ABSTRACT

Title of Document:	STATED PREFERENCE METHODS AND MODELS: ANALYZING RECREATIONAL ANGLING IN NEW ENGLAND GROUNDFISHERIES
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Policy analysis of nonmarket goods requires accurate knowledge about the behavior of economic agents. This dissertation explores several facets of behavior models in recreational angling for three New England groundfish species.

Stated preference methods are used frequently for nonmarket applications because data are scarce, but survey design can affect the results of behavior models via changes in respondents' cognitive processes. Methodological biases due to task complexity, measured by survey length, number of alternatives, and the degree of information overlap are observed in discrete choice experiment questionnaires, evidenced by differences in estimated model parameters and error variances. Additionally, ignoring task complexity increases mean marginal willingness-to-pay estimates. Information processing and decision heuristics should be considered in survey design and accounted for in estimated models.

Empirical specifications for utility models of recreational angling are also explored because numerous variants are employed in analyzing stated preference data. Inclusion of responses from different survey subpopulations affect estimated utility function parameters and mean marginal willingness-to-pay values. Utility models that are nonlinear in catch are as statistically robust as their linear counterparts but allow for diminishing marginal utility in fish, which is more consistent with recreational angling behavior. Failure to account for sources of heterogeneity such as angler avidity, species familiarity, and demographic information affect behavioral interpretations considerably.

Recreational fisheries are commonly managed using bag (creel) and minimum size restrictions. Many surveys include regulations as attributes in choice experiments, but models of angler behavior should not contain regulatory variables explicitly because they rarely factor into angler participation decisions directly. Because catch is random, regulations affect angler decisions indirectly by changing the underlying distributions for keep and release. A framework for understanding the effect of regulations on angler behavior given the stochastic nature of catch is developed. Short-run and long-run fishery implications are evaluated using a bioeconomic simulation.

STATED PREFERENCE METHODS AND MODELS: ANALYZING RECREATIONAL ANGLING IN NEW ENGLAND GROUNDFISHERIES

by

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Dedication

To Joseph and Ming-Hsia, who shaped the past;

Chookeat, who drives the present;

and Sylvia, William, Galen, and the Baby Jedi who will define the future.

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Table of Contents

List of Tables

List of Figures

Chapter 1: Dissertation Overview and Background

Introduction

Understanding the behavior of economic agents and having accurate estimates of demand are important elements in constructing natural resource policy. These issues are more difficult to address in the context of nonmarket goods because behavior is often unobservable and researchers must resort to using stated preference approaches for analysis. The three analytical chapters in this dissertation explore survey design and model construction in natural resource and environmental economics.

The first analytical chapter examines the impact of task complexity on survey responses in choice experiment (CE) surveys, which are also known as discrete choice experiment (DCE), discrete choice analysis (DCA), stated choice (SC), and choice based conjoint (CBC) surveys. The method is flexible and suitable for many applications where existing data are either inadequate or nonexistent; however, there are concerns regarding the effect of survey design elements on outcomes. Sophisticated, off-the-shelf commercial packages enable economists to generate CEs easily, but arbitrarily choosing a survey administration design may result in bias from unwanted cognitive responses, with broader implications for policy analysis. Questionnaire design affects a respondent's perception of task complexity and can result in adverse behaviors that violate standard axioms of consumer behavior (completeness, reflexivity, transitivity, continuity, and monotonicity), thereby affecting quantitative outcomes via changes in estimated model parameters and variance dispersion. Though some sources of task complexity were investigated in other studies (Adamowicz, Louviere, & Swait, 1998; Arentze, Borgers, Timmermans, & DelMistro, 2003; Bradley & Daly, 1994; Brazell & Louviere, 1998;

Carlsson & Martinsson, 2001, 2008; Chung, Boyer, & Han, 2011; DeShazo & Fermo, 2002, 2004; Hensher, 2006a, 2006b; Hensher, Stopher, & Louviere, 2001; Johnson & Orme, 1996; Kits, Adamowicz, & Swait, 2009; Maddala, Phillips, & Johnson, 2003; Malhotra, 1982; Ryan & Bates, 2001; Ryan & San Miguel, 2000; Sælensminde, 1998; Stopher & Hensher, 2000), very few examine multiple complexity types in a single nonmarket mail application. Questionnaire length, number of alternatives, degree of information overlap, and overall task complexity effects on model parameters and valuation estimates are examined for the New England recreational groundfishery.

The second analytical chapter attempts to recover preferences for attributes of recreational angling behavior using stated preference data. The framework for estimating recreational fishing demand has changed over the years and many theoretical and empirical specifications are currently in use. Different opinions exist about the inclusion of regulatory attributes (Aas, Haider, & Hunt, 2000; Dorow, Beardmore, Haider, & Arlinghaus, 2010; Hicks, 2002; Oh & Ditton, 2004; Oh, Ditton, Gentner, & Reichers, 2005; Paulrud & Laitila, 2004; Roehl, Ditton, Holland, & Perdue, 1993) and angler heterogneity (Breffle & Morey, 2000; Johnston, Arlinghaus, & Dieckmann, 2010; Provencher & Bishop, 2004) in econometric models, and nested (Hicks, 2002; Kaoru, 1995; Milon, 1988; Morey, Waldman, Assane, & Shaw, 1995) versus non-nested (Oh et al., 2005) models. Such diversity is problematic when direct comparisons of results are required due to differences in assumptions and estimation techniques. The effect of model specification on estimated parameters and willingness-to-pay (WTP) are explored.

The third analytical chapter constructs a framework for understanding responses to changes in recreational fishery policy. Management analyses using stated preference choice data often ignore the stochastic nature of catching fish. Further, the impact of fish size distributions on angler behavior is not well known, even among revealed preference studies. Economic outcomes under different regulatory scenarios are evaluated using a simulation driven by preferences derived from the CE survey. The simulation incorporates biological factors, generating realistic assessments that align more closely with ecosystem-based management goals.

The remainder of this chapter describes the application and survey data.

Fishery Overview

Groundfishing in New England has been in practice for over 400 years and continues to define communities such as Gloucester and New Bedford, Massachusetts, economically and socially. Though the North Atlantic Ocean was once abundant with cod, haddock, pollock, redfish, flounders, and other bottom-dwelling fish, advances in technology, the development of new markets, and failure of the management system to adequately control fishing effort have depleted the resource (Northeast Fisheries Science Center, 2004). Thirteen of the nineteen groundfish stocks assessed in 2007 show such diminished numbers that the biomass will remain permanently reduced despite any rebuilding efforts (Northeast Fisheries Science Center, 2008a). Figure 1 shows the status of the groundfish stocks based on the ratio of 2007 harvest (F_{2007}) and biomass (B_{2007}) levels to the harvest and biomass levels that produce the maximum sustainable yield (F_{MSY}, B_{MSY}). The maximum sustainable yield (MSY) is the highest level of harvest that can be taken from a species' stock without affecting the population's ability to reproduce. Theoretically, species that are harvested at or below MSY will maintain the same population levels indefinitely, barring any environmental disasters.

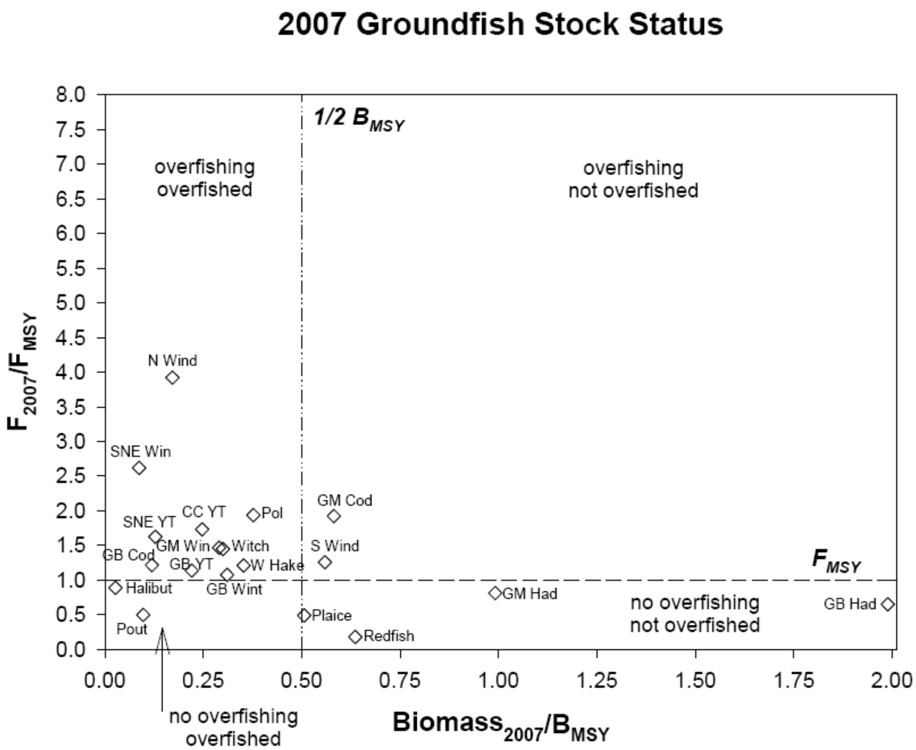

Figure 1. Status of 19 groundfish stocks in 2007 with respect to F_{MSY} and B_{MSY} or their proxies from the GARMIII review (Northeast Fisheries Science Center, 2008a).

Prior to the 1920s, cod was the primary species of interest for most commercial and recreational fishermen in the North Atlantic. The species was widely abundant between the Northeast shores of the US (north of New Jersey) and the banks of Newfoundland, and the flaky white flesh of this fish stood up well to the preservation methods of the time, mostly drying and curing the product with salt. The average size of the species was much larger in the past (currently ranges from 11 to 26 pounds), which made it an economical fish to harvest using baited lines and schooners. The fishery was so well known that many popular authors including Rudyard Kipling wrote stories based on the lives of these fishermen.

Haddock did not become popular until the Industrial Revolution because of the small size (three to five pounds on average) and difficulty in preserving the flesh using the methods available at the time. Steam-powered trawlers and technological advances in cold storage made fresh fillet distribution possible and the product became extremely popular as a lighter substitute for cod as evidenced by its status in many places as the staple ingredient in the popular dish “fish and chips.”

Atlantic pollock is traditionally popular only among certain ethnicities and poorer populations due to the strong flavor and a gray, veiny appearance when cooked. The appearance and fattiness of the fish make it most suitable for use in stews and chowders. Though it is considered a delicacy in Northern European countries, pollock is generally a weak substitute for cod in American markets; however, rapid declines in cod and haddock stocks have increased pollock fishing in recent years and it is often caught as bycatch in the bottom trawls and gillnets of commercial fishermen targeting the other two species. Commercially, Atlantic pollock comprises less than 2% of all pollock landed in the US and is mainly used in the manufacturing of fish sticks and fillet sandwiches.

Recreational anglers in New England on party boats, head boats, and charter boats target these species more than anglers in other modes because the fish prefer deep, cold water. This sector of the recreational fishery is considered a “meat fishery” because the anglers keep their catch to supplement their supper tables and are reluctant to discard any fish. The annual recreational harvest of these species is estimated to be between 7% and 13% of commercial landings based on landings values reported in *Fisheries of the United States 2008* (National Marine Fisheries Service, 2010). Cod appears to be the most popular recreational angling target, with haddock a close second. Some anglers consider pollock a nuisance fish that interferes with their ability to catch cod or haddock, but others enjoy the flavor and fishing experience. Though all three species respond to the same bait, pollock fishing is more flexible as these fish can be caught with still or trolling lines whereas cod and haddock respond mostly to still lines, a limiting factor on some for-hire trips. Pollock is also more accessible to near-shore anglers than the other species.

Rapidly advancing harvesting technology and steady consumer demand have continued to deplete stocks at an unsustainable rate. Cod is currently considered overfished overall with overfishing still occurring in the Georges Bank stock, and regulations will probably become stricter in the coming years, particularly for recreational fishermen (see Table 1 for current management levels). Haddock was recently elevated to not overfished with no overfishing. Pollock is overfished.

Table 1. *2010 Regulations by Species and State*

Species	**State**	**Minimum Size**	**Daily Bag Limit**	**Fishing Season**
Cod	GOM RMA†	24”	10/angler	Apr 1 – Oct 31
	Federal	22”	10/angler	Year Round
	CT	22”	10/angler	Year Round
	MA, Spring CCZ‡	N/A	No Keep	May 1 – Jun 30
	MA, Winter CCZ‡	N/A	No Keep	Dec 1 – Jan 31
	MA, N of Cape Cod	24”	10/angler	Apr 1 – Oct 31
	MA, S & E of Cape Cod	22”	2/angler or 75lbs./boat	Nov 1 – Mar 31
		22”	10/angler	Year Round
	ME	24”	10/angler	Apr 16 – Oct 31
		N/A	No Keep	Nov 1 – Apr 15
	NH	24”	10/angler	Apr 1 – Oct 31
	NJ	21”	None	Year Round
	NY	22”	10/angler	Year Round
	RI	22”	10/angler	Year Round
Haddock	Federal	18”	None	Year Round
	CT	19”	None	Year Round
	MA	18”	None	Year Round
	ME	18”	None	Year Round
	NH	18”	None	Year Round
	NJ	21”	None	Year Round
	NY	18”	None	Year Round
	RI	19”	None	Year Round
Pollock	Federal	19”	None	Year Round
	CT	19”	None	Year Round
	MA	None	None	Year Round
	ME	19”	6/angler/day under 19"	Year Round
	NH	None	None	Year Round
	NJ	19”	None	Year Round
	NY	19”	None	Year Round
	RI	19”	None	Year Round

† “GOM RMA” denotes the Gulf of Maine Restricted Management Area (Federal).
‡ “CCZ” denotes the Cod Conservation Zone.

New management amendments and recent policy proposals have caused a great deal of concern among recreational anglers and fishing communities because current policies are already considered fairly restrictive. Many fishermen allege that severe reductions in quota, changes to size and bag limits, and shorter fishing seasons for the recreational sector lead to significant losses for anglers. Additionally, fishing communities that rely on angling related expenditures such as bait, tackle, and chartered fishing trips worry that more stringent regulations have broader implications through impacts on local economies.

The Northeast Fisheries Science Center (NEFSC) in the National Marine Fisheries Service (NMFS) initiated the survey research underlying this dissertation to evaluate the economic consequences of altering current regulations. Though there is interest all New England groundfish species, cod and its substitutes are the primary species of interest in this study. The NMFS Office of Science and Technology provided funding for the data collection.

Survey Motivation

The NMFS has collected data on recreational angling since 1979 via dual-mode complementary phone and in-person interviews since the passage of the Magnuson Fishery Conservation and Management Act (16 USC §§ 1801-1882). Throughout the year, hundreds of trained field staff conduct several hundred thousand interviews at various sites along the US coastline, Hawaii, and Puerto Rico following a complex proportional random statistical sampling process stratified by fishing mode, geographic location, and time. Anglers are intercepted at boat ramps, marinas, beaches, piers, and other fishing access points to collect data about the species, length, weight, and number of fish caught, and other angler-specific fishing trip information. In 1994, this effort was expanded to include periodic collection of social and economic information to support characterization of recreational fisheries and fishermen for fisheries management. Intercepted anglers are occasionally asked to voluntarily participate in follow-up mail surveys to obtain more detailed information regarding specific topics of management interest. The data used in this dissertation were obtained from one such mail survey.

Survey Administration

The mail survey was distributed using information collected from the Marine Fisheries Recreational Statistics Survey (MRFSS) administered by NMFS. From March through December 2009, anglers intercepted at select fishing access sites from the coastal Northeastern states between Maine and New Jersey were asked to voluntarily participate in a follow-up mail survey. Recreational anglers intercepted in other states were not included in the sample population because the primary species of management interest, cod, does not typically inhabit waters south of New Jersey.

