effort × daily catch rate) are presented in Table 15.28. We decided to calculate catch at the daily level, rather than for a longer period of time, because it is easy to calculate the variance between days. In Table 15.29, total effort and total catch estimates are

Table 15.27 Calculation of daily effort (e, hours) for a stratified random sampling, roving–access survey of a river. Daily effort calculations based on expanding instantaneous counts.

Day and week or statistic	Period	Three counts	Average count ($\bar{I}_i$)	Work period effort[a]	Daily effort ($\hat{e}_i$)[b]
			Weekdays		
Thu–Week 1	PM	2, 6, 3	3.67	3.67 × 6 = 22.00	22.00/0.7 = 31.43
Wed–Week 2	AM	5, 1, 1	2.33	2.33 × 6 = 14.00	14.00/0.3 = 46.67
Thu–Week 3	PM	6, 4, 3	4.33	4.33 × 6 = 26.00	26.00/0.7 = 37.14
Tue–Week 4	AM	2, 6, 7	5.00	5.00 × 6 = 30.00	30.00/0.3 = 100.00
Mean ($\bar{e}_1$)					53.81
			Weekend days		
Sat–Week 2	AM	11, 15, 20	15.33	15.33 × 6 = 92.00	92.00/0.3 = 306.67
Sun–Week 2	AM	20, 21, 8	16.33	16.33 × 6 = 98.00	98.00/0.3 = 326.67
Sun–Week 3	PM	9, 18, 16	14.33	14.33 × 6 = 86.00	86.00/0.7 = 122.86
Sat–Week 4	PM	22, 17, 10	16.33	16.33 × 6 = 98.00	98.00/0.7 = 140.00
Mean ($\bar{e}_2$)					224.05

[a]Work periods were 6 hours long.
[b]Sampling probabilities were 0.3 for morning periods and 0.7 for afternoon periods.

presented by stratum and then overall. Standard errors are also presented based on the same method used in the first roving–roving example (Section 15.6.1.5; Tables 15.21–15.22).

15.6.3 Aerial Effort Designs

Aerial effort designs are conceptually identical to the roving effort designs discussed in Sections 15.6.1 and 15.6.2. However, they are presented briefly here because they have different practical implications. The two relevant designs are aerial–access design and aerial–roving design.

An airplane or helicopter is used to obtain the counts, which are expanded to obtain total effort. A ground survey is carried out either at access points to obtain interviews of anglers for complete trip catch rates or by roving clerks who conduct incomplete trip interviews to determine catch rates.

Table 15.28 Calculation of daily catch rate (c) for a stratified random sampling, roving–access survey on a river.

Day and week or statistic	Daily effort	Daily catch rate[a]	Daily catch ($\hat{c}_i$)[b]
		Weekdays	
Thu–Week 1	31.43	1.71	53.75
Wed–Week 2	46.67	3.27	152.61
Thu–Week 3	37.14	2.73	101.39
Tue–Week 4	100.00	3.61	361.00
Mean ($\bar{c}_1$)			167.19
		Weekend days	
Sat-Week 2	306.67	3.15	966.01
Sun-Week 2	326.67	4.81	1571.28
Sun-Week 3	122.86	2.33	286.26
Sat-Week 4	140.00	4.93	690.20
Mean ($\bar{c}_2$)			878.44

[a]Total catch divided by total effort for all completed trip interviews made on each sampling day.
[b]Daily catch = daily effort × daily catch rate.

Table 15.29 Total effort and total catch estimates for weekday and weekend strata and overall for a stratified random sampling, roving–access survey on a river. Standard errors are also presented.

Stratum	Variable	Estimate	SE
Weekday	Effort	1,076.20	314.29
	Catch	3,343.80	1,353.68
Weekend day	Effort	1,792.40	429.95
	Catch	7,027.52	2,158.77
Overall	Effort	2,868.60	532.57
	Catch	10,371.32	2,548.09

These designs could be useful for boat-based fisheries on rivers, lakes, or estuaries. They are less likely to be useful for bank fisheries because of visibility problems. There are practical advantages and disadvantages of using airplanes, as we discussed in Section 12.5. Planes can cover large regional fisheries very quickly, but they may not be able to fly in inclement weather although fishing may still be occurring. Overall, however, these are useful and important designs.

15.7 GENERAL DISCUSSION

A comprehensive synthesis of angler survey methodology for effort and catch estimation has never been attempted before this chapter was written. Most fisheries biologists and most biometricians viewed creel surveys as either access or roving types and that was about as far as it went. We have attempted to show that the field is much more comprehensive. Off-site and on-site methods can both be used, and effort and catch estimation may use separate methods, giving rise to complemented designs.

In Section 3.8 and then again in this chapter, we have attempted to present some information on estimation of variances and standard errors of estimators. However, we emphasize that a lot more work needs to be done in this area. The book by Wolter (1985) is a useful reference but it is quite mathematical. We suspect that in the near future, much more use will be made of Monte Carlo computer methods to estimate variances for complex designs. An example of this approach is the analysis of the bus route design by Jones et al. (1990).

Chapter 16

Surveys for Economic Analysis

16.1 INTRODUCTION

Managers and others who work with fisheries resources increasingly need economic values for both specific fisheries and for the fisheries systems they administer. The most frequent use of economic values in fisheries up to the present has been for providing a measure of the worth of fisheries for purposes related to program justification and for seeking higher operating budgets. Other important uses include estimating the economic impact of fishing on local communities and the value of human-induced resource damages requiring mitigation.

Economists often look askance at the frequent use of angler expenditure data to justify larger program budgets (Brown 1987). Expenditures alone do not accurately estimate the types of fishing values administrators and managers are trying to characterize. Moreover, the data are being used in a persuasive rather than an objective mode. For example, an administrator might say to the state legislature, "Anglers in our state spent $100 million last year. Fishing has a tremendous economic impact. We need more dollars for quality fishing programs." Perhaps the fisheries administrator is successful in this persuasive attempt. But the amount of additional funding that will be allocated to the fisheries program as a result has not been determined by any direct measure of either need or benefit. Thus, it really matters little whether anglers spent $80, $100, or $120 million. The funding decision has been determined simply by the forcefulness of the administrator's argument.

Persuasive arguments will probably continue to be important influences on what government funds and at what level. However, government budget staffs increasingly realize that decisions based purely upon persuasive arguments have little objective merit. For legislators to make good decisions, they need the answers to questions such as, How much will each dollar spent on fisheries programs generate in public benefits? and, What groups will benefit, and by how much, if more money is put into fisheries at the expense of other programs?

Information on economic impacts of fishing or on the values that individuals place on fishing can answer specific economic questions and can be incorporated into quantitative decision-making analysis. Adhering to the correct methods is critical, however, to deriving accurate estimates of economic values. Just as it often appears that anyone can conduct a survey, it may appear very simple to gather economic data by including a question that asks anglers how much they spent on their fishing trip. However, a casual approach to gathering economic data runs a strong risk of asking for the wrong type of data or asking the questions in ways that produce biased or inaccurate responses.

This chapter provides a framework for conducting economic analyses related to fisheries topics. For a given type of question or problem, this chapter clarifies the type of economics study that is needed and provides an overview of the methods that should be used. Individual sections provide guidance on the degree to which fisheries staffs without training in economics should proceed on their own. The methodology for some economics studies is sufficiently detailed that such studies should be conducted only by, or in close association with, a resource economist. We recommend that the objectives, draft survey instrument, and methodology for any economic survey be reviewed by a resource economist before the survey is undertaken.

For a further introduction to economics concepts used in fisheries resource valuation, refer to Swanson and McCollum (1991) and Ozuna and Stoll (1991).

16.2 DEFINING THE STUDY

The first steps in an economics study, as in other studies, are to carefully determine and specify the goal and objectives of the study. These decisions lead to the determination of the geographic area of study, the study population, the survey methods, and ultimately to the design of questions and the type of analysis that will be used. Other chapters cover many of the initial concerns. Refer especially to Chapter 2 for planning aspects of a study.

At the early stage of planning, one should beware of simplistic solutions and ascertain that all relevant types of values or impacts are listed and that the most important ones are considered for study. Suppose a chemical spill had killed an estimated 1,000 fish and one needs to bring charges of an appropriate amount against the responsible party. A simplistic approach would be to check the price of fish at a grocery or fish market, perhaps $4.00 per pound, and to conclude that damages should amount to $4,000. This approach presupposes first that the grocery price is the appropriate value to place on the fish, rather than replacement cost of the fish if they were restocked. It also presupposes that the value of the fish as food is the only loss. It ignores much of the value of the fishing experiences anglers will be giving up and the local economic impacts of reduced fishing. The public benefits lost because of the chemical spill might include more than the value of angler trips foregone if the fishery had to be closed for a time. Perhaps the quality of the fishing experience was reduced even after the fishery reopened: the size structure of the population had changed; catch rates were lower; fish were contaminated. These are the kinds of lost benefits for which value estimates are often needed. Such estimates when totaled will be quite different from a ''back of the envelope'' estimate of $4,000. Because the approach advocated here takes into account all of the values lost, the estimate derived will usually be larger than that derived from the casual type of estimate.

Thus, a better approach to arriving at the appropriate study method is to

(1) answer the question, ''What types of (usually public) benefits have been lost (or are threatened)?''

(2) characterize those types of lost or threatened benefits as specifically as possible, and

(3) find a valid means of estimating the relevant economic values with acceptable precision.

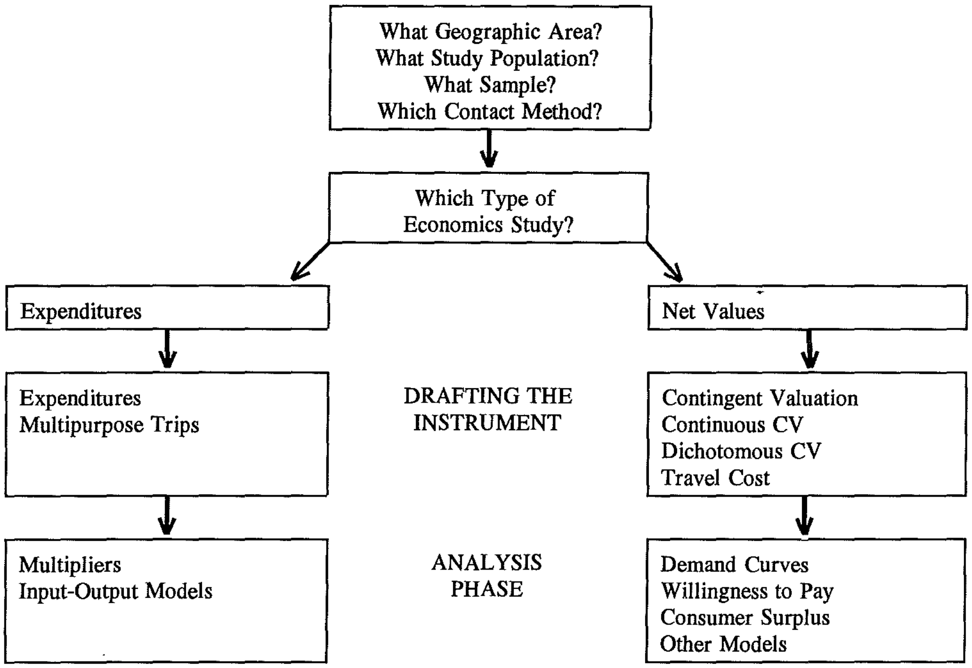


Figure 16.1 Decision and procedural framework for conducting fisheries economic studies (CV is contingent valuation).

16.3 CHOOSING THE CORRECT VALUES FRAMEWORK

Figure 16.1 illustrates the process of delimiting the study, choosing the appropriate methodology, designing the survey instrument, and carrying out the analysis. Because the initial considerations are covered in other chapters, they are lumped in the top box of Figure 16.1. The next consideration is to determine the appropriate type of economics study. Most fishery economics studies are conducted to estimate either economic impacts or net values. *Economic impact* is the extent to which a business, community, region, or other entity is changed economically (e.g., in quantities of sales, jobs, or income) by some event such as improvement or degradation of a fishery. *Net value* is the amount of benefit received by an individual or group from a product, service, or experience over and above the cost of obtaining it (cost can include such factors as time and inconvenience as well as money). The appropriate choice between these two types of analysis depends upon the purpose of the study.

To specify the type of economics study needed and whether the economic impact or the net value pathway should be followed, one first must answer two key questions about the economic values sought: value of what? and, value to whom? Consider first the "of what?" question. Is it the value of the fish found in a particular fishery? The value of the real estate (lands and waters)? Or the value of the fishing experiences produced by the fishery?

Next, consider the "to whom?" question. To the anglers? To businesses where the anglers spend money during their trips? Or to the entire community near the fishery through which angler expenses circulate? The economic value estimate for each of these three options will differ, and the methods and procedures for estimating each will be different.

To further emphasize this point, suppose three friends make a weekend trip of 100 miles (each way) to a popular fishing spot, where one of the three has a boat berthed at a local marina. They arrive Friday evening after work and fish through noon Sunday. Their fishing-related expenditures (food and beverages, lodging, gasoline for car and boat) for this weekend trip total $250. On their way home Sunday, they stop at an outlet mall where they spend $180 on boots and rain gear for future use. These trip expenditures do not take into account the $400 per year the boat owner pays for his berth at the marina or the annual maintenance, insurance, and depreciation on the boat.

In this example, positive values or impacts accrued to the individual anglers, to the group of three anglers, to the businesses the anglers patronized, and to the communities and region where the businesses were located. Values that accrue to the anglers are estimated in a different way from the economic impacts on businesses, workers, and the community, so it is important to be clear about what is to be measured. As the next sections will show, an expenditure-based estimate may or may not be appropriate. Further, even if an expenditure-based estimate is wanted, a trip-based estimate may not be. For example, if the impact of fishing on the local economy is to be estimated, the $400 per year that the boat owner pays for his marina berth is a relevant expenditure. Thus, clearly defining the value parameters one wishes to estimate is a prerequisite to choosing the methods and implementing the study.

16.3.1 Expenditure versus Net Value Estimates

Economic impact estimates are based on expenditures. Expenditure data are frequently misused by laypersons to represent the value of trips to anglers. Expenditures typically account for a significant portion of the total value of a trip to individual anglers. With rare exception, however, expenditure is not a valid measure of either the net value or the total value of an angler's trip, as will be shown later. Studies of the value of trips to anglers use a different concept—net value—and estimates produced by the two types of study are sometimes distinguished as "expenditure versus net value estimates."

In the previous example of three anglers who took the weekend fishing trip (and who might make additional trips to the same site), economic impact analysis would be undertaken to determine the degree to which spending by these anglers, who are part of a larger population of anglers, economically benefitted a defined geographic area over a specified period of time. Such an analysis could examine both the economic impact on particular sectors of the economy, such as marinas and sporting goods stores, and the collective economic impact across all sectors. Economic impact can be measured by the amount of money that flows into the community or particular economic sectors from anglers, the number of additional jobs that are created, or the total amount of personal income that is added. Of these, new monetary expenditure is the most straightforward measure because it is derived directly from angler activities and it represents an outside flow of money into, and an expansion of, the local economy.

Why might an economic impact analysis be undertaken? Suppose the local community, which owns lakefront property, was asked to develop a boat launch ramp and parking area. Local government leaders would surely ask what proportion of the projected costs of constructing and maintaining the facility would be recouped directly or indirectly from increased angler spending, perhaps including a launch fee during peak use times. The specific analysis criteria they set would depend upon how construction of the facility was to be financed (e.g., with a municipal bond or federal grant) and the extent to which local anglers would also benefit from the facility. Whatever their goals for covering costs, community leaders would need an economic impact analysis to estimate the amount of new tax money that would be generated for local government from retail sales to anglers or from property assessments on businesses that would be created or expanded to serve anglers.

As opposed to the economic impact generated by the three anglers' spending, consider now the value of the trip to the anglers themselves. A fishing trip is not simply the purchase of meals, lodging, fuel, and some fishing supplies. A fishing trip is an experience that includes such dimensions as relaxation or escape from work-related pressures, friendship, enjoying the out-of-doors, challenge, and the opportunity to consume the fish that are caught. A fishing trip has a planning phase and a recollection phase as well as the event itself. Each of these phases is generally viewed positively by anglers and therefore has benefits that accrue to anglers.

The three anglers were not required to pay for many aspects of their fishing experience, so the total value of that experience almost certainly was worth more than their $250 in fishing-related expenditures. The total value of the trip to the three anglers is defined by economics theory as the maximum amount they would have been willing to pay rather than forgo the trip. The difference between this total value they would have been willing to pay and their trip-related expenditures is known as the net value of their trip or the consumer surplus.

Thus, applications of economics to recreational fisheries embrace two fundamentally different types of economic measure: the economic impact of fishing on a geographic area, and the value of fishing to groups of individual anglers. These two measures have very different uses. Economic impact analysis treats fishing as if it were an industry. Expenditures of anglers in the geographic area of concern comprise the basis for economic impact estimates. Value analysis concerns the value of the fisheries resource to the anglers who use it or to the broader society. The consumer surplus or net value can be used to estimate the comparative value to society of a trout fishery and a power generating facility that would reduce the quality of trout fishing. This measure can also be used to evaluate the loss to society when a fishery is contaminated. Angler expenditures, the basis for economic impact estimates, provide little help in estimating the value that anglers or the larger society place on fisheries resources.

It should now be apparent why Figure 16.1 splits into two columns of considerations once the purpose of the study and the target population and region have been identified.

The remainder of this chapter describes how to use creel and angler surveys to obtain economic impact and net value estimates.

16.4 ECONOMIC IMPACT SURVEYS

Economic impact surveys are used to estimate angler expenditures during fishing trips. "Trip" has a particular meaning in this context. In most creel surveys designed to estimate catch or fishing effort, a trip is considered finished (complete) when an angler leaves the water; a return to the water on the same or a later day is treated as another trip. For economic surveys, in contrast, an "angler trip" or "complete trip" is the entire period of time that the person or party spends in the locality of the fishery—if the trip is made primarily for fishing. This period might be days or even weeks. If the economic survey design calls for expressing data in per-angler-day units, all expenditures an angler makes in the fishing locality during a particular 24-hour day (e.g., midnight to midnight) would be relevant, including a motel bill and purchases totally unrelated to fishing.

Angler expenditures in a local community on a particular day have little direct relationship to the amount of time spent fishing (logic suggests that the less time spent fishing, the more time available for local expenditures). Thus, on-site surveys cannot be used to obtain bias-free angler expenditures needed for estimating economic impacts, because anglers have not yet completed their stay in the area when they are interviewed by access point or roving clerks. They often do not know what their remaining expenditures will be, and their remaining expenditures have no relationship to their past expenditures.

The most accurate way to obtain anglers' expenditures is to record their telephone numbers or mailing addresses at the end of a creel interview, and to recontact them by telephone or mail within a few days of the fishing trip (as quickly as possible to minimize memory recall biases). Differences in expenditure estimates between on-site and off-site surveys have rarely if ever been checked experimentally. If the vast majority of trips to a fishery are day trips, if most of the anglers come from short distances, and if the study area is very rural with few retail shopping opportunities, the bias resulting from anglers' estimates of their expenditures for the rest of the trip may be acceptably low. However, when anglers make multiple-day trips over long distances to fish near unique retail opportunities, they frequently make unanticipated expenditures.

Fisheries that are most successful at attracting anglers from outside the region to make significant expenditures are the most likely subjects of an economic impact study. Because anglers in these situations are least able to accurately project their remaining expenditures during a creel survey interview, off-site studies will be needed despite the added time and cost entailed.

16.4.1 Biases Affecting Expenditure Estimates

Whether an expenditure questionnaire will be administered on or off site, a sample of anglers drawn during on-site creel surveys is subject to length-of-stay and avidity biases: anglers who fish longer per trip or who make more trips per unit time are more likely to be intercepted than other anglers. These biases are more likely with roving than with access point creel surveys, and they are serious to the extent that sampled anglers differ in behavior from anglers in general at the fishery.

Length-of-stay and avidity biases can be eliminated by weighting data inversely to trip duration or frequency (Nowell et al. 1988; Thomson 1991). Setting aside off-site survey biases for the moment, the above weighting yields an unbiased estimate of expenditures per trip. This estimate multiplied by an unbiased estimate of total trips, derived from the creel survey, provides an unbiased estimate of total angler expenditures.

16.4.2 Defining the Activity and Region of Interest

The purpose of an economic impact study is to determine the effect of some activity or event on the economy of a specifically defined locality or region. Both the subject of the study and the geographic area must be carefully specified before the research instruments can be designed to gather the needed data.

Economic impacts may be either positive or negative. Studies can examine the impact of a short-term event such as a fishing derby, a seasonal fishery such as a salmon run, or all fishing in a given area for as long as a calendar year. By comparison with a previous year for which data are available, one can estimate the economic impact of a reduction in the standing crop or average size of a particular fish species, or of the discovery of a contaminant in fish that results in health advisories related to fish consumption.

An economic impact study must be designed to measure only the impacts related to the subject of the study. A useful framework for study design is the phrase "with versus without." Suppose the objective is to measure the economic impact of a weekend fishing derby on a nearby community. Unless the waterway is closed on that weekend to all anglers except derby participants, it would be inappropriate to count the expenditures resulting from all fishing activity as derby-related, because some fishing would likely take place in the absence of the derby. The measure needed is the net value of economic activity resulting from the derby, which is the difference between the fishing-related economic activity *with* the derby and that which would have occurred *without* the derby.

Anglers who come to a fishing derby make trip-related expenditures in many places. They may buy some of their fishing tackle and a tank of gasoline before they leave home. They may stop for a meal or fuel en route to the fishing site. Some expenditures such as restaurant and lodging expenses will occur in the community near the fishing site. If the fishing site is in a very rural area with few services, anglers may travel many miles to the closest community to find accommodations, perhaps to a community not on the waterway they are fishing. These factors must be considered carefully in specifying the area or region chosen for measuring the economic impacts. Whatever area is specified, expenditures made within that area are counted, and those made outside the area are excluded.

The choice of study area must be linked not only to fishing patterns, but to the purpose of the study. For example, the village of Smithville is considering improvements in its boat launch ramp on Green Reservoir, which is used primarily by anglers. The mayor and village council want to know the amount fishing contributes to the village economy now, and they want to project how community revenues would increase with improved facilities. Whether current fishing-related expenditures in Smithville amount to 10% or 95% of all of expenditures related to the fishery in question, these village officials are justified in considering only expenditures that occur within their village limits. If the same boat launch ramp were owned by the county, the appropriate impact area would be the county rather than the village.

Alternatively, the study objective could be to determine the general economic impacts of a particular fishery. In this case, one should specify the location and

Please enter any expenses below that you made within Green, Shelby, or Wayne counties in conjunction with your most recent fishing trip to Green Reservoir. Enter your expenses in the appropriate column for the location where they were made:			
	PLACE WHERE EXPENDITURE OCCURRED:		
EXPENDITURE:	Within Village of Smithville	Outside Smithville but in Green County	Within Shelby or Wayne County
Restaurant/bar	$	$	$
Grocery	$	$	$
Lodging	$	$	$
Bait, tackle, sporting goods	$	$	$
Misc. recreation/ amusement expenses	$	$	$
Boat rental, launch fees	$	$	$
Gasoline and oil	$	$	$
Car or boat parts/repairs	$	$	$
Charter or guide fees	$	$	$
Souvenirs or other retail purchases	$	$	$
Other misc. expenditures	$	$	$

Figure 16.2 Example questionnaire to establish angler expenditures for a three-county economic impact analysis of the hypothetical Green Reservoir fishery.

expenditure categories to be analyzed and design a format for obtaining the data. Suppose the study region comprised the three counties (Green, Shelby, and Wayne) abutting Green Reservoir. As part of the regional study, the expenditures made specifically in Smithville (Green County) are to be broken out.

Figure 16.2 shows a questionnaire matrix of expenditures and locations that could be used to collect the economic data. Three criteria were used to select expenditure categories: (1) likely types of angler expenditures in the setting of the particular fishery, (2) expenditure categories for which the researcher needs specific estimates, and (3) categories that will help anglers recall all of their expenditures. Even if the researcher is not interested in expenditures by specific categories, it is advisable to use several categories to help responding anglers remember their various expenditures.

The specified region must be accurately interpreted to anglers when they are surveyed. If the survey is conducted by mail, a map of the study area can be included. If it is conducted by telephone, the names of the villages included can be provided to the anglers. Sometimes, as in Figure 16.2, the region is defined as one or more counties. Anglers who reside in the state and who visit the area from time to time may be generally familiar with the location of county borders, but many—perhaps most—anglers will have to be given a further description of the study area.

16.4.3 Deciding Which Expenditures to Include

Once the geographic area is defined, the next concern is specifying the angler population of interest and the expenditures to include. Should the expenses of local as well as nonlocal anglers be included? This depends upon the purpose of the study. If the question is how much new revenue an improved fishery brings into the impact area, the goal should be to estimate the differences in local economic activity with versus without the fishery. Thus, expenditures by local residents would be excluded (they should be represented in the baseline estimate) unless it is known that local people take money outside the area in the absence of the fishery. On the other hand, if the question is how much the fishery helps support specific types of businesses (e.g., marinas, bait shops), expenditures made by visiting *and* local anglers in relation to the fishery would be applicable.

Should the analysis include just expenditures related to fishing, or all expenditures made by anglers in the area? Just the expenditures of the anglers, or also those of accompanying family members or friends who did not fish? The answers to these questions also depend largely on the purpose of the study. Is the interest in knowing the impact of only the fishing-related expenditures, in knowing more specifically how fishing has affected a particular economic sector such as the boating or marina industry, or in knowing more generally how much revenue of any type is coming into an area because of the presence of the fishery?

Assume the interest is in total new revenues. Again in a "with versus without" mode, the question becomes, What is the total economic measure of revenues (or jobs or household income) that come into a community or region that would not do so if the fishery were not there. This question establishes the criterion for wording survey questions. With this criterion, if a fishing party from outside the region travels to the region specifically to fish, and while there, they buy other goods or services such as clothing, tools, or automobile repair services, those expenditures are of interest and should be included. If a spouse who does not fish comes along and shops, the shopping expenses meet the criterion if the spouse would not otherwise have made a separate trip to the same area to do that shopping. Whenever a party visits an area because of the fishing (i.e., they would not have made the trip in the absence of the fishery), all of the expenditures are of interest to a general economic impact study.

Multipurpose trips are more difficult from an accounting stance. When, for example, a family from outside the region visits friends who live within the region, and a portion of that visit is devoted to fishing, it is difficult to allocate the expenditures fairly, but the criterion remains the same: Would the visit and the various expenditures have occurred in the absence of the fishery? Only those expenditures that would not have occurred in the absence of the fishery should be included.

If the study is concerned with a more limited range of expenditures, the questionnaire can be designed accordingly. It is very important to the accuracy and subsequent interpretation of any expenditure survey that the criteria for inclusion of various expenditures are clear, and that the questionnaire itemizes the expenditures of interest by category (e.g., restaurant and bar, fuel, groceries).

Respondents can be much more accurate if they are asked for expenses by type than if they are simply asked for an overall estimate of their trip expenses.

16.4.4 Indirect Expenditures: Multipliers

Thus far, only direct or first-round expenditures have been discussed. Direct angler expenditures generate additional local economic impacts as monies are respent by merchants to pay salaries and to purchase supplies and services. The number of times, on average, that a dollar of expenditure is respent in the area of study before it leaves the area, added to the original dollar, is known as the expenditure or sales multiplier. (Jobs and new household income also have multipliers, but each is calculated differently from the sales multiplier.) For example, for each dollar spent at restaurants in Smithville, perhaps 60 cents is spent locally and 40 cents is spent outside of the region. Of the 60 cents that stays in the region, perhaps one-third (20 cents) is respent in the region, and in each succeeding round one-third is spent locally and the rest is exported. The restaurant sales multiplier would then be

$$1 + 0.60 + (0.60)(0.33) + (0.60)(0.33)(0.33) + \ldots,$$

or approximately 1.90.

It should be apparent upon reflection that the multiplier for restaurant expenditures is different from that for expenditures at a service station. A restaurant normally has a higher multiplier than a service station because it is more labor intensive, and most of the labor is local. Furthermore, petroleum sold at most service stations comes from outside local areas, whereas some of the produce and baked goods used in the restaurant is produced locally. Every economic sector in which anglers spend money has a different sales multiplier because different portions of sales receipts are respent in the area or region of interest.

Multipliers must be connected not only to a given economic sector, but also to a specified geographic region. The larger the region in terms of population and economic activity, the larger the multiplier of an economic sector generally will be. If Smithville is a very small village with no food-processing services and no bakery, its restaurant will have to buy almost all its goods from outside the village. Furthermore, if Smithville has very limited shopping opportunities, the restaurant staff will spend much of their wages outside Smithville. Thus, the restaurant sector in Smithville will have a rather low sales multiplier (perhaps 1.2 to 1.4).

Suppose instead that Smithville is a suburb in a major metropolitan area. Smithville no longer can be considered a separate trading center in an economic sense, because even local residents spend as much money in the primary metropolitan city and other surrounding suburbs as in Smithville itself. Thus it would rarely make sense to estimate the overall economic impact of fishing on Smithville; rather, the impact on the entire metropolitan area should be estimated. The metropolitan area has many bakeries that can supply the Smithville restaurant and many stores where the restaurant staff can shop. Most of the consumer goods purchased are not manufactured in the metropolitan area, but a few of them are. As a result, the economic impact of fishing on the larger metropolitan area in which Smithville is located is larger than that of Smithville because the dollars originally spent at the restaurant in Smithville stay in the metropolitan area longer than in Smithville. A typical sales multiplier for restaurants in a metropolitan area is between 1.6 and 2.2.

Similarly, one might estimate the impact of money spent by anglers in Smithville on the state in which Smithville is located. The multiplier for restaurants in larger, more populated states and provinces with very diversified economies is larger yet, approaching 3.0. (Even in the broad economies of states and provinces, sales multipliers rarely exceed 3 for any economic sector, because eventually goods are needed that must be imported from elsewhere.)

Thus far we have discussed sales multipliers in terms of specific sectors such as restaurants or service stations, rather than in terms of a broader economic grouping such as "recreation sector." Economic data are collected by government agencies in specific sector categories such as eating and drinking places, lodging establishments, sporting goods stores, and automotive service stations. Data from these sectors are used in models to estimate multipliers for these specific sectors. If these multipliers are to be used in conjunction with economic impact surveys, the categories in the survey must be consistent with categories in the model. Most models allow combining two or more sectors into a single category. However, the data are often more meaningful and more easily interpreted if those sectors in which significant angler expenditures are made are kept disaggregated.

In theory, an average retail sales multiplier exists for any community or region. However, such average multipliers usually are not known. Furthermore, the average sales multiplier for angler expenditures may not be similar to the average for all retail spending in the area. Thus, an average sales multiplier, even when known, may be a crude multiplier for angler expenditures, and we do not recommend its use. Even worse would be to assume that the multiplier estimated for one location is transferrable to another location. Economists at major universities or at government economic development agencies often can recommend or help develop appropriate multipliers for particular studies.

For a good overview of economic impact analysis and the several methods used, see Propst (1985). The method most frequently used to estimate sales, employment, and income multipliers for the various sectors is input–output (I–O) analysis. Miller and Blair (1985) is a comprehensive text on I–O analysis. Other useful I–O references include Miernyk (1965) and Hewings (1985). In past decades, I–O analysis was done by direct studies of businesses and households to determine expenditures by economic sector both within and outside the region in question. In recent years, several computer models have been developed that use indirect methods based on economic sector flows at the national level to estimate I–O coefficients at the county level for all counties in the USA. One of the best of these models and the one most readily available to public sector employees in the USA is IMPLAN, developed by U.S. Forest Service economists at the Rocky Mountain Forest and Range Experiment Station in Fort Collins, Colorado. At least one resource economist in most land grant universities has had training in the use of IMPLAN and has the microcomputer program for that state. The Forest Service has contracted with the University of Minnesota's Department of Agriculture and Applied Economics to maintain the IMPLAN data base and to conduct training sessions. Plans have also been made to transfer the data base to the private sector.

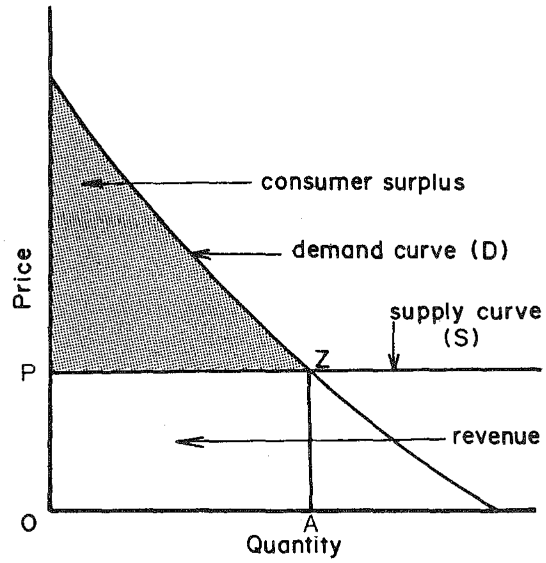


Figure 16.3 Generalized economic demand and supply curves, illustrating consumer surplus. Point P is the current price at which activity is available, and point Z indicates consumption at that price. (Adapted from Talhelm et al. 1987.)

16.5 NET VALUATION SURVEY METHODS

When one wishes to examine the value of a fisheries resource to society or the value of fishing experiences to groups of anglers, the appropriate economic measure is not expenditures, which are measures of direct economic impact, but the net value over and above the expenditures. This is illustrated conceptually in Figure 16.3 (Talhelm et al. 1987).