Sampling Strategy

The number of anglers included in the sample varied by month and state due to seasonal fluctuations in recreational fishing activity, cultural attitudes towards government surveys in certain states, and the NMFS intercept sampling strategy. The sample of intercepted anglers agreeing to the follow-up mail survey was stratified into two populations based on expected per trip expenditures. Sample A contained individuals perceived to have small average angling expenditures per trip, which were anglers intercepted in shore mode. Anglers in Sample A were randomly assigned a version of the Shore treatment only. Sample B included all individuals who were intercepted from private or rental boat mode, head boat or party boat mode, and charter boat mode, and any anglers that reported having purchased at least one charter trip in the previous 12 months. Sample B also included half of the shore mode anglers from New York and New Jersey. Though some Atlantic anglers switch fishing modes (Salz, Loomis, Ross, & Steinback, 2001), anecdotal evidence suggests this behavior is most likely to occur in wealthier coastal states, namely New York and New Jersey (personal communication, E. Zlokovitz, October 15, 2008). Therefore, a random sample of shore-mode anglers from these two states was included in Sample B. Anglers in Sample B were randomly assigned a version of the non-Shore treatments. See Chapter 2 for a more detailed explanation of the respondent samples and survey treatments.

Mailing Schedule

The address data was compiled on a monthly basis resulting in the mail survey being administered in a series of 10 waves. The intercept interview process prevented the surveys from being mailed immediately following intercept data collection, so addresses were collected over the course of each month and surveys mailed out in batches at the beginning of the following month. The original survey administration plan was a modified Dillman Tailored Design (Dillman, 2000). The entire administration process was to take no more than a month so that respondents could easily recall the connection between the mail survey and the intercept interview; however, some difficulties were encountered during the mailing process that prevented strict adherence to this timeline in all but one wave. Additionally, mailings scheduled to take place during the 2009 winter holidays (Christmas and New Year’s Day) were purposely delayed to avoid being lost amid holiday correspondence. Table 2 outlines the mailing schedule in detail and Table 3 lists the actual mailing dates. The inconsistencies in the mailing schedule may have affected response rates, but the extent of this problem is unknown.

Table 2. *Original Mailing Schedule*

Activity	Scheduled Time
Pre-contact: Brochure	At time of intercept interview
First Mailing	3 to 6 weeks after intercept interview
Postcard Reminder	2 weeks (14 days) after First Mailing
Second Mailing	4 weeks (28 days) after First Mailing

Table 3. *Actual Mailing Schedule*

Intercept Month	First Mailing	Postcard	Second Mailing	Explanation
March 2009	5/17/09	5/27/09	6/16/09	Contractor Delay
April 2009	5/17/09	5/27/09	6/16/09	Contractor Delay
May 2009	7/9/09	7/17/09	9/02/09	Contractor Delay
June 2009	8/14/09	8/21/09	10/07/09	Contractor Delay
July 2009	9/8/09	9/30/09	10/07/09	Contractor Delay
August 2009	10/26/09	11/02/09	12/04/09	Contractor Delay
September 2009	11/16/09	11/23/09	12/16/09	Contractor Delay
October 2009	1/15/10	1/22/10	2/22/10	Held for Holidays
November 2009	1/20/10	1/27/10	2/24/10	Held for Holidays
December 2009	3/05/10	3/16/10	4/6/10	Contractor Delay

Survey Instrument

The survey instrument has five components: a species information page, screener questions, the CE questions, and some demographic questions. The species page provided respondents with a picture of each of the species in the survey as well as some basic information about the species and current management. Because of difficulties in effectively pre-screening candidate respondents, it was necessary to include questions that assessed a respondent's familiarity with and avidity for the species in the survey. The demographic questions followed standard US Census groupings for income, age, ethnicity, and education. The instrument and cover letters were designed based on recommendations and critique from members of the NMFS Office of Science and Technology at NMFS Headquarters, staff at NMFS Science Centers, various state representatives, participants from focus groups held in New Hampshire and Massachusetts, and Dr. Rebecca Hamilton, Associate Professor of Marketing from the

Robert H. Smith School of Business at UMCP. Sample pages from the survey instrument and the pre-contact brochure can be found in Appendix A.

CE Alternatives

Every CE in all versions of the survey contained two or three alternatives plus an opt-out ("Do something other than saltwater fishing."). The number of alternatives included in the CE depended on the treatment group (see Chapter 2 for further explanation). An opt-out alternative was included in each CE based on strong suggestions from the literature (Banzhaf, Johnson, & Mathews, 2001; Batsell & Louviere, 1991; Freeman, 1991; Huber & Pinnell, 1994; Louviere, Hensher, & Swait, 2000; Olsen & Swait, 1997). Neglecting to provide respondents with an opt-out will limit the researcher's information on preferences as the respondents can only provide a choice conditional on choosing one of the alternatives present. Such forced choices are not reflective of most choice situations that individuals face and the researcher is unable to discern whether the individual would in fact choose any of the available alternatives. Opt-out alternatives give insight on participation and total demand, which are important for policy analysis.

CE Attributes

The attributes included in the CE were bag and size limits for each species, the number of legal-sized fish, the number of illegal-sized fish, the number of fish of the other species that could be legally kept, the trip length, and the trip cost. Separate categories for legal-sized versus illegal-sized fish were included to allow respondents to infer the current biological status of the stocks and the "catchability" of the species on any particular trip and to assess fishermen's preferences for trips with many discards versus trips with few discards.

Attribute levels were assigned to include historical and potential future values. The regulation levels chosen reflect management scenarios used in the past, and some potential future alternatives. The minimum size limits also account for the biological parameters of each species because the average species size made some lengths unreasonable. For example, haddock larger than 26" are rare, so a 26" minimum size is not realistic. The price and trip length vectors in the survey were constructed using an informal Internet and phone survey of nearly 100 party and charter boat companies from Maine to New Jersey conducted independently by the author. Trip packages were evaluated for amenities, the number of individuals included in the price, and the number of hours at sea to arrive at a representative range of trip costs per hour that approximated a observed total trip costs for one-day trips ($30 – $1,080). Feedback from focus group participants and interviews with fishermen at various docks verified the suitability of the chosen price and trip length vectors. Table 4 summarizes the attribute levels used in the survey.

Table 4. *Attributes and Levels Used in Choice Experiments*

Attribute	Level
Bag limits	2, 4, 8, 10
Size limits:	
Cod	18", 20", 22", 23", 24", 26"
Haddock	12", 16", 17", 19", 21", 22"
Pollock	17", 19", 20", 21", 23", 26"
Number of legal sized fish	1, 3, 6, 10
Number of undersized fish	1, 3, 6
Number of other fish	1, 3, 6, 10
Trip length (hours)	2, 4, 6, 8, 10, 12
Trip cost, shore mode only ($/trip)	$15, $35, $60, $90, $120, $150
Trip cost, all other modes:	
Hourly trip cost ($/hr.)	$15, $35, $60, $90
Total trip cost ($/trip=$/hr. x # hrs.)	$30-$1080

Experimental Design

The experimental design was constructed to optimize the statistical stability of the model results based on the maximum sample size allowed by the contract budget for the project and contractor printing specifications. All of the surveys were printed on sheets of 11"x17" paper folded in half, so the number of choice experiment questions asked in each survey needed to be in increments of four. The initial mailing sample size had to be reduced because the number of survey versions and the structure of the survey instruments (package weight) required to test the different task complexity treatments (see Chapter 2) increased administration costs significantly.

Additionally, response rates for the sample were difficult to estimate. The NMFS had not previously surveyed anglers about the three fish species in this survey using stated preference methods, and few economic studies had been conducted in the Northeast. Moreover, the results of other economic studies revealed state-specific idiosyncrasies in response type and response rates. The number of anglers agreeing to follow-up economic mail surveys from the intercept interviews ranged from 7% to 32% of the total MRFSS intercept sample in each state.

Consultations with experts familiar with the recreational fishery suggested that approximately 30% of the total saltwater angling population in the Northeast targets at least one of three groundfish species. Given that mail surveys usually have a 50% response rate, the original estimated response rate for this survey was 15% of the mail survey sample. The expected number of completed surveys after factoring in estimated levels of cooperation from MRFSS for each state was approximately 500. Candidate designs requiring sample sizes greater than the conservative estimate were eliminated from the list of possibilities. The experimental designs used in the survey were generated using SAS 9.2 Kuhfeld Macros and had the highest relative D-efficiency scores (≈ 73) of all candidate designs meeting the sample size requirements (see Appendix B for further explanation of experimental designs and efficiency scores).

<u>Survey Responses</u>

Response Rates

From March through December of 2009, 22,740 anglers were intercepted in the study's geographic area with 5,667 agreeing to participate in the follow-up survey, but only 4,579 surveys were mailed due to contractor error. The response rate for the mail survey was 37%, which was much higher than originally anticipated. The response rate relative to the total number of intercepted anglers was 7.5%. Tables 5 through 7 give detailed breakdowns of the return rates by month, geographic location, and mode. The total number of surveys mailed and received in Table 5 will not match the total number mailed and received in subsequent tables because the intercept data for two of the respondents were "lost" during the subcontractor's data cleaning process.

Table 5. *Response Rates by Month and Status*

Intercept Month	# Mailed	Non-Deliverable†	Refused‡	Completed after 1st Mailing	Total Completed	Completion Rate
March	57	3	0	14	19	33%
April	316	21	18	65	94	30%
May	612	37	31	155	229	37%
June	722	47	30	132	221	31%
July	803	40	33	106	249	31%
August	806	57	33	173	256	32%
September	629	33	22	147	222	35%
October	382	25	31	89	134	35%
November	219	30	22	32	62	28%
December	33	3	3	2	7	21%
Total	***4,579***	***296***	***223***	***915***	***1,493***	***33%***

† Non-deliverable responses are surveys mailed to invalid addresses.
‡ Refusals are respondents that returned blank surveys or called stating that they were not participating in the survey.

Table 6. *Response Rates by State and Residency*

Intercept State	Mailed	Resident Completed	Non-resident Completed	Total Completed	Completion Rate
Connecticut	34	10	3	13	38%
Maine	265	67	58	125	47%
Massachusetts	1238	272	168	440	36%
New Hampshire	536	124	66	190	35%
New Jersey	1421	310	124	434	31%
New York	725	157	7	164	23%
Rhode Island	358	48	77	125	35%
Total	***4,577***	***988***	***503***	***1491***	***33%***

Table 7. *Respondents by Fishing Mode*

Fishing Mode	No. Respondents	% Sample
Shore	288	19.4%
Head boat	515	34.5%
Charter boat	96	6.4%
Private/Rental boat	592	39.7%

Over 93% of the responses came from the anglers intercepted by MRFSS; other household members filled out the remaining surveys. Participation rates have historically varied across states due to cultural attitudes and species availability, resulting in uneven geographic representation. States where the survey species did not comprise a large portion of available fish tended to have lower response rates than the states where the fish were much more prevalent. Most of the non-residents in the survey came from the same states included in the intercept sampling. Only 2% of the completed surveys were from residents of the West Coast or Midwest regions of the United States, and 9% came from the South and Mid-Atlantic states south of New Jersey.

Respondent Characteristics

Demographic information for the respondents is listed in Table 8. The majority of the respondents are Caucasian males and 45 years or older. This sample had a higher incidence of persons 65 years old or older than in US Census estimates for the 2009 population (12.9%). The respondents in this survey are also better educated with higher income levels compared to the US median income of $52,029 and 24.4% with a bachelor’s degree or higher (US Census Bureau, 2010). The survey sample’s socioeconomic properties are consistent with other studies of recreational anglers.

Table 8. *Respondent Demographics*

Variable	Percentage
Age of respondent	
18-24 years old	3%
25-44 years old	24%
45-64 years old	56%
65 years old or older	16%
Refused/no answer	1%
Gender of respondent	
Male	94%
Female	5%
Refused/no answer	1%
Educational attainment	
Some high school	4%
High school graduate or GED completion	25%
Some college	19%
2-year degree or trade school graduate	14%
4-year degree	20%
Some graduate school	4%
Master’s degree	8%
Doctorate degree	5%
Refused/no answer	1%
Ethnic background	
Caucasian	91%
Black or African-American	3%
Hispanic or Latino	2%
Asian or Pacific Islander	1%
American Indian or Other (specify)	1%
Refused/no answer	2%
Household income (USD before taxes)	
Less than $20,000	4%
$20,000-39,999	10%
$40,000-59,999	16%
$60,000-79,999	16%
$80,000-99,999	14%
$100,000-149,999	20%
$150,000-199,999	8%
$200,000 and over	7%
Refused/no answer	5%

Chapter 2: Complexity and Survey Design

Introduction

Resource values are often obtainable through stated preference or hypothetical surveys only because other nonmarket valuation methods capture partial resource values or are unsuitable. Consideration of potential sources of bias is crucial for making appropriate policy decisions. Despite many advances in nonmarket valuation techniques, experimentally induced biases in valuation estimates may occur when evident variations WTP estimates are due to characteristics of the applied research method and not systematically to attributes of the environmental good or policy in question (Johnston, Ranson, Besedin, & Helm, 2006). In particular research has demonstrated the dependence of preferences on the framing of choice tasks and complexity of decisions (Tversky, 1996), and the implications of CE questionnaire structure must be considered.

Few nonmarket studies explicitly examine potential sources of methodological bias in valuation from using a choice-modeling framework. Additionally, most studies examine only one or two sources of task complexity using a given application. Because every application has unique qualities that may produce exogenous behavioral variations, combining results from different studies to derive conclusions about the relationship between survey responses and task complexity may not be advisable. Furthermore, the structures of some surveys in the literature make it difficult to parse out design effects and conclusions about the presence or absence of behavioral anomalies are not necessarily definitive (e.g., the two treatments in Kits et al., 2009, differ in both survey length and number of alternatives, yet conclude that the number of alternatives has no significant effect on survey responses).

Given that the perceived value of an environmental good is often "nebulous, complex, and ill-considered" (Shapansky, Adamowicz, & Boxall, 2002, p. 4), care must be taken in constructing such studies to ensure that results accurately reflect participant preferences, not decision heuristics based on task difficulty. Effective, well-designed surveys are especially critical in nonmarket studies considering that many target populations necessitate the use of mail surveys, which suffer from a lack of respondent monitoring, high printing and mailing costs compared to other survey modes (Kaplowitz, Hadlock, & Levine, 2004), high item nonresponse rates when compared to face-to-face interviews (Nicholaas, Thomson, & Lynn, 2000) and telephone interviews (de Leeuw & van der Zouwen, 1988; Harris, Weinberger, & Tierney, 1997), and the researcher's inability to modify survey instruments or design during the administration process.

This chapter addresses the effect of different CE designs on modeling outcomes and WTP estimates for nonmarket goods, and the degree of tradeoff associated with particular survey design choices in mail surveys. Specifically, this study examines the consequences of different forms of task complexity that can be easily changed within an experimental design, namely the number of questions, number of alternatives per CE, and degree of information overlap. By comparing response rates, response type, model parameters, variances, and WTP estimates between a controlled base survey and variants of the base incorporating different types of task complexity, adjustments in respondent behavior induced by different CE structures can be measured.

Discrete Choice Experiments and Experimental Design

The CE method asks individuals to choose between several alternatives (or profiles) depicting decompositions of the goods or policies in question. This exercise is usually repeated several times with different levels of each attribute. When specified correctly, CEs simulate actual choice decisions, allowing the researcher to use the choice selection probabilities for estimating taste parameters. Figure 2 depicts a sample CE.

Please compare Options A and B, then mark the option you like best.

Trip Attributes	Option A	Option B
NUMBER OF FISH CAUGHT	10	15
SIZE OF FISH CAUGHT	12 inch	9 inch
TRIP COST	\$20	\$15
I like this trip best:	TRIP A ☐	TRIP B ☐

Figure 2. Example of a CE.

The challenge in constructing a CE survey lies in finding an efficient experimental design (see Appendix B for further explanation of experimental designs) that addresses the fundamental questions of the research project, caters to the resource limitations of the study (e.g., budget or sample size), and reduces adverse respondent behaviors. Unlike data collected via other methods, the researcher using CEs has full control over the selection of vectors in each choice set; that is, the researcher is free to determine the number of levels (values) and range for each attribute selected so long as attribute levels adhere to study objectives and reality constraints.

Using preliminary preference models as guides, the researcher characterizes the decision problem for the respondent by defining the amount and type of information communicated via the attributes and alternatives presented in each choice set. CEs with many attributes and alternatives convey a lot of information, allowing the researcher to address many pertinent questions, but may exceed a respondent's cognitive capabilities resulting in nonsensical or non-utility-theoretic behaviors. Long surveys may affect response rates and result in respondent fatigue, but short surveys typically require more questionnaire versions and tend to increase administration costs and the potential for sampling issues (e.g., too few observations per survey version).

Task Complexity and Behavioral Outcomes

Though some studies have been conducted to test the statistical significance of changes in a survey's experimental design (e.g., the effect of different efficiency scores or search algorithms on parameter estimates), the behavioral implications are much more important. Whereas problems with statistical properties of a given design can be rectified using appropriate corrections in the final estimation model through the use of probability weights from the design matrix to eliminate design effects or increasing the sample size (see for example Lusk & Norwood, 2005), it is difficult to correct for respondent behavior. The source of the issue is rarely identifiable and may differ depending on the application or even a particular question within a survey.

Psychologists acknowledge that processing more than six pieces of information is difficult (Miller, 1956) and CEs often approach the limits of "how much information can be successfully evaluated before respondents quit, glaze over, or start to employ sub-optimal shortcut methods for making choices" (Orme, 1999). Respondents may ignore certain attributes, consider alternatives only if select attributes lie within certain ranges, reject alternatives on the basis of a single flaw, choose the first satisfactory alternative rather than the best alternative, or choose an alternative at random (Harris, 1998; Payne, Bettman, Coupey, & Johnson, 1992). Additionally, the respondent may not be consistent in his strategy. The respondent may not have well-formed preference structures *a priori* and may learn or adapt internal utilities to the survey while completing the survey. Or, the respondent may have an adaptive technique where different amounts of information are contemplated depending on the alternatives presented or the context of the question (Payne et al., 1992). Though any combination of *a priori* preference structures, behaviors, and outcomes is theoretically possible, certain preference structures and levels of task complexity predispose respondents towards particular behaviors. Respondents may feel more inclined to avoid decision-making or use simplification strategies with highly complex tasks, which may lead to anomalous responses and poorly estimated or biased results.

Coping strategies employed by individuals to deal with complex choice environments are inconsequential to stated preference studies only if they do not induce response biases and respondents continue to choose rationally; that is, respondents' preferences follow the axioms for completeness, transitivity, monotonicity, and continuity, and outcomes remain consistent through the entire survey regardless of the decision-making processes employed. Observed behaviors that violate any of these axioms invalidate the standard economic assumption of rational, utility-maximizing consumers that consider every attribute and alternative in each choice set required for using CEs and may have serious consequences for any valuation estimates generated using such data. Estimated model parameters may be confounded with design effects or other variables, may lose explanatory power, or have significantly larger error terms (De Palma, Meyers, & Papageorgiou, 1994; Heiner, 1983). It is also possible that WTP estimates are unaffected by changes in estimated model parameters, in which case task complexity is unimportant. Table 9 outlines respondent behavior and survey outcome scenarios.

Table 9. *Summary of Possible Respondent Behaviors and Outcomes*

Behavioral Antecedents	Initial Response Strategy	Secondary Response Strategy	Response Outcomes
Well-formed preferences with concrete values (external scale)	Consider all attributes and alternatives	Consistent with initial response strategy	*Observed behavior:* Consistent responses
Well-formed internal scale, anchor/adjust external values using first (several) CEs	Consider some attributes and all alternatives	Consistent with initial response strategy, but become fatigued and make errors	Inconsistent responses based on question order
Partially-formed preferences (may have some general internal scale)	Consider all attributes and some alternatives	Preferences become more refined with task repetition	Inconsistent responses based on context
Ill-formed preferences (no scale, has some small idea of preferences or is vaguely familiar with the survey subject)	Consider some attributes and some alternatives	Learn how to perform task and become more efficient (choices more closely reflect preference function)	Inconsistent responses, no clear pattern
No preferences *a priori*	Consider alternatives only if attributes fall in certain ranges	Change decision-making strategy based on fatigue	Lexicographic preference structure
	Select best alternative	Change decision-making strategy based on learning	Some “irrational” responses
	Select the first acceptable alternative	Change decision-making strategy based on context	All “irrational” responses
	Select alternative based on additional information inferred from attribute values or alternatives presented	Change decision-making strategy based on exogenous factors	Unreasonable proportion of certain types of responses (“status quo”/“opt-out” effect)
	Decide task too difficult, select first alternative	Change overall preferences	Other anomalies
	Decide task too difficult, select “opt-out” alternative	Change preferences regarding specific attributes	*Statistical ramifications:* Well-estimated utility function (no effect)
	Decide task too difficult, select alternative at random	Change preferences depending on context	Confounded parameter estimates
		Random	Some parameters statistically insignificant
		Any combination of the above	All parameters statistically insignificant
			Greater dispersion in estimated parameters, no effect on WTP means or dispersion
			Greater dispersion in estimated parameters, affects WTP means or dispersion
			Shifts in estimated parameters, no effect on WTP means or dispersion
			Shifts in estimated parameters, affects WTP means or dispersion

DeShazo and Fermo (2004) found that increasing cognitive costs increased choice inconsistency, and acknowledging the presence of rationally adaptive choice behaviors resulted in significantly higher WTP estimates for all attributes, some by more than 100%. Hensher and Rose (2005) and Hensher, Rose, and Bertoia (2007) also found that ignoring attribute processing strategies deflates the means and variances obtained for Value of Time Travel Savings (VTTS); however, Hensher (2006a) reported 18-62% inflation of mean VTTS from overlooking task complexity effects. Chung et al. (2011) also found significant inflation in mean WTP when ignoring task complexity. Though the direction of bias is unclear, task complexity does affect estimated values.

Recognizing anomalous outcomes can be difficult because the source of the issue is rarely identifiable and highly dependent on the particular application and survey design. Any assumptions about root causes are not likely to be global, but it is necessary to either account for "irrational" behaviors[1] in cases where it is predominant or design surveys to minimize these effects. Concerns about possible altered observed behaviors due to changes in a respondent's cognitive strategies have led to recommendations of using shorter, more concise CE tasks (Jedidi, Kohli, & DeSarbo, 1996; Malhotra, 1986); however, following these recommendations usually requires the researcher to compromise with either the statistical properties of the survey or the research questions that can be addressed in the study. The benefits of such compromises will be evaluated in the nonmarket valuation mail survey context.

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[1] Some anomalous behaviors are utility-theoretic. Individuals may exhibit lexicographic or intransitive preferences that are entirely rational, or read additional information into the attribute levels presented, such as the inference that higher-priced alternatives are of higher quality; however, responses that do not exhibit trading between attributes (non-compensatory behavior) cannot be adequately measured using the CE method.

Types of Task Complexity

Though there are many sources of task complexity, this study only addresses a few that are easily altered through changes in the CE design.

Survey Length

One difficulty in designing CE surveys occurs when selecting the number of CEs to ask each respondent, or the length of the survey. Sampling error is inversely related to the square root of sample size and is minimized by increasing the number of responses per respondent and the number of respondents. Reducing sampling error is usually accomplished by lengthening the survey instrument when sample size is a constraint; however, efficiency gains can only be realized if there are no subsequent cognitive reactions (Green & Srinivasan, 1990). The tradeoff between statistical precision and the number of choice tasks per respondent is not mathematically straightforward when cognitive burden is taken into consideration.

Survey Length Consideration: Fatigue Effect

Respondents may experience task overload and be either unwilling or unable to respond, resulting in inconsistent preferences over time. There are several different theories regarding this matter. One school of thought believes that exposing respondents to numerous sets of stimuli to evaluate incurs the risk of a fatigue effect or irrational behaviors resulting from weariness (Alriksson & Öberg 2008) that exacerbate response error and result in inconsistent model estimates (Greene, 2000). Behavioral psychology has shown that the quality and accuracy of a subject's response deteriorates toward the end of long experiments (Dong, 1983; Melles, Holling, & Reiners, 1998). Though early economic studies found no preference inconsistencies based on the number of choice situations (Bradley & Daly, 1994), more recent studies have shown that individuals do exhibit erratic behaviors, such as choosing dominated options or selecting the least preferred alternative (Ryan & Bates, 2001; Ryan & San Miguel, 2000). Maddala et al. (2003) examined preferences for HIV testing methods using intercept interviews in California. A fatigue effect, interpreted as statistically different parameter estimates, was noted when modeling the first six questions and the last six questions separately. Weariness may also be expressed simply as an unwillingness to respond. A meta-analysis by Johnson and Orme (1996) could not identify any parameter differences from surveys of varying lengths but the authors did observe more frequent usage of the "neither" alternative in later tasks, and individuals spent approximately 14% less time on questions where "neither" was the preferred alternative, which may be interpreted as an alternate symptom of survey fatigue.

Survey Length Consideration: Learning Effect

Another school of thought suggests the presence of learning effects based on psychological literature asserting that respondents learn over the course of repeated trials (Morrison, 2000). Sælensminde (1998) found inconsistent answers in the first CEs shown to respondents and postulated this phenomenon occurred because respondents are inexperienced in answering the exercise and spend time learning the task as opposed to having ill-formed preferences. Carlsson and Martinsson (2001) also suggested the presence of a learning effect. Responses were inconsistent among repeated questions when the questions occurred early in the survey; however, empirical analysis did not confirm the result. If learning effects exist, longer survey lengths are needed to compensate for possible increased randomness in response.

Survey Length Consideration: No Effect

Other studies insist that preferences are stable regardless of survey length. Louviere (2004) argued, “It is widely believed that ‘modeling’ individuals requires ‘smallish designs,’ but in contrast to the equivalent of widely held ‘academic urban myths’ in marketing and transport research, there is considerable evidence that humans will ‘do’ dozens (even hundreds) of [CEs].” Several studies conclude that longer surveys have negligible decreases in response rates, and differences in estimated parameters and error variances are not statistically significant (Arentze et al., 2003; Brazell & Louviere, 1998; Hensher et al., 2001; Stopher & Hensher, 2000). Carlsson and Martinsson (2008) argued that longer surveys are more efficient because the longest version of their survey resulted in 65% more completed CEs than the shortest version; however, the study did not use equal sample sizes, so this is theoretical and not actual gain. Additionally, the survey response rate for the longest version was 33% less than for the shortest version, so selection effects may be present, and calculations based on the published regression results reveal 3 to 47% differences in mean marginal WTP estimates for different length treatments. No clear direction of bias was detected.

Number of Alternatives

The number of alternatives (or profiles) in a choice set may increase the difficulty of the task and affect respondent behavior. Having more alternatives increases the statistical efficiency of a design (Zwerina, Huber, & Kuhfeld, 2005) but increases the amount of information a respondent must face simultaneously. Having more options increases the risk of choice overload and the possibility that respondents become paralyzed and unable to make decisions (Diehl & Poynor, 2007; Gourville & Soman, 2005; Iyengar & Lepper, 2000; Mogilner, Rudnick, & Iyengar, 2008).

Malhotra (1982) found that increasing the number of choice profiles only affects the dispersion of parameter estimates when the number of attributes is high; however, respondents reported higher degrees of information overload when forced to evaluate more attributes. Arentze et al. (2003) tested two and three alternative choice sets for work transport modes. They did not find any increase in error variance or parameter estimates with more alternatives, but did caution that their results could be due to transport mode-type dominance effects. Kits et al. (2009) examine the demand for carbon offsets using two different surveys. One version contained two alternatives and fifteen CEs, whereas the other had three alternatives and ten CEs. The coefficients for their complexity measures were only weakly significant or insignificant, and the authors conclude that there is no strong evidence that increasing the number of alternatives influences decisions; however, the analysis in this study is not robust because the authors fail to consider the unequal number of tasks between the two surveys. There may be implicit behavioral tradeoffs presently unaccounted for in the model.

Degree of Information Overlap

Another way of controlling the amount of information in each CE is with information overlap, which occurs when several alternatives in the same choice set have identical values for certain attributes. The chosen number of levels and value range for each attribute most frequently influences the degree of information overlap. Researchers using large numbers of levels and broad attribute ranges often construct experimental designs using the principle of minimal overlap because the greatest efficiency comes from comparing alternatives that differ across all attributes; however, the degree of information overlap may affect a respondent's perception of task complexity and attribute-processing strategy. Studies of decision quality and information load have shown that increases in the diversity of information presented have a detrimental effect on decision quality (Chewning & Harrell, 1990; Iselin, 1988), with lower mean accuracy and higher standard deviations in responses (Hwang & Lin, 1999).

Mazzotta and Opaluch (1995) suggested that individuals have difficulty making choices when more than three attributes vary between alternatives, and that individuals simplify their decision heuristic as decisions become more complex. Maddala et al. (2003) found that though the mean perceived difficulty score was statistically equivalent between surveys with differing levels of information overlap, attribute-level parameter estimates and model fit were unequal. Respondents in the group with higher degree of information overlap had higher price sensitivity. Additionally, mean WTP estimates were higher with narrower confidence intervals for the survey with less information overlap.

Summary of Previous Findings

Theory and empirical evidence from other applications provide little guidance regarding how much consideration should be given to task complexity when designing CE surveys. The results of other studies are inconclusive for almost all forms of task complexity. Because previous findings do not agree on the existence or the direction of bias from choice of CE design, predictions regarding the effect of task complexity on survey responses are not possible based on current information. This study adds to the quantitative knowledge on CE design available to survey researchers.

Methods

The CE method is based on Lancaster's approach to consumer theory (1966, 1971) and random utility theory. The basic assumptions are that the utility of a good consists of the utility of the attributes characterizing the good and the researcher is only able to observe a component of the consumer's utility function. Classically, the utility function is assumed to be a linear-in-parameters function of product attributes and net income,

$$V_{jn} = V\,(\mathrm{M}_n, \mathrm{p}_{jn}, \mathrm{x}_{jn}, \varepsilon_{jn}) = \beta x_{jn} + \lambda\,(\mathrm{M}_n - \mathrm{p}_{jn}) + \varepsilon_{jn}, \qquad (2.1)$$

where V_{jn} is the indirect utility of individual n for choosing alternative j, M_n is the respondent's annual household income, p_{jn} is the price of alternative j shown to individual n, x_{jn} is a vector of attribute levels, and the taste parameters or "part-worths" of the individual's utility function attributable to particular aspects of an environmental good or policy are represented by the vector β. Whether M_n represents a respondent's annual household income or individual income is immaterial here because all choices are based on the difference between price and income, so M_n drops out of the choice equation because it is constant across choices for an individual. Although it is possible for the marginal utility of income to vary across individuals, this model assumes that all respondents have the same marginal utility of income because prices are relatively small compared to income and income effects are negligible. The random component or error term ε_{jn} may include characteristics of the alternative omitted by the researcher, measurement errors, unobserved characteristics of the individual, or the choice context. Individual n is assumed to choose alternative j if the utility of that alternative exceeds the utility associated with any other alternative in the choice set *S*, which can be expressed as a probability:

$$\Pr(Y_{n|j} = 1) = \frac{\exp(V_{jn})}{\sum_{k\in S}\exp(V_{kn})} = \frac{\exp\left(\beta x_{jn}+\lambda(\mathrm{M}_n-\mathrm{p}_{jn})\right)}{\sum_{k\in S}\exp\left(\beta x_{kn}+\lambda(\mathrm{M}_n-\mathrm{p}_{kn})\right)}. \tag{2.2}$$

Depending on the assumptions made about the distribution of the random component, the parameter estimates can be derived from several different probabilistic choice models, including probit models, multinomial logit (MNL) models, conditional logit (CL) models, nested logit (NL) models, and mixed logit (MXL) models. Typically, MNL or CL model specifications are used with the assumption that the error terms are independent and identically distributed (i.i.d.) according to Gumbel's distribution. The i.i.d. assumption implies that all choice scenarios have independent irrelevant alternatives (IIA) and errors are uncorrelated across alternatives. IIA assumes that the ratio of probabilities of choosing an alternative remains constant regardless of the contents of other alternatives in the choice set.