In Figure 16.3, assume that the *X* (horizontal) -axis is number of angler trips and the *Y* (vertical) -axis is price or cost per angler trip. In this illustration, the number of trips the resource or the fisheries agency can supply is assumed to be independent of the price paid by anglers; hence the supply curve shown is horizontal. Also in this illustration, although the price per trip can vary over time, everyone fishing at a particular time pays the same prevailing price. (Normally this is not true, because people travel different distances, use different fishing equipment, etc.) Finally, the demand curve is a connected curve that shows the total amount that anglers would be willing to pay for various numbers of fishing trips. The demand curve slopes downward. This implies that there is a theoretical price (where the demand curve touches the *Y*-axis) for which no angler trips would be taken. On the other hand, many trips would be generated at zero price (where the demand curve touches the *X*-axis).

The rectangle OPZA in Figure 16.3 represents total expenditures generated by the angler trips taken at the constant price of P. However, in addition to the actual expenditures OPZA, the same anglers would be willing to spend, if necessary, the amount encompassed by the shaded portion above the rectangle. This shaded value is known as the net willingness to pay or the consumer surplus. It represents benefits received by participating anglers over and above the price actually paid.

Consumer surplus values do not involve monetary exchanges and therefore can not be observed directly in the way that expenditures can be observed. Economists have devised two general methods of estimating the consumer surplus associated with any type of natural resource experience. In the contingent valuation approach, anglers or other resource users are asked to estimate their own net benefits. In the travel cost approach, net benefits are estimated from the travel patterns of users. These methods are described in the following sections.

16.5.1 Contingent Valuation Method

The contingent valuation (CV) method was designed to estimate the extent to which relevant segments of the public value changes in the quality of public goods. The technique is based on survey questions that ask for individuals' willingness to pay for those changes. Contingent valuation also has been used to estimate the net value of current resources for specific purposes. With respect to fisheries, CV has been used to estimate the net value of angler (and other recreational) trips to specific sites. It has also been used to estimate whether or not anglers and others would be willing to pay for habitat restoration that would allow a particular species to flourish in given waters. The objective of CV is always to determine the maximum amount that users or consumers would be willing to pay for the change in question.

In contingent valuation of a change in a resource, the willingness-to-pay questions are used to establish hypothetical markets for unpriced goods or amenities. The prologue to these questions should define for respondents the good, service, or amenity under consideration, the current (status quo) level at which the good is provided, the change in the resource that is contemplated, any change in the institutional structure under which the good is to be provided, and the method of payment (Randall et al. 1983). Contingent valuation is now the most frequently used technique in environmental benefit assessment, because it is far more adaptable than alternative methods. Whether the resource facing or undergoing change is a particular fishery in a local lake or the marine sport fisheries of North America, CV has potential application. Whether the topic is fluctuating water levels, improvements at a park or other recreation site, improvements in fishing, or decreased vistas at national parks due to air pollution, the theory and methodology are similar. The situation is summarized as concisely as possible, and a sample of the relevant human population is surveyed to determine how much more or less they would be willing to pay under the revised condition. (The obverse measure—willingness to accept payment as compensation for change—is addressed in Section 16.5.1.1.)

Economists are still evaluating the use of CV in situations involving a resource currently in use, but it is the most straightforward method available for estimating the net value of a current resource to anglers. Furthermore, carefully designed survey instruments, in combination with rigorous survey procedures, have generally produced credible estimates. When current users of a resource are surveyed, the methodology consists of first ascertaining trip expenditures, then asking respondents to estimate the maximum amount over and above those expenditures that they would have been willing to pay before they had chosen instead to forego the trip.

The CV concept in economics has been around for decades, but little was done to improve it and apply it to environmental and natural resource settings until 1978. This hesitation was due to two early criticisms of the technique. The first concern can be succinctly stated: "Ask a hypothetical question and you will get a hypothetical answer." Potential biases have several dimensions (Cummings et al. 1986). People may genuinely not know what they would do in the hypothetical case because they have never been in that particular situation. People may not understand all of the ramifications of the situation they are being asked to value. Furthermore, the more hypothetical the situation, the less incentive the respondent has to seriously consider the scenario and try to arrive at an accurate answer. Despite these problems, interest in CV has grown over the past decade because requirements to quantify environmental costs and benefits have increased and because no other usable method exists for many applications. The very limited experimental research that has been done to date has shown that in specific situations, individuals can reasonably estimate their behavior (willingness to pay) in hypothetical situations.

The second early concern about CV was expressed by the noted economist Paul Samuelson (1954), who argued that CV would fail because it is in everyone's personal interest to send false signals—to underestimate what he or she would really pay for something (in case a higher charge becomes reality) or to overestimate what they would have to be paid for something that would be taken from them (in case the loss is realized). Freeman (1979) pointed out that the more hypothetical the question, the less incentive a respondent has for "strategic bias." As already noted, however, the motivation for a careful response also declines as the question becomes more hypothetical.

One of the first to consider CV as a means of valuing nonmarket goods was Ciriacy-Wantrup (1952), who saw that the additional incremental values people would be willing to pay correspond to a market demand schedule. The first known field implementation of CV was Davis's (1963) classic study of hunting in the Maine woods. In the early 1970s, Bohm (1971, 1972) conducted experiments with survey methods that generally refuted Samuelson's concerns about strategic bias, and interest in CV was renewed. More modern versions of CV date from work by Randall et al. (1974), who imposed a rigorous survey design involving a "bidding game" to establish a marketlike context. For further historical material on the development of this method, see Cummings et al. (1986) and Mitchell and Carson (1989).

Contingent valuation is now widely used and accepted, but economists realize that slight variations in how questions are worded can produce answers that differ substantially. A better understanding is needed of respondents' cognitive process when they answer CV questions, and psychologists and other social scientists have begun to team with economists to address this problem. This research should lead to better ways of stating situations and phrasing CV questions. Several excellent papers on CV by economists and social scientists were compiled by Peterson et al. (1988).

Bishop and Heberlein (1990) pointed out that any good CV study must deal with the following points.

Population Definition. All groups whose values are to be measured must be carefully defined. Interest might be only in active anglers who currently fish for a given species in a given waterway. Or it might be in a broader group of people who do not now fish for that species, but who may wish to someday. Or it might be in a still broader group of the general public who have no plans ever to fish, but who derive satisfaction from knowing the fishery is there as a symbol of an unspoiled environment, or who may wish to help ensure that the fishery remains viable for future generations to enjoy. The premise and questions of a CV survey will differ according to the target group identified.

Product Definition. "Product" in the CV context is broad enough to include not only changes in number or size of fish stocked, but any experiential changes that will affect the fishing experience. Survey respondents must understand the subject, condition, or change in condition that they are asked to value. This information must be portrayed in relatively simple language. In some cases, photographs or other visual aids may be helpful in portraying the product or the change in condition.

Payment Vehicle or Mechanism. Contingent value responses may differ according to the method by which respondents are asked to pay. Payment could take the form of an additional license fee, additional taxes, or a contribution. Mitchell and Carson (1989) suggested that the payment vehicle be as realistic as possible and neutral in the minds of respondents. Others have asserted that there is no unbiased payment vehicle; rather, the payment vehicle becomes a part of the "product" respondents are asked to value (Cummings et al. 1986).

How the CV Question Is Asked. The method of questioning includes both the structure of the question and its phrasing. This is discussed in more detail in Sections 16.5.1.3 and 16.5.1.4 on continuous and dichotomous values.

Additional Data Needs. Income, recreation participation, and other variables may be needed either to carry out the analysis or to evaluate the validity of the information obtained.

Type of Analysis. The appropriate form of analysis depends largely on whether a continuous (open-ended) approach or a dichotomous model is used. This will be discussed further in Sections 16.5.1.3 and 16.5.1.4.

16.5.1.1 Willingness to Pay Versus Willingness to Accept Payment

Economists generally agree that when a new or expanded resource is developed, or a resource is improved, the maximum willingness to pay (WTP) of the relevant public(s) for the development or improvement is the theoretically correct valuation measure of consumer surplus. On the other hand, when a resource to which the public has a right is taken away, the correct valuation measure is the least amount that the public would be willing to accept (WTA) to give up that resource. Thus, for a proposed new multipurpose reservoir that would be stocked for fishing, WTP would be the appropriate measure of consumer surplus, because the proposed reservoir represents a new fishing resource not now available to the public. On the other hand, the removal of a public fishery, perhaps because of hydropower development that destroys fish habitat, represents a situation for which WTA is the appropriate measure of consumer surplus; WTA will estimate the net value of that portion of the fishery removed from the public domain.

Several leading resource economists have argued that, theoretically, WTP should approximate WTA in a given situation. That is, a resource should be valued similarly whether people want to use it or fear its loss. Also in theory, WTP should be less than WTA because WTP is income-constrained (peoples' total and disposable income affect the amount they can pay regardless of how important they feel the resource is), whereas WTA is not; but the difference between the values should be small. In many empirical, comparative studies, however, WTA values have been several times as large as WTP values. Even in limited experimental cases in which real money rather than hypothetical money has been used, differences have remained large. Many CV experts have looked askance at WTA studies because the value estimates were so high. They rationalized using a WTP format in studies where WTA was the theoretically proper format because WTP was thought to be a close approximation of an unbiased WTA estimate. More recently, though, these experts have found conditions under which WTP and WTA values could legitimately be quite different (Knetsch and Sinden 1984; Mitchell and Carson 1989).

Several explanations for the large difference between the two measures have been put forth. Most experts agree that it is a strong disadvantage for respondents to bid on or price resource commodities or activities with which they have had no experience. Bishop and Heberlein (1986) suggested that in the absence of this experience, people bid very conservatively. In a WTP study, such people thus would bid comparatively low or safe values that they are sure they would pay, without giving careful consideration to the matter. Similarly in WTA studies, people would bid comparatively high values that they are sure they would accept, without thinking carefully about whether they would accept less. For reasonable compensation amounts and for most resources, the difference between $0 and WTP is substantially less than the difference between maximum WTP and many values given in WTA surveys. This provides further evidence that WTA values are more heavily biased than WTP.

Mitchell and Carson (1989) pointed out that the key factor for determining whether WTP or WPA should be used is property rights. If a resource to which users have use rights is being taken, WTA is appropriate; otherwise WTP is appropriate. In many cases for which WTA may seem appropriate, WTP is actually the better choice because the users do not have exclusive use rights. In the case of many rivers and streams, for example, users have the right to fish. However, they do not have exclusive rights to the watershed. Farming the riparian lands adds pesticides and nutrients to the water; boating on the river leaves fuel residues, and so on. If such waterways are to be cleaned up, many people will have to share in the cost. Thus, WTP is an appropriate measure for an angler survey on this topic.

In summary, given sufficient understanding of the methodology, contingent valuation is a straightforward and reasonable method for estimating net benefits of existing fisheries (i.e., of angler trips) or of changes in the quality of fisheries. Willingness-to-pay methodology is now well established and accepted, given that the technique is applied with care and that several potential sources of bias are minimized. On the other hand, perhaps much is left to be understood about willingness to accept payment. Results of WTA studies are not widely accepted by many economists because of a widespread belief that the estimates are heavily inflated over true minimum payments that would be accepted. Research to further understand WTA is continuing. For multidisciplinary papers on these topics see Fisher et al. (1989) and Gregory and Bishop (1989).

16.5.1.2 General Contingent Valuation Biases

Strategic bias is among the most frequently discussed general types of CV bias. It is caused by individuals who give artificially high or low payment answers to CV surveys when they perceive it to be in their interest to do so. For example, if the survey dealt with valuing a fishery threatened by a development, strategic bias would cause people favoring the development to devalue the resource from what they really think it is worth, and anglers and environmentalists to overstate what they would be willing to pay to keep the resource in an undeveloped state.

Brookshire et al. (1976) argued that one would expect true willingness-to-pay bids to be distributed approximately normally along a rather flat curve. In their study involving recreation visitors to Lake Powell, the curve plot was very nearly normal, leading the researchers to believe that strategic bias was minimal. They further suggested that if one carefully considers and possibly discards zero and extremely large bids (outliers), strategic bias, if it exists, will have a negligible effect on the bid distribution.

In another investigation of strategic bias, Rowe et al. (1980) first obtained WTP bids, and later asked respondents to place themselves on a continuum of conservationist to developer. The correlation between placement on the development–conservation continuum and the WTP value was insignificant, leading the authors to conclude that strategic bias was minimal. In a third study, Mitchell and Carson (1981) argued that the distribution of WTP bids should approach the average U.S. income distribution rather than the normal distribution, because income is a good indicator of people's WTP for water quality, the subject of the study. They found that 83% of the bids above $0 were in their "normal" category, and the rest were fairly evenly divided at each end of the distribution. As a result, they concluded that strategic bias was not a problem in the study.

Another concern about the CV method is whether or not individuals internalize CV questions, relating them to their own budgets and to the alternative purchases they must make and the expenses they must incur. Do they give realistic answers that reflect their own financial situations, or do they give responses that may be reasonable and consistent with their preferences but inconsistent with their true willingness to pay in a real situation? Schulze et al. (1983), Sorg and Brookshire (1984), and Walbert (1984) studied this problem experimentally by reminding some but not all respondents to answer in terms of their budget constraints. They typically found no statistically significant difference in WTP between the two groups.

Vehicle bias (method of payment bias) has also been investigated. Respondents are usually given a payment mechanism as part of the WTP scenario; entry fees, sales taxes, income taxes, utility bills, and fishing licenses have been used in one study or another. Some evidence indicates that the payment vehicle offered may influence the WTP answer given, though not necessarily in a way that can be anticipated. For example, if the amenity considered is cleaner water and respondents already feel that water bills are too high, a proposed increase in utility bills may trigger a lower WTP than a proposed increase in some tax.

Users of CV should be aware of the existence and potential of these biases. Strategic bias probably will not be a major concern except when a subject has become highly politicized, and the distribution of WTP value results often can indicate the extent to which strategic bias is a problem. Answering CV questions with regard to one's own budget is not likely to be a serious source of bias if the study deals with one situation; it is unclear how serious this bias might be if a survey asked willingness to pay to improve fishing at 20 waterways. Finally, the payment mechanism should be carefully considered both in terms of appropriateness and public perceptions. If resident fishing licenses have just increased by 20% (not an unusual amount for fee increases that occur infrequently), anglers may not be receptive to a proposed further license fee increase to pay for fisheries improvement; they may view a special fishing stamp differently, however.

Whether a CV study examines the value of fishing trips to a site or the one-time willingness to pay for a resource improvement, decisions must be made about the validity of zero and very large WTP values. If a study is conducted by mail, responses also should be examined carefully for their correspondence to the study population. Response rates to mail CV surveys are often poor, and a telephone survey of nonrespondents may be needed to refine the mean WTP estimate.

16.5.1.3 Continuous Contingent Valuation

A continuous CV model is one in which a surveyor attempts to elicit from respondents the maximum amount they would be willing to pay in a given situation; a dichotomous or discrete choice model (Section 16.5.1.4) is one in which respondents are asked whether or not they would be willing to pay one specified value. Perhaps the greatest challenge with a continuous CV is assuring that a respondent's greatest WTP really has been elicited. Many economists have advocated some type of bidding procedure to this end. In the bidding process, which is most easily used in face-to-face or telephone interviews, the interviewer asks respondents if they would be willing to pay increasingly higher amounts than the value they initially indicated, until they say "no." A maximum WTP is thereby ascertained with greater certainty.

An illustration of a bidding structure comes from the 1985 National Survey of Fishing, Hunting, and Wildlife-Associated Recreation conducted by the U.S. Fish and Wildlife Service. Anglers were asked how many trips they took in the past year to fish for smallmouth or largemouth bass, and how many fish they caught on a typical trip. Then they were asked to estimate their expenses for a typical fishing trip. Next, they were asked if they would still have gone fishing if their costs had been twice what they actually were. Those replying in the affirmative were asked if they still would have gone if expenses had been three times the actual amount; then, four times the actual amount. After that exercise, respondents were asked to indicate the maximum cost they would have been willing to pay before deciding not to go on the trip. A format similar to this was successfully adapted to a mail survey of fishing in New York (Connelly and Brown 1990). Johnson and Walsh (1987) used another continuous CV model to determine willingness to pay for stocking of trout and salmon in a Colorado reservoir.

Biases Associated with Continuous Models. When a bidding process is used, a potential concern is whether the starting point in the bid, which is chosen somewhat arbitrarily, affects the distribution of values given. The two potential sources of starting point bias are (1) that people may interpret the value asked as an appropriate amount to pay, not as a value-neutral example, and (2) people who have limited patience with surveys may quickly become bored with an iterative bidding procedure and may not carefully consider their responses to successively higher WTP values. Research results on this topic are inconsistent. Some people (e.g., Rowe et al. 1980; Boyle et al. 1985) have found significant differences in WTP when starting bids differed, but others (e.g., Brookshire et al. 1980, 1981; Thayer 1981) have found either no or insignificant differences.

Schulze et al. (1983) and Sorg and Brookshire (1984) found that the WTP obtained from iterative bidding was up to 40% higher than the highest value chosen by respondents from a range of bids listed on a card. This suggests that iterative bidding may be a vital technique for obtaining maximum WTP. (Iterative bidding is somewhat analogous to auction bidding, so it is not a novel human experience.)

16.5.1.4 Dichotomous Contingent Valuation

Continuous valuation models that use some type of bidding procedure risk starting point biases. Furthermore, consumers usually do not make purchasing decisions by bidding, but rather by accepting or rejecting a posted price. Partly for these reasons, the dichotomous CV model has gained increasing acceptance. In addition, the dichotomous model can be easily adapted for use in mail surveys, whereas bidding formats are much more difficult to implement by mail.

In the dichotomous CV model, the range of plausible WTP values is divided among respondents. Each individual is asked his or her willingness to pay a single amount. A "yes" answer is coded "1" and a "no" answer is coded "0." Maximum willingness to pay is then derived from a logit or probit analysis of the probability that a given respondent will pay various amounts. The logistic function is simpler to deal with and a logit analysis is most frequently used; see Hanushek and Jackson (1977), Daganzo (1979), and Stynes and Peterson (1984) for discussions of these models.

Some pretesting with an open-ended WTP format is recommended to determine the range of WTP values for the survey. Because pretesting is usually limited, however, a few values that are higher than any found in the pretest should be incorporated in the final survey. Logit estimation does not require repeated observations of given values (Loomis 1988).

As an example of how values have been chosen and divided among respondents, Kay et al. (1987) included a dichotomous CV question in a mail survey sent to a sample of the general public to determine the degree of support for restoration of Atlantic salmon in 14 New England rivers. The survey question previous to the dichotomous choice question asked respondents to make a "payment vehicle" choice between federal income taxes, state income taxes, sales taxes, or higher electricity bills (some older hydropower plants would likely be shut down to accomplish restoration objectives). The dichotomous choice question then read:

> Using an increase in (the payment means chosen above), would you be willing to pay $_____ more next year in order to help the Salmon Restoration Program succeed in bringing Atlantic Salmon back to New England rivers?

Seventeen values ranging from $1 to $100 were used and randomly written into the questionnaires. From the results, a probability distribution for positive responses indicating willingness to pay was developed corresponding to the logit function.

Bishop and Heberlein (1990) recommended the following procedure.

(1) Use an open-ended pretest question to develop a preliminary logit function $f(x)$.

(2) For a final sample size of N, draw $N/2$ probabilities from the interval 0.00 to 1.00. Estimate the dollar amounts associated with those probabilities from the preliminary logit $f(x)$ and assign those to half of the questionnaires.

(3) For the other half of the sample, use 1.00 minus each of the above probabilities. Estimate the dollar amounts associated with these new probabilities from the preliminary $f(x)$ and assign them to the remaining questionnaires.

This procedure provides a balanced set of values for observations across the expected range of dollar amounts from which the final logit function will be derived. McCollum et al. (1990) noted that the critical areas for estimating a logit function are the median and the points of inflection. They suggested concentrating data points in this range.

Ordinary least-squares regression cannot be used to estimate this type of dichotomous dependent variable. The expected function $f(x)$ is nonlinear, and linear regression would allow predicted probabilities less than 0 or greater than 1. Instead, the logit function and logistic regression are used. The general form of the logit equation is

$$\Pr(\text{yes}) = 1/\{1 + \exp[-f(x)]\};$$
$$\Pr(\text{no}) = [1 - \Pr(\text{yes})] = 1 - \langle 1/\{1 + \exp[f(x)]\}\rangle;$$

Pr denotes probability, and $f(x)$ is a function of the variables that predict respondents' answers to the value question (Boyle and Bishop 1988).

The first equation above can also be expressed in a logistic regression format as

$$\log_{10}\left(\frac{\Pr(\text{yes})}{1 - \Pr(\text{yes})}\right) = \alpha + \beta(\text{amount}).$$

Additional independent variables—number of fish caught when respondents visited the site, socioeconomic characteristics such as income, quality of the fishing experience, and so on—can be added to this model to investigate which variables are statistically significant and how they affect willingness to pay. The area under the logit curve is the expected value (mean) of maximum willingness to pay. The expected value can be calculated either mathematically through integration or geometrically. Geometrically,

$$\text{WTP} = \sum_{i=1}^{N}\left[(P_i - P_{i-1})\,\frac{(X_i + X_{i-1})}{2}\right];$$

X_i is a particular monetary amount and P_i is the probability of paying X_i (Loomis 1988).

Many of the leading packaged statistical programs (e.g., SAS, SPSS) have logistic regression programs that can facilitate the analysis. For a thorough discussion of the use of logistic regression to estimate WTP in dichotomous choice models, see Loomis (1988).

Biases Associated with Dichotomous Models. A frequent dilemma in the use of dichotomous values is whether to truncate the model at some very high WTP value, above which the probability of a "yes" response is very low. Calculating the expected value from a cumulative density function in theory requires integrating from zero to infinity. If the function had a very fat upper tail, total WTP could produce a value of the resource that probably would be unjustified. Previous authors have handled this problem in different ways. Hanemann (1984) recommended using the median of the distribution (where the probability of rejection equals 0.5) rather than the mean (or expected value). Bishop and Heberlein (1990) felt that the mean should be used, because the median would exclude those who value the resource most. They believed that if WTP values are chosen for the survey as they recommended (presented earlier in this section), and if the model is truncated at a probability of 0.99 that payment will be refused, fat tails are not likely to pose a problem. Other authors have pointed out that the income distribution for the general public has an upper tail, and it would be reasonable for WTP functions to have a similar upper tail.

16.5.2 Travel Cost Models

The travel cost method (TCM) is an indirect way of estimating benefits associated with specific recreation sites. It can be used to infer the benefits that accrue to visitors at existing, new, or improved sites. It can also be used to estimate the benefits associated with improvements at recreation sites. It cannot be used to estimate the benefits that accrue to the general population (which includes nonusers as well as users) or the population's willingness to pay. The method is most effective in estimating the benefits associated with travel to a specific site when visiting that site is the sole purpose of the trip.

The concept of TCM dates from Hotelling's (unpublished) suggestion in 1949 that examination of travel costs could be a way to estimate values for visits to national parks. The method was further developed and implemented by Clawson (1959), who used data from several national parks. Clawson's early use of TCM was primarily to estimate how fee increases would affect attendance at federal park and recreation areas. The TCM was illustrated from this perspective in the classic text on outdoor recreation economics by Clawson and Knetsch (1966). For additional detail on the development of the TCM and its use through the mid-1970s, see Dwyer et al. (1977). Many resource economists have made subsequent refinements to the TCM.

The essence of the TCM is that the cost of travel to a recreation site can be used as a proxy for the price that people pay for the site's nonmarket services. Therefore, a demand curve can be estimated for the site that shows how many people will visit the site in relation to alternative costs of traveling to it.

The basic idea of the travel cost model can be seen in Figure 16.4. If the number of trips taken by an individual to a particular fishery, the travel cost (price) of reaching the site, and the cost at which the angler is no longer willing to travel to the site are known, a simplified demand curve can be constructed for that individual. With this typical downward-sloping demand curve, the individual derives greatest consumer surplus or net benefit for the first trip and less for each succeeding trip, until for the fifth and last trip (in this example), the benefits received are only equal to the costs paid for the trip (there is no consumer surplus). This is consistent with economic theory of diminishing marginal benefits

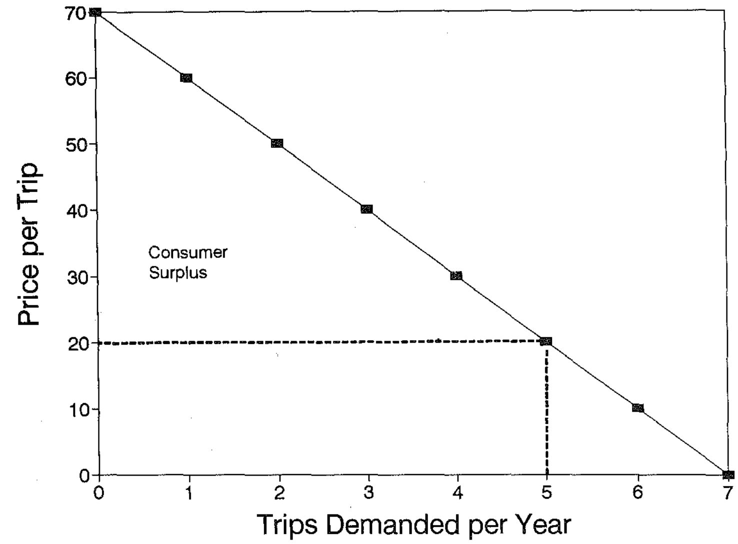


Figure 16.4 Simplified individual demand curve from travel cost data.

as consumption of a good or service increases. The demand curve can be used to estimate the number of trips an individual would take if the price were increased or decreased. This is particularly useful in estimating the demand for a new site.

The method of estimating individual demand curves is quite limited. Unless individuals visit a particular site several times a year and also visit similar sites at various distances from their residences, insufficient information will be available to derive a demand curve. In the more robust and useful form of the travel cost method, a demand curve is derived for a site with data on per capita visitation from various distances or distance zones around the site. This is illustrated in the next section.

16.5.2.1 Travel Cost Zonal Model for a Site

The general approach to the zonal TCM has two steps: (1) estimating a per capita demand curve, and (2) using the per capita curve to derive an aggregate site demand curve. The basic assumption of the TCM is that other factors held constant, the number of trips to a recreation site will decrease as the monetary and time costs of travel to the site increase. By integrating or otherwise estimating the area under the demand curve, the TCM provides an indirect estimate of consumer surplus benefits (Walsh 1986).

Use of the general model requires at least the following information: (1) estimates of population by distance zones surrounding the recreation site, (road distances from visitors' residences to the site), (2) site visitation data, (3) an estimate of motor vehicle variable costs, and (4) income data.

A simplified illustration of the basic zonal TCM comes from the National Economic Development Procedures Manual (U.S. Army Corps of Engineers 1986). Potential benefits are to be estimated for a proposed multipurpose reservoir that will have day use recreation and overnight camping. Good Time Lake, a similar existing reservoir in the same geographic area, is used to develop day use projections for the proposed reservoir. Good Time Lake does not have overnight camping.

Table 16.1 Distance zones and annual use estimates for Good Time Lake.

Zone	Distance from lake (miles)	Annual visitor days	Percentage distribution
1	0–25	353,345	55.8
2	26–50	190,420	30.1
3	51–75	33,685	5.3
4	76–100	28,185	4.5
5	101–125	27,495	4.3
All		633,130	100.0

The TCM uses distance traveled as a proxy for price, so the first step is to create several distance zones for plotting visitors' residences relative to the site. Distance zones traditionally take the general shape of concentric rings around the recreation site (or partial rings if the site is coastal). Each zone should be constructed so that actual road distance to the site from any point within the zone is similar. Thus, zones will be deformed from circular or elliptical shapes when barriers such as mountains force residents to drive particularly great distances to the site. Good Time Lake draws all of its visitors from distances not exceeding 125 miles. The researchers decided to create five concentric distance zones, each approximately 25 miles in width, around the lake. The annual day use visitation was then cataloged by zones, as shown in Table 16.1.

The next step is to estimate the population of each distance zone. This is best done by county for each county that falls within the distance zone (if counties are fully divided into townships, the townships can be used instead). If a county only partially falls within a zone, the populations of all urban centers within the zone are counted, and then the rural population is prorated according to the proportion of the county's area that falls within the distance zone. If cities overlap distance zones, census tract population data can be helpful in allocating the population to a particular zone.

The population data for Good Time Lake zones are shown in Table 16.2. Because each successive zone is larger in area than the previous zone, it contains (in this example) more counties and higher population than the next closer zone to the site. (In some regions, however, population may decline with distance from an amenity.)

The last step in the first phase is to derive the zonal per capita use rate for the reference lake. For Good Time Lake, the total visitation from each zone (Table 16.1) is divided by the population of each zone (Table 16.2) to get the visitation rates shown in Table 16.3.

Table 16.2 Counties and populations within distance zones around Good Time Lake.

Zone	Distance from lake (miles)	Number of counties partly within zone	Estimated population
1	0–25	2	79,741
2	26–50	8	801,178
3	51–75	10	2,472,318
4	76–100	18	4,307,937
5	101–125	25	4,361,719
All			12,022,893

Table 16.3 Estimated per capita visitation to Good Time Lake by distance zone.

Zone	Distance from lake (miles)	Annual visitor days	Per capita population	Per capita visitation
1	0–25	353,345	79,741	4.4311
2	26–50	190,420	801,178	0.2377
3	51–75	33,685	2,472,318	0.0136
4	76–100	28,185	4,307,937	0.0065
5	101–125	27,495	4,361,719	0.0063

It is assumed that per capita visitation rates from each zone will be the same for the new multipurpose reservoir as they are for Good Time Lake. Those rates thus are applied to zonal populations around the new reservoir site to obtain the visitation schedule shown in Table 16.4. The resulting estimate of projected use, 3.2 million visitor-days, represents the initial point on the X-axis of the demand curve, where $Y = 0$. This estimate presumes that travel costs will be no different than they are for trips to Good Time Lake. The next step is to derive the rest of the demand curve (or net benefit or consumer surplus curve). This is done by estimating the amount of visitation that would be expected if travelers from each distance zone confronted higher travel costs associated with the more distant zones. To derive the second point on the demand curve, visitation rates are calculated as though the site were an additional zone (25 miles) away from all visitors' residences. If the site were 25 miles further away, people in zone 1 would not be expected to participate at their original rate of 4.4311 per capita, but at the rate of 0.2377 visits per capita, the former rate for zone 2 participants. Similarly, zone 2, 3, and 4 participants would now be expected to participate at the initial rates of zone 3, 4, and 5 participants, respectively. Zone 5 participants would not be expected to participate at all because they now are more than 125 miles from the site, and their travel costs would be prohibitive. These calculations are shown in Table 16.5.

The same procedure is continued for estimating the third, fourth, and fifth points on the demand curve. This information is summarized in Table 16.6, and the resulting demand curve is shown in Figure 16.5. The area under the curve represents the consumer surplus or the additional amount people would be willing to pay in the form of travel to use the reservoir.

To estimate the dollar benefits under the curve, the mileage increments must first be converted into dollars. The value of travel has two components, the out-of-pocket or variable cost of operating a vehicle, and the value of travel time.

Table 16.4 Expected visitation to the multipurpose reservoir by distance zone.

Zone	Distance from lake (miles)	Number of counties partly within zone	Base year projected population (P)	Per capita visits to Good Time Lake (V)	Projected reservoir visits (P × V)
1	0–25	3	679,444	4.4311	3,010,684
2	26–50	7	491,958	0.2377	116,938
3	51–75	13	3,394,276	0.0136	46,162
4	76–100	15	4,425,762	0.0065	28,767
5	101–125	22	2,675,484	0.0063	16,856
All					3,219,407

Table 16.5 Participation estimates for the second point on the demand curve representing a 25-mile increase in travel distance to the multipurpose reservoir.

Zone	Zone population (P)	Per capita visits (V)	Estimated visits (P × V)
1	679,444	0.2377	161,504
2	491,958	0.0136	6,691
3	3,394,276	0.0065	22,061
4	4,425,762	0.0063	27,882
5	2,675,484	0	0
All			219,138

When this illustration was formulated in 1981, the average mileage rate was US$0.141 per mile. Survey results from Good Time Lake indicated that the average party size was 3.5. Thus, the average mileage cost per person was estimated at $0.141/3.5 or $0.041 per mile.

Income data were not available for the sample of Good Time Lake visitors, but an average wage rate of $7.15 per hour was derived from state employment data. The U.S. Water Resource Council estimated (in 1979) that the value of adult travel time is one-third the wage rate (here, $2.38) and the value of children's travel time is one-twelfth the wage rate ($0.60). (See Section 16.5.2.3 for valuation of travel time.) The average vehicle had 2.0 adults and 1.5 children, so the average weighted value of travel time per person per hour was $1.61.

Given an average vehicle speed of 45 miles per hour, the time cost to drive a 25-mile segment was estimated at $0.90 one way (25 miles × $1.61 per hour/45 miles per hour) and $1.80 round trip. The vehicle cost per person for the 25-mile increment was $2.05 (50-mile round trip × $0.041/mile). Table 16.7 was constructed with this information.

Plotting total cost per visit on the Y-axis and total visits on the X-axis and

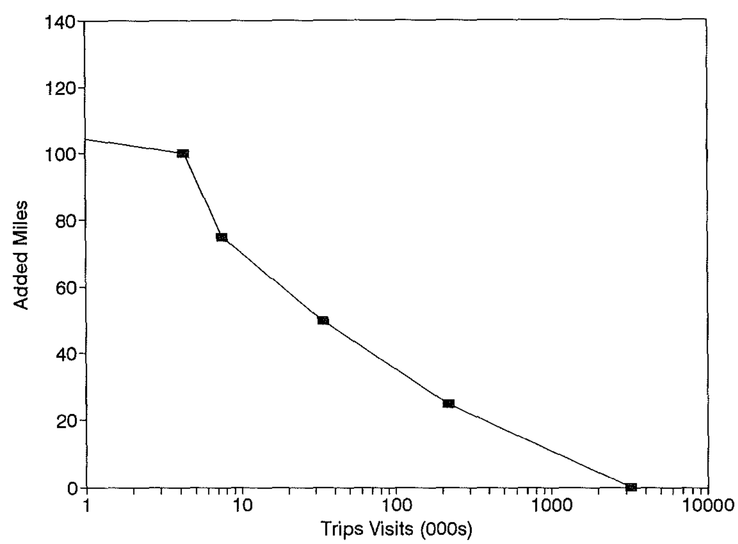


Figure 16.5 Second-stage demand curve for the multipurpose reservoir. (X-axis is shown in log scale).

Table 16.6 Second-stage use demand schedule for the multipurpose reservoir.

Added miles	Visits from distance zone: 1	2	3	4	5	Total visits
0	3,010,684	116,938	46,162	28,767	16,856	3,219,407
25	161,504	6,691	22,061	27,882	0	219,138
50	9,240	3,198	21,384	0	0	33,822
75	4,416	3,099	0	0	0	7,515
100	4,280	0	0	0	0	4,280
125	0	0	0	0	0	40

connecting the points provides the second-stage aggregate dollar demand curve (Figure 16.6). To estimate dollar benefits, one must determine the area under the curve. This can be done geometrically by successive intervals, as illustrated in Figure 16.6. For example, the area under the top part of the curve between total costs of \$19.25 and \$15.40 (from Table 16.7) and from 0 to 4,280 visits (from Table 16.6) is simply

$$0.5[(\$19.25 - \$15.40)\ (4.28 - 0)] = 0.5[\$3.84 \times 4{,}280] = \$8{,}239.$$

Summing all of these increments yields a total of \$7,217,400 (rounded to the nearest hundred dollars). This is the estimate of total day use benefits for the multipurpose reservoir. The average net benefit per individual per visit can be calculated by dividing this total by the number of visits estimated at zero additional miles (the sum of visits from the top row of Table 16.6 or the total from Table 16.4):

$$\$7{,}217{,}358/3{,}219{,}407 = \$2.24.$$

16.5.2.2 Defining Distance Zones

Use of the zonal version of the travel cost method requires delineation of several zones for which per capita visitation can be estimated. Fisheries that are used almost entirely by local residents or by local residents plus those from an urban center 20 miles away are not good candidates for the zonal method, because it would be difficult to identify enough zones from which to construct a demand curve. The zones chosen are less susceptible to biases if they are similar in size. They need not be in concentric rings around the site; for example, counties of residence can be used as zones (Swanson and McCollum 1991).

Invariably a few outlier points will reveal that some anglers came long distances (hundreds of miles; sometimes more than a thousand miles) to fish a water body that has only regional significance. Such outliers often involve multipurpose trips,

Table 16.7 Estimated dollar values of travel cost to the multipurpose reservoir.

Increments (miles)	Round-trip mileage	Time cost of travel	Vehicle cost per person	Total cost
25	50	\$ 1.80	\$ 2.05	\$ 3.85
50	100	3.60	4.10	7.70
75	150	5.40	6.15	11.55
100	200	7.20	8.20	15.40
125	250	9.00	10.25	19.25

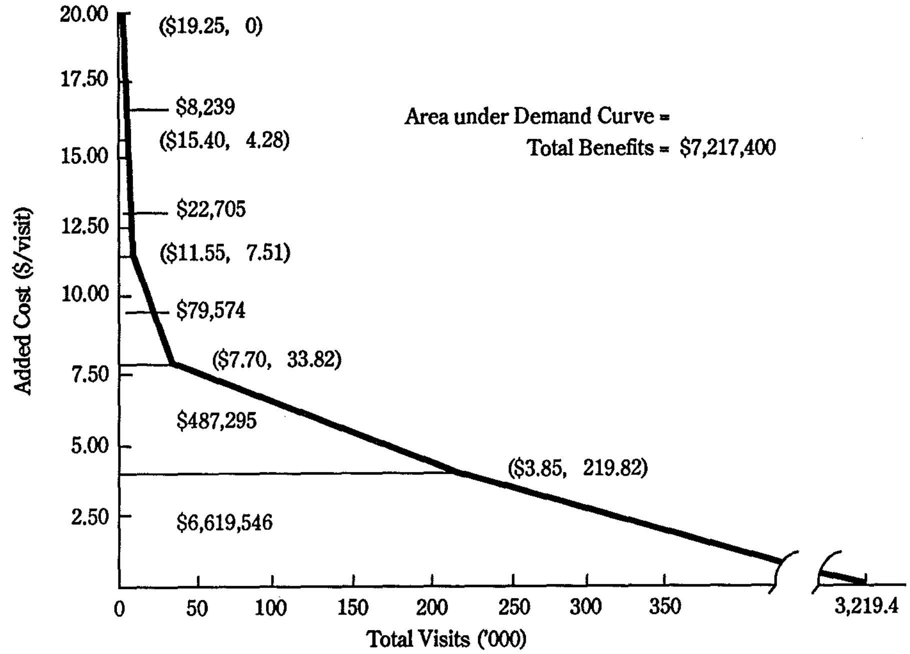


Figure 16.6 Day use benefit estimation for the multipurpose reservoir.

which the travel cost method is poorly equipped to handle. A rule of thumb sometimes used is to cut off observations beyond the distance from which 95% of visitors travel to reach a site.

16.5.2.3 Accounting for the Value of Time

As noted in the multipurpose reservoir example, people who take recreational trips invest time as well as money in a fishing experience. To some anglers pondering a more distant fishing visit than usual, the added time cost is a larger deterrent than the out-of-pocket cost of driving the additional round-trip distance. Several researchers have investigated the value of travel time, but no empirical values have gained widespread acceptance. Cesario (1976) and others have estimated that the value of nonwork travel time for adults is between 25% and 50% of the wage rate (thus a figure of one-third of the wage rate is often used). For children, the value of one-twelfth of the adult wage rate has been used.

What about the time anglers and other recreationists spend at the recreation site? Knetsch and Cesario (1976) and Mendelsohn and Brown (1983) indicated that the value of on-site time should not be included unless no on-site benefits are expected. These on-site costs are not related to an individual's marginal cost (i.e., the last incremental financial or time cost) of reaching the site for recreational use.

16.5.2.4 Which Expenses to Include

The only expenditure used in the example of the multipurpose reservoir was a mileage cost of operating a vehicle. When visitors to a recreation site must travel so far that food and lodging have to be bought, these expenses should be added to the time and vehicular costs of travel. However, costs that are constant for people from different zones, such as an admission fee to the site or a camping fee where all visitors are campers, need not be included, because such costs do not affect the slope of or the area under the demand curve (Walsh 1986).

16.5.2.5 Estimation Issues

Estimation issues for travel cost models have two primary dimensions: the functional form of the demand equation, and the variables included in the demand equation.

Several functional forms are used to improve upon the simplified assumptions of linear demand curves. These include quadratic ($Y = a - bX + cX^2$), semilog ($\log Y = a - bX$), and double-log ($\log Y = \log a - b\log X$) models. These are all consistent with the theory that the larger the price variable, the smaller will be the marginal effect of price on the number of trips demanded (Walsh 1986). Each form has some advantages and disadvantages. For example, the logarithmic forms never touch the axes, and a point must therefore be estimated where the curve should reach each axis. The double-log is the only form in which the regression coefficient corresponds to the elasticity (the percent change in the dependent variable associated with a 1% change in each of the independent variables). The semilog form allows easy estimation of the average consumer surplus (1.0 divided by the regression coefficient for direct cost or price).

The simplest travel cost studies do not have accompanying survey research data. They are limited to analyses of registration forms or vehicle license plates from which place of residence can be discerned, allowing estimation of travel costs. Such analyses, in which a linear demand equation is derived, are similar to the one illustrated above for the multipurpose reservoir. The demand equation is simply

$$V_i/N_i = a + b\text{TC}_i;$$

V_i/N_i = per capita visitation from origin i to the destination site, and
TC_i = travel cost from origin i to the destination site.

However, economic theory suggests that demand curves often are not linear and that several factors besides number of trips taken affect trip cost. Furthermore, in addition to travel cost, socioeconomic characteristics of each zone, such as income and age distributions, influence the rate of participation in a particular fishery. Thus, the above equation should be broadened to

$$V_i/N_i = f(\text{TC}_i, E_i),$$

where E_i represents the socioeconomic variables (Swanson and McCollum 1991). Running the regression analysis requires first converting the average travel distance for each zone into travel cost (Sections 16.5.2.2 and 16.5.2.3). Once the equation is specified, it can be used to estimate per capita consumer surplus.

Substitute sites are important to consider in estimating recreational demand. If the visitation radius of a proposed reservoir overlaps the visitation radius of an existing reservoir with similar fishing opportunities, the demand for the new reservoir will be overestimated if the existing (substitute) site is ignored. Some additional, "induced" demand will be created when the new reservoir is built, but presently there is no way to estimate induced demand except by analogy with previous studies. The existing demand for all similar fishing sites within the market area of the new reservoir will be reallocated by anglers in the region, based largely on travel cost. If the various sites are not true substitutes for each other (e.g., one has larger fish, another has more aesthetic appeal), the differences between them will affect choices of fishing location.

16.5.2.6 Biases Associated with the TCM

One of the greatest limitations of the TCM is the inherent assumption that people willingly travel the distances and spend the time they do for the sole purpose of recreating at one specific site. This often is not the case. Anglers may fish at several sites within a region. They may combine fishing with other activities such as visiting friends and relatives, sightseeing, or business travel. Unless survey data containing this information are available and travel cost models can be modified appropriately, the estimates of net benefits derived from the models probably will be overstated. The most straightforward way of dealing with this problem is to cut off the highest 5% of observations and assume that these represent multipurpose trips. Rather than be discarded, these observations might be assigned the mean consumer surplus value for the 95% of observations retained in the analysis.

16.5.2.7 Extension of the TCM to Multiple Sites

The TCM also may be also be applied to multiple sites within a region, state, or province. The multisite or regional model operates on the same general principles as the single-site model, but it is based on data from all similar sites in the region. However, most regional models take into account that participation cost is not the only variable that influences angler demand for fishing at specific sites. These models require survey data from participants and use additional variables in the regression equation to estimate per capita trips to each site:

$$V_{ij}/N_i = f(\mathrm{TC}_{ij}, E_i, S_j, Q_j);$$

V_{ij}/N_i = number of trips per capita by residents of origin i to site j (from all origins to all sites);
TC_{ij} = average round trip travel costs (including value of travel time);
E_i = socioeconomic characteristics of origin i visitors;
S_j = price of substitute sites available to origin i residents; and
Q_j = measure of quality of site j.

In this and all models, the type of fishing compared should be similar among sites, such as coldwater or warmwater fishing. Trips for both warmwater and coldwater fishing can be combined into either group, but if aggregate consumer surplus over all trips is being estimated, combining mixed trips in both warmwater and coldwater categories would result in double counting.

A measure of substitute sites might simply be the distance to the nearest site that is a reasonable substitute for the site chosen. Typical quality measures are the rate of catch (fish per hour or day) or total catch at a given site.

Sorg et al. (1985) used a regional TCM to estimate the net economic value of cold- and warmwater fishing in Idaho. Because the study involved users statewide as well as from out of state, and users who visited many fishing sites, a regional travel cost model (RTCM) was developed. The basic RTCM is

$$\mathrm{TRIPS}_{ij}/\mathrm{POP}_i = b_0 - b_1\mathrm{DIST}_{ij} + b_2\mathrm{QUALITY} - b_3\mathrm{SUB}_{ik} \pm b_4\mathrm{INCOME}_i;$$

$TRIPS_{ij}/POP_i$ = trips per capita from origin zone i to site j;
$DIST_{ij}$ = round trip mileage from residence county (i) to fishing site (j);
QUALITY = a measure of fishing quality at the site (catch per unit effort);
SUB_{ik} = a measure of the cost and quality of substitute fishing site k to origin i anglers relative to the site j under consideration; and
$INCOME_i$ = a measure of ability of county i households to incur fishing costs, and a proxy for other taste variables.

This study was conducted by a combination mail–telephone technique in which the sample was notified of the study by mail, provided a map of 51 Idaho fishing areas, and asked to list their fishing trips in advance. Data were then gathered by telephone. The contingent valuation method was also used for comparison. The analysis separated coldwater from warmwater fishing trips. Visitor origins were counties or county groups to facilitate calculation of trips per capita from each zone of origin.

Several independent variables were tested. For the QUALITY variable, fish caught per hour was not statistically significant in the regression equation, but total fish caught on each trip was significant for both warmwater and coldwater fishing, and hence was used. For the substitute site variable SUB, an index was derived that reflected both the location (travel cost) and the quality of substitute sites. First, an average distance was calculated from each origin zone to each of the $k = 50$ regional fishing areas and to the comparison site j. A ratio of harvest (coldwater or warmwater) to distance traveled from zone i was calculated for each site k and for site j. Any site k for which this ratio was larger than for site j was a cost-effective substitute for site j. The ratios for all sites that exceeded that for site j were then summed, and the sum became the value of SUB. The larger the value of SUB, the more cost-effective are the substitute sites, relative to site j, and (other factors constant) the less fishing would be expected at site j. Thus, the coefficient of SUB is expected to be negative.

Sorg et al. (1985) chose the functional form for their data after carefully reviewing both the data and economic theory related to functional form (readers who are not well versed in regression and econometric analysis should contact a statistician and a resource economist). The final form chosen (TOTFISH is total fish caught) was

$$\log_e(\text{TRIPS/POP}) = a - b\text{DIST} + c\text{TOTFISH} - d\text{TOTFISH}^2 - g\log_e(\text{SUB}) + h\text{INCOME} - m\text{INCOME}^2 .$$

The per capita demand curves were then used to derive a second-stage aggregate demand curve for each fishing site. This was done by setting TOTFISH at that site's value and setting INCOME at the origin's value. Then distance was set at its current value for a given origin to calculate estimated visits per capita at current distance. Visits per capita were then multiplied by the origin's population to calculate visits from the origin. Next, 200-mile increments were successively added to distance until the maximum observed distance was reached or until visits from that origin fell to less than one. This provided an upper limit for integration of the demand curve, which is necessary for the logarithmic form of visits per capita. Results showed that the average net value of coldwater fishing in Idaho was \$42.93 per trip or \$25.55 per day. Warmwater fishing values averaged \$42.18 per trip or \$26.36 per day.

16.5.2.8 Summary of the Travel Cost Method

Simplified travel cost models are useful because they require no survey data. Data requirements for more detailed single-site models are not extensive and could be obtained in conjunction with creel surveys that use complete trip sampling methods. The TCM has the potential advantage over contingent value surveys of being based on actual angler behavior rather than on hypothetical values reported by anglers.

The TCM has several limitations. Dealing with multipurpose trips, estimating the value of travel time, specifying measures of site quality and substitute sites, and choosing the functional form of the equation have been discussed above. In addition, the TCM deals only with use-related values. People also value recreation sites for their convenience, for their uniqueness, or just for their existence ("existence value"). For example, Bishop et al. (1987) found that Wisconsin residents assigned a total value of $12 million per year for the preservation of striped shiner, an endangered species present in a tributary of Lake Michigan. Existence values can be incorporated into the contingent valuation method, but not into the TCM.

The more sophisticated TCM models involve further estimation complexities and potential biases not dealt with in this introduction. For additional references, see Adamowicz and Phillips (1983), Menz and Wilton (1983), and Strong (1983).

Chapter 17

Surveys for Social and Market Analysis

17.1 INTRODUCTION

Human aspects of fisheries management have gained increased attention in recent years. This has stemmed from several fisheries use and allocation issues, and also from the increased realization that the goal of fisheries management is to optimize society's total benefit from fisheries resources and the environments surrounding those resources (Nielsen 1976). It has been noted that good science becomes good management only if the management plan or regulations are accepted by anglers (Pringle 1985). Matlock (1991) argued that a science-based fisheries decision must incorporate human dimensions as well as biological information. Voiland and Duttweiler (1984) challenged fishery managers to develop a better understanding of the users of fisheries resources as a step toward more progressive management.

Creel surveys, the most common means of obtaining data from fisheries users, have traditionally been used more to obtain information about the fish people catch (and related effort) than about the people themselves. Carl (1982) illustrated how changes in the users of a fishery would have been missed and the satisfaction of those anglers would have been misunderstood if research had been restricted to traditional creel survey data. Portions of the Huron River in southern Michigan were treated with rotenone in 1972 to reduce populations of rough fish. Seven species of sport fish were subsequently introduced. Although the catch rate dropped markedly from 1972 to 1974, creel data showed fishing effort and angler satisfaction to be unchanged over this period. Further information revealed, however, that the fishing clientele changed markedly over that period. Many African American anglers who enjoyed the higher catch rate of rough fish stopped fishing; they were replaced by a group of largely white anglers who sought the newly stocked species. Carl suggested that without the latter information, the evaluation of angler reactions to the fisheries management treatment would have been quite different.

Orbach (1980) noted that fisheries managers need a clear picture of the ways in which people participate in fisheries and of the benefits and satisfactions anglers receive. Each user derives somewhat different benefits from the resource and has a different set of preferences as to how the resource is used. Information from these users is needed to understand the impact of potential management decisions on the people involved in fisheries activities, to better allocate finite fishery resources among competing user groups, and to better understand how the complete fishery system (including managers, scientists, and users) works.

The limited research undertaken to date suggests that fishery scientists are only partially aware of angler preferences and values. Miranda and Frese (1991) found that fishery scientists had only 54% success in predicting average angler responses to a variety of preference and value questions. Scientists were least able to predict the factors that anglers felt were associated with fishing quality. Thus, information from anglers is a necessary ingredient in providing the fishing experiences sought by anglers.

Developing an understanding of the users of fisheries is important not only for direct management purposes such as establishing regulations, but also for marketing purposes. Many fisheries provide economic development opportunities for nearby communities. By knowing the fishing interests and motivations of those who live near a particular fisheries resource, as well as the types of benefits the resource can produce, the fishery can be enhanced (whether by stocking or facilities development) and successfully marketed for increased angler benefits and local economic impact.

Social scientists have used a number of techniques and concepts to develop a better understanding of human behavior in relationship to fishing and other recreational activities. This chapter covers the most frequently used social concepts. Its objective is to provide enough information that readers will be able to conduct straightforward studies of angler attitudes and preferences in relation to fisheries issues, of angler satisfaction and fishing involvement, and of fisheries-related marketing.

Information of the type discussed in this chapter is usually obtained from anglers off site via telephone or mail surveys, although a limited number of questions could be asked as part of an on-site creel survey. It is important to the success of any fishing survey that the questions be carefully worded, that the nature and scope of the material be appropriate to the survey technique, and that as much of the sample as possible be completed. These concerns are covered in earlier chapters. Refer especially to Chapter 4 for material on the wording of survey questions.

17.2 MEASURING PREFERENCES

The term "preference" is an uncomplicated public opinion concept that refers simply to a given choice or option that people like or desire more highly than one or more alternatives. Preferences concerning alternative fishing regulations or policies or preferences for particular species or methods of fishing are among the easier human behavioral concepts to measure. As an example, consider the following question that was asked in a statewide survey of New York anglers.

> Some waters can be managed to produce more large (15 inches or more) largemouth and smallmouth bass, but this usually requires that anglers keep fewer fish. Or, these waters can be managed to provide greater numbers of bass for anglers to harvest, but with fewer large fish. Which option do you prefer?
>
> ______ More large (15 inches and greater) bass, but with fewer fish available for harvest
>
> ______ More bass available for harvest, but with fewer large fish
>
> ______ No preference

Preference questions, if sufficiently specific, can provide important public input to managers. The above question was designed to help managers determine whether current bass catch and size regulations were in line with angler preferences, or whether most anglers would prefer an adjustment in the regulations. Often a single question will be sufficient to measure preferences on any single topic (the New York survey asked a similar question with regard to trout).

Preference questions can also be used in situations involving regulations to determine which type of additional harvest constraint is least objectionable. Dawson and Wilkins (1981), with separate samples of New York and Virginia saltwater boat anglers, determined that setting minimum size limits on fish kept was less objectionable than daily catch limits, rod or line limits, or limits on sale of fish caught by sport anglers.

The primary limitation to preference questions, if used alone, is that they do not provide insight into the underlying attitudes and beliefs that influence a particular preference. Preferences can be based on misinformation or on correct information about other relevant factors. To illustrate, in waters where contaminants are a problem, many anglers might indicate as a survey response that they prefer a creel of more smaller fish to fewer larger fish, not because they genuinely prefer to catch more smaller fish, but because they believe the larger fish contain higher contaminant levels and are unsafe to eat. Thus, armed only with answers to the above question, managers might use the data and implement new regulations that are in the short-term interest of most anglers. However, the managers might well misjudge the true fishing preferences of anglers and their concern about contaminants.

Although the above preference question is rather straightforward, it incorporates principles of good question design that are important to heed. First, the body of the question provides sufficient information and the basic options. Second, the category options for anglers to choose from follow directly from the options stated in the body of the question. Third, the answer categories include the full range of options, including "no preference."

17.3 MEASURING ATTITUDES

"Attitudes" refer to feelings or dispositions of people toward some entity that is generically referred to as the object of the attitude. These "objects" may be physical (e.g., particular fish species, types of waters, landscapes adjacent to waters), social (e.g., other anglers of particular types, biologists, law enforcement staff; the behavior of these or other individuals), or institutional (e.g., the fisheries agency, its regulations or policies). Attitudes differ from interests or preferences in that they always measure *feelings* toward some *object* (Nunnally 1978; Eagly and Chaiken 1993). Examples of attitude statements are the following.

- The intentional foulhooking of any fish is unethical.
- A large rainbow trout is too beautiful to keep and should be returned to the water.
- The Sport Fishing Institute does a good job of keeping American fisheries issues visible to the U.S. Congress.

Angler attitudes are important to fisheries managers because they are important factors that affect anglers' ultimate behavior (e.g., obeying a fishing regulation, buying a license, voting for an environmental bond act that will help finance fishing facilities). Attitude toward a behavior is defined as a linkage of beliefs about the behavior itself and the results of engaging in the behavior. According to a widely accepted model of human behavior (Ajzen and Fishbein 1980), an individual's attitudes toward engaging in a particular behavior, together with influences or pressure from others (e.g., peers, family members), guide the formation of the individual's intention to engage in the behavior. The stronger the intention, the more likely that the behavior will be performed.

Thus, attitudes are imperfect predictors of specific behaviors. This is in part because attitudes about several related aspects of a topic may be relevant to determining a specific behavior. Moreover, a number of external (i.e., other social) as well as internal forces come into play in shaping human behavior. For example, some individuals may feel generally that foulhooking is unethical. However, if their social group enjoys snagging, and if regulations permit it for particular species, these individuals may participate largely because of peer pressure. If they are snagging for carp, which they disdain, or spawning salmon that will soon die, these circumstances may also help them rationalize a behavior that they would normally consider to be unethical.

Attitudes, like preferences, may reflect lack of information or misinformation. However, they should first be recognized for what they are: people's feelings about particular objects. As such, attitudes should not be examined in the first instance in terms of being "correct" or "incorrect." In the above examples, questions could be designed to determine whether most people think any intentional foulhooking is unethical, or that all rainbow trout are too beautiful to be harvested. An evaluation process could even be designed to determine over some period of time whether or not the Sport Fishing Institute did a good job (which would have to be operationally defined) of keeping certain fisheries issues before the U.S. Congress. But managers should remember that "reality" in the minds of anglers or other groups of interest is often what they think and feel. It is this *perception* of "reality" that researchers seek to measure through learning more about attitudes.

Attitudes may be measured by single statements or by a group of statements, each of which measures a different component of a broader topic. As an example of the latter, suppose an agency is faced with the question of whether to stock trout in remote streams to enhance the catch rate. A simple preference or attitudinal statement could be designed to measure user preferences or attitudes about this option. However, managers may want to examine this question in the larger context of user attitudes toward wilderness or wild areas. To what degree do various user segments see stocking trout in a remote area as enhancing the recreation experience versus detracting from the natural conditions of wilderness? Attitude statements might be designed to cover such topics as the appropriateness or desirability of (1) allowing small motors on boats in such an area, if appropriate water bodies are present; (2) paving pathways or undertaking other physical measures to enhance access and limit erosion; (3) allowing recreational vehicle camping in the area; (4) having a nature trail displaying the names of various trees and plants; (5) using fire suppression methods if a fire should start in the area; (6) stocking trout in appropriate streams; and perhaps a number of other statements about types of public use and management.

For any particular remote stream, anglers could be asked a preference question about any of the specific topics listed above. Several formats are possible, but at least three options would be offered: "implement or allow"; "do not implement or allow"; and "no opinion." Attitude statements, on the other hand, would typically focus on obtaining a measure of the respondent's orientation or feelings toward the idea or concept as distinguished from implementation at a specific place and time.

Several formats have been used to measure attitudes. The following discussion is limited to basic formats that measure respondents' reactions to particular statements along a unidimensional continuum. For each format, we want to clearly convey the attitudinal stance and present several points along a continuum from which respondents can choose the point closest to their feelings about the statement. Additional information about these formats can be found in Edwards (1957), Maranell (1974), Nunnally (1978), and Eagly and Chaiken (1993).

17.3.1 Likert-Type Format

In the Likert format (Edwards 1957), a set of attitude statements is introduced with an explanation of the response options, such as SA = strongly agree, A = agree, N = neutral or undecided, D = disagree, and SD = strongly disagree. The first attitude statement might then appear in a format like the following.

Stocking brook trout in streams in remote areas is desirable in situations where it will allow greater catch limits.

___ SA ___ A ___ N ___ D ___ SD

A slight variation of this format would be to follow the attitude statement with:

Strongly Agree	___ :	___ :	___ :	___ :	___ :	___ :	___ :	___ :	___	Strongly Disagree
	1	2	3	4	5	6	7	8	9	

Although the traditional Likert format has 5 points, as shown in the first illustration, the number may vary. Most researchers prefer an odd number of response options because it allows those with no opinion to take a midway or neutral position. Occasionally, if a topic has been sufficiently visible that the vast majority of respondents should have an opinion about it, an even number of options is used to force respondents to take a position on one side or the other of neutral.

Some researchers prefer a larger number of response options (e.g., 9). They may attempt to design an interval-level rather than ordinal-level measure that would allow them to use parametric statistics such as analysis of variance in comparing results across subgroups. However, several assumptions are necessary to use interval-level analyses, even with a 9-point scale, and one is safer using nonparametric statistics. Furthermore, respondents may not be able to mentally discriminate the difference between a score of 2 versus 3, or 7 versus 8 on a 9-point scale. Attitude statements should be pretested. If pretesting shows that some response options are rarely used, the number of response categories probably should be collapsed.

17.3.2 Principles of Good Attitude Statements

Attitude statements should be briefly worded, preferably in a simple sentence. They should use language that all respondents will understand and they should avoid the use of double negatives. Attitude statements must be straightforward and unambiguous, and they should deal with only one topic or idea per statement. For example, consider the statement "American Fisheries Society dues should be increased so the Society can devote more attention to international issues." This phrasing might be satisfactory under certain circumstances, but "agree" and "disagree" responses to this attitude statement will reveal neither how many members would be willing to pay a dues increase nor how many think more attention should be devoted to international issues. Some members may be very interested in giving increased attention to international issues, but feel that funds should be diverted from some existing activity. Others may feel that further international activities would be worthwhile but that a dues increase should be limited to members involved in the new initiative. Still others might support a dues increase but only for another activity. One way to explore this topic is to first determine the interest in giving further attention to international issues. Those who have that interest then would be asked how resources for that work should be mounted.

Attitude statements are typically used as variables in attempts to distinguish between various groups. A statement such as "Fisheries regulations are often necessary to assure protection of the resource" would likely be answered positively by an overwhelming majority of any group. Thus, it would be of little help in differentiating between, for example, those who think the minimum size limit for possession of muskellunge should be increased and those who believe the current size limit is satisfactory.

17.3.3 Semantic Differential Items

Another type of attitude instrument, the semantic differential, focuses on a given subject or concept and anchors it with succeeding pairs of adjectives that are opposites. As an illustration consider:

Catch and Release Fishing

Interesting	___ 1	___ 2	___ 3	___ 4	___ 5	___ 6	___ 7	Boring
Satisfying	___ 1	___ 2	___ 3	___ 4	___ 5	___ 6	___ 7	Unsatisfying
Innovative	___ 1	___ 2	___ 3	___ 4	___ 5	___ 6	___ 7	Traditional
Conserving	___ 1	___ 2	___ 3	___ 4	___ 5	___ 6	___ 7	Exploitive
Simple	___ 1	___ 2	___ 3	___ 4	___ 5	___ 6	___ 7	Complex
Challenging	___ 1	___ 2	___ 3	___ 4	___ 5	___ 6	___ 7	Unchallenging

Semantic differential scales have been used in a wide variety of social, political, and marketing contexts. They are widely applicable because attitudes toward most topics can be conveyed by adjectives. Most adjectives have near or exact opposites; for those that do not, an opposite can be created with a prefix such as un- or in- (satisfying and unsatisfying; effective and ineffective). A semantic differential format can use just a few contrasts for a topic (as above) or many items.

As is true of Likert-type items, semantic differential items can be analyzed strictly on an individual basis, or the scales can be further analyzed into a limited number of factors by factor analysis. A "factor" in this context is a grouping of items that people often answer similarly. For example, in a broad listing of items that people might enjoy about a fishing trip, some anglers will rate "catching the most fish in my group," "catching the biggest fish," and "beating the catch on my last fishing trip" as being important to a satisfying trip. If so, factor analysis would statistically group these items together with high coefficients. It would then be up to the researcher to interpret the statistical grouping, in part by naming the factor (in this case, competitive or achievement-oriented anglers). Factor analysis is explained in the leading statistical computing program manuals; see also Nunnally (1978).

17.3.4 Wording and Format Considerations

Regardless of whether the Likert-type or semantic differential format is used, each item should have "face validity." That is, it should clearly contribute to some facet of the overall concept being evaluated. Items must also be clear and unconfusing. If too many items are used, the attention of respondents is likely to wane. One way to enhance respondents' attention is to vary the wording of items such that some Likert-type items are worded negatively or in a manner such that many people would disagree. Similarly, semantic differential items should be varied so that sometimes the negative rather than the positive adjective is placed on the left-hand side.

17.3.5 Summated Rating Scales

Responses to specific attitude statements can provide an incomplete picture of anglers' perspectives on a given issue if the issue has several components. In such cases, anglers' attitudes about several related statements need to be considered simultaneously. Grouping related statements via factor analysis is one means of considering anglers' responses to several statements simultaneously. Another method is creating an additive scale, sometimes referred to as a summated rating scale (Spector 1992). Development of such a multi-item scale requires expertise beyond the scope of this chapter. However, the use of such a scale and the general construction procedures can be easily communicated. An understanding of the following material will enable a fisheries staff member to work effectively with a social scientist to develop an appropriate summated rating scale for the situation being considered.

Consider the earlier illustration of whether to stock trout in a remote stream. The following attitude statement should provide valid information on angler attitudes about this specific topic:

> Hatchery-reared brook trout should be stocked in Smith River to supplement the native population so that the catch limit can be raised.

However, the basis for an attitude response by many anglers will be larger than the specific fishery issue, and managers might want to understand the broader basis, particularly if they are also considering other improvements such as easier access, or facilities of any type. Often in areas that are as yet undeveloped, some anglers and other recreationists value the "wilderness," regardless of whether the area has such a formal designation. Different groups will have different ideas about the level of development that is acceptable for a remote area.

To create a summated rating scale, one first carefully defines the overall attitudinal construct or domain to be measured. In this case, it might be termed "naturalness." A naturalness construct in its simplest form (and the only one dealt with here) is represented by a linear scale somewhat analogous to a multiple-choice test, except that there are no correct or incorrect answers. Each item is scored (e.g., 1 to 5 for a 5-point scale) as to degree of naturalness selected for that item, and the item scores are added to arrive at a total naturalness score.

The naturalness scale for this example should have end points ranging from completely undeveloped to rather highly developed. At the undeveloped end, the scale should encompass the view that improvements of any kind, including stocking of fish, would not be desirable. The "highly developed" end of the scale can correspond to issues being considered for the study area. In addition to including an attitude item about the stocking of fish, the scale should include enough other plausible types of development that respondents who approved of these would not object to the stocking of fish, at least in terms of modification to the natural environment.

The second step is to design the scale. This consists of determining the scale format and designing a pool of scale items. The scale format typically consists of item statements with which respondents can agree or disagree. A heading with instructions for completing the scale must be designed. In addition, the number of item points and labels for those points must be designed. For example, in a scale dealing with attitudes about people's jobs, Spector (1992) used a 6-point scale with point labels "very much agree," "moderately agree," "slightly agree," "slightly disagree," "moderately disagree," and "very much disagree." As with all even-number point scales, this scale has no neutral category.

The individual items are designed to contribute specific elements to the overall construct. For our example, the different elements that collectively define naturalness in the context of the area in question need to be determined. Below are possible subjects for a set of items.