To compute welfare measures for the random utility function, let

$$\tilde{V}(M_n, p, x) = \max_J\{V_{jn}\}$$

be the maximum random utility for an individual *n* facing *J* choice occasions. The expected compensating variation (CV) for a change in prices and attributes from (p_a, x_a) to (p_b, x_b)is defined as

$$\mathrm{E}[\tilde{V}(M_n, p_a, x_a)] = \mathrm{E}[\tilde{V}(M_n - CV, p_b, x_b)] \tag{2.3}$$

from Hanemann (1982), where the CV represents the expected maximum amount of money required to compensate individual *n* for a change to present conditions. For unit changes in *x*, holding price constant, the marginal WTP (MWTP) is simply

$$\mathrm{MWTP} = \frac{-\beta_x}{\lambda}. \tag{2.4}$$

Qualitative Measures: Response Rates and Type

Qualitative evaluation provides the most basic measure for evaluating reactions to survey complexity. Most studies regarding mail survey length and response rates show that longer surveys produce lower response rates (Adams & Gale, 1982; Burchell & Marsh, 1992; Dillman, Sinclair & Clark, 1993; Heberlein & Baumgartner, 1978), result in underrepresentation of respondents who place a high value on their time in the survey sample, and increased rates of item nonresponse (Anderson, Basilevsky, & Hum, 1983). Item nonresponse is particularly problematic for CE surveys because low variances in parameter estimates obtained through statistically efficient design are only realized when all items in the design structure are completed. Additionally, respondents who wish to avoid making difficult decisions may select the opt-out or "neither" alternative even if the option does not provide the highest utility level among all possible choices (Huber & Pinnell, 1994). Changes in decision heuristics, including increased random selection of alternatives, may alter observed response type ratios and parameter estimates.

Swait-Louviere Scale Parameter Test

One quantitative measure of treatment effects can be obtained from the Swait-Louviere scale parameter test (Swait & Louviere, 1993). Traditionally, the scale parameter test has been used for combining two different datasets generated from the same choice process, such as in stated preference-revealed preference (SP-RP) studies when one dataset is suspected to be "noisier" than another. The scale parameter test checks whether two different datasets share the same population parameters assuming that the specification of both MNL models is identical (Swait & Louviere, 1993).

For MNL models, the error terms ε_{jn} are assumed to be independently and identically distributed Gumbel variates. The generalized extreme value (GEV) distribution is characterized by both a location parameter and a scale factor, which is inversely proportional to the variance of the error term $\left(\mu = \pi/\sqrt{6\sigma_\varepsilon^2}\right)$. Explicitly rewriting Equation 2.1 to include the scale parameter μ gives:

$$V(M_n, p_{jn}, x_{jn}, \varepsilon_{jn}) = \beta x_{jn} + \lambda (M_n - p_{jn}) + \varepsilon_{jn}/\mu. \tag{2.5}$$

Because it is not possible to simultaneously identify β, λ, and μ, μ is either normalized to one or the researcher estimates the products $\mu\beta$ and $\mu\lambda$. Therefore, the estimated parameters from any dataset are confounded with the scale parameter specific to that individual dataset.

The scale parameter test determines whether differences in parameter estimates should be attributed to scale parameter differences:

$$H: \beta_1 = \beta_2 \text{ and } \mu_1 = \mu_2.$$

The Swait-Louviere procedure involves a variant of the two-stage Chow test. First, estimated parameters are tested to determine if differences are due entirely to a difference in scale factors (*i.e*. the sum of log-likelihoods for two different data sets differ significantly from the log-likelihood of a model estimated from pooled data with a parameter restriction):

$$H1: \beta_1 = \beta_2 = \beta.$$

Assume that $\mu_1 = \mu_2$. A grid search is conducted over some hypothesized region over two stacked datasets for a scalar value (ω):

$$W = \begin{bmatrix} data1 \\ \omega \cdot data2 \end{bmatrix}. \tag{2.6}$$

The scalar value (ω) can be interpreted as a variance scale parameter ratio. The optimal ω should optimize the log-likelihood of a multinomial logit model fitted to the pooled dataset *W*, which imposes that the vector of coefficients for the first dataset β_1 must be equal to the vector of coefficients for the second dataset β_2. Then, the test statistic (log-likelihood ratio) is calculated using the formula

$$LR1 = -2\left(logL[x_{1|2}] - (logL[x_1] + logL[x_2])\right), \tag{2.7}$$

where $logL[x_1]$ is the log-likelihood score for the model estimated using the first dataset, $logL[x_2]$ the log-likelihood score for the model using the second dataset, and $logL[x_{1|2}]$ is the log-likelihood score for the pooled dataset. The LR1 test statistic follows an asymptotic χ^2 distribution with R+1 degrees of freedom where R is the number of parameters specified in the MNL.

The second part of the Swait-Louviere procedure tests the hypothesis

H2: $\mu_1 = \mu_2 = \mu$.

Assume that $\beta_1 = \beta_2$. Estimate a model using a stacked dataset as before, except without the scalar value (ω). The log-likelihood from this estimation ($logL[x_{pool}]$) is then used in the test statistic

$$LR2 = -2\left(logL[x_{pool}] - logL[x_{1|2}]\right), \tag{2.8}$$

which also follows an asymptotic χ^2 distribution, but with only one degree of freedom.

Both hypotheses must not be rejected for the main hypothesis (H) to be accepted at a given confidence interval. If the first hypothesis (H1) is rejected, then the principal hypothesis (H) must also be rejected. If H1 is not rejected, then the scalar value (ω) measures the degree of heterogeneity between the error variances of the two datasets; otherwise, the scalar value (ω) is interpreted simply as the optimally scaling average multiplier of the second dataset that offsets the imposition of the estimated parameter vector β equality assumption (Swait & Louviere, 1993).

Scale Heterogeneity Model

Scale heterogeneity models, also known as heteroscedastic logit models, assume that all individuals have the same parameter vector β. Independence across choice sets is retained, but individuals are allowed to have different error variances or "noisiness" in their decisions (Breffle & Morey, 2000). This specification is equivalent to a homoscedastic model with parameter proportionality where the β's are scaled up or down depending on the individual or strata; however, it is more parsimonious to describe the data using a single scale parameter than a vector of parameters, one for each stratification.

Several other studies (Chung et al., 2011; DeShazo & Fermo, 2004; Swait & Adamowicz, 2001a) use scale heterogeneity models to quantify response inconsistencies due to task complexity. The scale parameter from Equation 2.5 can be represented as an exponential function (DeShazo & Fermo, 2002; Hole, 2007), which allows for nonlinearity in parameters and yet converges:

$$\mu_n\,(C_n) = \exp\,[C_n\,\gamma]. \tag{2. 9}$$

C_n represents a vector of m individual characteristics and the vector γ measures the degree of influence of C_n on the error variance. Heterogeneity due to task complexity is captured using scale factors that vary by treatment.

Changes in scale factors alter the steepness of the choice probability function (see Figure 3). As the scale factor increases, the probability (vertical axis) associated with the utility difference between alternatives (horizontal axis) rises more sharply. The utility values associated with different alternatives are more distinct for individuals with large scale factors because the observed factors (and corresponding parameter β) are large relative to the unobserved factors ε. As a result, choice probabilities are better defined, resulting in steep curves and easily discernible preferences. The random component dominates with small scale factors, resulting in less responsive choice probabilities and greater model variance.

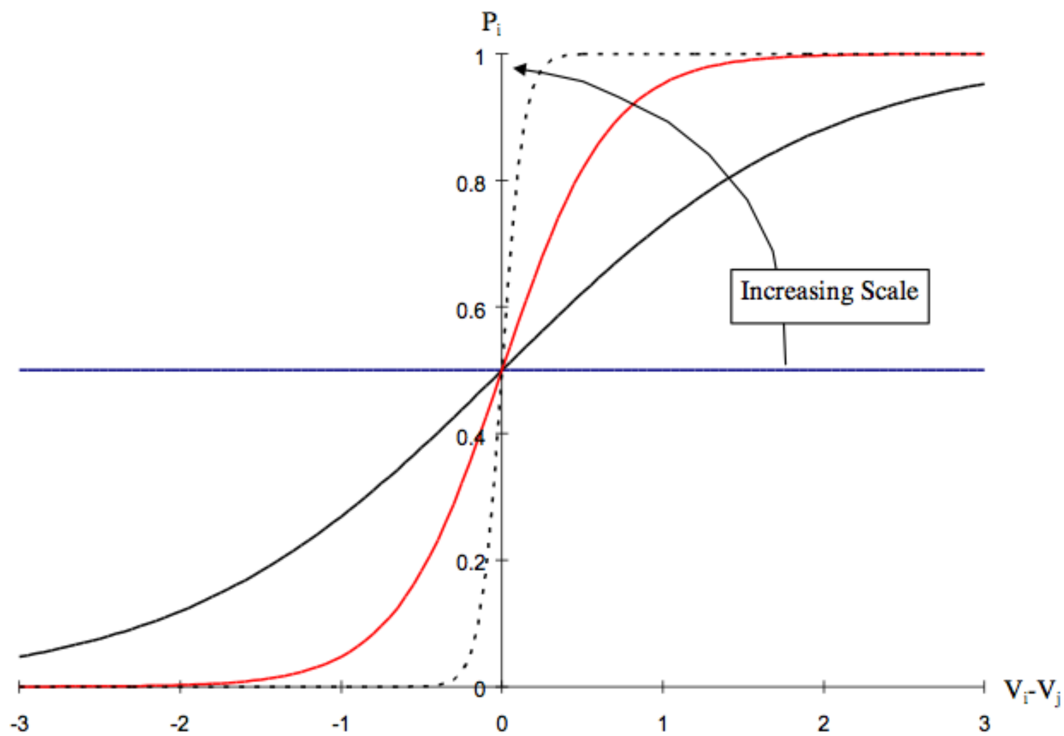


Figure 3. Scale parameter effect on choice probabilities (Adamowicz et al., 1998).

Scale heterogeneity models are also convenient for valuation studies because changes in the scale parameter reflect systematic differences without affecting marginal WTP measures. Using Equation 2.9, the MWTP is

$$mwtp = \frac{\partial U / \partial x}{\partial U / \partial p} = \frac{\frac{\partial}{\partial x}\left(\mu_n(C_n)\left(\beta x_{jn} + \lambda(M_n - p_{jn})\right) + \varepsilon_{jn}\right)}{\frac{\partial}{\partial p}\left(\mu_n(C_n)\left(\beta x_{jn} + \lambda(M_n - p_{jn})\right) + \varepsilon_{jn}\right)}$$
$$= -\frac{\mu_n(C_n)\beta}{\mu_n(C_n)\lambda} = -\frac{\beta}{\lambda}. \quad (2.10)$$

The scale heterogeneity model is identical to the scale parameter test for a treatment effect from a single treatment when C_n is specified as a dummy variable for some treatment because γ rescales the variance for the treatment effect dataset. If H1 is rejected, then γ should be statistically insignificant, and vice versa.

The scale heterogeneity model will be used in determining whether task complexity can be captured parametrically. The scale parameter test only analyzes one specific treatment case at a time, which does not allow for extrapolation to other possible study designs. The scale heterogeneity model allows for continuous specifications of C_n, which addresses the correlation between response and survey complexity measures more thoroughly. For the number of alternatives, γ can be specified as a linear trend parameter. Because there are multiple length treatments, the effect of survey length on scale parameter can be specified as either linear or quadratic.

Random Parameters Logit Models

Recent studies have also used random parameters logit (RPL) or MXL models to explain complexity (Boxall, Adamowicz, & Moon, 2009; Meyerhoff, 2006). In a random parameters model, preferences are allowed to vary randomly between subpopulation groups. This method relaxes IIA assumptions without imposing a specific structure on the heterogeneity by assuming that two additive parts comprise the model parameters β_n, a fixed, observable component and an unobserved (random) component:

$$\begin{aligned} V_{jn} &= \beta_n \begin{bmatrix} x_{jn} \\ M_n - p_{jn} \end{bmatrix} + \varepsilon_{jn} \\ &= b'_n \begin{bmatrix} x_{jn} \\ M_n - p_{jn} \end{bmatrix} + \eta'_n \begin{bmatrix} x_{jn} \\ M_n - p_{jn} \end{bmatrix} + \varepsilon_{jn}. \end{aligned} \tag{2.11}$$

The more general specification for the MXL includes a random component for the error term (random intercept)

$$V_{jn} = b'_n \begin{bmatrix} x_{jn} \\ M_n - p_{jn} \end{bmatrix} + \eta'_n \begin{bmatrix} 1 \\ x_{jn} \\ M_n - p_{jn} \end{bmatrix} + \varepsilon_{jn}. \tag{2.12}$$

The two parameter components can be alternatively interpreted as the population mean and individual deviation in tastes. The error terms ε_{jn} in this specification are still identically and independently distributed; however, the unobserved portion of the utility function now includes the η' term, which induces correlation over alternatives by assuming that respondents within a subpopulation evaluate all alternatives using the same tastes. By specifying the model in this way, all of the observed and unobserved attributes are represented.

Substituting Equation 2.11 into Equation 2.2 gives the RPL (MXL) probabilities

$$P_n^m(n) = \int_{\mathrm{B}} \frac{\exp\left[\beta\begin{bmatrix} x_{jn} \\ M_n - p_{jn} \end{bmatrix}\right]}{\sum_K \exp\left[\beta\begin{bmatrix} x_{kn} \\ M_n - p_{kn} \end{bmatrix}\right]} f(\beta|\varphi) d\beta, \tag{2.13}$$

where $f(\beta|\varphi)$ is the density function of β described by parameters φ (mean and variance). Because the RPL models violate standard i.i.d. assumptions for CLs, estimation of the parameters relies on assuming a distribution for $f(\beta|\varphi)$. The RPL models in this dissertation assume that the parameters η'_n are randomly and normally distributed for simplicity, but any distribution could be used.

The distribution of coefficient heterogeneity is addressed in both the RPL and the scale heterogeneity model. Whereas the scale heterogeneity model requires model parameters to vary based on specific proportionality factors, the RPL model is more liberal as the specification allows for more general variation in the parameters; however, the RPL requires the estimation of a larger number of parameters, which can lead to convergence issues.

Model Specification

The models estimated in this chapter will include only the most important trip attributes (determined empirically) and ignore species stratification for ease of model interpretation. Combining catch into generic groupings reduces the number of parameters in the model without significant loss of meaning. The general model used in empirical estimations is

$$\begin{aligned} V_{jn} = \beta_1 fishkept_{jn} + \beta_2 fishreleased_{jn} + \beta_3 triplength_{jn} \\ + \lambda\, Optout + \lambda\left(\mathrm{M}_n - \mathrm{p}_{jn}\right). \end{aligned} \tag{2.14}$$

Marginal WTP mean and confidence intervals are calculated using the Krinsky-Robb method (see Haab & McConnell, 2002).

<u>Data</u>

Variants of experimental design elements were tested using the survey instrument described in Chapter 1. To minimize interaction effects between different forms of task complexity, questions were identical in wording and layout across treatments; only elements of the CEs reflected differences between treatments. Additionally, the experimental designs chosen had approximately the same efficiency scores. This study focuses on questionnaire length and number of alternatives per CE because they are the easiest way to adjust required sample size and control printing costs (page length) in a mail survey and most common variants among studies in the literature

To ensure a standard basis of comparison, a Base survey with eight questions and two alternatives plus an opt-out option was considered the reference survey. The most common questionnaire lengths found in discrete choice studies were four, six, and eight discrete choice questions. Four questions seemed too few to accurately measure respondent preferences if psychological effects, such as learning, were present, and the literature recommended eight (see Carson et al., 1994). All treatments varied only one experimental design element from the base so that any differences attributed to a particular design choice could be accurately measured. Three different survey length variants were tested based on frequently observed numbers in the literature (12, 16, and 24 CEs) and one treatment varied the number of alternatives.

To account for potential differences from income and fishing mode, an additional survey treatment was created with one less attribute and a reduced price vector (Shore). The fishing trip length attribute was included in the Base survey and variations on the Base survey because most party and charter boat fees explicitly outline time as one of the determinants of trip cost; however, time is not determined exogenously for shore mode anglers. Shore mode is also a less expensive form of angling and thus has a smaller price vector with fewer levels than the other treatments. Fishing mode, the number of attributes, the smaller price range, and the number of price levels are all potentially confounded in the Shore treatment. The data from the Shore treatment will not be used in the task complexity analyses because distinguishing between the multiple treatment effects and the mode effect is impossible.

The experimental design variations allow for tests of questionnaire length and number of alternatives. The degree of information overlap, though not explicitly controlled for by any specific experiment, can also be tested. Changes in the number of attributes, attribute range, and attribute leveling cannot be tested due to confounding. The full sampling strategy was described in Chapter 1. Sample A respondents were assigned to the Shore treatment. Sample B respondents were randomly assigned to one of the other five treatments. Table 10 summarizes the treatments.

Table 10. *Summary of Survey Treatments*

Survey Version	Number of Questions	Number of Alternatives	Number of Attributes	Sample Assignment
Base	8	2 + Opt-out	8	Sample B
Length 1	12	2 + Opt-out	8	Sample B
Length 2	16	2 + Opt-out	8	Sample B
Length 3	24	2 + Opt-out	8	Sample B
3-Alternative	8	3 + Opt-out	8	Sample B
Shore	8	2 + Opt-out	7	Sample A

Results

Qualitative Measures: Respondent Characteristics

Differences in general population characteristics (race, age, gender, education, ethnicity, household income) were not detected between treatment groups.

Qualitative Measures: Response Rates

Z-tests on sample proportions were conducted to determine if task complexity affected response rates. The response rate for the Base survey is statistically different from the response rates of all of the length treatments at the 90% confidence level, indicating that the number of CE questions influences respondents' willingness to complete and mail back surveys. The length treatment response rates are not statistically different from each other; however, the response rate for Length 3 is significantly different from the 3-Alternative treatment, which is not statistically different from the Base. Table 11 summarizes this qualitative evidence.

Table 11. *Response Rates by Treatment*

Treatment	Mailed	Returned	Return Rate	Z-Score (Base)	Z-Score (Length 3)
Base	1,173	428	36%	—	3.823^{***}
Length 1	585	188	32%	1.748^{**}	1.204
Length 2	585	189	32%	1.677^{*}	1.132
Length 3	392	101	26%	3.823^{***}	—
3-Alternative	1,166	410	35%	0.628	3.364^{***}

$^{***}p < .01$, $^{**}p < .05$, $^{*}p < .10$ for two-tailed Z-test.

Qualitative Measures: Item Nonresponse Rates

Table 12 summarizes item nonresponse rates by treatment and question subgroup. Surprisingly, the average item nonresponse rate is not statistically different between treatments; however, this result is consistent with other mail survey studies showing that item nonresponse is independent of questionnaire length (Craig & McCann, 1978; Hing, Schappert, Burt, & Shimizu, 2005).

Table 12. *Item Nonresponse by Treatment and Question Order*

Treatment	Average non-response rate in Q1-Q8	Average non-response rate in Q9-Q12	Average non-response rate in Q13-Q16	Average non-response rate in Q17-Q24	Average non-response rate for all Qs
Base	20%	—	—	—	20%
Length 1	16%**	16%	—	—	16%**
Length 2	19.4%	20%	21%	—	20%
Length 3	17.6%	20.5%	19.5%	19.4%	19%
3-Alternative	22%	—	—	—	22%

***p < .01, **p < .05, *p < .10 for two-tailed Z-test compared to Base.

Qualitative Measures: Response Composition (Type of Response)

A comparison of choice distribution for different survey designs can also be revealing. Each length treatment was structured to include the base survey CEs. Inclusion of the base CEs in the 3-Alternative or Shore treatments was not possible for obvious reasons. Tables 13 through Table 17 give response breakdowns for a few questions. Though some variation is expected due to heterogeneity in respondent preferences, the proportion of opt-out responses were significantly higher in the length treatments than in the base in most cases. There also appear to be significant differences in the distribution of response type between treatment groups for each of the questions shown in the tables.

Table 13. *Question A Response Breakdown*

Treatment	No. Responses	Option A	Option B	Opt-Out	Nonresponse
Base	428	37%	38%	6%	19%
Length 1	188	19%***	40%	10%***	31%***
Length 2	189	39%	30%***	14%***	17%
Length 3	101	27%***	45%**	8%	20%

***p < .01, **p < .05, *p < .10 for two-tailed Z-test compared to Base.

Table 14. *Question B Response Breakdown*

Treatment	No. Responses	Option A	Option B	Opt-Out	Nonresponse
Base	428	39%	18%	11%	32%
Length 1	188	32%***	25%***	10%	32%
Length 2	189	42%	20%	15%**	23%***
Length 3	101	48%***	16%	18%***	18%***

$^{***}p < .01$, $^{**}p < .05$, $^{*}p < .10$ for two-tailed Z-test compared to Base.

Table 15. *Question C Response Breakdown*

Treatment	No. Responses	Option A	Option B	Opt-Out	Nonresponse
Base	428	10%	44%	25%	21%
Length 1	188	9%	41%	14%***	36%***
Length 2	189	12%	55%***	19%***	14%***
Length 3	101	14%**	49%*	18%***	19%

$^{***}p < .01$, $^{**}p < .05$, $^{*}p < .10$ for two-tailed Z-test compared to Base.

Table 16. *Question D Response Breakdown*

Treatment	No. Responses	Option A	Option B	Opt-Out	Nonresponse
Base	428	25%	35%	17%	23%
Length 1	188	9%***	41%**	24%***	26%
Length 2	189	14%***	41%**	22%**	23%
Length 3	101	8%***	48%***	26%***	18%**

$^{***}p < .01$, $^{**}p < .05$, $^{*}p < .10$ for two-tailed Z-test compared to Base.

Table 17. *Question E Response Breakdown*

Treatment	No. Responses	Option A	Option B	Opt-Out	Nonresponse
Base	428	6%	45%	18%	31%
Length 1	188	7%	43%	26%***	24%***
Length 2	189	12%***	32%***	23%**	33%
Length 3	101	4%	52%**	28%***	16%***

$^{***}p < .01$, $^{**}p < .05$, $^{*}p < .10$ for two-tailed Z-test compared to Base.

Qualitative Measures: Consistency—Ratings and Choices

The last qualitative measure tests consistency within responses by respondents. Each CE in this survey required the respondent to select the most preferred trip option, as is standard in most CE questionnaires, and then rate each alternative in the CE. Including the rating exercise allows for tests of rationality and consistency, as ratings should align with trip selections. Many different scales appear in previous studies, but this survey utilized the most common scale, 1 (dislike) to 10 (like) (Bigsby & Ozanne, 2002). Table 18 summarizes the percentage of inconsistent responses by treatment, which are responses where the highest rated alternative is not the chosen alternative, excluding nonresponses. The Length 3 treatment group had the highest percentage of inconsistent responses per respondent, which was significantly different from the base. Surprisingly, the Length 2 and 3-Alternative treatments had the fewest number of inconsistent responses, suggesting these respondents paid more attention to the choice tasks.

Table 18. *Percent Inconsistent Responses by Treatment*

Treatment	0-25% Responses Inconsistent	25-50% Responses Inconsistent	50-75% Responses Inconsistent	75-100% Responses Inconsistent
Base	76.3%	13.9%	3.9%	5.8%
Length 1	76.5%	13.4%	4.3%	5.9%
Length 2	78.2%	12.8%	3.2%	5.9%
Length 3	69.3%***	15.8%	5.9%	8.9%*
3-Alternative	78.7%	10.8%**	4.2%	6.4%

$^{***}p < .01$, $^{**}p < .05$, $^{*}p < .10$ for two-tailed Z-test compared to Base.

Swait-Louviere Scale Parameter Test

Parameter estimates for individual treatments are listed in Table 19. The coefficients for fish kept and trip length are positive and statistically significant as expected. The opt-out and trip cost are negative and significant. The number of fish released is not statistically significant, but it is an important part of the theoretical model. There are some noticeable differences in parameter estimates between the treatments and the control group, which is also reflected in calculated MWTP values. For the tables in this chapter, the number of observations is the total number of items (alternatives) and the number of groups indicates the number of clusters used in computing the standard errors (N), which equals the number of individuals. Models in this chapter cluster observations by individual to control for error correlation in responses unless noted otherwise. Differences in parameter estimates between non-clustered and clustered models are not statistically significant.

Table 19. *Estimation Results for Individual Treatments*

Variable	Base	Length 1	Length 2	Length 3	3-Alts.
# Fish kept	0.0556***	0.0389***	0.0422***	0.0473***	0.0652***
	(0.00603)	(0.00762)	(0.00651)	(0.00741)	(0.00558)
# Released	0.00257	0.0123	0.0145	0.0127	-0.0110
	(0.0108)	(0.0132)	(0.0113)	(0.0130)	(0.00689)
Trip length	0.0444***	0.0527***	0.0249***	0.0233*	0.0666***
	(0.00914)	(0.0129)	(0.0115)	(0.0129)	(0.00967)
Opt-out	-0.941***	-1.162***	-0.853***	-1.155***	-0.654***
	(0.123)	(0.145)	(0.124)	(0.142)	(0.120)
Trip cost	-0.00460***	-0.00569***	-0.00457***	-0.00556***	-0.00553***
	(0.000284)	(0.00342)	(0.00278)	(0.000325)	(0.000246)
LR (χ^2)	943.0***	710.3***	790.6***	961.0***	1,388.8***
Log-Likelihood	-2,829.57	-1,511.40	-2,108.42	-1,657.42	-2,518.92
No. Obs.	8,166	5,097	6,837	5,838	10,168
No. Groups	354	151	153	86	336
MWTP (keep)	$12.08	$6.83	$9.24	$8.50	$11.78
	(9.01 – 15.14)	(3.98 – 9.67)	(6.10 – 12.39)	(5.60 – 11.40)	(9.45 – 14.10)

$^{***}p < .01$, $^{**}p < .05$, $^{*}p < .10$.

Pooled models for the scale parameter test are summarized in Table 20, Table 21, and Table 22. The test statistics for H1 (LR1), which considers whether differences in parameter estimates are due to scale factors only, are listed in Table 20. *R* is 5 for this model and the degrees of freedom for LR1 are 6. Because LR1 is rejected for all of the length treatments versus base at the 99% confidence level, the hypothesis of equal parameter estimates is also rejected, indicating that the underlying choice models for the length treatments have different parameters than the base. Increasing survey length either alters respondent behavior or changes unobserved sample characteristics.

LR1 fails to be rejected for the 3-Alternatives treatment (Table 20), indicating that the relative scale factor (ω) is a measure of heterogeneity of the error variances between the base and the 3-Alternatives treatment. In this case, it appears that the two datasets are fairly homogeneous as ω is very close to one. LR2 (Table 22), which tests whether there are differences in parameter estimates assuming that scale parameters are the same, also fails to be rejected in this case. Thus, the hypothesis (H) is accepted, meaning that the number of alternatives does not have a statistically significant effect on response process.

The 3-Alternatives treatment was not compared with any of the length treatments because it has already been demonstrated that the length treatments are significantly different from the control treatment, which has the same underlying parameters and error variance as the 3-Alternatives treatment according to the scale parameter test.

Table 20. *Pooled Estimation Results (Base + ωTreatment) for Swait-Louviere H1*

Variable	Base + L1	Base + L2	Base + L3	Base + 3-Alts.
# Fish kept	0.0474***	0.0522***	0.0499***	0.0584***
	(0.00458)	(0.00475)	(0.00460)	(0.00396)
# Released	0.00589	0.00931	0.00825	-0.00666
	(0.00811)	(0.00840)	(0.00814)	(0.00552)
Trip length	0.0449***	0.0407***	0.0371***	0.0527***
	(0.00725)	(0.00757)	(0.00736)	(0.00641)
Opt-out	-1.036***	-1.004***	-1.054***	-0.828***
	(0.0915)	(0.0943)	(0.0916)	(0.0825)
Trip cost	-0.00497***	-0.00506***	-0.00506***	-0.00493***
	(0.000212)	(0.000213)	(0.000208)	(0.000176)
LR (χ^2)	1,642.31***	1,718.44***	1,884.71***	2,312.8***
Log-Likelihood	-4,035.81	-4,634.94	-4,185.97	-5,357.97
No. Obs.	13,263	15,003	14,004	18,334
No. Groups	505	507	440	690
MWTP (keep)	$9.55	$10.33	$9.85	$11.84
	(7.51 – 11.59)	(8.23 – 12.42)	(7.84 – 11.86)	(9.99 – 13.70)
Scale ratio (ω)	1.05	0.83	1.02	1.05
LR1 Test Statistic	610.32	606.10	602.04	-18.96

*** $p < .01$, ** $p < .05$, * $p < .10$.

Table 21. *Pooled Estimation Results (Treatment$_A$ + ωTreatment$_B$) for Swait-Louviere H1*

Variable	L1 + L2	L1 + L3	L2 + L3
# Fish kept	0.0449***	0.0433***	0.0408***
	(0.00548)	(0.00529)	(0.00448)
# Released	0.0139	0.0116	0.0125*
	(0.00951)	(0.0380)	(0.00781)
Trip length	0.0427***	0.0381***	0.0219***
	(0.00949)	(0.00913)	(0.00786)
Opt-out	-1.101***	-1.164***	-0.910***
	(0.104)	(0.101)	(0.0855)
Trip cost	-0.00565***	-0.00564***	-0.00460***
	(0.000240)	(0.000235)	(0.000194)
LR (χ^2)	1,497.26***	1,666.75***	1,751.07***
Log-Likelihood	-3,621.65	-3,171.07	-3,766.10
No. Obs.	11,934	10,935	12,675
No. Groups	304	237	239
MWTP (keep)	$7.94	$7.68	$8.86
	(6.85 – 10.04)	(5.66 – 9.69)	(6.73 – 10.99)
Scale ratio (ω)	0.82	1.00	1.20
LR1 Test Statistic	-3.66	-4.50	-0.52

*** $p < .01$, ** $p < .05$, * $p < .10$.

Table 22. *Pooled Estimation Results (Treatment$_A$ + Treatment$_B$) for Swait-Louviere H2*

Variable	Base + 3-Alts.	L1 + L2	L1 + L3	L2 + L3
# Fish kept	0.0602***	0.0404***	0.0433***	0.0446***
	(0.00407)	(0.00492)	(0.00529)	(0.00489)
# Released	-0.00641	0.0134	0.0116	0.0139*
	(0.00572)	(0.00855)	(0.00920)	(0.00851)
Trip length	0.0541***	0.0382***	0.0381***	0.0237***
	(0.00656)	(0.000857)	(0.00913)	(0.00859)
Opt-out	-0.849***	-0.971***	-1.164***	-0.987***
	(0.0845)	(0.0939)	(0.101)	(0.0932)
Trip cost	-0.00508***	-0.00502***	-0.00564***	-0.00501***
	(0.00181)	(0.000215)	(0.000235)	(0.000211)
LR (χ^2)	2,312.48***	1486.26***	1666.75***	1737.50***
Log-Likelihood	-5,358.14	-3,627.15	-3,171.07	-3,772.89
No. Obs.	18,334	11934	10,935	12,675
No. Groups	690	304	237	239
MWTP (keep)	\$11.84	\$8.04	\$7.68	\$8.89
	(9.99 – 13.69)	(5.92 – 10.16)	(5.66 – 9.69)	(6.76 – 11.04)
LR2 Test Statistic	-19.30	-14.66	-4.5	-14.1

$^{***}p < .01$, $^{**}p < .05$, $^{*}p < .10$.