- Stocking of fish.
- Provision of automobile access and parking all the way into the site.
- Providing fire protection in case of a forest fire.
- Allowing motors on boats on Smith River.
- Installing a fish-cleaning station at the site.
- Allowing tent camping at the site.
- Building a recreational vehicle campground at the site.
- Paving walking paths at the site.
- Allowing snowmobiles and all-terrain vehicles to use the area.
- Building a convenience store at the site.

From the list, the specific wording of each item is constructed (e.g., "Snowmobiling on constructed trails would be an acceptable use of the Smith River site"). The draft scale is then pretested on several respondents to determine which items are ambiguous or confusing, and the items are reworded based on this feedback. If possible, the scale is then fully pretested on 100–200 respondents. The results of the pretest are subjected to an item analysis, and if necessary, a smaller set of items is chosen to form a scale that is internally consistent. Cronback's alpha is calculated to determine the reliability of internal consistency. Finally, the scale is implemented with a full sample of respondents.

The item responses, if a 5-point scale is used, can be scored 1 for strongly disagree to 5 for strongly agree. The individual item scores then can be added to arrive at a total "naturalness" score. Respondents might then be divided into three groups: those who have a strict naturalness orientation toward the area in question (i.e., "want no artificial improvements"), those who favor heavily developing the area to facilitate recreational use, and an intermediate group. By examining these groupings in comparison with their response to the specific attitude statement about stocking trout, fisheries staff can determine the extent to which attitudes expressed about the stocking of trout are related to their individual fishing preferences alone (e.g., an inherent preference for fishing for native trout), or whether their attitudes are also based on the level of management and development they feel is appropriate for the area.

An individual scale item may provide useful information by itself, yet not make a meaningful contribution to an attitude scale. An attitude scale should have both reliability (i.e., provide consistent measures for the same individual over time) and validity (i.e., measure the construct it is intended to measure). An item contributes meaningfully to a scale if respondents' scores on the item are significantly and positively correlated with their scores on the total scale. Particularly in reference to validating the scale, we recommend that fisheries staff work with a social scientist. For further references on attitude scales see Eagly and Chaiken (1993).

17.3.6 Summary of Attitude Measurement

Attitudes underlie and influence such behaviors as obeying fishing regulations (or not) and bringing political pressure against a fisheries agency. Attitudinal information can be valuable to fisheries agencies in clarifying the perspectives of anglers who espouse particular positions on fisheries issues. In combination with other questions, attitudinal information can also indicate whether anglers' positions are based on misleading or incorrect information.

To the extent that sufficient information can be obtained through one or more individual attitudinal statements per topic, fisheries staff can, with practice, develop valid attitudinal statements and satisfactorily implement public attitude surveys. In developing this experience, it will be helpful to circulate draft questions both to agency colleagues and to a human dimensions researcher for review.

17.4 MEASURES OF ANGLER INVOLVEMENT AND COMMITMENT

The attachment or relationship that anglers have to fishing and how this changes over time should be of concern to managers for several reasons. The types of experiences anglers seek affect the demands they place upon fishery resources. The degree to which anglers are involved in fishing has a bearing on the degree to which they support and work for causes related to fishing and water quality.

Finally, the degree to which anglers remain involved in fishing over time affects whether they continue to buy licenses and, as a result, contribute funds toward management and specific fisheries programs.

The literature on fishing involvement and commitment has evolved largely from two themes. The first involves trends in the level and consistency of fishing activity. Some people fish consistently year after year. Others fish sporadically, sometimes going a year or even several years between fishing trips. Still others fish for some period of time and then totally stop fishing. Many people have never fished. Some of the last group have some interest in fishing, and given the right set of circumstances (e.g., someone to fish with, to advise them on purchases of equipment, and to teach them how and where to fish), they may become active anglers. A better understanding of these groups (which are sometimes envisioned as market segments) would allow managers to better predict fishing trends and to design programs for increased and more sustained fishing involvement.

The second theme concerns changes in fishing behavior over time, particularly among consistent anglers (those who fish every year). Evidence that anglers specialize in their fishing over time and the ways in which specialization is measured are covered in Section 17.4.3.

17.4.1 Involvement and Related Concepts

The concept of involvement has been developed both in the social sciences and in the marketing and consumer behavior field. For an excellent review of its evolution and literature sources, see Havitz and Dimanch (1990). Involvement is a very broad concept with several components: enduring involvement (the extent to which anglers remain involved in fishing over time), situational involvement, personal involvement, and ego involvement. The component of greatest interest in fisheries is enduring involvement, although other types of involvement may influence enduring involvement.

Not all of the involvement literature classifies the type of involvement. Working from a marketing perspective, for example, Laurent and Kapferer (1985) found "involvement" to be a multidimensional concept consisting of four facets: (1) interest in or perceived importance of a "product" (e.g., fishing or a specific type of fishing), (2) emotional appeal of the product and its ability to provide pleasure or affect, (3) symbolic value the user assigns to using the product or participating in the activity (e.g., identifying as an angler; having a boat that could be used to take others fishing), and (4) risk associated with making a poor decision about buying a product (e.g., a boat used for fishing) and the perceived probability of a poor decision. An individual's level of involvement with an activity often changes over time.

The investigation of enduring involvement requires the introduction of several other social science concepts. From a *motivational* perspective, anglers have certain needs or desires that they *expect* particular fishing experiences to fulfill. The degree to which these *expectations* are fulfilled determines in large part anglers' *satisfaction* with the experience. The degree of satisfaction with the sum of fishing experiences over time, as well as such factors as available time and health, determines in large part whether or not anglers maintain an enduring involvement with fishing. This in turn largely determines the degree to which anglers personally identify with fishing and become *committed* to the activity as something they want to continue to participate in and identify with.

A related concept that has been recently associated with fishing is *personal investment theory*. Much of the theoretical work on this concept was developed and synthesized by Maehr and Braskamp (1986). Personal investment theory has been applied to Great Lakes fishing by Absher and Collins (1987) and by Siemer et al. (1989). This theory is useful in understanding fishing involvement and commitment because it combines the aspects of ego or personal involvement and the investment of resources (primarily time and money) in the activity. Thus, over time, through a combination of increased participation and acquisition of more equipment, individuals may become personally invested in fishing at increasing levels. One means of examining individuals' commitment to fishing (from a sociopsychological perspective) or the likelihood that they will continue to buy licenses and participate in fishing (from a marketing perspective) is to examine variables related to how personally invested they are in fishing. Some key investment variables to cover in a questionnaire are number of consecutive years anglers have fished, number of fishing trips taken in the past year, value of equipment owned that is used primarily for fishing, and whether anglers consistently read fishing magazines or watch television programs about fishing.

For the first variable, number of consecutive years fished, a measure such as "at least 5 years" is usually better than "total number of years," which would suggest (say) that 20 years represents a greater commitment than 15. Many relatively young people are active, highly committed anglers, and the measure or index of commitment should not penalize them just because they are young. At the opposite extreme, involvement often declines in later years; someone who has fished for 30 consecutive years may have a declining commitment to fishing, although he or she still fishes every year.

In addition to obtaining general estimates of current involvement in and commitment to fishing, it may also be important to investigate whether the level of commitment to fishing is increasing, decreasing, or remaining about the same.

17.4.2 Motivations for Fishing

Managers need a general understanding of the motivations of various angler groups for fishing because these affect the specific types of benefits anglers seek. In turn, the degree to which these benefits are obtained, as perceived by anglers, determines their level of satisfaction with the recreational experience.

Motivational studies have shown that recreationists identify certain needs or goals that they want their outdoor experiences to meet. These typically include an outing with friends, escape from work-related tensions, and enjoying the out-of-doors. Whether or not individuals choose to attempt to meet these goals through fishing depends on a combination of internal, external, and situational factors. Internal factors include anglers' perceptions of their own fishing skills, of fishing as an activity, and of people who fish. External factors include whether family and friends fish and their attitudes about fishing. Situational factors include such things as the weather, time available for the trip, and time of the year vis-a-vis fishing seasons.

Researchers have examined the reasons people fish and the attributes sought in a fishing experience both at the fairly superficial level of preferences and at the deeper underlying level of goals and motives. Driver and Knopf (1976) and Driver et al. (1984) have emphasized that fisheries management should be viewed as the management of a production process in which the products are not fish per se but rather particular types of recreational experiences or opportunities to use fisheries in particular ways. Within the settings in which particular fisheries occur (e.g., wilderness, rural, or urban setting; wild or stocked species), angler preferences or the attributes they seek should play a role in determining what "products" (i.e., experiences) will be emphasized by a given fishery.

Questionnaires that investigate reasons for fishing often list numerous possible reasons and ask respondents to indicate on a 5-, 7-, or 9-point scale the relative importance of each. At a first level of analysis, the results are examined as to the mean importance of each reason. The mean importance of various reasons is examined not only for the total sample, but for meaningful subgroups (e.g., types of fishing, socioeconomic groups). At a more detailed level of analysis, the results are sometimes subjected to factor analysis. Some of the more universal reasons for fishing that have emerged from many studies are relieving tensions or escaping from work pressures, being in the outdoors (appreciative), being with family or friends (affiliative), catching the limit or catching a trophy fish (achievement, challenge), and relaxing. Catch-related motives have often been reported to be higher for tournament than for nontournament fishing experiences (Falk et al. 1989).

An example of a straightforward survey approach to examining fishing motives is Hicks et al.'s (1983) study of visitors to Missouri's trout parks. A brief survey was printed on card stock and distributed on randomly chosen days to anglers who purchased a daily permit. The format for the question examining motives for fishing was as follows.

For each of the following reasons for fishing, please show (√) *how important* it is *to you* while fishing *here*.

REASON	HOW IMPORTANT		
	Very	*Somewhat*	*Not*
Escape daily routine	( )	( )	( )
Relax	( )	( )	( )
Catch a limit of trout	( )	( )	( )
Enjoy nature	( )	( )	( )
(etc.)			

Items used were adapted from a list used previously by Driver (1977).

17.4.3 Fishing Specialization

Fisheries social scientists have a long-standing interest in how anglers' interests and fishing behavior change over time. To the extent that different angler groups seek different fishing experiences and evaluate given experiences differently, the specialization concept is also important to fisheries managers. Much of the fishing specialization work has been summarized by Hahn (1991).

There is not yet a clear consensus as to which variable(s) specialization should be based upon. Bryan (1977), who did the seminal work on this topic, defined specialization in terms of moving from the general to the particular. He placed trout anglers in four categories: occasional anglers, generalists, technique specialists, and technique and setting specialists. Thus, amount of fishing was a factor, at least for the two most general groups, but type of equipment used was the dominant variable. Chipman and Helfrich (1988), in a study of anglers on two Virginia rivers, also found that four general measures were useful in deriving angler groupings: (1) resource use (type of equipment; species sought; harvest rate); (2) experience (years of experience; frequency of fishing); (3) investment (equipment owned; fishing expenditures); and (4) centrality of fishing to total lifestyle (club memberships, magazine subscriptions, maximum fishing trip distances, etc.). Applying cluster analysis to these four dimensions, the authors defined six types of anglers. Types 1–3, which had low specialization, preferred more liberal creel limits, whereas types 4–6, which had high specialization, tended to favor more restrictive limits.

Ditton et al. (1992) used the sociological concept of social worlds and subworlds to reexamine fishing specialization. The social world is a useful concept because of its breadth. It has been defined as "an arena in which there is a kind of organization," a "culture area" whose boundaries are "set neither by territory nor formal membership but by the limits of effective communication" (Shibutani 1955), and as "an internally recognizable constellation of actors, organizations, events and practices which have coalesced into a perceived sphere of influence and involvement for participants" (Unruh 1979). The social subworld is similar to the concept of segmentation in marketing; it recognizes that as groups evolve and expand in size, specific interest groups (e.g., bass anglers, fly-fishers) emerge and attain their own identities. Aside from examining specialization, the social world concept is relevant to examining the meaning of fishing to groups of people who have some involvement in the activity but who may not actually participate. Examples include sellers of fishing equipment and family members who accompany anglers on camping trips and eat the fish caught.

Based on social worlds literature and the work by Bryan (1977), Ditton et al. (1992) reconceptualized recreational specialization as (1) the process in which recreational social worlds segment into new subworlds, and (2) the subsequent ordered arrangement of these subworlds and their members along a continuum from least to most specialized. The authors proposed eight propositions about specialization in conjunction with Bryan's (1977) previous work. The first states that participants in a recreation activity are likely to become more specialized in the activity over time. The remaining seven state that as specialization in a recreation activity increases,

- the time and monetary costs of obtaining and using equipment increases;
- the centrality of that activity in a person's life increases;
- acceptance of the norms and rules of the activity likely increases;
- the importance attached to equipment and the use of that equipment increases;
- dependence on a specific resource likely increases;
- use of the media for information about the activity increases; and
- the importance of activity-specific elements of the experience decreases relative to that of other elements (e.g., experiential aspects).

Further research is needed to determine the extent to which these propositions are valid. Ditton et al. (1992) developed hypotheses from the last three propositions above and tested them with data from 4,200 Texas saltwater anglers.

Unfortunately, they defined specialization operationally (de facto) in terms of frequency of annual fishing participation, which seems more closely related to involvement than to specialization. Nevertheless, they showed that angler traits were mutually consistent with the last three predictions in the list. Using four groups roughly equal in size, Ditton et al. found that those who fished most frequently (1) were more interested in catching large fish or trophy fish and in fishing where there were several species of fish, (2) had more contact with the media (including the state management agency's magazine) concerning fishing, and (3) attached less importance to activity-specific elements of the fishing experience and more importance to nonactivity-specific elements. That is, they were more likely to be happy than anglers in other groups if they did not catch fish or keep their fish. They attached more importance to catching fish for eating and for the experience of the catch. Finally, they attached more importance to experiential aspects such as having a different experience, experiencing natural surroundings, and getting away from the demands of other people.

The number of specialized fishing organizations, such as the Bass Anglers Sportsman Society or Muskies, Inc., and the number of specialized fishing tournaments are ample proof that many anglers do specialize according to species and equipment. It is less clear, however, to what extent active anglers move through various stages over time. Bryan (1977) predicted that more experienced anglers would have less interest in harvest motives and greater interest in specialized fishing equipment and the resource setting than less experienced anglers. More recent research has verified these tendencies, but also demonstrated that not all anglers move through these stages. Dawson et al. (1991), for example, found that approximately one-third of the anglers studied did not fall into the specialization category one would expect from Bryan's theory. Brown and Siemer (1991) concluded that a given angler likely has a different set of goals for different fishing experiences. Across all of one's annual fishing experiences, these goals may show particular tendencies, but they may also vary considerably from one fishing trip to the next. Within a given year, an angler may take children fishing for sunfish, go fly-fishing for trout, and go deep-sea fishing. Thus, the view that most anglers fall within a specific stage of specialization that describes their current fishing activity appears too rigid and simplistic.

Research on fishing specialization demonstrates the complexity of human behavior. If one views specialization as a tendency, however, it is a useful concept to help understand the fishing orientations and preferences of anglers, and how these tend to change over time. The variables used by Chipman and Helfrich (1988), described above, are reasonable to use in exploring specialization.

17.5 MEASURING SATISFACTIONS: CONCEPTS AND METHODS

Within the constraints imposed by policy, legislation, or funding on how fisheries resources are to be managed (e.g., as wilderness; to preserve native fish or wildlife populations), a major goal of management should be to optimize human benefits or user satisfactions. Research from hunting, fishing, and other outdoor activities has consistently shown that recreationists seek a variety of satisfactions from their outdoor experiences. As suggested by the previous section, anglers do not seek just to catch fish; they also seek relaxation, enjoyment of the outdoors, companionship, and a number of other benefits from a fishing experience. Thus, understanding anglers' satisfaction with the overall fishing experience involves understanding their level of satisfaction with all of the components that they view to be important to a fishing experience.

The simplest type of satisfaction measure is one that rates the angler's general level of satisfaction with the fishing experience. "The fishing experience" can be very specific (e.g., the last striped bass fishing trip to a particular place), or it can be much more general (e.g., all 1994 freshwater fishing trips in Arkansas). A typical wording and format for a general question might be as follows.

On a scale of 1 to 9, where 1 means extremely dissatisfied and 9 means extremely satisfied, how satisfied were you with your overall fishing experience for rainbow trout in Colorado in 1994? (Circle one number):

Extremely Dissatisfied		Generally Dissatisfied		Neutral	Generally Satisfied		Extremely Satisfied	
1	2	3	4	5	6	7	8	9

More detailed questionnaires would usually retain an overall measure such as that shown above, but would also investigate various satisfaction components (e.g., number of fish caught, size of fish caught, evidence of fish at the site, cleanliness of the site, ability to relax).

Several other types of scales have been used to measure satisfaction. Some have ranged from 0 to 10, 0 to 100, −10 to +10, and −100 to +100. Matlock et al. (1991) found that a 0-to-10 scale worked best for a face-to-face interview that was conducted as part of a creel survey.

Research has shown that the relevant components of satisfaction sometimes vary from one setting to another. Satisfaction components have been derived for work settings, overall quality of life, and several leisure activities. One cannot assume that the components listed in any study are suitable for a particular fisheries application. Some satisfaction components could even vary between freshwater and saltwater fishing or between (say) sunfish and salmon fishing. Thus, a preliminary list of satisfaction components should be field-tested with a modest sample of relevant anglers and then augmented and refined as necessary before the primary study is conducted.

Most of the satisfaction models used for outdoor recreation activities fall within two general constructs that are sometimes used in conjunction with each other: *discrepancy theory* (also known as contrast theory) and the *sum-of-satisfactions* approach. With discrepancy theory, the amount of a satisfaction component (catching fish, getting a strike, being in a pleasing environment) actually experienced is compared with either a preferred amount (sometimes worded as an ideal amount) or an expected amount. The degree to which expectations are good reference points for discrepancy analysis of fishing satisfaction probably is related to the degree to which anglers are familiar with both the form of fishing in question and the fishing site. Negative discrepancy or disparity from the expected or ideal amount is presumed to imply less satisfaction; zero or positive disparity (e.g., catching more fish than expected) is presumed to mean more satisfaction. Few fishing studies have used this concept; for applications to hunting and camping, see Bultena and Klessig (1969), Peterson (1974), and Decker et al. (1980).

Some studies have also obtained a measure of the relative importance of each satisfaction component used in the study and have weighted each item by this importance score. Generally, the weighted item scores have been no more closely correlated with overall satisfaction than unweighted scores (e.g., Decker et al. 1980).

In the sum-of-satisfactions approach (e.g., Decker et al. 1980; Graefe and Fedler 1986), the sum of the scores of individual angler satisfaction components (each of which can be positive or negative) is assumed to be highly correlated with the angler's overall or total level of satisfaction with the fishing trip. When this approach is combined with discrepancy theory, one calculates the difference between the ideal or expected amount of each component experienced and the amount actually realized, and then sums these scores (some of which may be negative) for all satisfaction components. Analysis can then provide an indication of the overall level of satisfaction of anglers, which can be compared with a single-item overall measure and with the score of each component. At a more advanced level of analysis, some studies have used principal components analysis, usually with varimax rotation, to reduce the 15 to 25 individual satisfaction components to a smaller number of factor groupings.

17.5.1 Brief Review of Recreational Satisfaction Literature

Much of the literature on recreational satisfaction has dealt with the relationship of an individual's overall satisfaction with an experience to his or her preferences for, expectations for, and satisfaction with specific components of the experience. Dorfman (1979) indicated that specific expectations or preference levels correlated less well with overall satisfaction than did satisfaction with individual components deemed by recreationists to be particularly valuable. Connelly (1987) built upon this finding by introducing the concept of critical factors. A critical factor is one that, if not met at an angler's minimally acceptable level, causes dissatisfaction with the overall experience, even if other components received positive ratings. At the aggregate level, Connelly developed several criteria for a critical factor: (1) its mean importance level must be above neutral, (2) it must be at least moderately correlated with the overall satisfaction score, (3) it must enter a regression equation predicting overall satisfaction with a statistically significant t-score, (4) it must predict at least 50% of the variance that can be explained when all factors are included in a regression model, and (5) it and all other critical factors must predict 90% of the variance that can be predicted with all factors. The Connelly application was to camping; such an application has not yet been made to fishing.

Buchanan (1983) investigated fishing satisfaction at a U.S. Army Corps of Engineers reservoir in central Illinois. The leading satisfaction components rated by these anglers, in decreasing order of mean importance rating, were catching fish, physical rest, escaping personal or social pressures, being with friends, family togetherness, escaping physical pressures (e.g., noise), experiencing nature, exercise, showing equipment, learning, nostalgia, security, meeting new people, leadership, values, achievement, creativity, change of temperature, and escaping the family. These 19 items (of 20; the 20th was risk taking) received importance scores above a mean of 4.50 on a 9-point scale. Graefe and Fedler (1986) examined satisfactions of marine charter anglers in Maryland and Delaware. Based on previous research, they used the following six-item scale as a measure of overall satisfaction, the dependent variable. Each item was significantly correlated with the total scale score and had a reliability coefficient (Cronback's alpha) of at least 0.80:

- I thoroughly enjoyed the fishing trip.
- The fishing trip was not as enjoyable as expected.
- I can not imagine a better fishing trip.
- I do not want to go on any more fishing trips like that one.
- I was disappointed with some aspects of the trip.
- The trip was well worth the money I spent to take it.

Five situational factors were investigated as independent variables: weather conditions, number of fish caught personally, number of fish caught by the group, number of people on the boat, and qualities of the captain and crew. The first two of these factors were not significant for the Maryland anglers; all factors were significant for Delaware anglers. Twelve subjective evaluation measures were also used. Those that were statistically significant in predicting overall satisfaction were related to being outdoors, catching the type and amount of fish desired, crowding, learning to be a better angler, and enjoying the challenge and sport. The combination of the subjective evaluations and situational factors accounted for 56% and 57% of the variance in satisfaction in the two states.

Schoolmaster (1986) examined the tolerance of anglers along the Madison River in Montana for seeing other anglers. This concept is closely related to general satisfaction research because it compared the number of anglers that respondents expected to see, desired to see, and actually saw. The primary use conflict was with the number of float anglers seen by bank anglers. This study was accomplished by a mail survey that followed from a creel survey in which names and addresses were obtained.

17.5.2 Summary of Satisfaction Measurement

The above studies were cited to demonstrate the nature of satisfaction research and its application to fisheries research. Although such research has occurred in nonspecific settings (e.g., for all of one's fishing in the past year), satisfaction research is probably most valid and most useful when it is carried out in specific contexts involving particular types of fishing on specific waterways. To develop a component scale, some exploratory research should first be conducted with anglers on site to determine the aspects of fishing that are most important to them. Those results plus other components taken from the literature should be pretested with a group of relevant anglers to determine which components are significantly correlated with overall fishing satisfaction. Depending on the nature of the waterway and the type of fishing, all of the factors discussed above, as well as others such as ease of access and availability of services (e.g., restaurants, bait and tackle shops) in the area, could affect satisfaction. Pretesting may significantly reduce the number of satisfaction items that will be needed.

A further analysis option is to perform factor analysis to determine the degree to which general groups of anglers can be identified in terms of the types of experience they seek (this ties in with Section 17.7.1.4 on psychographic market segmentation). If this proves to be successful, managers can work to provide the overall types of experiences sought by the largest groupings of anglers. If these management efforts are successful, agencies and offices of tourism can further use the sociological data to develop marketing aimed toward these groups in the larger population within a reasonable distance of specific fisheries.

17.6 EVALUATION AND ASSESSMENT SURVEYS

Evaluation has become much more prevalent over the past two decades as government programs have expanded and both the public and policy leaders have sought measures of program accountability. Fisheries agencies have not been immune from this trend. Although a substantial portion of fisheries budgets come from dedicated funds, government audit and control agencies, legislators, and angling organizations all seek periodic measures of an agency's effectiveness. Evaluations also can serve meaningful program functions within an agency. Among the reasons for evaluating a fisheries program are to ascertain (1) the degree to which anglers or other groups participate in and approve of the program, (2) the impact of the catch authorized by the program on the fisheries resource, (3) the cost-effectiveness of the program, (4) the numbers and changes in types of anglers who use the fishery, and (5) the degree to which a new or expanded program has received redistributed effort from other fisheries (Brown 1984).

It may prove useful to distinguish between program assessment and program evaluation. A program assessment may be exploratory or open-ended. An agency may want to know how many people are using a new urban fishing program, how they like it, and how it can be improved from the users' perspectives. If the results of the survey are not compared with previously established objectives or criteria, this will be referred to as a program assessment. On the other hand, if the results are compared with some previously set standards or objectives, this will be referred to as a program evaluation. (Some literature uses the term "evaluation" in both contexts.)

Perhaps the most basic and central point to any type of program evaluation is that one must have criteria to evaluate against. To know that 5,000 angler-days were spent on a new urban fishery the first year it was stocked will provide an assessment, but it can not be used to provide an evaluation without further information. What use objective was established for the program in its first year—to attract anglers who fished already or new anglers? Is there an existing standard against which the program's cost-effectiveness can be measured? What population criteria were set for the stocked fish? Evaluations address the question "To what extent were objectives realized?" Natural resources evaluations usually examine either or both of two dimensions, a quantitative program goal (e.g., the number of anglers who use a fishery, number of licenses sold, amount of litter reduction), and a measure of cost-efficiency (the program gain per dollar or person-day expended).

Many programs, including fisheries programs, are established at least in part to bring about social or economic change. Local economic development, usually in the form of fisheries-related tourism, is one objective of some fisheries programs. Urban fisheries programs may have objectives as far-reaching as reducing delinquency among urban youth. Fishing sometimes is used as a vehicle for promoting interest in the environment that, over time, will result in a more environmentally aware citizenry.

Sometimes large projects such as reservoir construction or introduction of new fish species are associated with fisheries program objectives. A type of evaluation used as a part of such a project evaluation is impact assessment (Rossi and Freeman 1982). Economic impact assessments are dealt with in Chapter 16. Social impact assessments, which often are conducted hand-in-hand with economic assessments, may address changes in such dimensions as local population demographics, local work force, public services, community infrastructure, and social organization and values within the community (Leistritz and Murdock 1981). Full social impact assessments are rarely undertaken for fisheries projects. However, material presented in Chapter 4 and this chapter should be sufficient for straightforward estimates of use impacts and satisfactions of users for the program or project being evaluated.

17.6.1 Evaluating Agency Regulations and Policies

Agency regulations and policies are often set initially with particular biological objectives in mind (e.g., maintaining natural reproduction of a given species). However, these regulations and policies must be at least minimally satisfactory to anglers and other fishing interests, or these stakeholders will use the political process to attempt to get the policies or regulations changed. The optimal regulation or policy is the alternative that provides maximum human benefits while still attaining biological objectives. Information on measuring preferences and satisfactions presented earlier in this chapter can be used to evaluate public reactions to particular regulations and policies.

17.6.2 Assessments of Communications and Agency Image

Two other related types of periodic assessment that can prove valuable for a fisheries agency involve the agency's image and its communications programs. When a controversial issue is being debated and there is strong opposition that can hold up an agency's progress on an important program, the agency may wish to know its public image and the degree to which its communications are being received by anglers. For a conceptual model of a natural resources agency and its relationship to other agencies and specific publics see Decker (1985).

The three areas for which public image of a resource agency are generally sought are (1) personnel characteristics (e.g., competence, responsiveness); (2) management function (e.g., appropriateness of objectives and methods; degree of successful implementation); and (3) communications behavior (e.g., use of the media, timeliness of news releases, balanced perspective in communications). Any of these image components can be measured through standard Likert or semantic differential formats. Some example Likert-style statements, with positive and negative formulations and a 5-point scale from "strongly agree" (SA) through "don't know" (DK) to "strongly disagree" (SD), are listed below.

Personnel Characteristics.

The Nebraska fisheries agency is a trustworthy organization.

SA A DK D SD

Nebraska state fishery biologists seem to lack training in some of the latest management methods.

SA A DK D SD

Management Function.

The Manitoba provincial fisheries agency is successfully reducing the population of rough fish in lakes and reservoirs where these fish have been a problem.

SA A DK D SD

Manitoba provincial fisheries staff lack sufficient research data and manage primarily on a trial-and-error basis.

SA A DK D SD

Communications Function.

The National Marine Fisheries Service does not make an adequate attempt to explain its fisheries programs and objectives to the public.

SA A DK D SD

National Marine Fisheries staff are very receptive to insights and information from the public.

SA A DK D SD

17.7 MARKET RESEARCH SURVEYS

Market research information is not just for the private sector; it is equally useful to fisheries and other public agencies. Market research can provide such information as the characteristics and preferences of anglers residing in various geographic localities, the proportion of current anglers likely to continue fishing, and the number of potentially interested people who may start fishing within the next year. The number of people who are interested in various species of fish, in fishing various waterways, and in fishing with various types of boats and fishing equipment can be estimated through market research, as can the number of people who would be interested in a new type of fishery or in fishing a given waterway under modified regulations.

Much of the material covered in earlier sections of this chapter can be used in the context of market research. If at least one purpose of the study is to learn how to satisfy anglers in order to keep them active as anglers, or to determine how to attract new people into fishing, the study has a market research component. Information from market research studies then can be used not only to attract and serve various fishing markets, but also to project the number of people who will be active anglers at some future date.

17.7.1 Market Segmentation

An activity as diverse as fishing encompasses users with a wide array of characteristics. Anglers vary widely in age, place of residence, education, income, and many other characters. Their interest in fishing varies from slight to intense and their skills vary tremendously. They may like to fish alone, with family or friends, or on a charter fishing trip; they may prefer fishing for warm- or coldwater species, on lakes, streams, or oceans. The kind of benefits (benefit package) they seek most in a fishing trip (e.g., catching their limit or just relaxing) also varies.

No one type of fishing satisfies or is even desired by all anglers. As a result, it is more useful in managing fisheries for human needs and interests to think of groupings of people with similar interests and "product needs." These groupings are known as market segments. Anglers can be segmented by any of the categories suggested in the previous paragraph and by others as well (e.g., shore versus boat anglers). Four general types of market segmentation can be attempted: socioeconomic or demographic, geographic, product-related (i.e., the type of fishing-related trip of interest), and psychographic (Pride and Ferrell 1983). This section will be limited to a few applied segmentation types that have been found particularly useful to management agencies and to tourism officials who often work closely with fisheries officials. It is possible and often desirable to use combinations of market segments (e.g., nonresident anglers who fish for trout in mountain streams).

Any market segmentation effort that may be identified for possible use faces the pragmatic problem of how to reach the segment of interest. Must members of this group buy a special license that would provide a listing of names? Do they live in a particular locality where they can be reached through the local media? Are they more likely than others to read particular magazines or tune in to particular radio or television shows? This type of information is often asked for in market research, and questions to gain it can be added easily to most general angler surveys. However, the concept of market segmentation has little use if there is no means of reaching the segments of interest.

17.7.1.1 Socioeconomic Market Segmentation

One of the oldest types of market segmentation is by socioeconomic groupings such as age, education, and income. Although these factors may be important in projecting participation in angling (see Section 17.8), they have not proven to be particularly useful in separating out people who desire various types of fishing experiences. As an illustration, the average income of fly-fishers may be above the national average income, but many fly-fishers would be missed by focusing solely on upper-income groups. Socioeconomic segmentation may have particular application in conjunction with urban fishing programs that are directed toward less mobile and disadvantaged inner-city residents. It has not proven to be particularly useful elsewhere.

17.7.1.2 Geographic Market Segmentation

Geographic segmentation is also traditional, but has proven to be among the most useful types of market segmentation. From a state- or province-wide mail or telephone survey with a large sample, one can estimate the proportion of visitors to a given county or waterway who come from various geographic residence zones. Similarly, the total amount of money spent in a given locality can be traced back to residence zones. This information provides an excellent indication of which residence areas are worth various proportions of the locality's overall advertising budget.

Geographic marketing research often uses zip or postal codes to group residents who visit particular areas or buy particular products in such areas. This approach can be useful for fisheries that are only of interest to people living within, say, 100 kilometers of the fishing site. For more popular fisheries, individual postal areas are too small to be of great use. Even though they later can be combined into larger groupings, anomalies often occur because neighboring areas may have zip codes that are quite different.

A preferred method, used by Brown (1983) and also by marketing professionals, is to segment by what has been termed media regions or areas of communication influence. States and provinces can be divided into metropolitan media regions that, among them, encompass all counties or ridings (applicable jurisdictions in bordering states and provinces also may be included). The counties that sell more newspapers from a given city than from any other, or that depend more heavily on television coverage from a given metropolitan area than from any other, can be grouped together into media regions. Thus, the number of media regions in a state or province is correlated fairly closely with the number of metropolitan centers. However, most media regions are considerably larger than standard metropolitan statistical areas because they also include rural counties. It seems imminently practical to group anglers who visit particular fisheries into these media regions because television and newspapers represent likely means of promoting fisheries, which is a primary reason for conducting market segmentation research in fisheries.

Information on media coverage and regions is developed and maintained by national marketing companies that monitor the primary service areas and readership, listenership, and viewership of individual newspapers and radio and television stations. Their information is sold for a fee and permission is required for its use. However, these companies are often generous in making a prior year's data available to universities and public agencies free of charge for uses such as delineating media regions. The publications of some companies will map out media regions.

17.7.1.3 Product-Related Market Segmentation

Product-related segmentation divides the market by type of use (e.g., striped bass fishing, fly-fishing) or by expectations of the experience. Often several categories are used in conjunction with some type of factor or cluster analysis to provide groupings of people with similar interests. This type of segmentation has received increased interest in recent years. As an example, Ditton and Mertens (1978) divided marine charter boat anglers in Texas into four groups based on party relationships: families, friends, work colleagues, and clients.