Comparisons between length treatments were conducted to determine if treatment effects varied between treatments. LR1 (Table 21) and LR2 (Table 22) fail to be rejected for comparisons of the length treatments, meaning that H also fails to be rejected; therefore, the hypothesis of equal parameter estimates and equal scale parameters is accepted. The length treatment models are not significantly different from each other, only from the base.

Scale Heterogeneity Models

Table 23 lists the results for the scale heterogeneity models by treatment type. The 3-Alternatives case was only examined as a linear case as there was only one treatment for this type of complexity. The scale heterogeneity parameter for the number of alternatives is not statistically significant, which is not surprising because the scale parameter here replicates the Swait-Louviere scale parameter test and H for the 3-Alternatives case was not rejected. The length treatments are examined as both a linear (Model A) and quadratic (Model B) effect on error variance. In the linear specification, the scale parameter term for questionnaire length is not statistically significant; however, both the linear and quadratic terms are highly significant in the quadratic specification. The convexity of the scale parameter indicates that the error variance initially increases, plateaus, then decreases in response to increases in survey length. The parameters are significantly different from the homogeneous base model in Table 19 (LR (χ^2) = 15,648.05), and the MWTP estimates for the quadratic length specification are much lower than the MWTP estimates for the base. The confidence intervals just barely overlap. Contrary to findings in other studies (Bradley & Daly, 1994; Brazell & Louviere, 1998; Carlsson & Martinsson, 2008; Hensher et al., 2001; Louviere, 2004), preferences do change depending on survey length.

Table 23. *Scale Heterogeneity Models: Single-Complexity Type*

Variable	**Base + 3-Alts.**	**Base + Length (Model A)**	**Base + Length (Model B)**
# Fish kept	0.059***	0.0453***	0.0727***
	(0.00435)	(0.00400)	(0.0123)
# Released	-0.00658	0.00974*	0.0170*
	(0.00560)	(0.00591)	(0.00991)
Trip length	0.0532***	0.0367***	0.0597***
	(0.00660)	(0.00589)	(0.0129)
Opt-out	-0.836***	-1.020***	-1.635***
	(0.0857)	(0.0830)	(0.270)
Trip cost	-0.00498***	-0.00502***	-0.00808***
	(0.000229)	(0.000293)	(0.00126)
γ (#alternatives)	0.0324	—	—
	(0.0473)		
γ (#questions)	—	0.000396	-0.0680***
		(0.00331)	(0.0215)
γ ((#questions)^2)	—	—	0.00212***
			(0.000658)
LR (χ^2)	0.47	0.01	10.53***
No. Obs.	18,334	25,938	25,938
No. Groups†	5,264	8,646	8,646
MWTP (keep)	\$11.84	\$9.02	\$8.99
	(9.99 – 13.70)	(7.57 – 10.48)	(7.54 – 10.44)

$^{***}p < .01$, $^{**}p < .05$, $^{*}p < .10$.

† No. Groups indicates the number of unique observations for the scale parameter variable.

Learning and fatigue effects are best examined in terms of question order, but this effect is difficult to measure explicitly in mail surveys. Respondents are not constrained to answering questions in a particular order and can easily change the answers for previously completed questions at any time. For example, Carlsson, Frykblom, and Liljenstolpe (2003) found no question order effects in study of wetland valuation in Sweden despite administering half of the surveys in reverse question order. The inability to parse out any question order effects is typical of mail surveys but does not conclusively indicate the absence of order effects. If learning effects are present, the impact of inconsistent responses early in the survey (Carlsson & Martinsson, 2001; Sælensminde, 1998) should result in higher variance in short surveys compared to long surveys. Fatigue effects occur at the end of long surveys (Maddala et al., 2003; Ryan & Bates, 2001; Ryan & San Miguel, 2000) and are correlated with survey length, so longer surveys should exhibit higher variances than short surveys. The shape of the scale parameter in response to questionnaire length does not support either theory as middle-length surveys have the highest error variances.

To explore this further, a scale heterogeneity model (Table 24) was estimated including a question order term (question number in the survey). The parameters are not statistically different from the quadratic model in Table 23 but are significantly different from the homogeneous base model in Table 19 (LR (χ^2) = 15,647.21). After controlling for questionnaire length effects, there is a slight increase in error variance with questions near the end of the survey, which can be construed as a fatigue effect. Though statistically significant, this effect is dwarfed by the questionnaire length effect.

Table 24. *Scale Heterogeneity Models: Testing for Learning and Fatigue Effects*

Variable	Base + Length (C)
# Fish kept	0.0709***
	(0.0120)
# Released	0.0158*
	(0.00966)
Trip length	0.0598***
	(0.0128)
Opt-out	-1.633***
	(0.269)
Trip cost	-0.00799***
	(0.00125)
γ (question order)	-0.00805*
	(0.00424)
γ (#questions)	-0.0623***
	(0.0216)
γ ((#questions)^2)	0.00209***
	(0.000658)
LR (χ^2)	14.10***
No. Obs.	25,938
No. Groups†	8,646
MWTP (keep)	$8.87
	(7.44 – 10.31)

*** p < .01, ** p < .05, * p < .10.

† No. Groups indicates the number of unique observations for the scale parameter variables.

Random Parameters Logit Models

Due to convergence issues, the RPL models estimated only specify random parameters for the number of fish kept. The first set of results in Table 25 is from a dataset containing the control group responses and the 3-Alternative group responses. The parameter estimates differ from the scale heterogeneity model and the pooled model without a consistent direction. The random parameter on fish kept is positive, indicating that responses from the 3-Alternatives group weighted the number of fish kept more heavily than the base group. Surprisingly, the results for the length treatments were not as conclusive. The random parameter in this case indicates that respondents with longer surveys have a greater dispersion in their utility weight for number of fish kept but no shift in the mean. The pooled treatment model includes data from all of the design treatments. In the pooled treatment model, the random parameter implies that any survey design treatment results in a higher mean value and greater dispersion in taste parameters compared to the base. These results suggest that there is a treatment effect caused by the 3-Alternatives scenario, but it is not captured using the parametric specifications used in the previous section unlike the treatment effects for the different length scenarios.

Table 25. *RPL Models with Randomness in Fish Kept*

		Base + 3-Alts.		**Base + Length (All L Treatments)**		**Base + All Treatments**	
Variable		*Coef.*	*Std. Err.*	*Coef.*	*Std. Err.*	*Coef.*	*Std. Err.*
# Fish kept	Mean	0.0704***	0.0136	0.0540***	0.00348	0.0591***	0.00976
($\eta_{treatment}$)	*S. D.*	*0.0182****	*0.00924*	*0*	*0.00145*	*0.0161****	*0.00667*
# Released	Mean	-0.0161**	0.00632	0.0152***	0.00589	0.00381	0.00472
Trip length	Mean	0.0738***	0.00690	0.0399***	0.00573	0.0502***	0.00490
Opt-out	Mean	-0.985***	0.0875	-1.271***	0.0705	-1.144***	0.0598
Trip cost	Mean	-0.00545***	0.000182	-0.00521***	0.000143	-0.00547***	0.000124
Wald (χ^2)		1,051.57***		3,536.25***		2,390.44***	
No. Obs.		18,334		25,938		36,106	
MWTP		$12.91		$10.36		$10.80	
		(7.93 – 17.89)		(8.89 – 11.83)		(7.25 – 14.34)	

$^{***}p < .01$, $^{**}p < .05$, $^{*}p < .10$.

Degree of Information Overlap

Because this study did not explicitly control for the degree of information overlap, the Swait-Louviere method could not be used as the data could not be subset; however, the degree of information overlap directly affects perception of choice difficulty and is important in survey design considerations. Therefore, analyses of the degree of information overlap will be conducted using only the control sample.

Previous studies have used variables such as the percentage of attributes with unequal levels (Maddala et al., 2003) and entropy measures (Danthurebandara, Yu, & Vandebroek, 2011; Swait & Adamowicz, 2001a, 2001b) to represent the degree of information overlap. Here, the degree of information overlap (or information diversity) is defined as the distance or Euclidean mean between attribute vectors in the CE. Smaller values indicate that the information between vectors is very similar, whereas large values indicate very different vector values. Though this is a departure from methods used in the literature, the Euclidean mean is a simple but apt description that captures the degree of difference between attribute levels and the number of attributes with unequal levels.

The test results for degree of information overlap are shown in Table 26. The scale parameter in the scale heterogeneity model is not statistically significant. The random parameters in the RPL model have a positive and significant standard deviation from the estimated mean coefficient, which indicates decreases in error variance with increases in degree of information overlap. The magnitude of all parameters in the RPL model are greater than those in the homogeneous model, but differences in MWTP confidence intervals are not significant. As with the 3-Alternatives treatment, the degree of information overlap is best captured non-parametrically.

Table 26. *Regression Results for the Degree of Information Overlap (Base Only)*

Variable	Base Homogeneous	Base Scaled		Base-RPL *Coef.*	Base-RPL *Std. Error*
# Fish kept	0.0556***	0.0533***	Mean	0.0969***	0.0130
	(0.00603)	(0.00656)	*S. D.*	*0.0555****	*0.0080*
# Released	0.00257	0.000803	Mean	0.00993	0.0169
	(0.0108)	(0.0107)			
Trip length	0.0444***	0.0439***	Mean	0.0741***	0.0131
	(0.00914)	(0.00897)			
Opt-out	-0.941***	-0.889***	Mean	-1.184***	0.168
	(0.123)	(0.136)			
Trip cost	-0.00460***	-0.00433***	Mean	-0.00726***	0.000401
	(0.000284)	(0.000435)			
γ (overlap)	—	0.000225		—	—
		(0.000293)			
LR (χ^2)	943.0***	0.59		491.82***	
No. Obs.	8,166	8,166		8,166	
No. Groups	354	2,722†		—	
MWTP	$12.08	$12.28		$13.35	
	(9.01 – 15.14)	(9.11 – 15.45)		(9.56 – 17.14)	

$^{***}p < .01$, $^{**}p < .05$, $^{*}p < .10$.

† No. Groups indicates the number of unique observations for the scale parameter variable.

Complete Complexity Analysis

The previous sections examined the effect of various forms of task complexity individually, as has been done before in the literature (Arentze et al., 2003; Carlsson &

Martinsson, 2008; Hensher, 2006a, 2006b); however, the practice may result in omission bias because some sources of task complexity, namely the degree of information overlap, are present in all treatments. Though other studies have examined the effect of total survey complexity on survey response (Danthurebandara et al., 2011; Swait & Adamowicz, 2001a, 2001b), the all-encompassing entropy measure is unable to differentiate contributions from specific forms of task complexity. The scale heterogeneity model in Table 27 uses pooled treatment data with separate scale parameters for each complexity type so that relative importance can be measured. A homogeneous specification using the pooled treatment data is included for comparison. No RPL was estimated due to technological constraints.

Table 27. *Regression Results for Complete Task Complexity Model (Pooled Data)*

Variable	**Homogeneous**	**Scale Heterogeneity**
# Fish kept	0.0507***	0.0567***
	(0.00287)	(0.00164)
# Released	0.00236	0.00439
	(0.00444)	(0.00519)
Trip length	0.0428***	0.0474***
	(0.00479)	(0.0147)
Opt-out	-0.961***	-1.141***
	(0.0568)	(0.321)
Trip cost	-0.00516***	-0.00602***
	(0.000127)	(0.00168)
γ (overlap)	—	-0.000212***
		(0.0000803)
γ (# alternatives)	—	0.103***
		(0.0601)
γ (question order)		-0.00313
		(0.00400)
γ (# questions)	—	-0.0598***
		(0.0216)
γ ((#questions)^2)	—	0.00197***
		(0.000656)
LR (χ^2)	4725.6***	20.13***
No. Obs.	36,106	36,106
No. Groups	1,080	11,188†
MWTP	$9.82	$9.43
	(8.58 – 11.06)	(8.21 – 10.64)

***p < .01, **p < .05, *p < .10.

† No. Groups indicates the number of unique observations for the scale parameter variables.

The estimated model parameters for the scale heterogeneity model are generally larger than the model parameters in the homogeneous model though some of the attributes are mean-invariant. The likelihood ratio test statistic for difference in parameters between the two models is 20.124, which statistically different at the 99% confidence level. The scale parameters for survey length are comparable to those estimated using the base and length pooled data (Table 23 and Table 24). The effect of questionnaire length is statistically significant and inverted-U shaped, as in the previous model, and the parameters suggest that error variances increase until questionnaire length reaches 15 questions, then decreases. Additionally, MWTP values are very similar between the homogeneous pooled data model and the heterogeneous pooled data model, as expected; however, the pooled data models shift the confidence intervals significantly in a negative direction compared to the results from the base data only model (Table 19). This is similar to the result found by Hensher (2006a) for VTTS when overlooking task complexity effects.

Some results from the single-source examinations were not consistent with the pooled treatment results. Question order was not statistically significant in the presence of other sources of task complexity. Degree of overlap and number of alternatives were both statistically significant in this model though they were not in single-source scale heterogeneity models. This is most likely due to correlation with another regressor; however, examination of the covariance matrix for the estimated coefficients did not reveal any significant correlations between variables in the model (all covariance measures are small). The degree of information overlap variable, which increases with increases in information diversity, is negative and statistically significant, indicating that increasing information diversity increases error variance in the model.

The number of alternatives increases choice accuracy, which is counterintuitive and contradicts previous findings in the literature that increasing the number of alternatives produces no effect (Arentze et al., 2003; Kits et al., 2009; Malhotra, 1982) or has a negative effect on the decision-making process (Dhar, 1997a, 1997b; Green & Srinivasan, 1990; Haynes, 2009; Redelmeier & Shafir, 1995; Tversky & Shafir, 1992). In this case, it could be that a larger assortment of choices allows for more direct comparisons and a greater understanding of possible options (Hutchinson, 2005), which may reduce random selection behaviors. Additionally, some studies have suggested that the relationship between choice and number of alternatives can be characterized as an inverted-U (Reutskaja & Hogarth, 2009). The theory is that individuals are better off with more options but prefer to make decisions from only a few alternatives, and the shift in dominant effect results in a convex shape. Because there were only two treatments for the number of alternatives and the number of alternatives was small in each treatment, it could be that the scale parameter is identifying the beginning of the inverted-U function.

Conclusion

Methodological biases are important considerations in the design of stated preference studies as they are often the only source of economic information for nonmarket goods and services. This chapter examined several CE survey designs for a mail survey of recreational groundfish anglers in New England. The results of this experiment show that individuals do respond to increases in task complexity as evidenced by variations in response rates, qualitative choice consistency measures, model parameters, and estimated MWTP.

Three different methods were used to test for evidence of response changes due to treatment group: the Swait-Louviere scale parameter test, scale heterogeneity modeling, and RPL modeling. The results suggest that researchers should be most concerned with survey length and number of alternatives when designing CE questionnaires. Survey length has an inverted-U shaped relationship with error variance, and longer surveys deflate MWTP estimates. Increasing the number of alternatives increases response consistency, but only having two treatments addressing this form of task complexity may be influencing this result.