Many market segmentation efforts have focused on anglers who fish for particular species or species groups. Thus, questions in a general survey designed to assist in later market segmentation should solicit information on the species anglers fish for and on the species they prefer to fish for (if different). If information on place of residence is also obtained, the market can be further segmented by geographic region.

17.7.1.4 Psychographic Market Segmentation

Psychographic segmentation has become increasingly popular in recent years because researchers have realized that the type of experience recreationists seek and the way they evaluate particular experiences have a great deal to do with what the recreationists hope to gain from the experience. Psychographic segmentation attempts to divide anglers or other recreationists into distinct groups based on variables that reflect their lifestyles and personalities. Psychographic segmentation is believed to be particularly useful for advertising and promotion because it can be used to reach groups of people by targeting the way they feel (i.e., their attitudes and values).

As one example of psychometric segmentation in fisheries, Driver et al. (1984) used cluster analysis of 23 attitudinal variables to divide Wyoming anglers into preference dimensions that would be meaningful to managers. The derived dimensions were general outdoors, yield, solitude, wild, social, general recreation, and trophy. Two of these, general outdoors and social, were dropped. (Over 95% of respondents scored high on the general outdoors dimension; thus it was not useful in segmenting anglers. The social dimension was deemed to have limited utility for managers.) Each angler was then assigned to the category of his or her highest score, so long as that score was as least 3.5 on a 5-point scale. In research related to fishing, psychographic and product segmentation have had some similarities or likely cross-correlations. That is, differences in preferences for various products (e.g., fly-fishing versus fishing from a large boat) may be largely due to differences in attitudes or personalities that cause people to seek different attributes from the fishing experience (e.g., appreciative versus social experiences).

17.7.2 Methods of Segmentation

Segmentation of anglers can occur either before or after a study. A study can be designed to interview anglers from different residence areas, or to compare shore anglers, boat anglers, and charter anglers by sampling each group. More frequently, however, angler surveys are conducted primarily for other purposes and markets are segmented afterward. This does not usually pose a problem if the segmentation work is planned in advance so that the segmentation variables of interest are incorporated into the questionnaire.

Whereas geographic segmentation into media areas produces obvious segmentation categories, psychographic segmentation does not, and even product segmentation does not necessarily produce obvious categories (e.g., anglers may pursue various combinations of fishing types). For this reason, cluster analysis is frequently used to empirically group respondents according to the similarity of their multivariate profiles. For further information on cluster analysis, see Romesburg (1984) and Norusis (1985).

17.7.3 Summary of Market Segmentation

Market segmentation is of particular interest to fisheries agencies for promotion purposes or for other types of targeted communications. If these needs are anticipated at the time surveys are conducted, appropriate information can be gathered to facilitate the market segmentation.

Pragmatically, the greatest problem in pursuing market segmentation is having a means to reach the target groups once the segments have been measured and described. For this reason, geographic market segmentation may be more useful to fisheries agencies because of the ability to use the media that serve particular regions. If understanding the fisheries product sought is the objective rather than active target marketing, the product-related or psychographic methods should provide a deeper understanding of the experiences sought by anglers.

17.8 SURVEYS TO PREDICT PARTICIPATION

It is important to be able to project fishing participation, for several reasons. License sales constitute a major portion of revenues for most fisheries agencies. Support for agency programs and fishing-related causes is provided by the angling clientele. Fishing demand is also important to assess for general planning purposes.

Two types of database have been used to project participation or license sales: longitudinal or time-series databases, and cross-sectional databases, usually obtained through a social survey. The longitudinal database typically does not involve surveys, but rather a long annual data series (of perhaps 25 years) in which license sales is the dependent variable and several demographic and resource variables, as well as the license fee, are independent variables. Because survey research is not involved, the longitudinal database will not be dealt with further here. For an example of such a model applied to hunting and fishing, see Brown and Wilkins (1975).

Participation is most frequently projected through use of a cross-sectional survey such as the national surveys conducted by the U.S. Fish and Wildlife Service at 5-year intervals. A variety of projection techniques have been used. Those most frequently used can best be evaluated by first examining factors related to fishing demand.

Many socioeconomic and resource factors affect fishing demand. Among socioeconomic factors, the number of people in a given geographic area cataloged by gender and certain age and income groups is usually significantly associated with the number of people who fish. In association with the income of the population, the various costs associated with fishing also affect the number of people who will fish. In addition, what economists refer to as tastes and preferences affect fishing demand. Over time, from a sociocultural perspective, fishing may become a more or less important activity to a given human population.

Resource variables that reflect the quality of fishing and access to fishing resources also affect fishing demand. Adams et al. (1993) found that at the state level, the amount of public lands, the per capita fisheries budget, and the amount of water affect the amount of fishing. The demand for fishing at a particular site will be influenced by the availability of substitute sites.

Some variables may be nonlinearly associated with demand. In highly urbanized regions, for example, additional development at some point may impinge upon fisheries resources such that additional people will have a negative rather than a positive effect upon fishing demand. We have poor measures of some variables, such as those that reflect tastes (or attitudes) and preferences. Often we also do not have good data on resource-related variables. As a result, some models used are greatly simplified and incorporate only some of the variables that actually affect fishing demand.

A further problem in using predictive models of fishing demand is population change. Unless one assumes no change in the human population of interest (which might be permissible for a short-term projection, such as the impact of a license fee increase on license sales next year), values of independent variables also will be needed for the year of projection in order to predict the dependent variable. Because the U.S. Bureau of the Census and Statistics Canada have population projections by major socioeconomic groupings, some models use only variables for these groups. Models of this type that project only a few years into the future may be reasonably accurate for fishing because fishing has been a very stable activity, with strong interest across many sectors of the population. However, it is important to realize that such models tacitly assume that the influence of unincluded variables will remain constant. A major environmental calamity such as an oil spill or a large chemical discharge into a major water body could have a sudden adverse effect on fishing. Similarly, attitudes and preferences could change dramatically. Demand data based on a 1970 cross-sectional study of snowmobiling in the United States, for example, would have projected roughly twice as many snowmobilers today as there actually are. This is because snowmobiling turned out to be a short-term fad to many who initially tried it. In addition, most of the northern United States has had few severe winters with extended heavy snow in the past 20 years.

17.8.1 Age-Cohort Analysis

In its simplest form, age-cohort analysis (ACA) projects fishing participation strictly by projecting the population of various age-groups, weighted by the proportion of each age-group that currently fishes. Age-cohort analysis assumes that the proportion of the population in the various age-groups that fish will remain unchanged from the current period to the year of the projection. In its crudest form, ACA would be done nationally or at the state or provincial level without further disaggregation. More disaggregated groupings would improve the projections. For example, the various age-groupings could be broken down by sex, by urban–rural groups, and by race. These divisions could furthermore be done for each county, for a state- or province-wide model, or for each state or province in a national model to provide insight into the residence areas where fishing participation could be expected to experience the greatest change.

Because of the ease of obtaining age, sex, race, and residence data from surveys, ACA is very simple to use, and it can provide important insights into future fishing patterns. The precautions mentioned above need to be stressed, however. Population structure is only one of several categories of variables that affect fishing demand, and notable changes in other factors will be influential. Furthermore, over long periods of time, the proportions of people in various age-groups who fish may not remain constant for a wide variety of reasons (e.g., changes in leisure time). Thus, we recommend restricting ACA to short-term projections (5 to 10 years maximum).

Murdock et al. (1990) projected fishing and several other recreational activities by multiplying current age-, race-, or ethnic-specific rates of participation by expected changes in those demographic variables. The authors noted the risks inherent in such projections. They suggested that it is better to use such projections for sensitivity analysis of alternative future trends, rather than to use the projections as point estimates.

17.8.2 Forecasting From a Demand Function

An extension of age-cohort analysis is to develop equations from survey data that model current participation and then to use the model to project future participation (the dependent variable); census data for, or other projections of, the independent variables would be input for this purpose. Early studies used multiple linear regression models in which days of participation or number of trips was the dependent variable and socioeconomic and attitudinal variables were independent variables. More recent studies have used logit or probit analyses to develop models that project the likelihood of participation. For further information on demand modeling, see Walsh (1986) and Stynes and Peterson (1984); fishery managers should be sufficiently familiar with demand models to make sure that the proper variables are incorporated in surveys.

17.8.3 Summary of Fishing Projections

All socioeconomic projections, including recreational demand, will be imperfect. However, projections of activities such as fishing, for which participation rates have changed relatively slowly, can be made more accurately than projections for outdoor activities that are more faddish. The simplest types of projections use a single variable, as in age-cohort analysis. More detailed analyses might use further breakouts of a population by age, sex, and ethnic group. Even these analyses and projections can be subject to large projection errors due to inaccurate population projections or to changes in the rate of participation of various demographic groupings. As a result, we suggest that projections be limited to 5–10 years in the future, and that resulting projections be noted as tentative because of factors beyond an analyst's control.

Chapter 18

Biological Uses of Angler Data

18.1 INTRODUCTION

Most angler surveys are designed primarily to estimate total angler catch or harvest and total angler effort (Chapter 15), although the frequency of angler surveys for economic (Chapter 16) or social (Chapter 17) purposes is increasing. In this chapter, we consider biological uses of survey data that are collected mainly for estimation of catch and effort. We recommend that Chapter 15 be read before this chapter.

Angler surveys typically are directed toward some or all of the following parameters (Section 15.2):

- *fishing effort* (angler-hours, party-hours, or trips), a measure of how heavily a fishery resource is used by anglers over a particular time period;
- *catch,* the total number or weight of fish caught (kept and released) in a fishery over a particular time period;
- *harvest,* that part of the catch that is kept by anglers; and
- *catch rate* or *catch per unit effort* (CPUE, the more common term in biological analyses), the number or weight of fish caught per angler-hour or per trip.

As well as providing effort and harvest estimates, an angler survey may also provide biological samples for a wide range of standard analyses such as estimation of growth rates, age structures, food habits, length–weight relationships, maturity schedules, and contaminant loadings (Figure 18.1) Angler surveys are not commonly used for these purposes because of the time required to obtain the samples and the resistance of anglers to mutilation of their fish. With proper planning and public relations, however, biological sampling can be incorporated into surveys. We do not discuss standard biological analyses here because they are well covered in many standard fisheries texts, but two very important considerations arise when angler data are used for biological inferences. First, angler surveys do not provide random samples of fish populations because angling is notoriously selective with respect to length, age, or other variables (see, for example, Santucci and Wahl 1991). Ignoring this can result in badly biased estimates and very misleading conclusions. Second, a simple random sampling design is assumed for many standard analyses, whereas most angler surveys have a complex sampling design (stratified, multistage, cluster, etc.). Again, serious biases and misleading conclusions may result from ignoring the sampling design.

This chapter treats a variety of techniques used in studying the population demography and dynamics of exploited fisheries populations (Figure 18.2). Most of these methods rely principally on the catch and effort statistics typically obtained in angler surveys. They have been used mainly for commercial fisheries,

STANDARD BIOLOGICAL ANALYSES
•Estimation of Growth Rates
•Estimation of Age Structure
•Estimation of Stomach Contents
•Length-Weight Relationships
•Maturity-at-Age Ogives
•Detection of Environmental Contaminants

Figure 18.1 Standard biological analyses that may be carried out in conjunction with on-site angler surveys.

but they are applicable to recreational fisheries as well. When they are used, the following provisos should be kept in mind.

- In angler surveys, catch (and effort) estimates may be incomplete and therefore may not apply to the whole fishery. Night fishing may not have been surveyed for safety reasons, for example, or bank fishing may not have been covered for budgetary reasons. Many of the models we discuss require total catch and effort for the whole fishery population.
- The catch obtained from anglers is unlikely to be a random sample of the entire fish population. Among other problems, this means that catch per unit effort may not be proportional to population size, although CPUE often has been used as an index of population abundance.
- Effort may have a variable relationship to catch over a fishing season. Climate, weather, and fish behavior change over time, which can affect the utility of catch and CPUE statistics.
- Catch is estimated periodically at best in angler surveys, whereas many biological models assume it is known from continuous monitoring. Continuous monitoring and direct inventory of catches are more likely in commercial fisheries. If the angler survey is well designed, the catch estimate should be unbiased and have a small standard error, but it is still not the same as having

POPULATION DEMOGRAPHY AND DYNAMICS APPROACHES
•Catch Curves (18.2)
•Tag Return Models (18.3)
•Catch-Effort Models (18.4)
•Change-in-Ratio Models (18.5)
•Catch-at-Age Models (18.6)
•Stock Production Models (18.7)

Figure 18.2 Population demography and population dynamics modeling approaches that may be used in conjunction with data collected in angler surveys. Parenthetic numbers indicate the sections of Chapter 18 in which these approaches are discussed.

a known catch. Similar concerns apply to effort estimates, because some models also assume total effort is known exactly.

- The sampling design of an angler survey (e.g., stratification, multistage sampling) can affect model performance. Some of the models require a simple random sampling design. Violation of this requirement may not always introduce substantial bias, but the consequences of design conflict should be checked in each case.
- Some of the models, in particular the catch-at-age and stock production models, assume detailed knowledge of the total catch (and perhaps effort) over many years. Angler surveys may not be regular enough to meet this requirement, and even regular surveys may not give complete coverage in space or time. If (for example) a catch-at-age analysis is an important objective, a complete angler survey (including valid subsampling of fish to be aged) must be run for many years, requiring substantial commitments of time and money.
- These biological models may require additional types of information beyond those usually obtained in angler surveys. Tag recovery, scale or otolith sampling, and other procedures are beyond the scope of most angler surveys. Special planning, coordination, and training efforts may be needed if a "new" technique is to be grafted onto an angler survey without degrading the survey itself.

In addition to the population dynamics approaches considered in this chapter, we also briefly discuss several important management activities that might be evaluated by an angler survey: regulation setting (e.g., bag limits, size limits: Section 18.8), stocking programs (Section 18.9), and environmental impact assessment (Section 18.10).

18.2 CATCH CURVES

One method of estimating total survival (and total mortality) of fish in a population is from the abundances of successive age-groups. This method is called the analysis of catch curves. A good detailed overview and historical review of this topic was given by Ricker (1975). Seber (1982) provided a more mathematical treatment of mortality and survival estimates from age data. Ebbers (1987) gave examples of catch curves and length-converted catch curves. Colvin (1991a) provided an example of catch curve analysis with data from angler surveys of Missouri reservoirs.

If the probability p of catching an individual is constant for all ages, if survival is constant over all ages and years, and if the year-classes are of equal strength, the expected (E) catch (n) at age x is

$$E\,(n_x) \;=\; pN_0S^x;$$

N_0 is the population size of age-0 fish and S is the annual survival rate (Seber 1982:426). In logarithmic form, this equation is

$$\log_e(n_x) \;\approx\; \log_e(pN_0) + x\,\log_e(S),$$

which is a linear relationship between catch (n_x) and age (x) with intercept $\log_e(pN_0)$ and slope $\log_e(S)$. The slope can be estimated by least-squares linear regression. Chapman and Robson (1960) and Robson and Chapman (1961) treated the statistical models in detail. If S is per year, the instantaneous total mortality rate (Z) is $-\log_e(S)$ per year.

Ricker (1975) listed (in a different order) the following four stringent assumptions that must be met if catch curves are to be used to estimate survival rates.

(1) *The survival rate is uniform among ages, over the range of age-groups in question.* In practice, catch curves often are nonlinear. One can examine the empirical plot of $\log_e$ (catch) versus age to determine if the relation becomes linear beyond some minimum age. If it does, the analysis can be used for the older age-groups with uniform survival.

(2) *The survival rate is uniform among years.* In combination with (1), this assumption implies that fishing and natural mortality (the two components of total mortality) are both constant over ages and years. Although it is theoretically possible for fishing and natural mortality to change in such a way that total mortality remains constant, such precise balancing is not likely in practice.

(3) *The age-groups in question are equal in numbers at the time each is recruited to the fishery.* That is, recruitment is constant. Assumptions 2 and 3 together imply a stationary age distribution (see Caughley 1977a, for example). A stationary age distribution and a constant survival rate (assumption 1) are unlikely in natural populations, so this method of estimating mortality rates is likely to have some bias. If recruitment fluctuates randomly and several years of data are combined, recruitment fluctuations should average out, reducing the severity of the bias.

(4) *The sample is randomly drawn from the age-groups in question.* Random samples of fish are rare in angler surveys. Most fishing gear is very size selective. However, some older age-classes may have approximately equal vulnerabilities. Nonlinearity of a catch curve may be due to differential vulnerabilities to capture, as well as to differential survival rates among age-groups. Many catch curves have an ascending "left arm," due (presumably) to lower vulnerability of younger age-groups, and a descending "right arm," which may or may not be linear according to (presumably) how constant the survival rate is for older age-groups.

In most angler surveys, only a subsample of the fish examined can be aged, although many more may be measured for length. Ricker (1975) discussed how to exploit the correlation of length with age to estimate the catch curve more efficiently than might be possible if only aged fish are used.

If its underlying assumptions are not badly violated, catch curve analysis is a simple way to estimate survival in a fish population. It can be applied to only 1 year's angler survey data without the expense of collecting the many auxiliary data that other methods require. It cannot, however, produce separate estimates of fishing and natural mortality. Under some conditions, such a distinction can be made by tag return models, which are considered next.

18.3 TAG RETURN MODELS

18.3.1 Background

We distinguish "tag return models" for fish tags returned by recreational or commercial fishers from "capture–recapture models" for marked fish that are

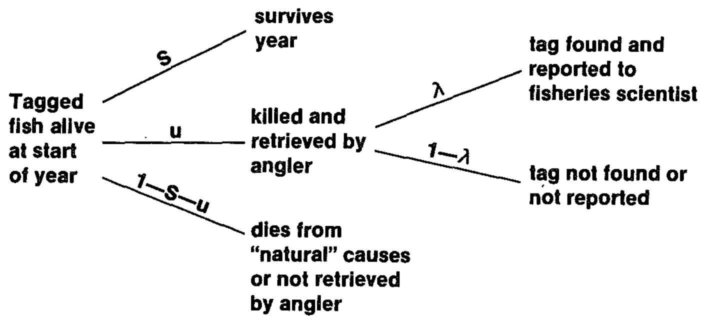


Figure 18.3 Possible fates of a fish tagged at the start of the year, based on diagrams in Brownie et al. (1985); S is finite annual survival rate, u is finite annual exploitation rate, and λ is tag-reporting rate. (Reproduced from Pollock et al. 1991.)

captured and recaptured alive by research biologists. Tag return models were reviewed by Brownie et al. (1985) and capture–recapture models by Burnham et al. (1987), Pollock et al. (1990), and Lebreton et al. (1992). Here we emphasize tag return models run in conjunction with angler surveys. Tags may be solicited from anglers on site by a survey agent or voluntarily reported by anglers to a fisheries agency. Papers by Jagielo (1991) and Pollock et al. (1991) form the basis of what follows. We present these models in some detail because they are not well known by fisheries biologists.

18.3.2 Estimation of Mortality

In this section we consider estimation of mortality when a multiyear tagging study is run in conjunction with an angler survey during at least one year—but ideally during all years—of the survey. The methodology is an extension of the band return models presented for birds by Brownie et al. (1985).

18.3.2.1 Concepts

Consider the possible fates of a fish tagged at the start of the year (Figure 18.3). The notation (from Ricker 1975) is

S = the finite annual survival rate (the probability of surviving the year),
u = the finite annual exploitation rate (the probability of being harvested during the year), and
λ = the tag-reporting rate (the probability that a tag will be found and reported, given that the fish has been harvested).

If all fish killed are retrieved by anglers,

$$v = 1 - S - u$$

is the finite natural mortality rate (the probability of dying from natural causes in the presence of fishing mortality). Annual survival (S) can be estimated with a multiyear tagging study (Ricker 1975), but the only tag return parameter that can be estimated is the product $f = \lambda u$, the tag recovery rate, because information only comes from reported tags. The component rates λ and u are not estimable without additional information, such as that generated by reward tags or angler surveys

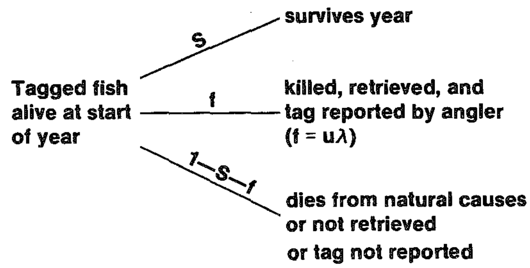


Figure 18.4 Possible fates of a fish tagged at the start of the year, modified to show only the quantities S (finite annual survival rate), f (tag recovery rate), and $1 - S - f$ that can be estimated with basic tagging studies, based on Brownie et al. (1985). (Reproduced from Pollock et al. 1991.)

(discussed later). Figure 18.4 is a modification of Figure 18.3 that shows which quantities are estimable by basic tagging studies.

18.3.2.2 Model Structure

The basic models of Brownie et al. (1985) accommodate multiple-year tagging and recovery data for animals that are not stratified by age-class. In its most general form, model 0, S_i is the year-specific annual survival rate, f_i^* is the year-specific annual recovery rate for newly tagged fish, and f_i is the year-specific annual recovery rate for previously tagged fish. Separate recovery rates may be needed for previously (f_i) and newly tagged fish (f_i^*) if fishing begins before all tagging is completed, if the newly tagged fish are more difficult to capture, or if reporting rates change with distance from the tagging locations. Table 18.1 shows the matrix of expected recoveries for the example of three tagging years and four recovery years.

Model 0 can be successively restricted by assuming that certain parameters remain constant over years, cohorts, or both. Model 1 eliminates the distinction between recovery rates of newly and previously tagged fish by setting $f_i^* = f_i$ for all i. Model 2 makes survival constant ($S_i = S$ for all years) and model 3 makes survival and recovery rates constant ($S_i = S$ and $f_i = f$ for all years). Although it is advantageous to restrict the model as much as possible, thereby reducing the number of parameters to be estimated and increasing the precision of those that remain, models 2 and 3 are too restrictive for most fisheries studies. The computer

Table 18.1 Matrix of expected recoveries of tagged fish according to model 0 of Brownie et al. (1985) for three tagging years and four recovery years; S_i is annual survival from year i to year $i + 1$; f_i^* is recovery rate in the first year after tagging, and f_i is recovery rate in subsequent years. (Reproduced from Pollock et al. 1991.)

Year of tagging	Number of fish tagged	Expected number of recoveries in year:			
		1	2	3	4
1	N_1	$N_1 f_1^*$	$N_1 S_1 f_2$	$N_1 S_1 S_2 f_3$	$N_1 S_1 S_2 S_3 f_4$
2	N_2		$N_2 f_2^*$	$N_2 S_2 f_3$	$N_2 S_2 S_3 f_4$
3	N_3			$N_3 f_3^*$	$N_3 S_3 f_4$

program ESTIMATE (Brownie et al. 1985) can be used to determine the best model and estimate survival and recovery rates.

If fish ages are known (from scales sampled when fish were tagged), differential survival and recovery rates of age-classes can be analyzed; the computer program BROWNIE is available for this situation (Brownie et al. 1985). The approach we describe below—using estimated tag-reporting rates to convert recovery to exploitation rates—also can be generalized to handle age-structured tagging data. These models can be applied to any time periods of equal length, not just to years.

18.3.2.3 Model Assumptions

Six assumptions underlie the multiyear tagging models (Nichols et al. 1982; Pollock and Raveling 1982; Brownie et al. 1985:6). The following discussion of them is based on Pollock and Raveling (1982), as presented by Pollock et al. (1991).

(1) *The tagged sample is representative of the target population.* This assumption is very important, especially if survival and recovery rates vary (which would violate assumption 6). If, for example, most fish are tagged in or near areas with heavy angling pressure, tag recovery rates are likely to be high and inferred survival low, which would give a distorted result for the entire region. To avoid this, tagging should be dispersed over the region, preferably in proportion to the population density in each part of the region (if this is known). Otherwise, one must assume that tagged fish mix thoroughly throughout the region, which is usually unrealistic.

(2) *No tags are lost.* Tag loss produces a negative bias on survival estimates that is relatively worse for species with high survival rates (Nelson et al. 1980). Recovery rate estimates are also negatively biased. A double-tagging study often is needed to adjust survival and recovery rates (Seber 1982:94).

(3) *Tagging does not affect survival.* If tagging substantially increases mortality, the survival estimates will not apply to untagged fish. Sometimes it may be practical to hold fish in enclosures to evaluate short-term tagging mortality.

(4) *Recovered tags are correctly attributed to year.* Sometimes anglers report tags from fish caught last year as if the fish had been caught this year. The incidence of delayed reporting is rarely known, but it positively biases survival estimates to the extent that it occurs.

(5) *Tagged fish have independent fates.* This assumption is probably violated in almost all tag return studies, because fish are not independent entities in terms of survival or other characteristics. Lack of complete independence will not bias any model estimators, but to the extent that fish behave in "concert" with one another, effective sample sizes will be smaller than actual sample sizes, true variances will be underestimated by the models, and calculated confidence intervals will be smaller than they should be.

(6) *All tagged fish within an identifiable class have the same annual survival and recovery probabilities.* Survival and recovery rates are likely to be heterogeneous (a violation of this assumption), but we do not know how serious this will be for fish-tagging studies. Simulation studies by Nichols et al. (1982) and Pollock and Raveling (1982) indicated that if only recovery rates are heterogeneous, survival estimates are not biased and recovery rate estimates can be averaged for the population (if the tagging sample is random). If survival probabilities are heterogeneous over the population, on the other hand, survival rate estimators

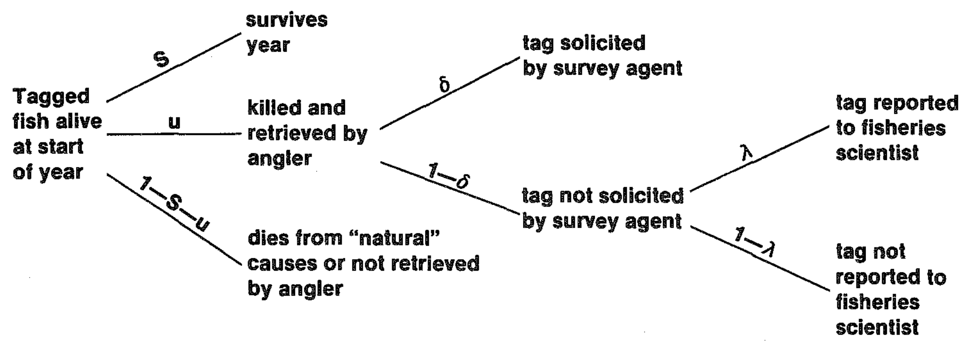


Figure 18.5 Possible fates of a fish tagged at the start of the year, extended from Brownie et al. (1985) to allow for tag solicitation from anglers with probability δ. (Reproduced from Pollock et al. 1991.)

generally will have a negative bias that is more serious when the average survival rate is high and the study is short. In theory, survival rate estimators could have a positive bias if segments of the population have markedly different survival rates but similar recovery rates. This would imply that survival is mainly controlled by heterogeneous natural mortality, the result of genetic or environmental variability.

18.3.2.4 Generalization to Allow for Tag Solicitation

The original models (Figure 18.3) did not allow for tags solicited by survey agents or other biologists, and a more realistic model incorporates the unknown probability δ that a survey agent will encounter an angler with a tagged fish and record the tag number (Figure 18.5). If a tag is solicited, of course, it is reported with certainty. Nevertheless, several quantities still are not estimable without further information. If the recovery rate of solicited tags is defined as $f_s = u\delta$ and the recovery rate of unsolicited tags (tags voluntarily returned by anglers in person or by mail) as $f_r = u(1 - \delta)\lambda$, Figure 18.5 can be transformed to Figure 18.6, which presents only the estimable quantities S, f_s, and f_r. Estimation is now more complex because there are two classes of tag recovery.

The generalization of model 0 for this situation is presented in Table 18.2. Estimates for this model and others can be obtained by using the general FORTRAN program called SURVIV (White 1983). From estimates of $\hat{f}_s$ and $\hat{f}_r$, exploitation rate u can be estimated if the reporting rate (λ) can be estimated:

$$\hat{u} = \hat{f}_s + \hat{f}_r/\hat{\lambda}.$$

The expected or average value of $\hat{u}$ is

$$E(\hat{u}) \approx u\delta + \frac{u(1-\delta)\lambda}{\lambda} = u\delta + u(1-\delta) = u,$$

so $\hat{u}$ will be unbiased in large samples. It is not necessary to estimate δ, because it drops out of the equation. Estimation of λ, being crucial, is discussed next.

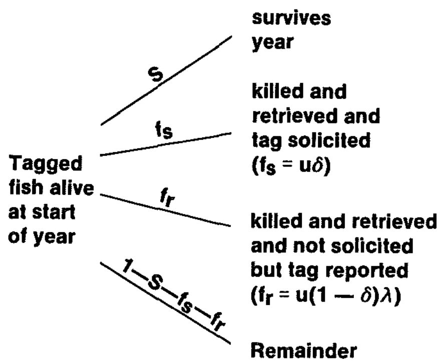


Figure 18.6 Possible fates of a fish tagged at the start of the year, modified to show only the quantities, including rates of solicited (f_s) and unsolicited (f_r) tag recoveries, that are estimable with basic tagging studies. (Based on Brownie et al. 1985 and reproduced from Pollock et al. 1991.)

18.3.2.5 Estimation of Tag-Reporting Rate (λ)

Use of Tag Return Rewards. One way to estimate λ is to use two types of tag in a special study, one (control) that offers no reward for being returned, and one that does. This approach to estimating λ was developed by Henny and Burnham (1976) and applied by them and by Conroy and Blandin (1984) (see Pollock et al. 1991 for details). A critical assumption is that all recaptured fish with special reward tags are reported, either voluntarily or via solicitation. Ideally, the reward notice should be displayed prominently on the tag so that it is not likely to be overlooked, although this can present operational difficulties in practical situations. If this assumption is violated, it causes a potentially serious positive bias in the reporting-rate estimator (Conroy and Williams 1981). Wildlife scientists have shown that as the reward increases, the recovery rate approaches 100% asymptomatically (Nichols et al. 1991); however, the amount necessary for near 100% reporting differs among species and locations.

Another assumption is that angler behavior does not change in response to the study. If a reward is advertised, anglers may become more likely to report regular tags and to fish specifically for reward tags. We argue against the use of lotteries if the objective is to measure tag-reporting rate. Lotteries may inspire a greater

Table 18.2 Matrix of expected tag recoveries for the generalization of model 0 to allow for tag solicitation. The matrix now contains both solicited and voluntarily reported tags for three tagging years and four recovery years. (Reproduced from Pollock et al. 1991.)

Year of tagging	Number of fish tagged	Expected number of recoveries in year: 1	2	3	4	Mode of tag recovery
1	N_1	$N_1 f_{1s}^*$	$N_1 S_1 f_{2s}$	$N_1 S_1 S_2 f_{3s}$	$N_1 S_1 S_2 S_3 f_{4s}$	Solicited
		$N_1 f_{1r}^*$	$N_1 S_1 f_{2r}$	$N_1 S_1 S_2 f_{3r}$	$N_1 S_1 S_2 S_3 f_{4r}$	Reported
2	N_2		$N_2 f_{2s}^*$	$N_2 S_2 f_{3s}$	$N_2 S_2 S_3 f_{4s}$	Solicited
			$N_2 f_{2r}^*$	$N_2 S_2 f_{3r}$	$N_2 S_2 S_3 f_{4r}$	Reported
3	N_3			$N_3 f_{3s}^*$	$N_3 S_3 f_{4s}$	Solicited
				$N_3 f_{3r}^*$	$N_3 S_3 f_{4r}$	Reported

return of tags, but they do not include a subset of tags that will always be reported, and so they provide no certain reference against which to estimate λ.

Use of Angler Surveys. The tag-reporting rate (λ) also can be estimated with an on-site angler survey. When an agent checks anglers' catches, the probability of a tag being reported is assumed to be 1.0; anglers not interviewed report tags with probability λ. The survey provides an estimate of the total number of tags that could be reported, and the fraction of this total that anglers actually report is the estimate of λ. This method, described in detail by Pollock et al. (1991), requires that the agent and the anglers not miss any tags on fish that are examined, that the angler not conceal solicited tags, and that the survey design be based on valid probability sampling so that the estimated total number of available tags is unbiased.

18.3.2.6 Separation of Fishing and Natural Mortality Estimates

Survival rate (S), solicited recovery rate (f_s), and reported recovery rate (f_r) can be estimated from a multiyear tagging study, and the tag-reporting rate (λ) can be estimated from a reward tagging study or an angler survey. The estimated exploitation rate ($\hat{u}$) is

$$\hat{u} = \hat{f}_s + \hat{f}_r/\hat{\lambda}.$$

The natural mortality estimator that occurs in the presence of fishing mortality (v, expectation of natural death: Ricker 1975) can be obtained by subtraction of fishing mortality ($\hat{u}$) from the total mortality ($1 - \hat{S}$):

$$\hat{v} = 1 - \hat{S} - \hat{u}.$$

Variances and covariances of $\hat{u}$ and $\hat{v}$ were presented by Pollock et al. (1991).

The natural mortality rate, v, depends on the amount of fishing mortality and the exploitation rate, u, on the amount of natural mortality. The two components of total mortality can be separated if one makes assumptions about their timing. Following Ricker (1975:11),

F = instantaneous fishing mortality rate (year^{-1}),
M = instantaneous natural mortality rate (year^{-1}),
Z = instantaneous total mortality rate = $F + M$,
$S = e^{-Z}$ = annual survival rate or probability of surviving a year,
$m = 1 - e^{-F}$ = conditional fishing rate or probability of dying in a year from fishing mortality if there is no natural mortality, and
$n = 1 - e^{-M}$ = conditional natural mortality rate or probability of dying in a year from natural mortality if there is no fishing mortality.

The estimator of the instantaneous rate of total mortality is

$$\hat{Z} = -\log_e \hat{S}.$$

In some fisheries (the type I fishery of Ricker 1975), fishing activity is restricted to a small part of the year such that fishing and natural mortality occur sequentially rather than concurrently. In others (type II), fishing and natural mortality occur concurrently. In either case, F, M, m, and n can be estimated by

Table 18.3 Anglers' tag recoveries for a hypothetical 3-year tagging program, based roughly on a study by Youngs and Robson (1975). (Reproduced from Pollock et al. 1991.)

Year of tagging	Number of fish tagged	Number of tags recovered in year: 1	2	3	Mode of tag recovery
1	1,000	58	31	16	Solicited
		48	24	12	Reported
2	1,000		60	29	Solicited
			49	23	Reported
3	1,000			61	Solicited
				49	Reported

manipulation of the relationships given in this section, as demonstrated by Pollock et al. (1991).

Sometimes only a single-year tagging study is possible; then the data will be the observed proportion of tags recovered in the solicited and reported categories. The exploitation rate (u) can still be estimated as before, but neither total survival (S), natural mortality (v), nor the quantities that depend on them can be estimated. Published studies of this type abound, except that solicited and unsolicited tag returns have not been reported separately.

Example. Table 18.3 shows tag return data for solicited and reported tags during the first 3 years of tagging and the first 3 years of recovery, based on a study by Youngs and Robson (1975). Table 18.4 gives expected values for the model fitted, which is a generalization of model 1 of Brownie et al. (1985), and Table 18.5 presents the survival and recovery rate estimates generated by White's (1983) program SURVIV.

Suppose that during the second year, a creel survey on the lake in this example produced the reporting rate estimate $\hat{\lambda} = 0.2086$, with $\hat{SE}(\hat{\lambda}) = 0.022$. If this reporting rate can be assumed for all years, estimates of $\hat{u}_i$ and $\hat{v}_i$ can be obtained (Table 18.5); for example,

$$\hat{u}_1 = \hat{f}_{1s} + \hat{f}_{1r}/\hat{\lambda} = 0.0580 + 0.0480/0.2086 = 0.2881$$

and

Table 18.4 Matrix of expected tag recoveries for the study presented in Table 18.3. The model is a generalization of model 1 (Brownie et al. 1985) that allows consideration of solicited and recovered tags. (Reproduced from Pollock et al. 1991.)

Year of tagging	Number of fish tagged	Expected number of recoveries in year: 1	2	3	Mode of tag recovery
1	N_1	$N_1 f_{1s}$	$N_1 S_1 f_{2s}$	$N_1 S_1 S_2 f_{3s}$	Solicited
		$N_2 f_{1r}$	$N_1 S_1 f_{2r}$	$N_1 S_1 S_2 f_{3r}$	Reported
2	N_2		$N_2 f_{2s}$	$N_2 S_2 f_{3s}$	Solicited
			$N_2 f_{2r}$	$N_2 S_2 f_{3r}$	Reported
3	N_3			$N_3 f_{3s}$	Solicited
				$N_3 f_{3r}$	Reported

Table 18.5 Parameter estimates and standard errors (SEs, in parentheses) based on tagging data and a creel survey. The survival and recovery rate estimates were obtained with program SURVIV and the hypothetical tagging data in Table 18.3. The exploitation and natural mortality rates are based on a tag-reporting rate of $\hat{\lambda} = 0.2086$ [$\hat{SE}(\hat{\lambda}) = 0.022$] estimated from a (hypothetical) angler survey. (Reproduced from Pollock et al. 1991.)

Year	Survival rate ($\hat{S}_i$)	Solicited tag recovery rate ($\hat{f}_{is}$)	Reported tag recovery rate ($\hat{f}_{ir}$)	Exploitation rate ($\hat{u}_i$)	Natural mortality rate ($\hat{v}_i$)
1	0.5155 (0.0657)	0.0580 (0.0074)	0.0480 (0.0068)	0.2881 (0.0409)	0.1964 (0.0758)
2	0.4799 (0.0707)	0.0600 (0.0066)	0.0482 (0.0059)	0.2911 (0.0374)	0.2990 (0.0780)
3		0.0614 (0.0068)	0.0486 (0.0059)	0.2944 (0.0369)	

$$\hat{v}_1 = 1 - \hat{S}_1 - \hat{u}_1 = 1 - 0.5155 - 0.2881 = 0.1964.$$

Estimates of still other quantities are possible, depending on whether the lake supports a type I or a type II fishery.

18.3.2.7 Discussion

A multiyear tagging study combined with a reward tagging study or an angler survey allows estimation of both exploitation and natural mortality rates. Reasonable natural mortality estimates of exploited populations are difficult to obtain by other methods (Vetter 1988).

Other methods of estimating reporting rate have been proposed. Youngs (1974) showed that reporting rate (λ) can be estimated directly from multiyear tagging data if one can support strong assumptions of constant natural mortality rates and constant reporting rates over years. Green et al. (1983) estimated by surreptitiously planting tags in creeled fish, but we suspect that this approach might alter angler behavior and that anglers are likely to make their most attentive inspection of their catch before survey clerks have a chance to doctor it.

The use of reward tags to estimate λ and thereby to separate fishing from natural mortality depends critically on the assumption that reward tags are returned with certainty. Wildlife studies (Nichols et al. 1991) have shown that this assumption can be justified if the rewards are high enough, but the assumption (and the monetary consequences) needs to be investigated for important fisheries. The use of a lottery to boost the recovery rate of tags for estimation of λ is logically faulty; the money would be better spent on high-reward tags. The relative virtues of reward tagging studies and angler surveys for estimating tag-reporting rates also need research. Angler surveys are more expensive, but they provide important additional information about recreational fisheries.

Many fisheries are exploited by both commercial and recreational groups. If it is important to apportion the exploitation rate between the two user groups, the table of expected tag returns could be modified to account for three classes of tag returns: solicited tags, tags voluntarily reported by user group 1, and tags voluntarily reported by user group 2. White's (1983) program SURVIV can estimate the parameters.

Wildlife banding data have been used to study the question of whether natural and hunting mortality are additive or compensatory or a combination of the two. The first important paper on this question was by Anderson and Burnham (1976), who relied on the extensive banding data for mallards; other papers on mallards have followed (Anderson et al. 1982; Nichols and Hines 1983; Burnham and Anderson 1984; Burnham et al. 1984; Nichols et al. 1984). The evidence suggests some degree of compensation, at least for some age-classes. Pollock et al. (1989) found evidence of additivity in a population of quail subjected to a late-season hunt. It would be valuable to apply similar analyses to fisheries tagging data. Virtually all the traditional models used in fisheries population dynamics assume that natural mortality and fishing mortality are additive.

18.3.3 Estimation of Population Size

The Lincoln–Petersen estimator (or a modification of it; Seber 1982:60) can be used to estimate population size in a single-year or multiyear tagging study. The estimator (adjusted for bias) is

$$\hat{N} = \frac{(\hat{M}+1)(\hat{C}+1)}{\hat{r}+1} - 1,$$

where $\hat{M}$ is the estimated number of tags in the population, $\hat{C}$ is the estimated total catch, and $\hat{r}$ is the estimated total tags in the catch. The estimate of $\hat{r}$ requires adjustment of the observed tag returns to account for nonreporting. Jagielo (1991) discussed use of this estimator and how to extend it to the case where M is unknown in a multiyear study. The marked population size at time i can be obtained from the original marked population sizes and the appropriate survival rate estimates. We emphasize that although the estimator takes the same form as it does in other applications, the variance of $\hat{N}$ is larger and more complex to calculate because M, C, and r all have to be estimated in an angler survey; normally they are known statistics when used in traditional fisheries applications.

18.4 CATCH–EFFORT MODELS

18.4.1 Background

The catch–effort method is applicable when a population is fished until enough fish are removed to reduce significantly the catch per unit effort (CPUE), the latter being considered proportional to the stock size. For example, if removal of 5,000 fish reduces CPUE by 40%, the original stock (size) is estimated as 5,000/0.40, or 12,500 fish. Point values of CPUE can be affected by many fishery and environmental variables, so a series of CPUE measurements usually is made, and stock size is estimated from Ricker (1975:149).

Most applications of catch–effort models have been to commercial fisheries, but we believe that these methods could be usefully applied in some recreational fisheries that are subject to heavy exploitation and in which catch and effort are estimated by angler surveys over the fishing season. Here we briefly review the catch–effort models for closed and open populations following Ricker (1975) and Seber (1982). We emphasize model assumptions and the unique questions that arise when catch and effort data are estimated from angler surveys subject to bias and variability.

18.4.2 Closed Population Models

A "closed population" for present purposes is one in which the overwhelming source of mortality is fishing mortality; because a fishery is so short and intensive, natural mortality can be assumed to be zero. (Closed populations also have no immigration or emigration. Even if these processes occur, they sometimes can be ignored over short time periods.) Seber (1982) presented maximum-likelihood and regression catch–effort estimators for closed populations. Ricker (1975) outlined the two most common types of analysis for this situation. In the Leslie method (Leslie and Davis 1939), catch per unit effort (*y*-axis) is regressed against cumulative catch (*x*-axis). The straight line has negative slope because the heavily exploited population is decreasing in size. The line is extrapolated to the *x*-axis (CPUE = 0), and the cumulative catch at that point is equivalent to the initial population size. The slope of the regression is the catchability coefficient. In the DeLury method (DeLury 1947), the logarithm of catch per unit effort is plotted against cumulative effort, and the fitted straight line once again yields estimates of initial population size and catchability but the estimation of equations are a little different. The following brief summaries of these methods are drawn from Ricker (1975:150–154) with some differences in notation.

18.4.2.1 Leslie Method

The Leslie method exploits the linear relationship between expected or average catch per unit effort (C_i/e_i) and cumulative catch (K_i) at time i:

$$(C_i/e_i) \approx qN_0 - qK_i;$$

q is the catchability coefficient and N_0 is the initial population size before exploitation begins. The slope of the linear regression line is an estimate of q. The intercept of the fitted line on the x-axis is an estimate of N_0. The intercept of the fitted line on the y-axis is an estimate of (qN_0).

18.4.2.2 DeLury Method

The DeLury method uses the linear relationship between the logarithm of CPUE and cumulative effort (E_i):

$$\log_e(C_i/e_i) \approx \log_e(qN_0) - qE_i.$$

The slope of the fitted line gives an estimate of q, and the intercept on the y-axis is an estimate of $\log_e(qN_0)$. From these two estimates the initial population size (N_0) can be estimated.

18.4.2.3 Evaluation

Ricker (1975) recommended the Leslie method over the DeLury method for most applications, presumably because catch is usually known with less error than effort in commercial fisheries. This reasoning does not apply to recreational fisheries in which effort and catch are both estimated from angler surveys and for which effort is usually estimated with a smaller relative standard error than catch (see Chapter 15).

Statisticians usually recommend maximum-likelihood methods over regression methods because if the model assumptions hold, maximum-likelihood estimators theoretically have greater precision. Because estimates of catch and effort are used instead of the true values, however, maximum-likelihood estimators may not necessarily be better in practice. The regression methods have at least two advantages: calculations are simple (especially with standard computer packages), and the plots can be used to check for assumption violations. More research, perhaps with simulation modeling, is needed on the relative merits of these methods.

The following assumptions are made for the model.

(1) *The population is closed,* meaning that the only change in population size is the change caused by exploitation. If fishing goes on too long, natural mortality will become appreciable, fish may immigrate or emigrate (especially in fluvial waters), and younger fish may recruit into the exploitable population. Then an open population model will be needed (Section 18.4.3).

(2) *Catchability is constant for all fish over all sampling times.* Constant catchability is difficult to assure. Changes in weather and other environmental variables can both increase and decrease catchability. Also, individual fish may not all have the same catchability due to size selectivity or other factors; this heterogeneity of catchability (discussed in detail by Seber 1982) causes a negative bias in population size estimates.

(3) *The units of fishing effort are independent.* The models require that catch be proportional to fishing effort. In turn, this requires that all units of effort have the same expected effect on the fishery, as well as that catchability remain constant (assumption 2). If a concentration of anglers scares away the fish from one place, the units of effort of different anglers on the same or different days may have different efficiencies of capture and may not be independent.

(4) *Anglers all use the same type of gear, or different types of gear can be converted to a standard measure of effort.* This may be difficult to do in practice.

Sometimes marked fish are used to help evaluate some of the assumptions of the models. For example, if recruitment is suspected, a comparison of the regression lines for marked and unmarked fish should shed light on the issue, because the marked population will not have any recruitment. (Ricker 1975; Seber 1982).

18.4.3 Open Population Models

If fish are subject to substantial natural mortality during the fishing season, closed population models will not be adequate. Seber (1982) discussed a regression and maximum-likelihood approach to this problem. The regression model was first suggested by Chapman (1961).

A fish population studied over several seasons will experience natural mortality and recruitment in addition to fishing mortality. Seber (1982:344) presented a method of Chapman (1961) that involves tagging before the start of the study. The tagged population is used to estimate a catchability coefficient and a natural mortality rate, which are then applied to the unmarked population to estimate population size of and recruitment to the stock. Like previous models, this approach assumes catch and effort are known exactly, whereas in recreational fisheries they are estimated from angler surveys. Another, more complex model that has not been used much yet is that of Dupont (1983).

18.5 CHANGE-IN-RATIO MODELS

18.5.1 Background

The idea that population numbers could be estimated from a knowledge of the ratio of two categories (e.g., sexes or ages) before and after a differential harvest of the types goes back to Kelker (1940, 1944) in a wildlife setting (Seber 1982). The first stochastic models were developed by Chapman (1954, 1955) for closed populations. Other important papers were written by Chapman and Murphy (1965), who treated populations subject to natural and fishing mortality, and by Paulik and Robson (1969), who gave a good overall review.

Unlike the procedures for estimating demographic parameters discussed earlier in this chapter, the change-in-ratio or selective removal method has not been widely used with commercial or recreational fisheries. Ricker (1975:199) devoted only one page to it. However, this technique—especially in its modern forms—has a lot of potential for recreational and commercial fisheries, because fishing is often size selective and many fisheries are so heavily exploited that size ratios before and after the harvest are likely to differ substantially. Murphy (1952) used the technique to estimate the size of a salmon population. Hoenig et al. (1990) used a change-in-ratio approach to estimate the relative survival rates of two groups of fish.

We present an overview of the change-in-ratio model applied to closed and open populations, emphasizing model assumptions and the special considerations that arise when harvest is estimated from an angler survey.

18.5.2 Closed Population Models

Consider a closed population consisting of two types of fish (Seber 1982:353), designated as x-type and y-type fish (large and small fish, old and young, etc.). Suppose there is a differential change in the ratio of x-type and y-type between times t_1 and t_2, as indicated by the following notation.

X_i = number of x-type animals in the population at time t_i ($i = 1, 2$).
Y_i = number of y-type animals in the population at time t_i.
$N_i = X_i + Y_i$ = total population size at time t_i.
$P_i = X_i/N_i$ = fraction of x-type animals in the population at time t_i.
$C_x = X_1 - X_2$ = the harvest of x-type animals between times t_1 and t_2.
$C_y = Y_1 - Y_2$ = the harvest of y-type animals between times t_1 and t_2.
$C = C_x + C_y$ = the total harvest between times t_1 and t_2.

Usually C_x and C_y are assumed to be known exactly, although if an angler survey is used they will be estimated. In the simplest model, the population is closed to all gains and losses except the fishery harvest, so the time between t_1 and t_2 needs to be short to ensure that natural mortality, recruitment, emigration, and immigration are negligible.

Population estimation has the following basis. Express P_2 as

$$P_2 = \frac{X_2}{N_2} = \frac{X_1 - C_x}{N_1 - C}.$$

Substitution and rearrangement give first

$$P_2 = \frac{P_1 N_1 - C_x}{N_1 - C},$$

then

$$N_1 = \frac{C_x - CP_2}{P_1 - P_2}.$$

Suppose some research surveys can obtain samples of fish at times t_1 and t_2 that are unbiased with respect to type. The resulting estimates of P_1 and P_2 would give

$$\hat{N}_1 = \frac{C_x - C\hat{P}_2}{\hat{P}_1 - \hat{P}_2}$$

and

$$\hat{X}_1 = \hat{N}_1 \hat{P}_1,$$

the variances of which (Taylor Series approach) are

$$\text{Var}(\hat{N}_1) = \frac{N_1^2 \text{Var}(\hat{P}_1) + N_2^2 \text{Var}(\hat{P}_2)}{(P_1 - P_2)^2}$$

and

$$\text{Var}(\hat{X}_1) = \frac{N_1^2 P_2^2 \text{Var}(\hat{P}_1) + N_2^2 P_1^2 \text{Var}(\hat{P}_2)}{(P_1 - P_2)^2}.$$

These variance expressions show how important a large change in ratio is to the success of the estimation procedure. If $P_1 - P_2$ is small (i.e., if there is little change in the ratio) the variances will be large, because $(P_1 - P_2)^2$ occurs in the denominator.

In angler surveys, C_x and C_y have to be estimated, so

$$\hat{N}_1 = \frac{\hat{C}_x - \hat{C}\hat{P}_2}{\hat{P}_1 - \hat{P}_2}$$

and

$$\hat{X}_1 = \hat{N}_1 \hat{P}_1,$$

the associated variances being

$$\text{Var}(\hat{N}_1) \approx \frac{N_1^2 \text{Var}(\hat{P}_1) + N_2^2 \text{Var}(\hat{P}_2) + (1 - P_2)^2 \text{Var}(\hat{C}_x) + P_2^2 \text{Var}(\hat{C}_y)}{(P_1 - P_2)^2}$$

and

$$\text{Var}(\hat{X}_1) \approx \frac{N_1^2 P_2^2 \text{Var}(\hat{P}_1) + N_2^2 P_1^2 \text{Var}(\hat{P}_2) + P_1^2 (1 - P_2)^2 \text{Var}(\hat{C}_x) + P_1^2 P_2^2 \text{Var}(\hat{C}_y)}{(P_1 - P_2)^2}$$

(Seber 1982:371). These variances are larger than variances when C_x and C_y are known exactly.

This basic model rests on two main assumptions.

(1) *The population is closed except for the removal or fishing process.* This assumption is very important, but it has been weakened to allow for natural mortality by Chapman and Murphy (1965) (Section 18.5.3).

(2) *All fish have the same probability of being captured in the research surveys at times t_1 and t_2, irrespective of which type they are.* Although fishing can be size selective with this method, research surveys must not be. A practical limitation may be finding a gear that is not size selective for the species of fish being studied.

Work by Pollock et al. (1985), Udevitz (1989), and Udevitz and Pollock (1991) has generalized these models to allow for unequal catchability of fish in the research surveys. We believe these generalizations may be useful for recreational (and commercial) fisheries subject to heavy size-selective fishing pressure over a short fishing season such that the assumption of a closed population remains reasonable.

18.5.3 Open Population Models

Chapman and Murphy (1965) generalized the basic change-in-ratio model described above to allow for natural mortality. Generalizations along the lines of Udevitz and Pollock (1991) are also possible, and these models might be applied when the fishing season is too long for natural mortality to be ignored.

For the change-in-ratio method to be applied across several years, recruitment also needs to be included, which is much more difficult. One approach might be to use tagged animals to augment the data collected. Another approach might be to apply separate change-in-ratio procedures in each year. This discussion is brief because a lot more research is needed to clarify the use of change-in-ratio methods for open populations.

18.5.4 Combination of Catch–Effort and Change-in-Ratio Models

It is possible to combine catch–effort and change-in-ratio models in one study with impressive gains in precision of population size estimates. This has been done by Dawe et al. (1993) for a crab population. We expect further theoretical research on this important topic will occur in the near future.

18.6 CATCH-AT-AGE MODELS

A suite of methods under the general heading of catch-at-age models are considered here. These methods involve estimation of stock size and mortality rates from age-specific catch data over a period of years plus auxiliary information of various types. The terms virtual population analysis (Fry 1949), sequential population analysis (Ricker 1975), and cohort analysis (Pope 1972) are also commonly used for these models. This approach to stock assessment is widely used for commercial fisheries; it has achieved some measure of mathematical sophistication and is evolving rapidly. However, it appears to us (and to others) that the models are so severely overparameterized that many simplifying assumptions have to be made for estimation to occur. Good overviews were written by Jones (1984), Winters (1988), and Megrey (1989); Hilborn and Walters (1992) also discussed this topic in two chapters of their book.

Our brief description of the approach is based on Restrepo et al. (1992). The basic data are records of total catch by age-group for several consecutive years. These data track all the fishing removals from a cohort (fish born in a given calendar year). For simplicity, we use Pope's (1972) method of cohort analysis. Cohort analysis is a deterministic method that requires the catch from each cohort for several consecutive years, the value of the natural mortality rate (assumed constant over age and time), and one of the following for one year: the abundance of the cohort at the start of the year, the exploitation rate during the year, or the fishing mortality rate during the year. This auxiliary information usually is specified for the most recent year and then the computations proceed backwards in time. A thorough treatment of cohort analysis, including a version that uses length-specific catch data instead of age-specific data, can be found in Jones (1984).

For simplicity, it is assumed that all fishing mortality occurs at the midpoint of the year. (Other versions are possible.) Thus, if the finite natural mortality rate for the year is denoted by n (Ricker 1975), the number surviving at the midpoint of year t is $N_t\sqrt{(1-n)}$, the number present at the start of the year (N_t) times the average survival rate ($\sqrt{(1-n)}$). A catch of C_t fish is removed from the cohort, leaving $N_t\sqrt{(1-n)} - C_t$ fish in the water. These fish are subjected to natural mortality for another half year, so the number remaining at the end of the year is

$$N_{t+1} = (N_t\sqrt{(1-n)} - C_t)\sqrt{(1-n)}.$$

Solving for N_t gives the important basic result

$$N_t = [N_{t+1}/(1-n)] + [C_t/\sqrt{(1-n)}].$$

on which the method was built.

To use this equation, one needs estimates of everything on the right side. Typically the cohort catch (C_t) is known or can be estimated. Natural mortality rate (M) which gives n by the relationship $n = 1 - e^{-M}$ (Ricker 1975) is usually assumed to be constant over cohorts and years. Obtaining a good estimate of M is exceedingly difficult, and sometimes an assumed value is used; for example, $M = 0.2$ is sometimes assumed for groundfish stocks. Sometimes an estimate of natural mortality rate can be obtained from a tagging study (Section 18.3), from observations of the fishery or a similar fishery before the fishery was exploited, or from various rules of thumb that relate M to easily measured life history traits (e.g., longevity or growth: Pauly 1980; Hoenig 1983).

Obtaining an estimate of the abundance in the next year (N_{t+1}) may be difficult. Some of the previously discussed methods might be used, such as tag-return methods (Section 18.3), catch–effort models (18.4), and change-in-ratio methods (18.5). An estimate of the exploitation rate, $u_{t+1} = C_{t+1}/N_{t+1}$, allows for estimation of N_{t+1} because C_{t+1} is known or estimated. Also, an estimate of fishing mortality in the final year can be converted to an estimate of N_{t+1}.

Restrepo et al. (1992) pointed out that as one computes the size of a cohort at successively earlier ages, the results converge to values that are independent of the value of N_{t+1} used in the terminal year (but not of the value of M used). This means that these methods can be very useful for studying the history of stock biomass and exploitation for the years in which the analysis has converged sufficiently. The rate of convergence increases with increasing levels of fishing mortality. For moderately exploited stocks, convergence occurs by the time one moves 3 years back in time.

Restrepo et al. (1992) also emphasized that the models have many parameters and that there has been a lot of work to reduce their numbers. This reduction has required simplifying assumptions, which may be unrealistic. The current practice is to "tune" the cohort or virtual population analysis by choosing an abundance value for the final year so that the time series of computed population sizes matches, as closely as possible, another time series that indicates relative population abundances over time (e.g., research cruise indices).

One of the earliest papers on virtual populations was Fry's (1949) on the recreational fishery in Lake Opeongo, Ontario, where catch-at-age data were obtained from an angler survey. However, this was not a virtual population analysis in the modern sense. Serns (1986) used Pope's cohort analysis for the fishery in a Wisconsin lake. Carl et al. (1991) used these methods for the Lake Opeongo fishery for years since Fry (1949). Nevertheless, most data analyzed have been for important commercial fish stocks for which catch is closely monitored. We believe these analyses could be more widely used for recreational fish populations assessed by angler surveys. The main practical problem is that comprehensive angler surveys with age determinations have to be carried out every year for many years, a large commitment for a management agency. However, many recreational fisheries require this attention for effective management because of high exploitation and other pressures such as environmental degradation.

18.7 STOCK PRODUCTION MODELS

Stock production (surplus yield) models use catch and effort statistics compiled for a fishery over many years. They are described in most fisheries texts (Ricker 1975; Pitcher and Hart 1982) in the context of assessing commercial fisheries. The classical model was derived by Schaefer (1954), and it has since been modified and refined. Schaefer (1954) assumed a logistic growth curve relating biomass to time, a symmetric S-shaped curve. Fox (1975) used a more general Gompertz curve whose asymmetry is more realistic.

Given the appropriate growth curve, an equilibrium-yield biomass curve is derived. For the Schaefer model, this is a quadratic function with the maximum yield at half of the maximum biomass. The plot of catch per unit effort versus total fishing effort is a decreasing linear function, and the yearly catch and effort data can be used to calculate the slope of the line. This can be used to find the maximum sustainable yield (MSY) and the fishing effort that achieves it. Fitting the Fox (1975) model is slightly more complex but quite feasible with modern computer programs.

The major practical advantage of stock production models is that they require only catch and effort data, which have accumulated over many years for many commercial fisheries. The MSY is seductively easy to calculate, but the models ignore the real biological processes that actually generate the biomass. Changes in biomass integrate contributions from the separate but interacting processes of growth, recruitment, and mortality. Even if the model worked well for a steady state population, time lags in the response of one of these processes to altered fishing are quite likely to differ from time lags in another, rendering a single growth function inapplicable. Furthermore, the population processes themselves may be drastically altered by different age structures in the fish population, and age structure is also ignored by the surplus yield model (Jensen 1973). Pitcher and Hart (1982) illustrated the dangers of using stock production models by discussing the Peruvian anchovy fishery, which collapsed in the 1970s. Hilborn and Walters (1992) presented some modern variations of these methods, but they urged caution in the use of these methods, and they pointed out that surplus yield is often overestimated.

Given the stringency of the model assumptions and the years of data required, we question whether these models will be very useful for the management of most recreational fisheries assessed by angler surveys. Furthermore, the idea of maximum sustained yield has been largely replaced by the concept of optimum sustainable yield, particularly for recreational fisheries (Roedel 1975; Larkin 1977). Optimum sustainable yield embraces socioeconomic as well as biological considerations. Still, stock production models may provide useful insights if they are used in conjunction with other approaches.

18.8 EVALUATION OF REGULATIONS

An essential ingredient of management of recreational fisheries is the setting of sensible regulations and the ongoing assessment of such regulations for possible modification. Some important regulations are daily creel limits, size limits, season lengths, permits (to restrict total seasonal catch of a species to a very low level), and area closures (spawning areas or where fish are contaminated by pollutants). The purpose of these regulations is to manage the fisheries for equitable, safe enjoyment of the resources by anglers over the long term. Angler surveys may be very helpful to fisheries managers in assessing the biological effects of regulations in a variety of ways. They may also be helpful to managers in assessing angler attitudes to regulations and changes in regulations.

18.8.1 Assessment of Regulation Changes

The following list of ways in which angler surveys can be used to assess regulation changes is not exhaustive.

Before-and-After Surveys. Angler surveys before and after a regulation change may be used to assess the change in harvest and other important variables. Colvin (1991b) described such an approach for Missouri reservoirs where size and bag limits were imposed. One has to be careful to determine that the effect measured was caused by the new regulation and is not just an artifact of the passage of time. This potential confounding is similar to the one arising in environmental impact assessments (Section 18.10).

Theoretical Harvest Reduction. Sometimes angler survey data obtained before a regulatory change have been analyzed to compute the theoretical reduction in harvest that would have resulted if the bag limit or size limit had been in effect (see, for example, Colvin 1991b). This approach requires that each angler's catch be enumerated separately so that her or his reduction in catch under the bag limit or size limit can be estimated. It also assumes that anglers do not change their behavior in response to the regulation change, and it further ignores any additional unreported harvest due to evasion of the regulation or to the mortality of fish released to meet the regulation.

Angler Surveys plus Tagging Models. If tagging plus an angler survey (Section 18.3) has been used, fishing mortality and natural mortality can be separated. This information could be very valuable in deciding whether a regulation intended to change fishing mortality is sensible or not. For example, Reed and Davies (1991) stated that harvest restrictions on a fishery in Alabama did not seem to be warranted, based on an angler survey and tagging study that showed that natural mortality was much higher than fishing mortality.

Angler Surveys plus Catch–Effort Models. In a heavily exploited recreational fishery, it would be possible to estimate total catch and total effort for various periods with an angler survey. These estimates could be used in a catch–effort model (Section 18.4) to estimate stock size. If the same procedure were used after a regulation, the change in stock size could be estimated. One would have to be sure that the change in stock size was caused by the regulation (stock size may change even without a regulation change). Similarly, it may be possible to use change-in-ratio methods to this end (Section 18.5).

Catch-at-Age Models. Regulation changes could be assessed if angler surveys were ongoing over a period of years and catch-at-age methods (Section 18.6) were used. This approach, however, requires a high level of commitment from a management agency.

Simulation Modeling. Regulations may also be assessed by simulation modeling (e.g., Porch and Fox 1991). This approach has the advantage of assessing changes in angler behavior or in other effects of the bag limit. It is also cheaper than doing angler surveys to assess the bag limit. However, the simulation model will necessarily require making many assumptions that are hard to test in the field. Similar concerns apply to evaluating size limits or other regulations by simulation.

18.8.2 Assessing Angler Attitudes to Regulation Changes

Angler opinion surveys by mail (Chapter 6) can be very useful in assessing how anglers are responding to regulation changes. The cheapest method is to carry out the survey soon after the regulation has gone into effect, but ideally a management agency assesses angler opinion regularly and adds appropriate questions on regulation changes as the need arises.

18.9 EVALUATION OF STOCKING PROGRAMS

Sometimes fisheries agencies find it necessary to stock fish to enhance fisheries where inadequate natural reproduction occurs. Angler surveys may be used to assess the effectiveness of these programs in terms of catch, effort, and angler attitudes.

Two very important parameters that need to be estimated when fish are stocked are fishing mortality and the natural mortality of the stocked and native fish. These parameters could differ for stocked and native fish. One estimation method is to use tag return models combined with an angler survey as discussed in Section 18.3. Often it is of interest to compare two or more groups of tagged fish; for example, two sources of stocked fish or native versus stocked fish. An important monograph on this topic was written by Burnham et al. (1987).

18.10 ENVIRONMENTAL IMPACT ASSESSMENT

Sometimes fisheries managers want to use angler surveys to assess the environmental impact of some development project or some detrimental event such as a chemical spill or a new nuclear power plant. Impact assessment is a very difficult task because usually it is not possible to do an experimental test of the impact. Here we discuss experimental design and then follow this by discussing various study designs useful in assessing environmental impact when it is not possible to do a full-fledged experiment.

18.10.1 Experimental Design

A study becomes an experiment when some different experimental conditions (treatments) are applied to people, animals, or objects in order to observe and compare the responses. The objects on which the experiment is performed are the experimental units, and the specific experimental conditions applied to the units are called treatments (Moore and McCabe 1993).

The basic principles of experimental design are as follows.

Control. An attempt is made to control as many extraneous variables as possible; uncontrolled extraneous variables are neutralized by comparing treatments.

Randomization. In an experiment, treatments are randomly assigned to the experimental units so that any bias due to differences between the units is eliminated.

Replication. Repetition of each treatment condition on more than one experimental unit allows the assessment of variability of units treated alike. This enables valid statistical comparisons of treatment effects.

In many situations, experimental assessment of environmental impacts on fisheries is not feasible. One possible but very expensive example of an experiment is to compare the impact of stocking on reservoirs. Suppose six reservoirs with similar fisheries could be found where three could be randomly assigned to a control treatment (i.e., do nothing) and three could be randomly assigned to a stocking treatment. Later, angler surveys could be used to compare the treatment effect on the number of creeled fish.

18.10.2 Environmental Impact Design

Consider an assessment of the environmental impact a new nuclear power plant has on a river fishery. Randomization is impractical because the power plant is assigned to a river and the study has to be designed around this decision. True replication (in space) would require analysis of similar rivers with and without power plants, which is almost never possible.

Hurlbert (1984) discussed "pseudoreplication" in ecological studies. (A pseudoreplicate is some kind of replicate that cannot replace a true spatial replicate.) For the power plant example, a frequent design has been to compare a series of years before and after the power plant went on line. A year is a pseudoreplicate rather than a true replicate, so this is not an experiment in the true sense because it uses pseudoreplication and it also does not involve randomization of the treatment (power plant present or power plant not present). We now present some study designs for impact assessment with their strengths and weaknesses.

18.10.2.1 Ideal Design (Before and After, Control and Treated Sites)

We recommend that a long-term time series of angler surveys be conducted before and after an impact on some control sites as well as on the treated site. This is still not an experiment, because the treated site is not randomly assigned, but it should be possible to see any time trends and to separate them from the effect of the impact. In practice, this design may not be feasible for logistical or economic reasons.