Though some tests performed on individual complexity treatments did not reveal any behavioral changes, the pooled complexity model confirmed that task complexity does affect underlying decision-making processes as evidenced by differences in estimated model parameters, error variances, and MWTP confidence intervals. The contradictions between individual and the pooled treatment results suggest that interaction effects between different forms of task complexity may influence results unless all forms of task complexity are analyzed simultaneously and mean MWTP estimates are slightly higher when task complexity is ignored.

Nonmarket stated preference valuation studies should account for the possibility of survey design influences on analytical outcomes to reduce inherent study biases and prevent poor policy choices. Because changes in model parameters and error variances affect demand estimates, there may be additional consequences for valuations based on total WTP not explored in this study. This chapter extended previous research on task complexity by simultaneously examining several sources of perceived difficulty, but further exploration is warranted to understand the relationship between questionnaire length, question order, number of alternatives, and degree of information overlap on observed response behavior in mail CE surveys.

Chapter 3: Stated Preference Models for Recreational Fishing

Introduction

Stated preference methods are widely used in environmental and nonmarket contexts because adequate revealed preference data are not always available for quantitative analysis. Discrete choice stated preference methods are becoming more popular with applications including hunting trip valuation (Gan & Luzar, 1993; Mackenzie, 1993), public good valuation (Bennett, Rolfe, & Morrison, 2001; Johnson & Desvousges, 1997; Opaluch, Swallow, Weaver, Wessells, & Wichelns, 1993), and recreational angling (Banzhaf et al., 2001; Haider, 2002; Hicks, 2002; Roe, Boyle, & Teisl, 1996). Because CEs mimic real choice environments, more accurate predictions are obtained using this method than other stated preference methods (Elrod, Louviere, & Davey, 1992; Louviere, 1988; Louviere & Woodworth 1983). Despite many advances in discrete choice stated preference modeling techniques and technology, no consensus has been reached regarding model specification for recreational fisheries applications.

Though some angler behavior and characteristics vary between fisheries, most discrete choice stated preference studies of recreational angling utilize the same variables: regulations in the form of bag limits and minimum size limits; some combination of catch, keep, and discard of fish; and trip cost. Even with these commonalities, no single model is consistently utilized in CE stated preference studies of recreational angling. One concern is in estimating the economic impact of changes in management. Because most recreational fisheries studies support or inform policy decisions, having a standardized method for approaching such valuation problems would be useful. Previous models have included various numbers of regulatory variables from none (Roehl et al., 1993) to several (Aas et al., 2000; Dorow et al., 2010; Hicks, 2002; Oh et al., 2005; Oh & Ditton, 2004; Paulrud & Laitila, 2004). These studies include an array of specifications including linear and interaction terms.

Another issue concerns estimation structure. Econometric specifications in the literature include conditional logits, nested logits, scaled multinomial logits, and mixed logits. Conditional logits assume that anglers have singular decision-making processes and that all options within a choice scenario can be considered IIA. Nested logits assume that anglers have a branched decision-making process and that the assumption of IIA holds only within branches, not between. Scaled multinomial logit and mixed logit specifications both allow for heterogeneity, but the random elements of mixed logits may affect WTP estimates whereas the scale factor in scale heterogeneity models should not.

The type of respondent included in survey data analysis is also a concern. Recreational angling studies obtain their survey panels through a variety of methods, including license frames (Herrmann et al., 2001), marketing or third party lists, random digit dialing, and in-person intercept interviews (Oh et al., 2005). Studies vary in their ability to control for specific respondent characteristics through pre-screening. Inclusion (or exclusion) of different angling sub-populations may affect results.

The diverse range of models and estimation methods found in previous models present challenges when comparing outcomes from different studies or generating results using meta-analysis as variations in valuation estimates cannot be attributed solely to differences in resource characteristics. This chapter focuses on the consequences of model and population specification on inferences of the recreational value of Atlantic cod, haddock and pollock using the CE stated preference data described in Chapter 1.

Methods

Random utility models (RUMs) have been used to model recreational angling since Bockstael, McConnell, and Strand (1989) pioneered the usage with their study of Florida sportfishing. In discrete choice stated preference studies, anglers are asked to compare several trips simultaneously with different attribute levels. The collection of choice responses from various choice scenarios enables researchers to examine tradeoffs and behavioral responses to a variety of biological and regulatory changes.

The traditional model for a discrete choice study begins with a choice occasion *s*. According to random utility theory, the researcher cannot directly observe the actual utility function of any angler *n*; however, a significant proportion of the utility function can be understood using information from a well-designed choice study. That is,

1. $$U_{jns} = V_n\,(\mathrm{x}_{js}) + \varepsilon_{jns}, \qquad (3.1)$$

where U_{jns} is the latent, unobserved utility for option *j* at a given choice occasion *s*, V_n is the systematic or observed portion of an angler's utility, x_{js} is a vector of alternative-specific attributes, and ε_{jns} is the random or unobserved portion of the angler's utility. Given the random component, the utility function itself cannot be estimated; however, a researcher can interpret the probability of an angler's choice. The angler *n* will choose option *j* from set *s* if the utility obtained from that option outweighs the utility of all other options *k* in set *s*:

$$\mathrm{P}\,(j|s) = \mathrm{P}\,[(V_n\,(\mathrm{x}_{js}) + \varepsilon_{jns}) > (V_n\,(\mathrm{x}_{ks}) + \varepsilon_{kns})]\ \forall\, j \neq k \in s. \qquad (3.2)$$

Estimation of the choice probabilities requires an assumption of the distribution of the random component. Typically, either a Gumbel or normal distribution is assumed, which leads to estimation using models from either the multinomial logit or multinomial probit families. This general specification forms the basis for most studies of recreational angling using CE data; however, variations in the details can cause significant differences in results. Several classes of models commonly found in the literature are explored in the following sections.

Linear Utility Functions

The most common utility model used in economic analyses of recreational angling is linear. A simple linear-in-parameters utility function can be specified in general as

$$U_{jns} = \beta x_{jns} + \varepsilon_{jns} \; \forall \; n = 1, \ldots, N; j = 1, \ldots, J; s = 1, \ldots, S, \tag{3.3}$$

where β is a vector of utility weights homogeneous across individuals, and ε_{jns} is the random component or error term, which, for conditional logit models, ε_{jns} ~ i.i.d. generalized extreme value (GEV). The error term may include characteristics of the alternative omitted by the researcher, measurement errors, and unobserved characteristics of the individual or the choice context. The choice probabilities are given by

1. $$P(j|X_{ns}) = \frac{exp(\beta x_{jns})}{\sum_1^J exp(\beta x_{kns})}, \tag{3.4}$$

where X_{ns} is the vector of attributes of all alternatives $j = 1, \ldots, J$. WTP for marginal changes in attribute levels, as measured by consumer surplus, is given by the ratio of the partial derivative of the utility function with respect to a particular attribute x to the partial derivative of the utility function with respect to price p (or trip cost)

$$mwtp = \frac{\partial U / \partial x}{\partial U / \partial p}, \tag{3.5}$$

which is simply the ratio of the parameter estimates for x and p:

$$MWTP = \beta_x/\beta_p. \tag{3.6}$$

All MWTP confidence intervals reported in this chapter are constructed using the Krinsky-Robb procedure recommended by Haab and McConnell (2002).

Inclusion of Regulatory Attributes

One principal difference in the literature is the inclusion (or exclusion) of regulatory attributes. CEs are founded on the premise that the entire worth of the good or service equals the sum of its parts. Studies of market goods and services typically utilize all attributes shown to respondents in the CEs because the attribute list encompasses all aspects of the good that are perceptible to the consumer. For example, a study of breakfast cereals might include attributes such as brand, package size, grain type, sugar content, and cereal shape. Generating such definitive breakdowns for nonmarket goods is more difficult, and some attributes may overlap with others. Additionally, comprehensive characteristic lists for nonmarket goods may overwhelm respondents and are impractical for many applications.

Therein lies the debate for recreational fisheries: Should regulatory attributes be included in CE surveys? More importantly, should regulatory attributes be included in models using CE data? There are a large number of environmental goods and services that affect an angler's enjoyment (utility level) for a given fishing trip. Ambience, weather, water features, bird sightings, and catch are among the possible items contributing to pleasure derived from being outdoors. Personal considerations are also important in choosing angling trips, such as time with friends or family, or the ability to supplement the dinner table. Most researchers would list regulations such as size limits and bag limits in the list of deciding factors for angling trips; however, the importance of regulations varies depending on the application. For anglers who only participate in catch and release fishing (game fish), regulations have no bearing at all; all of the fish are released, so whether or not regulations bind is immaterial. For meat fisheries (food fish), the degree to which regulations bind does impact trip outcomes, effort, and participation levels. The problem lies in capturing these economic consequences for policy analysis using discrete choice stated preference methods if anglers ignore the attributes when answering CEs, as has been noted in other studies by NMFS.

Most recreational angling surveys do include regulatory attributes in the CE questions, utilizing direct tactics and explicitly listing regulation levels in the table of attributes for each question (Gillis & Ditton, 2002; Hicks, 2002; Oh et al., 2005; Paulrud & Laitila, 2004). Some authors believe that it is necessary to adhere to traditional marketing conjoint methods and include all attributes from the CEs in the estimation of choice probabilities because the value of changes to management is usually derived from the ratio of parameter estimates and reported as a MWTP for changes in management level (Hicks, 2002; Oh et al., 2005; Paulrud & Laitila, 2004). It is unclear what these types of values mean. Can a simple ratio of parameters define the economic impact of a change in management, especially when anglers insist that these attributes are ignored when they answer stated preference surveys? Moreover, are the model parameters meaningful when estimated in this fashion?

To address these issues, three different model specifications are compared. The first model is a linear in parameters utility function including all regulatory variables:

$$\begin{aligned} U_{jns} = {} & \beta_1(cod\ kept)_{jns} + \beta_2(cod\ released)_{jns} \\ & +\beta_3(haddock\ kept)_{jns} + \beta_4(haddock\ released)_{jns} \\ & +\beta_5(pollock\ kept)_{jns} + \beta_6(pollock\ released)_{jns} \\ & +\beta_7(cod\ bag\ limit)_{jns} + \beta_8(haddock\ bag\ limit)_{jns} \\ & +\beta_9(pollock\ bag\ limit)_{jns} + \beta_{10}(cod\ min.size)_{jns} \\ & +\beta_{11}(haddock\ min.size)_{jns} + \beta_{12}(pollock\ min.size)_{jns} \end{aligned}$$

$$+\beta_{14}\left[(trip\ length)_{jns} \times for\text{-}hire_n\right]$$
$$+\beta_{15}\left[(trip\ length)_{jns} \times for\text{-}hire_n\right]^2$$
$$+\beta_{16}\left(opt\text{-}out\right)_{jns} + \beta_{17}(trip\ cost)_{jns} + \varepsilon_{jns}. \quad (3.7)$$

The second model includes only bag limits because previous studies have stated that estimation failure occurs with minimum size limits (e.g., Hicks, 2002):

$$\begin{aligned} U_{jns} = {} & \beta_1(cod\ kept)_{jns} + \beta_2(cod\ released)_{jns} \\ & +\beta_3(haddock\ kept)_{jns} + \beta_4(haddock\ released)_{jns} \\ & +\beta_5(pollock\ kept)_{jns} + \beta_6(pollock\ released)_{jns} \\ & +\beta_7(cod\ bag\ limit)_{jns} + \beta_8(haddock\ bag\ limit)_{jns} \\ & +\beta_9(pollock\ bag\ limit)_{jns} + \beta_{10}\left[(trip\ length)_{jns} \times for\text{-}hire_n\right] \\ & +\beta_{11}\left[(trip\ length)_{jns} \times for\text{-}hire_n\right]^2 + \beta_{12}\left(opt\text{-}out\right)_{jns} \\ & +\beta_{13}(trip\ cost)_{jns} + \varepsilon_{jns}. \end{aligned} \quad (3.8)$$

The third model does not include any regulatory variables:

$$\begin{aligned} U_{jns} = {} & \beta_1(cod\ kept)_{jns} + \beta_2(cod\ released)_{jns} \\ & +\beta_3(haddock\ kept)_{jns} + \beta_4(haddock\ released)_{jns} \\ & +\beta_5(pollock\ kept)_{jns} + \beta_6(pollock\ released)_{jns} \\ & +\beta_7\left[(trip\ length)_{jns} \times for\text{-}hire_n\right] \\ & +\beta_8\left[(trip\ length)_{jns} \times for\text{-}hire_n\right]^2 + \beta_9\left(opt\text{-}out\right)_{jns} \\ & +\beta_{10}(trip\ cost)_{jns} + \varepsilon_{jns}. \end{aligned} \quad (3.9)$$

The catch for each species is defined as the number of fish kept and the number of fish released. Decomposition of the catch into keep and release is a better indicator of fishing success than total catch (Milon, 1991). Additionally, New England groundfish are classified as a meat fishery and the difference between the value of a kept fish and a discarded fish should be significant, which would not be captured with a grouped catch term. The value of catching an additional fish is arbitrary in this context and not informative. Separating the catch is also more suitable for estimating regulation impacts because regulations affect the composition of catch, not total catch.

Based on anecdotal evidence from focus groups, the variable "trip length" is modeled as a quadratic function to show that anglers enjoy longer trips with decreasing marginal utility due to increasing opportunity costs of time. Because shore anglers (Sample A) received a version of the survey that did not include trip length unlike for-hire anglers (Sample B), this variable is interacted with a dummy indicating for-hire (Sample B) inclusion status.

Diminishing Marginal Utility of Catch

Though some previous studies utilize interaction terms (e.g. Dorow et al., 2010; Hicks, 2002; Oh & Ditton, 2004; Oh et al., 2005; Paulrud & Laitila, 2004), very few have explored nonlinear specifications for catch. The linear-in-catch (LIC) utility models outlined above assume constant rates of substitution between income and catch, which may not best represent angler utility. A nonlinear-in-catch (NIC) model allows for diminishing marginal utility of catch and may be more appropriate. Though a log-linear transform is possible, the following model uses the square root of catch as in previous fishery studies (e.g., Daw, 2008; Haab, Hicks, & Whitehead, 2005; Hicks, Haab, & Lipton, 2004; Lipton & Hicks, 2003; Silvestre, 1998):

$$\begin{aligned} U_{jns} = {} & \beta_1\sqrt{(cod\ kept)_{jns}} + \beta_2\sqrt{(cod\ released)_{jns}} \\ & +\beta_3\sqrt{(haddock\ kept)_{jns}} + \beta_4\sqrt{(haddock\ released)_{jns}} \\ & +\beta_5\sqrt{(pollock\ kept)_{jns}} + \beta_6\sqrt{(pollock\ released)_{jns}} \\ & +\beta_7\left[(trip\ length)_{jns} \times for\text{-}hire_n\right] \\ & +\beta_8\left[(trip\ length)_{jns} \times for\text{-}hire_n\right]^2 + \beta_9\left(opt\text{-}out\right)_{jns} \\ & +\beta_{10}(trip\ cost)_{jns} + \varepsilon_{jns}. \end{aligned} \tag{3.10}$$

The marginal WTP estimates for catch differ from the linear-in-catch specification in that the ratio of partial derivatives is no longer constant:

$$mwtp_{\#cod\ kept} = \frac{\partial U / \partial(\#cod\ kept)}{\partial U / \partial p} = \frac{\beta_1 \left(\frac{1}{2}(\#cod\ kept)^{-1/2}\right)}{\beta_{10}}. \qquad (3.11)$$

First-Hand Knowledge of Fishery

Because it was not possible to pre-screen respondents prior to mailing the surveys, the first few questions asked respondents to indicate whether or not they had fished for the three species in the survey in the last year and in the last five years. Initially, these questions were designed to be screening questions to eliminate respondents who had not had direct experience with the species in the survey; however, most anglers who found the survey irrelevant either refused or did not respond to the survey. The anglers who responded to the survey but did not state having any direct experience with any of the species in the past five years form an intriguing group. These individuals are not users of the fishery as they stated lack of experience with the fish in the survey, yet insisted on expressing their opinions on the subject. Marginal WTP estimates from the non-experienced group are difficult to interpret and might represent existence or non-use values, option values, or latent effort values. The meaning of the MWTP estimate for this sample is unknown because there is a possibility that these species are not in the respondents' choice sets. Estimates for the latter sample should be smaller than the marginal WTP estimates for those with direct experience.