18.10.2.2 Other Designs

Two other designs that have been used in practice are examined to expose their weaknesses.

Before versus After (on Impact Site). One common design is to compare angler surveys only on the treated site before and after an impact has occurred. This suffers from the use of pseudoreplication (years). It also suffers from confounding the effect of time with that of the impact. It is impossible to tell if there is a real impact of (for example) a power plant on a fishery or simply a change due to weather or other environmental changes. This design also does not use any randomization.

Above versus Below (Impact Site). A second common design is to carry out angler surveys above and below an impact site such as a power plant (assuming it is on a river or lake with pronounced water flow in one direction). This design suffers from the weakness of confounding power plant effects with location effects; the fisheries above and below the impact site may be naturally different. This design also has no true replication or randomization.

Although the two highlighted designs are commonly used, we emphasize again that inferences drawn from them are weaker than biologists often realize. Skalski and Robson (1992) discussed environmental impact assessment in wildlife studies, and much of their discussion is applicable to fisheries.

18.10.3 Angler Attitude and Behavior

We conclude by emphasizing that angler surveys may also be used to assess anglers' attitudes toward fish contamination or other indicators of environmental degradation. A mail survey used for this purpose is described in Section 6.5.1.

Part V

EPILOGUE

Chapter 19

Future Prospects for Angler Surveys

19.1 ENVIRONMENTAL CHALLENGES

Throughout the world, growing human populations will put increasing pressure on commercial and recreational fisheries resources into the foreseeable future. Concurrently, worldwide industrial and agricultural development will degrade the water and habitats necessary to sustain fishery resources, unless management is effective in preventing environmental decline. User conflicts within the recreational and commercial fishery sectors—and between them—are likely to increase. The need for effective management will require information from angler surveys that is of higher quality than seems satisfactory today. Ongoing comprehensive surveys often will be needed to monitor important fish populations over many years.

19.2 ITERATIVE QUALITY IMPROVEMENT IN ANGLER SURVEYS

Effective angler surveys in the future will require an iterative approach to achieve continual quality improvement. We identify six tasks of importance through which the iterations must cycle (Figure 19.1). Management agencies must assemble teams of managers, biologists, economists, sociologists, population modelers, and statisticians to work on angler surveys. The teams must clearly define goals of the surveys. Conflicting goals must be identified and resolved before surveys begin, not left for discovery until it is too late to resolve the conflicts. A common problem with large regional surveys involving state and federal agencies in the United States illustrates this point. A federal agency often is content with regional estimates (over several states) of relatively low precision that can be obtained at relatively low (though absolutely high) cost. The state agencies, however, want higher precision (at much higher cost), because they are responsible for managing most fisheries. It is crucial that these conflicts be discussed and resolved before any sampling is done.

Effective angler surveys will require attention to tasks at the six stages of design: basic analyses, interpretation, modeling, survey comparison and consolidation, and improvements for future surveys. The first three stages are quite obvious and have been emphasized in previous publications. Statisticians will need to be heavily involved at all these stages to see that sound statistical methods are used so that estimates will have low bias and high precision. Biological

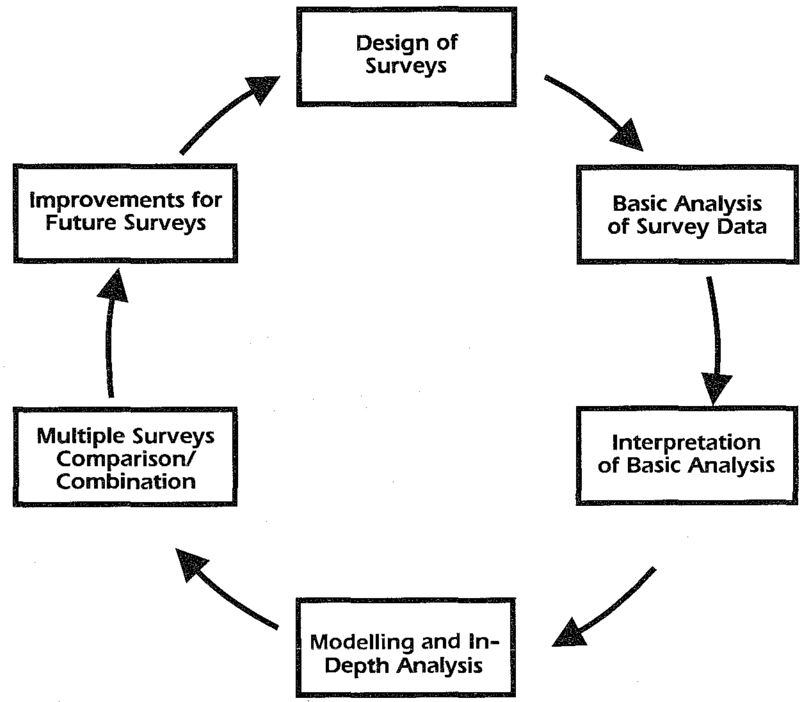


Figure 19.1 Overview of important steps for continually improving angler surveys.

modelers, econometricians, sociologists, and statisticians will need to be involved at the crucial fourth stage, in which the basic data are used in sophisticated analyses and models for a variety of purposes. This stage has been seriously neglected in past surveys. Reasonable data and estimates often have been obtained from surveys, but the data were not used to study important social, economic, or biological questions with sophisticated follow-up analyses. To fully use angler survey data to answer such questions, multiyear studies of the same fishery often will be needed. The fifth task—combining and contrasting similar surveys—will be crucial, especially for large regional fisheries. The final task—noting possible improvements in future surveys and carrying out survey research—leads naturally back to the first to continue iterative quality improvement.

19.3 FUTURE RESEARCH AREAS

We group future research on angler surveys in five general categories: survey design, applications, multidisciplinary research, technology uses, and statistical issues (Figure 19.2). The categories overlap, and they are not exhaustive.

19.3.1 Survey Design

Much research still needs to be done on the various angler contact methods. For example, we found that telephone contacts have not been used very much for

FUTURE RESEARCH AREAS

SURVEY DESIGN
- Contact Methods (Telephone, etc.)
- Combination Contact Methods (Telephone-Access, etc.)
- Large Regional Surveys

APPLICATIONS OF SURVEYS
- Catch and Effort Estimation Surveys
- Economic Surveys
- Social and Market Surveys
- Biological Uses of Angler Survey Data

MULTIDISCIPLINARY RESEARCH
- Research Teams
- Cross Fertilization and Cooperation
- Multiple Agency Cooperation and Funding

TECHNOLOGY (NEW AND IMPROVED)
- Use of Planes and Helicopters
- Portable Data Recorders and Computers
- New Data Collection Methods (Video Cameras, etc.)

STATISTICAL RESEARCH
- Catch Rate Estimation (Complete and Incomplete Trips)
- Variance Estimation
- Computer Simulation Modeling
    - Variance Estimation
    - Assessment of Assumption Violations

Figure 19.2 Overview of what future research on angler surveys may involve.

angler opinion surveys. An interesting research project would be to compare mail and telephone surveys in a small pilot project.

Combination of different contact methods in complemented surveys (Chapter 14) is very exciting, and a lot of important research can be done in this area. In a large regional boat-based fishery, for example, the results of a combination telephone survey (for effort estimation) and access survey (for catch estimation) could be compared with those from a bus route access–access or an aerial–access design. It will be important to know the comparative strengths and weaknesses of various complements in particular survey contexts.

19.3.2 Applications of Surveys

The traditional use of angler surveys to estimate catch and effort still has many difficult challenges, especially in large regional fisheries. For example, a river fishery may have both day and night and boat and bank anglers scattered over very large areas. Good estimation of catch and effort may only be possible with several different survey methods. Night fishing data might have to come from a telephone survey for safety reasons, boat fishing estimation might be feasible with a bus route access survey, and bank fishing assessment might require a roving survey.

The use of angler surveys for economic, social, and market purposes is new and needs much research. Economic and sociologic research methods are still evolving rapidly, and specialists in these methods should be involved to ensure that the latest methods are incorporated into creel and angler surveys. Although the importance of including economists and sociologists on survey research teams should be obvious, these disciplines sometimes have been overlooked in the past.

The use of angler survey data for biological purposes is poorly studied. Many techniques from commercial fisheries management appear applicable, but they need to be researched, applied to recreational fisheries, and reported in the literature if successful.

19.3.3 Multidisciplinary Research

Angler survey research must be multidisciplinary. Fisheries management is too complex and relevant disciplines are too numerous and specialized for the research to be otherwise. The days when an individual could be "sufficiently knowledgeable" about surveys have passed. However, the multidisciplinary approach will be expensive and will require multiagency cooperation.

19.3.4 Technology

Technology is developing so rapidly that the line between conjecture and reality is nearly invisible. Hardly considered a few years ago, field computers for recording data are increasingly common today. Because angler surveys are inherently expensive, relevant technology advances must be tested and, if worthy, adopted to improve productivity. Aerial observation (from planes, helicopters, balloons, etc.) is an important area of research; little of it seems to be done now. Further improvements in electronic data recording (wands, video cameras, etc.) are imminent. Abilities to record lengths and weights electronically already exist, and electronic shape recognition (for species identifications) may not be far away. Analytical survey software can be refined. Every aspect of surveys should be continually examined for technological improvement.

19.3.5 Statistical Issues

Statistical problems have been noted throughout this book. Among the most important issues is the proper catch rate estimator to use for complete and incomplete trips (Chapter 15). The question of how to estimate variances and standard errors of estimates in complex multistage designs is important for many surveys. More simulation modeling is needed to study the biases induced by violations of assumptions and to estimate variances and standard errors.

19.4 CONCLUDING REMARKS

Although the future holds difficult challenges and some negative aspects, it also holds some exciting and positive prospects. Knowledge of angler survey methodology has been much improved by recent publications (e.g., Guthrie et al. 1991), and there is a growing awareness of the need for an integrated approach to angler survey design. Technology is likely to improve the quantity and quality of data collected and may even allow new types of data to be obtained.

Writing this manual has been a challenge to us, but exciting and rewarding. We are deeply indebted to our colleagues who came before and to all the contributors to the International Symposium on Creel and Angler Surveys in Fisheries Management, who broadened our perspectives. They made the writing of this manual possible.

References

Absher, J. D., and J. R. Collins. 1987. Southern Lake Michigan sportfishery: angler profiles and specialization index for Illinois and Indiana. Illinois–Indiana Sea Grant Program, Technical Report IL-IN-SG-R-87-2, Champaign.

Adamowicz, W. L., and W. E. Phillips. 1983. A comparison of extramarket benefit evaluation techniques. Canadian Journal of Agricultural Economics 31:401–412.

Adams, C. E., J. K. Thomas, and W. R. Knowles. 1993. Explaining differences in angling rates in the United States. Fisheries (Bethesda) 18(4):11–17.

Ajzen, I., and M. Fishbein. 1980. Understanding attitudes and predicting social behavior. Prentice-Hall, Englewood Cliffs, New Jersey.

Amesbury, S. S., T. S. Sherwood, and G. W. Davis. 1991. Monitoring a tropical island fishery. American Fisheries Society Symposium 12:84–87.

Anderson, D. R., and K. P. Burnham. 1976. Population ecology of the mallard: VI. The effect of exploitation on survival. U.S. Fish and Wildlife Service Resource Publication 128.

Anderson, D. R., K. P. Burnham, and G. C. White. 1982. The effect of exploitation on annual survival of mallard ducks: an ultrastructural model. Pages 33–39 *in* Proceedings, XI International Biometric Conference, Toulouse, France.

Anderson, L. E., and P. C. Thompson. 1991. Development and implementation of the angler diary monitoring program for Great Bear Lake, Northwest Territories. American Fisheries Society Symposium 12:457–475.

Applegate, J. E. 1984. Nongame tax checkoff programs: a survey of New Jersey residents following the first year of contributions. Wildlife Society Bulletin 12:122–128.

Bailar, B. 1989. Information needs, surveys, and measurement errors. Pages 1–24 *in* D. Kasprzyk, G. Duncan, G. Kalton, and M. P. Singh, editors. Panel surveys. Wiley, New York.

Barnett, V. 1974. Elements of sampling theory. The English University Press, London.

Baxter, J. L., and P. H. Young. 1953. An evaluation of the marine sportfishing record system in California. California Fish and Game 39:343–353.

Bayley, P. B., and M. Petrere, Jr. 1989. Amazon fisheries: assessment methods, current status and management options. Pages 385–398 *in* D. P. Dodge, editor. Proceedings of the international large river symposium. Canadian Special Publication of Fisheries and Aquatic Sciences 106.

Bayley, P. B., S. T. Sobaski, and D. J. Austin. 1990. The Fisheries Analysis System (FAS): creel survey and lake analyses. Illinois Natural History Survey. Center for Aquatic Ecology Technical Report 90/12, Champaign.

Bayley, P. B., S. T. Sobaski, M. J. Halter, and D. J. Austin. 1991. Comparisons of Illinois creel surveys and the precision of their estimates. American Fisheries Society Symposium 12:206–211.

Beisser, G. S. 1989. Northwest region fisheries investigations: an evaluation of the access point and roving non-uniform probability creel surveys used in Georgia. Georgia Department of Natural Resources, Final Report GA F-036-9/Study IV, Atlanta.

Best, E. A., and H. D. Boles. 1956. An evaluation of creel census methods. California Fish and Game 42:109–115.

Bishop, R. C., K. J. Boyle, and M. P. Welsh. 1987. Toward total economic valuation of Great Lakes fishery resources. Transactions of the American Fisheries Society 116:339–345.

Bishop, R. C., and T. A. Heberlein. 1986. Does contingent valuation work? Pages 123–147 *in* R. G. Cummings, D. S. Brookshire, and W. D. Schulze, editors. Valuing environmental goods: an assessment of the contingent valuation method. Rowman and Allanheld, Totowa, New Jersey.

Bishop, R. C., and T. A. Heberlein. 1990. The contingent valuation method. Pages 81–104 *in* R. L. Johnson and G. V. Johnson, editors. Economic valuation of natural resources: issues, theory, and applications. Westview Press, Boulder, Colorado.

Blumberg, H. J., C. Fuller, and A. P. Hare. 1974. Response rates in postal surveys. Public Opinion Quarterly 38:113–123.

Boer, A. H., G. Redmond, and T. J. Pettigrew. 1989. Loran-C: a navigation aid for aerial surveys. Journal of Wildlife Management 53:228–230.

Bohm, P. 1971. An approach to the problem of estimating demand for public goods. Swedish Journal of Economics 73(1):55–66.

Bohm, P. 1972. Estimating demand for public goods: an experiment. European Economic Review 3(2):111–130.

Boyle, K. J., and R. C. Bishop. 1988. Welfare measurements using contingent valuation: a comparison of techniques. American Journal of Agricultural Economics 70:20–29.

Boyle, K. J., R. C. Bishop, and M. P. Welsh. 1985. Starting point bias in contingent valuation bidding games. Land Economics 61:188–194.

Brookshire, D. S., R. C. d'Arge, W. D. Schulze, and M. A. Thayer. 1981. Experiments in valuing public goods. Pages 132–172 *in* V. K. Smith, editor. Advances in applied microeconomics, volume 1. JAI Press, Greenwich, Connecticut.

Brookshire, D. S., B. C. Ives, and W. D. Schulze. 1976. The valuation of aesthetic preferences. Journal of Environmental Economics and Management 3:325–346.

Brookshire, D. S., A. Randall, and J. R. Stoll. 1980. Valuing increments and decrements in natural resource service flows. American Journal of Agricultural Economics 62:478–488.

Brown, B. E. 1971. Implications from the Oklahoma State lake creel survey to improve creel survey design. Proceedings of the Annual Conference Southeastern Association of Game and Fish Commissioners 24(1970):577–591.

Brown, T. L. 1976. The 1973–75 salmon runs: New York Salmon River sport fishery, angler activity, and economic impact. State University of New York, New York Sea Grant Institute, Publication NYSSGP-RS-76-025, Stony Brook.

Brown, T. L. 1977. Economic impact of New York's Great Lakes sports fisheries. Journal of Great Lakes Research 3:164–166.

Brown, T. L. 1983. A market segmentation of New York's Great Lakes anglers. New York Sea Grant Research Report, Ithaca.

Brown, T. L. 1984. Special problems in evaluating urban fishing programs. Pages 151–155 *in* L. J. Allen, editor. Urban fishing symposium proceedings. American Fisheries Society, Fisheries Management Section and Fisheries Administrators Section, Bethesda, Maryland.

Brown, T. L. 1987. Typology of human dimensions information needed for Great Lakes sport-fisheries management. Transactions of the American Fisheries Society 116:320–324.

Brown, T. L. 1991. Use and abuse of mail surveys in fisheries management. American Fisheries Society Symposium 12:255–261.

Brown, T. L., N. A. Connelly, and D. J. Decker. 1986. First-year results of New York's "Return A Gift to Wildlife" tax checkoff. Wildlife Society Bulletin 14:115–120.

Brown, T. L., and W. F. Siemer. 1991. Toward a comprehensive understanding of angler involvement. Pages 149–152 *in* Proceedings, northeastern recreation symposium. U.S. Forest Service General Technical Report NE-160, Radnor, Pennsylvania.

Brown, T. L., and B. T. Wilkins. 1975. Methods of improving recreation projections. Journal of Leisure Research 7(3):225–234.

Brownie, C., D. R. Anderson, K. P. Burnham, and D. S. Robson. 1985. Statistical inference from band recovery data: a handbook. U.S. Fish and Wildlife Service Resource Publication 156.

Bryan, H. 1977. Leisure value systems and recreational specialization: the case of trout fishermen. Journal of Leisure Research 9(3):174–187.

Buchanan, T. 1983. Toward an understanding of variability in satisfactions within activities. Journal of Leisure Research 15(1):39–51.

Bultena, G. L., and L. L. Klessig. 1969. Satisfaction in camping: a conceptualization and guide to social research. Journal of Leisure Research 1:348–364.

Burnham, K. P., and D. R. Anderson. 1984. Tests of compensatory vs. additivity hypotheses of mortality in mallards. Ecology 65:105–112.

Burnham, K. P., D. R. Anderson, G. C. White, C. Brownie, and K. H. Pollock. 1987. Design and analysis methods for fish survival experiments based on release-capture. American Fisheries Society Monograph 5.

Burnham, K. P., G. C. White, and D. R. Anderson. 1984. Estimating the effect of hunting on annual survival rates of adult mallards. Journal of Wildlife Management 48:350–361.

Calhoun, A. J. 1949. California striped bass records from the party boat fishery: 1938–1948. California Fish and Game 34:211–253.

Carl, L. M. 1982. Social impacts of a stream reclamation project on urban anglers. North American Journal of Fisheries Management 2:164–170.

Carl, L. M., B. J. Shuter, and J. E. Matuszek. 1991. Production and instantaneous growth rates of lake trout before and after lake herring introduction in Lake Opeongo, Ontario. American Fisheries Society Symposium 12:476–480.

Carlander, K. D., C. J. DiCostanzo, and R. J. Jessen. 1958. Sampling problems in creel census. Progressive Fish-Culturist 20:73–81.

Caughley, G. 1977a. Analysis of vertebrate populations. Wiley, New York.

Caughley, G. 1977b. Sampling in aerial survey. Journal of Wildlife Management 41:605–615.

Cesario, F. J. 1976. Value of time in recreation benefit studies. Land Economics 52:32–41.

Chadwick, H. N. 1962. Catch records from the striped bass sport fishery in California. California Fish and Game 48:153–177.

Chapman, D. G. 1954. The estimation of biological populations. Annals of Mathematical Statistics 25:1–15.

Chapman, D. G. 1955. Population estimation based on change of composition caused by selective removal. Biometrika 42:279–290.

Chapman, D. G. 1961. Statistical problems in dynamics of exploited fisheries populations. Pages 153–168 *in* Proceedings of the Berkeley symposium on mathematical statistics and probability 4. University of California Press, Berkeley.

Chapman, D. G., and G. I. Murphy. 1965. Estimates of mortality and population from survey-removal records. Biometrics 21:921–935.

Chapman, D. G., and D. S. Robson. 1960. The analysis of a catch curve. Biometrics 16:354–368.

Chipman, B. D., and L. A. Helfrich. 1988. Recreational specializations of Virginia river anglers. North American Journal of Fisheries Management 8:390–398.

Ciriacy-Wantrup, S. V. 1952. Resource conservation: economics and policies. University of California Press, Berkeley.

Clawson, M. 1959. Methods of measuring the demand for and value of outdoor recreation. Resources for the Future, Reprint 10. Washington, D.C.

Clawson, M., and J. L. Knetsch. 1966. Economics of outdoor recreation. Johns Hopkins Press, Baltimore, Maryland.

Cochran, W. G. 1977. Sampling techniques, 3rd edition. Wiley, New York.

Colvin, M. A. 1991a. Population characteristics and angler harvest of white crappies in four large Missouri reservoirs. North American Journal of Fisheries Management 11:572–584.

Colvin, M. A. 1991b. Evaluation of minimum-size limits and reduced daily limits on the crappie populations and fisheries in five large Missouri reservoirs. North American Journal of Fisheries Management 11:585–597.

Connelly, N. A. 1987. Critical factors and their threshold for camper satisfaction at two campgrounds. Journal of Leisure Research 19(3):159–173.

Connelly, N. A., and T. L. Brown. 1990. New York statewide angler survey, 1988. New York Department of Environmental Conservation, Albany, New York.

Conroy, M. J., and W. W. Blandin. 1984. Geographical and temporal differences in band reporting rates for American black ducks. Journal of Wildlife Management 48:23–36.

Conroy, M. J., and B. K. Williams. 1981. Sensitivity of band reporting rate estimates to violation of assumptions. Journal of Wildlife Management 45:789–792.

Cox, B. G., and S. B. Cohen. 1985. Methodological issues for health care surveys. Marcel Dekker, New York.

Crone, P. R., and S. P. Malvestuto. 1988. Comparison of harvest rates for the marine recreational fishery on the eastern and western sides of Mobile Bay, Alabama, October 1984–September 1985. Proceedings of the Annual Conference Southeast Association of Fish and Wildlife Agencies 40(1986):117–126.

Crone, P. R., and S. P. Malvestuto. 1991. Comparison of five estimators of fishing success from creel survey data on three Oklahoma reservoirs. American Fisheries Society Symposium 12:61–66.

Cummings, R. G., D. S. Brookshire, and W. D. Schulze, editors. 1986. Valuing environmental goods: an assessment of the contingent valuation method. Rowman and Allanheld, Totowa, New Jersey.

Daganzo, C. 1979. Multinomial probit: the theory and its application to demand forecasting. Academic Press, New York.

Davis, R. K. 1963. The value of outdoor recreation: an economic study of the Maine woods. Doctoral dissertation. Harvard University, Cambridge, Massachusetts.

Dawe, E. G., J. M. Hoenig, and X. Xu. 1993. Change-in- ratio and index removal methods for population assessment and their applications to snow crab (*Chionoecetes opilio*). Canadian Journal of Fisheries and Aquatic Science 50:1467–1476.

Dawson, C. D., T. L. Brown, and N. A. Connelly. 1991. The angler specialization concept applied: New York's Salmon River anglers. Pages 153–155 *in* Proceedings, northeastern recreation symposium. U.S. Forest Service General Technical Report NE-160, Radnor, Pennsylvania.

Dawson, C. P., and B. T. Wilkins. 1981. Motivations of New York and Virginia marine boat anglers and their preferences for potential fishing constraints. North American Journal of Fisheries Management 1:151–158.

Decker, D. J. 1985. Agency image: a key to successful natural resource management. Transactions of the Northeast Section of the Wildlife Society 41:43–56.

Decker, D. J., T. L. Brown, and R. J. Gutierrez. 1980. Further insights into the multiple-satisfactions approach to hunter management. Wildlife Society Bulletin 8(4):323–331.

DeLury, D. B. 1947. On the estimation of biological populations. Biometrics 3:145–167.

Deming, W. E. 1960. Sample design in business research. Wiley, New York.

Dent, R. J., Jr. 1986. Methods and parameters used in evaluating bass length limits on Pomme de Terre Lake, Missouri. Pages 65–72 *in* G.E. Hall and M.J. Van Den Avyle, editors. Reservoir fisheries management: strategies for the 80's. American Fisheries Society, Southern Division, Reservoir Committee, Bethesda, Maryland.

Dillman, D. A. 1978. Mail and telephone surveys: the total design method. Wiley, New York.

Ditton, R. B., D. K. Loomis, and S. Choi. 1992. Recreation specialization: re-conceptualization from social worlds perspective. Journal of Leisure Research 24(1):33–51.

Ditton, R. B., and T. J. Mertens. 1978. Characteristics, participation, and motivations of Texas charter boat fishermen. Marine Fisheries Review 40(8):8–13.

Ditton, R. B., and seven coauthors. 1990. Demographics, participation, attitudes, expenditures, and management preferences of Texas saltwater anglers, 1986. Texas Parks and Wildlife Department, Coastal Fisheries Branch, Management Data Series 18, Austin.

Dorfman, P. W. 1979. Measurement and meaning of recreation satisfaction: a case study in camping. Environment and Behavior 11(4):483–510.

Driver, B. L. 1977. Item pool for scales designed to quantify the psychological outcomes desired and expected from recreation participation. U.S. Forest Service, Rocky Mountain Forest and Range Experiment Station, Fort Collins, Colorado.

Driver, B. L., and R. C. Knopf. 1976. Temporary escape: one product of sport fisheries management. Fisheries (Bethesda) 1(2):21–29.

Driver, B. L., C. Phillips, E. P. Bergersen, and C. C. Harris. 1984. Using angler preference data in defining types of sport fisheries to manage. Transactions of the North American Wildlife and Natural Resources Conference 49:82–90.

Dupont, W. D. 1983. A stochastic catch-effort method for estimating animal abundance. Biometrics 39:1021–1033.

Dwyer, J. F., J. R. Kelly, and M. D. Bowes. 1977. Improved procedures for valuation of the contribution of recreation to national economic development. University of Illinois at Urbana–Champaign Water Resources Center Research Report 128.

Eagly, A. H., and S. Chaiken. 1993. The psychology of attitudes. Harcourt Brace Jovanovich College Publishers, New York.

Ebbers, M. A. 1987. Vital statistics of a largemouth bass population in Minnesota from electrofishing and angler-supplied data. North American Journal of Fisheries Management 2:249–256.

Edwards, A. L. 1957. Techniques of attitude scale construction. Appleton-Century-Crofts, Inc., New York.

Essig, R. J., and M. C. Holliday. 1991. Development of a recreational fishing survey: the marine recreational fishery statistics survey case study. American Fisheries Society Symposium 12:245–254.

Fabrizio, M. C., J. R. Ryckman, and R. N. Lockwood. 1991. Evaluation of sampling methodologies of the Lake Michigan creel survey. American Fisheries Society Symposium 12:162–176.

Falk, J. M., A. R. Graefe, and R. B. Ditton. 1989. Patterns of participation and motivation among saltwater tournament anglers. Fisheries (Bethesda) 10(4):10–17.

Fishbein, M., and I. Ajzen. 1975. Belief, attitude, intention, and behavior: an introduction to theory and research. Addison-Wesley, Reading, Massachusetts.

Fisher, A., G. H. McCelland, and W. D. Schulze. 1989. Measures of willingness to pay versus willingness to accept: evidence, explanations, and potential reconciliation. Pages 127–134 *in* G. L. Peterson, B. L. Driver, and R. Gregory, editors. Amenity resource valuation: integrating economics with other disciplines. Venture Publishing, State College, Pennsylvania.

Fleener, G. 1989. Recreational use survey of the Missouri River. Missouri Department of Conservation, Columbia.

Fox, W. W. 1975. Fitting the generalized stock production model by least squares and equilibrium approximation. U.S. National Marine Fisheries Service Fishery Bulletin 73:23–37.

Fraidenburg, M. E., and G. G. Bargmann. 1982. Estimating boat-based fishing effort in a marine recreational fishery. North American Journal of Fisheries Management 2:351–358.

Freeman, A. M. 1979. Approaches to measuring public goods demands. American Journal of Agricultural Economics 61:915–920.

Frey, J. H. 1983. Survey research by telephone. Sage Publications, Beverly Hills, California.

Fry, F. E. J. 1949. Statistics of a lake trout fishery. Biometrics 5:27–67.

Gems, B., D. Ghosh, and R. Hitlin. 1982. A recall experiment. Impact of time on recall of recreational fishing trips. Pages 372–375 *in* Proceedings of the section on survey research methods, American Statistical Association. American Statistical Association, Washington, D.C.

Goodstadt, M. S., L. Chung, R. Kronitz, and J. Cook. 1977. Mail survey response rates: their manipulation and impact. Journal of Marketing Research 14:391–395.

Graefe, A. R., and A. J. Fedler. 1986. Situational and subjective determinants of satisfaction in marine recreational fishing. Leisure Sciences 8(3):275–295.

Grambsch, A. E., and W. L. Fisher. 1991. Improving surveys: lessons from the national survey. American Fisheries Society Symposium 12:5–12.

Green, A. W., G. C. Matlock, and J. E. Weaver. 1983. A method for directly estimating the tag-reporting rate of anglers. Transactions of the American Fisheries Society 112:412–415.

Gregory, R., and R. C. Bishop. 1989. Willingness to pay and compensation demanded:

issues in applied resource valuation. Pages 135–141 *in* G. L. Peterson, B. L. Driver, and R. Gregory, editors. Amenity resource valuation: integrating economics with other disciplines. Venture Publishing, State College, Pennsylvania.

Groves, R. M., P. P. Biemer, L. E. Lyberg, J. T. Massey, W. L. Nicholls II, and J. Waksberg, editors. 1988. Telephone survey methodology. Wiley, New York.

Gunderson, D. R. 1993. Surveys of fisheries resources. Wiley, New York.

Guthrie, D., and seven coeditors. 1991. Creel and angler surveys in fisheries management. American Fisheries Society Symposium 12.

Hahn, J. 1991. Angler specialization: measurement of a key sociological concept and implications for fisheries management decisions. American Fisheries Society Symposium 12:1–4.

Hammarstrom, S. L. 1990. Angler-effort and harvest of chinook salmon and coho salmon by the recreational fisheries in the lower Kenai River, 1989. Alaska Department of Fish and Game, Sport Fish Division, Fishery Data Series 90-22, Anchorage.

Hammarstrom, S. L. 1991. Advantages of field data recorders in creel surveys of recreational fisheries. American Fisheries Society Symposium 12:232–235.

Hammerschmidt, P. C., and R. L. Benefield. 1986. Number and distribution of crab traps in Texas bays. Texas Parks and Wildlife Department, Coastal Fisheries Branch, Management Data Series 92, Austin.

Hanemann, W. M. 1984. Welfare evaluations in contingent evaluation experiments with discrete responses. American Journal of Agricultural Economics 66:332–341.

Hanushek, E. A., and J. E. Jackson. 1977. Statistical methods for social scientists. Academic Press, New York.

Harris, C. C., and E. P. Bergersen. 1985. Survey on demand for sport fisheries: problems and potentialities for its use in fishery management planning. North American Journal of Fisheries Management 5:400–410.

Haverland, P. 1990. Recreational use survey of the Lower Osage River. Missouri Department of Conservation, Federal Aid in Sport Fish Restoration, Project F-1-R-39, Study S37, Columbia.

Havitz, M. E., and F. Dimanche. 1990. Propositions for testing the involvement construct in recreational and tourism contexts. Leisure Sciences 12:179–191.

Haw, F., and R. M. Buckley. 1968. The ability of Washington anglers to identify some common marine fishes. California Fish and Game 54:43–48.

Hayne, D. W. 1991. The access point survey: procedures and comparison with the roving-clerk creel survey. American Fisheries Society Symposium 12:123–138.

Heineman, G. 1991. Use of machine-readable forms to record creel survey and biological data. American Fisheries Society Symposium 12:227–231.

Henny, C. J., and K. P. Burnham. 1976. A reward band study of mallards to estimate band reporting rates. Journal of Wildlife Management 40:1–14.

Hewings, G. J. D. 1985. Regional input-output analysis. Scientific Geography Series, volume 6. Sage Publications, Beverly Hills, California.

Hicks, C. E., L. C. Belusz, D. J. Witter, and P. S. Haverland. 1983. Application of angler attitudes and motives to management strategies at Missouri's trout parks. Fisheries (Bethesda) 8(5)2–7.

Hiett, R. L., and D. N. Ghosh. 1977. A recommended approach to the collection of marine recreational finfishing and shellfishing data on the Pacific coast. Final report to National Marine Fisheries Service, Washington, D.C.

Hilborn, R., and C. J. Walters. 1992. Quantitative fisheries stock assessment. Choice, dynamics and uncertainty. Chapman and Hall, London.

Hoenig, J. M. 1983. Empirical use of longevity data to estimate mortality rates. U.S. National Marine Fisheries Service Fishery Bulletin 82:898–903.

Hoenig, J. M., and C. M. Heywood. 1991. Use of model predictions as an auxiliary variable to reduce variance in a creel survey. American Fisheries Society Symposium 12:292–297.

Hoenig, J. M., F. B. Martin, and C. M. Heywood. 1989. Model-based sampling methods for effort and catch estimation. American Fisheries Society Symposium 6:181–189.

Hoenig, J. M., P. Pepin, and D. W. Lawling. 1990. Estimating relative survival rate for two groups of larval fishes from field data: do older larvae survive better than young? U.S. National Marine Fisheries Service Fishery Bulletin 88:485–491.

Hoenig, J. M., D. S. Robson, C. M. Jones, and K. H. Pollock. 1993. Scheduling counts in the instantaneous and progressive count methods for estimating sport fishing effort. North American Journal of Fisheries Management 13:723–736.

Hudgins, M. D., and S. P. Malvestuto. 1982. An evaluation of factors affecting creel clerk performance. Proceedings of the Annual Conference Southeastern Association of Fish and Wildlife Agencies 36:252–263.

Hurlbert, S. H. 1984. Pseudoreplication and the design of ecological field experiments. Ecological Monographs 54:187–211.

Jagielo, T. 1991. Synthesis of mark-recapture and fishery data to estimate open-population parameters. American Fisheries Society Symposium 12:492–506.

James, J. M., and R. Bolstein. 1990. The effect of monetary incentives and follow-up mailings on the response rate and response quality in mail surveys. Public Opinion Quarterly 54:346–361.

Jensen, A. L. 1973. Relation between simple dynamic pool and surplus production models for yield from a fishery. Journal of the Fisheries Research Board of Canada 30:998–1002.

Jensen, P. T. 1964. Landing estimates of California's marine recreational salmon fishery. California Fish and Game 50:48–52.

Johnson, D. M., and R. G. Walsh. 1987. Economic benefits and costs of the fish stocking program at Blue Mesa Reservoir, Colorado. Colorado Water Resources Research Institute Technical Report 49. Colorado State University, Ft. Collins.

Jones, C. M., and D. S. Robson. 1991. Improving precision in recreational angling surveys: the traditional access method versus the bus-route design. American Fisheries Society Symposium 12:177–188.

Jones, C. M., D. S. Robson, D. Otis, and S. Gloss. 1990. Use of a computer simulation model to determine the behavior of a new survey estimator of recreational angling. Transactions of the American Fisheries Society 119:41–54.

Jones, R. 1984. Assessing the effects of changes in exploitation pattern using length composition data (with notes on VPA and cohort analysis). FAO Fisheries Technical Paper 256.

Kalton, G. 1983. Introduction to survey sampling. Sage University Paper 35. Sage Publications, Beverly Hills, California.

Kasprzyk, D., G. Duncan, G. Kalton, and M. P. Singh, editors. 1989. Panel surveys. Wiley, New York.

Kay, D. L., T. L. Brown, and D. J. Allee. 1987. The economic benefits of the restoration of Atlantic salmon to New England Rivers. Cornell University Department of Natural Resources, Human Dimensions Research Unit Publication 87-6.

Kelker, G. H. 1940. Estimating deer populations by a differential hunting loss in the sexes. Proceedings Utah Academy of Sciences 17:6–69.

Kelker, G. H. 1944. Sex ratio equations and formulas for determining wildlife populations. Proceedings Utah Academy of Sciences 19:189–198.

Kish, L. 1965. Survey sampling. Wiley, New York.

Knetsch, J. L., and F. J. Cesario. 1976. Some problems in estimating the demand for outdoor recreation: comment. American Journal of Agricultural Economics 58:596–597.

Knetsch, J. L., and J. A. Sinden. 1984. Willingness to pay and compensation demanded: experimental evidence of an unexpected disparity in measures of value. Quarterly Journal of Economics August:508–521.

Krebs, C. J. 1989. Ecological methodology. Harper and Row, New York.

Labaw, P. J. 1980. Advanced questionnaire design. Abt Books, Cambridge, Massachusetts.

Lambou, V. W., and H. Stern, Jr. 1958. Creel census methods used on Clear Lake, Richland Parish, Louisiana. Proceedings of the Annual Conference Southeastern Association of Game and Fish Commissioners 12:169–175.

Larkin, P. A. 1977. An epitaph for the concept of maximum sustained yield. Transactions of the American Fisheries Society 106:1–11.

Larson, C. L., and D. B. Jester. 1991. Software for administering mail surveys. American Fisheries Society Symposium 12:262–270.

Larson, C. L., S. E. Moore, and D. C. Lee. 1986. Angling and electrofishing for removing nonnative rainbow trout from a stream in a national park. North American Journal of Fisheries Management 6:580–585.

Laurent, G., and J. N. Kapferer. 1985. Measuring consumer involvement profiles. Journal of Marketing Research 22:41–53.

Lebreton, J. D., K. P. Burnham, J. Clobert, and D. R. Anderson. 1992. Modelling survival and testing biological hypotheses using marked animals: a unified approach with case studies. Ecological Monographs 62:67–118.

Leistritz, F. L., and S. H. Murdock. 1981. The socioeconomic impact of resource development: methods for assessment. Westview Press, Boulder, Colorado.

Lepkowski, J. M. 1988. Telephone sampling methods in the United States. Pages 73–98 *in* Groves et al. (1988).

Leslie, P. H., and D. H. S. Davis. 1939. An attempt to determine the absolute number of rats on a given area. Journal of Animal Ecology 8:94–113.

Lester, N. P., M. M. Petzold, and W. I. Dunlop. 1991. Sample size determination in roving creel surveys. American Fisheries Society Symposium: 12:25–39.

Loomis, J. B. 1988. Contingent valuation using dichotomous choice models. Journal of Leisure Research 20:46–56.

Lowry, H. 1978. Preference survey of Oregon resident anglers. Oregon State University Survey Research Center, Corvallis.

Lucas, R. C. 1963. Bias in estimating recreationists' length of stay from sample interviews. Journal of Forestry 61:912–914.

MacKenzie, C. 1991. Comparison of northern pike catch and harvest rates estimated from uncompleted and completed fishing trips. American Fisheries Society Symposium 12:47–50.

Maehr, M. L., and L. A. Braskamp 1986. The motivation factor: a theory of personal investment. Heath, Lexington, Massachusetts.

Malvestuto, S. P. 1983. Sampling the recreational fishery. Pages 397–419 *in* L. A. Nielsen and D. L. Johnson, editors. Fisheries Techniques. American Fisheries Society, Bethesda, Maryland.

Malvestuto, S. P. 1991. The customization of recreational fishery surveys for management purposes in the United States. Pages 201–213 *in* I.G. Croux, editor. Catch and effort sampling strategies. Fishing News Books, Blackwell, London.

Malvestuto, S. P., W. D. Davies, and W. L. Shelton. 1978. An evaluation of the roving creel survey with nonuniform probability sampling. Transactions of the American Fisheries Society 107:255–262.

Malvestuto, S. P., W. D. Davies, and W. L. Shelton. 1979. Predicting the precision of creel survey estimates of fishing effort by use of climatic variables. Transactions of the American Fisheries Society 108:43–45.

Malvestuto, S. P., and E. K. Meredith. 1989. Assessment of the Niger River fishery in Niger (1983–85) with implications for management. Pages 533–544 *in* D. P. Dodge, editor. Proceedings of the international large river symposium. Canadian Special Publication of Fisheries and Aquatic Sciences 106.

Maranell, G. M., editor. 1974. Scaling: a sourcebook for behavioral scientists. Aldine, Chicago.

Massey, J. T. 1988. An overview of telephone coverage. Pages 3–8 *in* Groves et al. (1988).

Matlock, G. C. 1991. Use of surveys in decision making. American Fisheries Society Symposium 12:1–4.

Matlock, G. C., H. R. Osburn, R. K. Reichers, and R. B. Ditton. 1991. Comparison of response scales for measuring angler satisfaction. American Fisheries Society Symposium 12:1–4.

McCollum, D. W., A. H. Gilbert, and G. L. Peterson. 1990. The net economic value of day use cross country skiing in Vermont: a dichotomous choice contingent valuation approach. Journal of Leisure Research 22:341–352.

McNeish, J. D., and J. G. Trial. 1991. A cost-effective method for estimating angler effort from interval counts. American Fisheries Symposium 12:236–243.

Megrey, B. A. 1989. Review and comparison of age-structured stock assessment models from theoretical and applied points of view. American Fisheries Society Symposium 6:8–48.

Mendelsohn, R., and G. M. Brown, Jr. 1983. Revealed preference approaches to valuing outdoor recreation. Natural Resources Journal 23:607–618.

Menz, F. C., and D. P. Wilton. 1983. Alternative ways to measure recreation values by the travel cost method. American Journal of Agricultural Economics 65:332–336.

Miernyk, W. H. 1965. The elements of input-output analysis. Random House, New York.

Miller, R. E., and P. D. Blair. 1985. Input-output analysis: foundations and extensions. Prentice-Hall, Englewood Cliffs, New Jersey.

Miranda, L. E., and W. Frese. 1991. Can fishery scientists predict angler preferences? American Fisheries Society Symposium 12:375–379.

Mitchell, R. C., and R. T. Carson. 1981. An experiment in determining willingness to pay for national water quality improvements. Resources for the Future draft report to U.S. Environmental Protection Agency, Washington, D.C.

Mitchell, R. C., and R. T. Carson. 1989. Using surveys to value public goods: the contingent valuation method. Resources for the Future, Washington, D.C.

Moore, D. S., and G. P. McCabe. 1993. Introduction to the practice of statistics, 2nd edition. Freeman, New York.

Murdock, S. H., K. Backman, M. N. Hoque, and D. Ellis. 1990. The impacts of future change in population size and composition on recreational demand. Pages 147–195 *in* Proceedings, outdoor recreation trends symposium III. Illinois–Indiana Sea Grant Program, Champaign.

Murphy, G. I. 1952. An analysis of silver salmon counts at Benbow Dam, South Fork of Eel River, California. California Fish and Game 38:105–112.

Nelson, L. J. , D. R. Anderson, and K. P. Burnham. 1980. The effect of band loss on estimates of annual survival. Journal of Field Ornithology 51:30–38.

Neuhold, J. M., and K. H. Lu. 1957. Creel census methods. Utah State Department of Fish and Game Publication 8, Salt Lake City.

Nicholls, W. L., II. 1988. Computer-assisted telephone interviewing: a general introduction. Pages 337–385 *in* Groves et al. (1988).

Nichols, J. D., R. J. Blohm, R. E. Reynolds, R. E. Trost, J. E. Hines, and J. P. Bladen. 1991. Band reporting rates for mallards with reward bands of different dollar values. Journal of Wildlife Management 55:119–126.

Nichols, J. D., M. J. Conroy, D. R. Anderson, and K. P. Burnham. 1984. Compensatory mortality in waterfowl populations: a review of the evidence and implications for research and management. Transactions of the North American Wildlife and Natural Resources Conference 49:535–554.

Nichols, J. D., and J. E. Hines. 1983. The relationship between harvest rates and survival rates of mallards: a straightforward approach with randomly partitioned data sets. Journal of Wildlife Management 47:334–348.

Nichols, J. D., S. L. Stokes, J. E. Hines, and M. J. Conroy. 1982. Additional comments on the assumptions of homogeneous survival rates in modern bird banding estimation models. Journal of Wildlife Management 46:953–962.

Nielsen, L. A. 1976. The evolution of fisheries management philosophy. Marine Fisheries Review December:15–23.

Norusis, M. J. 1985. Stacking beers: cluster analysis. Pages 165–191 *in* M. J. Norusis. SPSSX: advanced statistics guide. McGraw-Hill, Chicago.

Nowell, C., M. A. Evans, and L. McDonald. 1988. Length-biased sampling in contingent value studies. Land Economics 64:367–371.

Nunnally, J. C. 1978. Psychometric theory. McGraw-Hill, New York.

Orbach, M. K. 1980. The human dimension. Pages 149–163 *in* R. T. Lackey and L. A. Nielsen, editors. Fisheries management. Wiley, New York.

Osburn, H. R., and M. F. Osborn. 1991. Increasing the efficiency of Texas saltwater creel surveys. American Fisheries Society Symposium 12:155–161.

Ott, L. 1988. An introduction to statistical methods and data analysis, 3rd edition. PWS-Kent, Boston.

Overton, W. S., and D. E. Davis. 1969. Estimating the numbers of animals in wildlife population. Pages 403–455 *in* R. H. Giles, Jr., editor. Wildlife management techniques, 3rd edition. The Wildlife Society, Washington, D.C.

Ozuna, T., and J. R. Stoll. 1991. The significance of data collection and econometric methods in estimating nonmarket resource values. American Fisheries Society Symposium 12:328–335.

Palsson, W. A. 1991. Using creel surveys to evaluate angler success in discrete fisheries. American Fisheries Society Symposium 12:139–154.

Paulik, G. J., and D. S. Robson. 1969. Statistical calculations for change-in-ratio estimators of population parameters. Journal of Wildlife Management 33:1–27.

Pauly, D. 1980. On the interrelationships between natural mortality, growth parameters and mean environmental temperature in 175 fish stocks. Journal du Counseil International pour l'Exploration de la Mer 39:175–192.

Payne, S. L. 1951. The art of asking questions. Princeton University Press, Princeton, New Jersey.

Peterson, G. L. 1974. Evaluating the quality of the wilderness environment: congruence between perception and aspiration. Environment and Behavior 6(2):169–194.

Peterson, G. L., B. L. Driver, and R. Gregory, editors. 1988. Amenity resource valuation: integrating economics with other disciplines. Venture Publishing, State College, Pennsylvania.

Pfeiffer, P. W. 1966. The results of a non-uniform probability creel survey on a small state-owned lake. Southeastern Association of Game and Fish Commissioners 20:409–412.

Phippen, K. W., and E. P. Bergersen. 1991. Accuracy of a roving creel survey's harvest estimate and evaluation of possible sources of bias. American Fisheries Society Symposium 12:51–60.

Pitcher, T. J., and P. J. B. Hart. 1982. Fisheries ecology. AVI Publishing, Westport, Connecticut.

Pollock, K. H., J. M. Hoenig, and C. M. Jones. 1991. Estimation of fishing and natural mortality when a tagging study is combined with a creel survey or port sampling. American Fisheries Society Symposium 12:423–434.

Pollock, K. H., and W. L. Kendall. 1987. Visibility bias in aerial surveys: a review of estimation procedures. Journal of Wildlife Management 51:502–510.

Pollock, K. H., R. A. Lancia, M. C. Conner, and B. L. Wood. 1985. A new change-in-ratio procedure robust to unequal catchability of types of animals. Biometrics 41:653–662.

Pollock, K. H., C. T. Moore, W. R. Davidson, F. E. Kellogg, and G. L. Doster. 1989. Survival rates of bobwhite quail based on band recovery analysis. Journal of Wildlife Management 53:1–6.

Pollock, K. H., J. D. Nichols, C. Brownie, and J. E. Hines. 1990. Statistical inference for capture-recapture experiments. Wildlife Monographs 107.

Pollock, K. H., and D. G. Raveling. 1982. Assumptions of modern band-recovery models with emphasis on heterogeneous survival rates. Journal of Wildlife Management 46:88–98.

Pollock, K. H., S. C. Turner, and C. A. Brown. 1993. Use of capture-recapture techniques to estimate population size and population totals when a complete frame is unavailable. Pages 200–209 *in* Proceedings of the international conference on establishment surveys. American Statistical Association, Washington, D.C.

Pope, J. G. 1972. An investigation of the accuracy of virtual population analysis using cohort analysis. International Commission on Northwestern Atlantic Fisheries Research Bulletin 9:65–74.

Porch, C. E., and W. W. Fox. 1991. Evaluating the potential effects of a daily bag limit from the observed frequency distribution of catch per fisher. American Fisheries Society Symposium 12:435–456.

Pride, W. M., and O. C. Ferrell. 1983. Marketing: basic concepts and decisions, 3rd edition. Houghton Mifflin, Boston.

Pringle, J. D. 1985. The human factor in fishery resource management. Canadian Journal of Fisheries and Aquatic Science 42:389–392.

Propst, D. P., compiler. 1985. Assessing the economic impacts of recreation and tourism. U.S. Forest Service, Southeastern Forest Experiment Station, Asheville, North Carolina.

Quertermus, C. J. 1991. Use of bass club tournament results to evaluate relative abundance and fishing quality. American Fisheries Society Symposium 12:515–519.

Randall, A., J. P. Hoehn, and D. Brookshire. 1983. Contingent valuation surveys for evaluating environmental assets. Natural Resources Journal 23:635–648.

Randall, A., B. Ives, and C. Eastman. 1974. Bidding games for valuation of aesthetic environmental improvements. Journal of Environmental Economics and Management 1:132–149.

Reed, J. F., and W. D. Davies. 1991. Population dynamics of black crappies and white crappies in Weiss Reservoir, Alabama: implications for the implementation of harvest restrictions. North American Journal of Fisheries Management 11:598–603.

Restrepo, V. R., J. M. Hoenig, J. E. Powers, J. W. Baird, and S. C. Turner. 1992. A simple simulation approach to risk and cost analysis with applications to swordfish and cod fisheries. U.S. National Marine Fisheries Service Fishery Bulletin 90:736–748.

Ricker, W. E. 1975. Computation and interpretation of biological statistics of fish populations. Fisheries Research Board of Canada Bulletin 191.

Riechers, R. K., G. C. Matlock, and R. B. Ditton. 1991. A duel-survey approach for estimating the economic aspects of fishing. American Fisheries Society Symposium 12:344–355.

Robson, D. S. 1960. An unbiased sampling and estimation procedure for creel censuses of fishermen. Biometrics 16:261–277.

Robson, D. S. 1961. On the statistical theory of a roving creel census. Biometrics 17:415–437.

Robson, D. S. 1991. The roving creel survey. American Fisheries Society Symposium 12:19–24.

Robson, D. S., and D. G. Chapman. 1961. Catch curves and mortality rates. Transactions of the American Fisheries Society 90:181–189.

Robson, D. S., and C. M. Jones. 1989. The theoretical basis of an access site angler survey design. Biometrics 45:83–98.

Roedel, Philip M., editor. 1975. Optimum sustainable yield as a concept in fisheries management. American Fisheries Society, Bethesda, Maryland.

Romesberg, H. C. 1984. Cluster analysis for researchers. Lifetime Learning Publications, Belmont, California.

Roth, K. J., and K. Delaney. 1989. Creel statistics for the Valdez Arm and Eshamy Bay sport fisheries of Prince William Sound, Alaska during 1988. Alaska Department of Fish and Game, Fishery Data Series 103. Juneau, Alaska.

Rossi, P. H., and H. E. Freeman. 1982. Evaluation: a systematic approach. Sage Publications, Beverly Hills, California.

Rowe, R. D., R. C. D'Arge, and D. S. Brookshire. 1980. An experiment on the economic value of visibility. Journal Environmental Economics and Management 7:1–19.

Rubin, G., and D. S. Robson. 1990. A single server queue with random arrivals and balking: confidence interval estimation. Queuing Systems 7:283–306.

Samuelson, P. A. 1954. Pure theory of public expenditure. Review of Economics and Statistics 36:387–389.

Santucci, V. J., and D. H. Wahl. 1991. Use of a creel census and electrofishing to assess centrachid populations. American Fisheries Society Symposium 12:481–491.

Schoolmaster, F. A. 1986. Bank- and float-angler perceptions of use levels on the Madison River, Montana. North American Journal of Fisheries Management 6:430–438.

Schulze, W. D., and eight coauthors. 1983. The economic benefits of preserving visibility in the national parklands of the southwest. Natural Resources Journal 23:149–173.

Seber, G. A. F. 1982. The estimation of animal abundance and related parameters, 2nd edition. Macmillan, New York.

Serns, S. L. 1986. Cohort analysis as an indication of walleye year-class strength in Escanaba Lake, Wisconsin, 1956–1974. Transactions of the American Fisheries Society 115:849–852.

Schaefer, M. B. 1954. Some aspects of the dynamics of populations important to the management of the commercial fisheries. Inter-American Tropical Tuna Commission Bulletin 1:26–56.

Shibutani, T. 1955. Reference groups as perspectives. American Journal of Sociology 60:562–569.

Siemer, W. F., T. L. Brown, and D. J. Decker. 1989. Fishing motivations and involvement of boating salmonid anglers on Lake Ontario. Cornell University Department of Natural Resources, HDRU Series 89-6. Ithaca, New York.

Sigler, W. F., and J. W. Sigler. 1990. Recreational fisheries: management, theory, and application. University of Nevada Press, Reno.

Skalski, J. R., and D. S. Robson. 1992. Techniques for wildlife investigations: design and analysis of capture data. Academic Press, New York.

Snedecor, G. W., and W. G. Cochran. 1980. Statistical methods. Iowa State University Press, Ames.

Sorg, C., and D. S. Brookshire. 1984. Valuing increments and decrements of wildlife resources—further evidence. Report to U.S. Forest Service, Rocky Mountain Forest and Range Experiment Station, Fort Collins, Colorado.

Sorg, C. F., J. B. Loomis, D. M. Donnelly, G. L. Peterson, and L. J. Nelson. 1985. Net economic value of cold and warm water fishing in Idaho. U.S. Forest Service Resource Bulletin RM-11.

Spector, P. E. 1992. Summated rating scale construction. Sage Publications, Beverly Hills, California.

Stephan, F., and P. J. McCarthy. 1958. Sampling opinions. Wiley, New York.

Strong, E. J. 1983. A note on the functional form of travel cost models with zones of unequal populations. Land Economics 59:342–349.

Stynes, D. J., and G. L. Peterson. 1984. A review of logit models with implications for modeling recreation choices. Journal of Leisure Research 16(4):295–310.

Sudman, S., and N. M. Bradburn. 1983. Asking questions: a practical guide to questionnaire construction. Jossey-Bass, Washington, D.C.

Swanson, C. S., and D. W. McCollum. 1991. Application of economics to recreational fisheries management: an overview. American Fisheries Society Symposium 12:299–315.

Sztramko, L. K. 1985. Effects of sanctuary on the smallmouth bass fishery of Long Point Bay, Lake Erie. North American Journal of Fisheries Management 5:233–241.

Sztramko, L. K., W. I. Dunlop, S. W. Powell, and R. G. Sutherland. 1991. Applications and benefits of an angler diary program on Lake Erie. American Fisheries Society Symposium 12:520–528.

Talhelm, D. R., J. E. Hanna, and P. Victor. 1987. Product travel cost approach: estimating acid rain damage to sportfishing in Ontario. Transactions of the American Fisheries Society 116:420–431.

Thayer, M. A. 1981. Contingent valuation techniques for assessing environmental impacts: further evidence. Journal of Environmental Economics and Management 8:27–44.

Thompson, C. J. 1991. Effects of the avidity bias on survey estimates of fishing effort and economic value. American Fisheries Society Symposium 12:356–366.

Thomson, C. T. 1991. Effects of the avidity bias on survey estimates of fishing effort and economic value. American Fisheries Society Symposium 12:356–366.

Thornberry, O. T., Jr., and J. T. Massey. 1988. Trends in United States telephone coverage across time and subgourps. Pages 25–49 *in* Groves et al. (1988).

Udevitz, M. S. 1989. Change-in-ratio methods for estimating the size of closed populations. Doctoral dissertation. North Carolina State University, Raleigh.

Udevitz, M. S., and K. H. Pollock. 1991. Change-in-ratio estimators for populations with more than two subclasses. Biometrics 47:1531–1546.

Unruh, D. R. 1979. Characteristics and types of participation in social worlds. Interaction 2:115–130.

U.S. Army Corps of Engineers (USACE). 1986. National economic development procedures model—recreation. Volume 1: Recreation use and benefit estimation techniques. USACE, IWR Report 86-R-4, Washington, D.C.

USEPA (U.S. Environmental Protection Agency). 1992. Consumption surveys for fish and shellfish: a review. Wiley, New York.

U.S. Fish and Wildlife Service (USFWS). 1988. 1985 National survey of fishing, hunting, and wildlife-associated recreation. USFWS, Washington, D.C.

Van Den Avyle, M. J. 1986. Measuring angler effort, success, and harvest. Pages 57–64 in G. E. Hall and M. J. Van Den Avyle, editors. Reservoir fisheries management: strategies for the 80's. American Fisheries Society, Southern Division, Reservoir Committee, Bethesda, Maryland.

Vetter, E. F. 1988. Estimation of natural mortality in fish stocks: a review. U.S. National Marine Fisheries Service Fishery Bulletin 86:26–43.

Voiland, M. P., and M. W. Duttweiler. 1984. Where's the humanity: a challenge and opportunity for the fisheries community. Fisheries (Bethesda) 9(4):10–12.

von Geldern, C. E., Jr. 1972. Angling quality at Folsom Lake, California, as determined by a roving creel census. California Fish and Game 58:75–79.

von Geldern, C. E., Jr., and P. K. Tomlinson. 1973. On the analysis of angler catch rate data from warmwater reservoirs. California Fish and Game 59:281–292.

Wade, D. L., C. M. Jones, D. S. Robson, and K. H. Pollock. 1991. Computer simulation techniques to access bias in the roving-creel-survey estimator. American Fisheries Society Symposium 12:40–46.

Waksberg, J. 1978. Sampling methods for random digit dialing. Journal of the American Statistical Association 73:40–46.

Walbert, M. S. 1984. Valuing policies which reduce environmental risk: an assessment of the contingent valuation method. Doctoral dissertation. University of New Mexico, Albuquerque.

Walsh, R. G. 1986. Recreation economic decisions: comparing benefits and costs. Venture Publishing Inc., State College, Pennsylvania.

Weithman, A. S. 1991. Telephone survey preferred in collecting angler data statewide. American Fisheries Society Symposium 12:271–280.

Weithman, A. S., and P. Haverland. 1991. Comparability of data collected by telephone and roving creel surveys. American Fisheries Society Symposium 12:67–73.

Westat, Inc. 1989. Investigation of possible recall/reference bias in national surveys of fishing, hunting and wildlife-associated recreation. Final report to the U.S. Fish and Wildlife Service, contract 14-16-009-87-008, Washington, D.C.

White, G. C. 1983. Numerical estimation of survival rates from band-recovery and biotelemetry data. Journal of Wildlife Management 47:716–728.

White, G. C., D. R. Anderson, K. P. Burnham, and D. L. Otis. 1982. Capture-recapture and removal methods for sampling closed populations. Los Alamos National Laboratory, LA 8787-NERP, Los Alamos, New Mexico.

Williams, J. S., G. C. Garman, D. M. Johnson, N. R. Westerberg, J. M. Baker, and J. A. Maker. 1986. Virginia trout anglers: results of a statewide survey. Virginia Commonwealth University Survey Research Laboratory, Richmond.

Winters, G. H. 1988. The development and utility of sequential population analysis in stock assessments. Pages 111–134 *in* D. Rivard, editor. Collected papers on stock assessment methods. Canadian Atlantic Fisheries Scientific Advisory Committee Research Document 88/61.

Wolter, K. M. 1985. Introduction to variance estimation. Springer-Verlag, New York.

Youngs, W. D. 1974. Estimation of the fraction of anglers returning tags. Transactions of the American Fisheries Society 103:616–618.

Youngs, W. D., and D. S. Robson. 1975. Estimating survival rates from tag returns: model tests and sample size denominations. Journal of the Fisheries Research Board of Canada 32:2365–2371.

Glossary

The terms in this glossary are defined in the context of recreational fishing, the focus of this book. Some terms may have slightly different (but analogous) meanings for commercial and research fishing.

Accuracy: Degree of conformity to a true value. An accurate estimator has a small mean squared error, implying little or no bias and small standard error. (Compare *Precision.*)

Age-cohort analysis: Method of forecasting future participation in an activity by measuring the proportions of each age-group that now participate and applying them to the expected future sizes of each age-group in the population.

Angler survey: General term for any survey of anglers by an off-site method (mail, telephone, door-to-door) or an on-site method (access, roving, aerial). (Compare *Creel survey.*)

Attitude: Disposition or feeling of a person toward some entity or object.

Avidity bias: Bias arising in on-site surveys when anglers are sampled in proportion to their fishing avidity (time spent fishing or frequency of fishing), not with equal probability.

Biased estimator: Estimator whose average value over many hypothetical repetitions of a study deviates from the true parameter value.

Catch per unit effort: Number or weight of fish caught per trip, per angler-hour, or per some other unit of fishing effort. This measure of catch rate or success rate also can be applied to harvest.

Catch: Number or weight of all fish caught, whether the fish are kept or released. Sometimes the term is also used (less precisely) to mean harvest. (Compare *Harvest.*)

Census: Sampling of every unit in the sampled population.

Complemented survey: Survey combining two or more contact methods (e.g., a telephone survey to estimate effort and an access survey to estimate catch rate).

Completed trip interview: Interview conducted as an angler leaves the water at the end of fishing. (Compare *Incomplete trip interview.*)

Consistent estimator: Estimator that gets closer and closer to the true parameter value as the size of the sample increases.

Consumer surplus: Difference between the amount consumers would be willing to pay for a good and the amount they actually do pay.

Contact method: Any method used to contact anglers for a survey (mail, telephone, door-to-door, access, roving, or aerial).

Contingent valuation: Method of estimating the net value of an unpriced good by establishing a hypothetical market for the good and eliciting the price respondents would be willing to pay for it.

Creel survey: On-site angler survey during which anglers' harvests are examined by the survey agent.

Demand curve: Relationship of the price charged for a unit good or service to the number of units a customer is willing to buy at that price.

Digit bias: See *Rounding bias*.

Directed fishing effort: Fishing effort directed at a particular species or group of species.

Economic impact: Extent to which a business, community, region, or other entity is changed economically by some event.

Effort: See *Fishing effort*.

Estimate: Realized value of an estimator calculated from a particular sample.

Estimator: Formula or sample statistic used to estimate a population parameter.

Expenditure multiplier: See *Sales multiplier*.

Fishing effort (fishing pressure): A measure of resource use by anglers. Typical units of effort are number of trips on the water, angler-hours, party-hours, and boat-hours.

Frame: See *Sampling frame*.

Harvest: Number or weight of the fish caught that are kept, not released. (Compare *Catch*.)

Incomplete trip interview: Interview conducted before an angler has finished fishing. (Compare *Completed trip interview*.)

Input-output analysis: Regional economic analysis that traces goods and services from their creation (or import) to their final consumption (or export), used to estimate sales and other multipliers.

Instantaneous count: Count of anglers or boats made quickly from an airplane, a vantage point (bridge, hilltop, etc.), a fast-moving boat, or an automobile. (Compare *Progressive count*.)

Instrument: See *Survey instrument*.

Length-of-stay bias: Bias arising in roving surveys when anglers are interviewed with probability proportional to the length of their fishing trip, not with equal probability.

Likert question: Attitude question in which a statement is posed and respondents indicate their agreement or disagreement with it along a 5-point (or larger) response scale.

Mean squared error: Average or expected value of the squared deviations of an estimator from its true parameter value. It combines variance and bias in one measure.

Multiplier: See *Sales multiplier*.

Net value: Amount of benefit received by an individual or group over and above the cost of obtaining it.

Nonresponse bias: Bias arising when people refuse or are unable to answer a survey question.

Panel survey: Any longitudinal survey (i.e., a survey in which people are interviewed repeatedly over time).

Parameter: Characteristic of the population under study.

Precision: Degree of variation. A precise estimator has a small standard error (or variance). (Compare *Accuracy*.)

Preference: Option chosen or most favored by a person.

Prestige bias: Bias arising when surveyed anglers exaggerate the number and size of the fish they caught.

Probability sampling: Sampling in which all possible samples have known probabilities of being drawn.

Progressive count: Count of anglers or boats made over time as a survey agent moves through a fishery area. Within each small subarea, the count may be instantaneous. (Compare *Instantaneous count*.)

Recall bias: Bias arising when anglers misremember past events or the time in which they occurred.

Response error: Error arising because of recall, prestige, or rounding bias, or because an angler lied, misinterpreted a question, misidentified a species, or measured fish incorrectly.

Rounding bias (digit bias): Bias arising because anglers round their catch or other data to numbers ending in 0 or 5.

Sales multiplier: Average number of times a dollar of expenditure is respent in a defined area before it leaves the area, added to the original dollar. A dollar that immediately leaves the area has a multiplier of 1.0; one that is respent twice in the area has a multiplier of 3.0.

Sample: Group of sampling units drawn from the sampled population.

Sampled population: Actual population from which information is collected. (Compare *Target population*.)

Sampling error: Error arising from improper sample selection, an incomplete sampling frame, duplications within the frame, avidity bias, or length-of-stay bias.

Sampling frame: Complete set or list of all sampling units.

Sampling unit: Basic unit of sampling (e.g., an angler or a particular combination of space and time).

Semantic differential question: Attitude question in which a topic is characterized with pairs of opposing adjectives (e.g., good, bad) and respondents indicate their choices along a 5-point (or larger) response scale.

Standard error: Square root of an estimator's variance.

Statistic: Characteristic of the sample drawn.

Stratified sampling: Independent sampling within two or more defined subgroups of a sampled population.

Summated rating scale: Scale along which summed numerical responses to a group of questions are arrayed to show the distribution of respondents' attitudes toward some larger issue, aspects of which were addressed by the specific questions.

Supply curve: Relationship of the price paid for a unit good or service to the number of units a provider is willing to supply at that price.

Survey error: General term embracing sampling, response, and nonresponse errors.

Survey instrument: Any questionnaire or form on which data are recorded during a survey.

Target population: Population about which information is desired. (Compare *Sampled population.*)

Two-stage sampling: Form of subsampling in which a primary sampling unit (PSU) is chosen first, and then a secondary sampling unit (SSU) is chosen from within the primary unit. In on-site surveys, the PSU usually is a day and the SSU is a part day or hour.

Travel cost method: Method of estimating a demand curve for a recreational amenity based on the relationship between use of the amenity and the cost of traveling to it.

Unbiased estimator: Estimator whose average (or expected) value over many hypothetical repetitions of a study is the true parameter value.

Variance: The average (or expected) value of the squared deviations of an estimator from its expected value.

Visibility bias: Bias arising in effort estimation when anglers or boats cannot be seen and counted during roving or aerial surveys.

Index