Shore Versus For-Hire

The previous models used a pooled dataset including both shore (Sample A) and for-hire (Sample B) respondents. As explained in earlier chapters, shore fishermen are less likely to encounter these species due to biological preferences for colder, deeper waters. Table 28 shows the proportion of anglers reporting experience with the species.

Table 28. *Self-Reported Species Familiarity by Fishing Mode*

Species	Shore	For-Hire
Cod	44%	56%
Haddock	31%	47%
Pollock	28%	47%
Any of the 3 Species	51%	71%
All 3 Species	21%	38%

The high proportion of individuals from shore mode having direct experience with the three species is surprising, considering only 19% of shore anglers reported being multi-mode fishermen. Shore anglers typically have smaller expenditures per trip than boat-mode anglers so comparing model results for the two different sub-populations should show different preference structures.

Alternative Econometric Specifications

Conditional logits are restricted by the assumption of i.i.d. error terms, which implies IIA, which means that the ratio of probabilities of any two alternatives is independent of any alternative-specific characteristics for all other alternatives and is constant regardless of the presence or absence of any additional alternatives:

$$\frac{P(j|X_{ns})}{P(k|X_{ns})} = \frac{\exp(\beta x_{jns})}{\exp(\beta x_{kns})} = \exp\left(\beta x_{jns} - \beta x_{kns}\right). \tag{3.12}$$

Most studies that relax the i.i.d. assumption do so using nested logits (Hicks, 2002; Kaoru, 1995; Milon, 1988; Morey, Waldman, Assane, & Shaw, 1995). The primary theoretical reason behind this practice is the assumption that anglers first make a decision about going fishing, and then make choices regarding particular trips, but some studies suggest that the nested logit is inferior to single-step conditional logit models for recreational participation decisions (Adamowicz, Swait, Boxall, Louviere, & Williams, 1997). Neither anecdotal evidence nor quantitative evidence supports a nested logit for this application. Nested logit models estimated from this data did not pass consistency conditions outlined by Herriges and Kling (1996) and Kling and Herriges (1995), ruling out this specification as a viable alternative.

A different econometric specification involves introducing systematic heterogeneity in the error term using the fact that the GEV distribution is characterized by both a location parameter and a scale factor. The scale heterogeneity model, or heteroscedastic logit, does not explicitly capture all of the heterogeneity in a dataset per se; rather, the scale factor implies a proportional scaling of the vector of utility weights across respondents with idiosyncratic error terms being larger for some anglers than for others. Explicitly rewriting Equation 3.3 with the scale parameter μ gives:

$$U_{jns} = \boldsymbol{\beta} \mathbf{x}_{\boldsymbol{jns}} + \varepsilon_{jns}/\mu \qquad \forall\ n = 1, \ldots, N; j = 1, \ldots, J; s = 1, \ldots, S. \qquad (3.13)$$

The scale factor is inversely proportional to the variance of the error term $\left(\mu = \pi/\sqrt{6\sigma_\varepsilon^2}\right)$. Usually, the scale parameter is normalized to one because it is not possible to identify both β and μ. If the scale parameter is allowed to vary by individual, then Equation 3.13 can be rewritten as

$$U_{jns} = (\mu_n \boldsymbol{\beta})\mathbf{x}_{\boldsymbol{jns}} + \varepsilon_{jns} \qquad \forall\ n = 1, \ldots, N; j = 1, \ldots, J; s = 1, \ldots, S, \qquad (3.14)$$

with the error term now distributed i.i.d. GEV with variance $\sigma_\varepsilon^2 = (\pi^2/6\mu^2)$. From Deshazo and Fermo (2002) and Hole (2007), the scale factor is represented as an exponential function to force the scale parameter to be positive (error terms are positive, so scale parameters must be positive across all subpopulation groups):

$$\mu_n(C_n) = \exp\,[C_n \gamma]. \qquad (3.15)$$

C_n represents a vector of m individual characteristics and the vector γ measures the degree of influence of C_n on the error variance. This specification is convenient as it allows for nonlinearity and converges.

When the only contribution to heterogeneity in error terms can be fully described by a scale parameter, MWTP is not affected. Rewriting Equation 3.5 gives

$$mwtp = \frac{\partial U/\partial x}{\partial U/\partial p} = \frac{\frac{\partial}{\partial x}\left((\mu_n \beta) x_{jns} + \varepsilon_{jns}\right)}{\frac{\partial}{\partial p}\left((\mu_n \beta) x_{jns} + \varepsilon_{jns}\right)} = \frac{\mu_n \beta_x}{\mu_n \beta_p} = \frac{\beta_x}{\beta_p} \tag{3.16}$$

under the assumption that μ_n is a scalar term and the utility function is linear in parameters. MWTP for the NIC utility function specified in Equation 3.10 is also unaffected:

$$mwtp_{\#cod\ kept} = \frac{\partial U/\partial(\#cod\ kept)}{\partial U/\partial p} = \frac{\mu_n \beta_1\left(\frac{1}{2}(\#cod\ kept)^{-1/2}\right)}{\mu_n \beta_{10}}$$
$$= \frac{\beta_1\left((\#cod\ kept)^{-1/2}\right)}{2\beta_{10}}. \tag{3.17}$$

Though the MWTP function is nonlinear, the scale factor cancels out.

This parametric specification for heterogeneity allows for heterogeneity in coefficients but does not affect MWTP because attribute and price coefficients are simultaneously scaled by the same parameter. That is not to say that all anglers have the same choice probabilities; differences in attribute sensitivities are fully captured by the scale heterogeneity model. For example, the derivative of the choice probability with respect to trip cost is

$$\frac{\partial P_n(j|X_{ns})}{\partial p_{jns}} = -P_n(j|X_{ns}) \cdot [1 - P_n(j|X_{ns})] \cdot \mu_n \cdot \beta_p. \tag{3.18}$$

As $\mu_n \to 0$, the deterministic portion of the utility function decreases in relative importance, the error term ε_{jns} dominates, and price sensitivity approaches zero. Anglers with larger scale parameters will exhibit bigger demand changes than anglers with small scale parameters, but it is possible for two anglers to have the same MWTP for an attribute with different choice probabilities.

Though many recent studies in nonmarket valuation have employed mixed logits for describing sample heterogeneity, critics have argued that mixed logits are poorly representative and far less parsimonious than scale heterogeneity specifications (Louviere et al., 2008). More specifically, if respondent preferences closely resemble lexicographic preferences, then scale heterogeneity models are better at explaining resulting behavior (Fiebig, Louviere, Keane, & Wasi, 2010). Individuals with lexicographic preference structures appear extreme by consistently ignoring a majority of the attributes in each choice set. For example, an individual would be interpreted by the model as being extreme relative to other respondents if he always chooses trips with the most haddock regardless of the values of other attributes. Scale heterogeneity models capture extreme behaviors through large scale parameters, which allow a few attributes to drive choices and indicate little randomness in observed behavior. For the same reason, scale heterogeneity models are better able to explain random behaviors than mixed logit models by putting more explanatory power in the error term than in the model parameters. Individuals with a seemingly large amount of random behavior are assigned small scale parameters.

Additionally, scale heterogeneity models are more computationally feasible. Though mixed logit and scale heterogeneity models perform about the same when responses are well behaved, reliance on residual taste heterogeneity alone severely restricts the information available to mixed logit specifications and limits the model's ability to adequately explain extreme or random circumstances (Fiebig et al., 2010). Many mixed logit specifications are too resource-intensive to be estimated using the average commercially available computer or have convergence problems (Vojáček & Pecáková, 2010) and researchers often compromise theory to achieve empirical results.

Given that many recreational anglers report making fishing decisions based on very few factors, scale heterogeneity models seem most appropriate for understanding heterogeneity in this application. Several different scale heterogeneity models are estimated assuming that error terms are similar or correlated among subgroups in the study sample. Heterogeneity based on the two population differences identified previously, mode and direct experience, are estimated. Additionally, heterogeneity based on angler avidity is also explored. The scale factor for this model is estimated as a function of number of trips taken over 12 months as reported by the respondent:

$$U_{jns} = [\mu_n\, ((\#\, trips)_n) \cdot \boldsymbol{\beta}]\, \mathbf{x}_{jns} + \varepsilon_{jns}. \tag{3.19}$$

A more detailed, or cumulative, scale heterogeneity model is also estimated to minimize misspecification and identify residual effects from multiple sources of heterogeneity. This model incorporates all survey-identified population differences (mode, knowledge of species, avidity) and socio-economic demographic variables:

$$U_{jns} = \left[\mu_n \begin{pmatrix} (direct\ experience)_n \\ (shore)_n \\ (\#trips)_n \\ (demographics)_n \end{pmatrix} \cdot \beta\right] x_{jns} + \varepsilon_{jns}. \tag{3.20}$$

<u>Results</u>

Inclusion of Regulatory Attributes

From Table 29, Model 1 follows classic conjoint techniques by utilizing all attributes shown to respondents in the CEs. Some of the estimated parameters behave as expected: kept fish have a higher weight for cod and haddock than released fish, but both types of catch are considered valuable to anglers. It is puzzling that released pollock is more valuable than kept pollock, though this could be due to high levels of excitement during capture and distaste for consumption of the fish. With regard to species importance, cod has the greatest value to recreational anglers in this fishery, followed by haddock and then pollock, as predicted. Trip cost is significant and negative, as expected. For the tables in this chapter, No. Obs. is the total number of items (alternatives) and No. Groups is the number of clusters used in computing the standard errors (N), which equals the number of individuals. Models in this chapter cluster observations by individual to control for correlation in responses by specific individuals unless noted otherwise. Differences in parameter estimates between non-clustered and clustered models are not statistically significant.

Table 29. *Analysis of Regulatory Attribute Inclusion*

Variable	Model 1	Model 2	Model 3
# Cod kept	0.0688 (0.00516)***	0.0631 (0.00478)***	0.0648 (0.00470)***
# Cod released	0.00277 (0.00948)***	0.0250 (0.00608)***	0.00938 (0.00526)*
# Haddock kept	0.0595 (0.00502)***	0.0614 (0.00499)***	0.0645 (0.00472)***
# Haddock released	0.0254 (0.00892)***	0.0286 (0.00719)***	-0.00377 (0.00588)
# Pollock kept	0.00614 (0.0127)	0.0201 (0.0118)*	0.0260 (0.00796)***
# Pollock released	0.0509 (0.0219)**	-0.0210 (0.0115)*	-0.0694 (0.00983)***
Bag limit cod	0.0386 (0.00775)***	0.0540 (0.00566)***	—
Bag limit haddock	0.0240 (0.00816)***	0.0247 (0.00619)***	—
Bag limit pollock	0.0767 (0.0145)***	0.0307 (0.00928)***	—
Min. size limit cod	0.0223 (0.00730)***	—	—
Min. size limit haddock	0.0166 (0.00707)**	—	—
Min. size limit pollock	-0.0130 (0.00934)	—	—
Trip length × For-Hire	0.182 (0.0191)***	0.182 (0.0190)***	0.188 (0.0181)***
(Trip length)^2 × For-Hire	-0.00928 (0.00133)***	-0.00914 (0.00133)***	-0.00987 (0.00128)***
Trip cost	-0.00557 (0.000131)***	-0.00550 (0.000130)***	-0.00549 (0.000128)***
Opt-out	-0.0740 (0.148)	-0.356 (0.0776)***	-0.710 (0.0604)***
LR (χ^2)	4,986.0***	4961.8***	4,869.0***
No. Obs.	39,151	39,151	39,151
No. Groups	1,214	1,214	1,214
MWTP Cod	$12.36	$11.48	$11.79
	(10.31, 14.24)	(9.55, 13.25)	(9.88, 13.55)
MWTP Haddock	$10.69	$11.18	$11.75
	(8.83, 12.39)	(9.32, 12.88)	(9.92, 13.37)
MWTP Pollock	$1.10	$3.65	$4.74
	(-3.27, 5.76)	(-0.45, 8.15)	(2.02, 7.74)

$^{***}p < .01$, $^{**}p < .05$, $^{*}p < .10$.

The interpretation of the parameters on the two management variables, bag limit and minimum size limit, is challenging. Though the positive sign on bag limits is expected, the magnitude of the parameters for bag limits is not. Anglers should be happier when allowed to keep more fish, but the empirical model shows that anglers place higher utility weights on the bag limits for pollock than they do on the cod and haddock they are keeping, which contradicts evidence from ongoing research at NMFS showing that bag limits have little or no impact on anglers' decisions to take fishing trips. These parameter estimates are not sensible.

The coefficient on minimum size is equally inexplicable. Raising the minimum size limit decreases the probability of keeping fish caught and anglers should consider increases undesirable. Three explanations are possible. The justifications used previously in the literature (Hicks, 2002) are that the survey population contains an unusually large proportion of conservation-minded anglers that believe in preserving juvenile species, or respondents are interpreting minimum size as a quality variable indicating the size of fish they are catching or keeping. Another possibility is that anglers do not keep small fish regardless of regulations, so increases in the minimum size are desirable (personal communication, S. Steinback, March 18, 2011). In species such as black sea bass, the minimum size could be 12", but because anglers will not retain anything smaller than 16", increasing the minimum size limit from 12" to 16" is actually agreeable to the anglers. None of these explanations are plausible in this case. The first two explanations are not supported by feedback from survey respondents and focus group participants. The third explanation cannot be true in this case because the smallest minimum sizes included in the survey are well above what is considered a small, inedible groundfish.

The positive sign on minimum size limits is a common phenomenon in models where only one regulation size variable is specified (Aas et al., 2000; Hicks, 2002) even though this result contradicts rational behavior. Slot limits, where both a minimum and maximum size limit are specified in the regulation, do not appear to suffer from the same estimation problems (Oh & Ditton, 2004; Oh et al., 2005). Though authors have attempted to work around this estimation failure by interacting minimum size with other attributes, such models make it difficult to isolate the effects of policy changes on angler behavior. For example, Hicks (2002) used an interaction between minimum size and catch, Paulrud and Laitila (2004) combined size and catch as one attribute. The interaction terms complicate policy analysis, as assumptions on the interacted variables may be restrictive. Dorow et al. (2010) reclassified the minimum size into a series of dummy variables, but half of the parameters still had negative signs. Additionally, binary transformations are impractical for considering policy changes outside the possibilities included in the survey.

Model 2 in Table 29 is slightly more sensible than Model 1. As in Model 1, the value of the relative species decreases from cod to haddock to pollock, but there are several differences, the most notable being the negative coefficient on released pollock and the relative worth of kept pollock. The first result is more consistent with anecdotal evidence as catching and discarding an undesirable fish uses up valuable time and resources that could be devoted to catching more desirable species. The relative worth of kept pollock is roughly one-third that of cod and haddock, which makes more sense than the miniscule and statistically insignificant parameter in Model 1. There are only marginal differences in the parameters for trip cost and trip length, but the opt-out constant is now statistically significant and markedly more negative, which reflects the “I’d rather be fishing” attitude of most recreational anglers.

The magnitude of the bag limit parameters in Model 2 is still puzzling. Though it is possible that anglers consider the bag limit for cod in making trip decisions almost as much as they consider the number of cod kept, it is still absurd to think that anglers would weight bag limits for pollock more heavily than the number of fish caught.

Model 3 in Table 29 does not include any regulatory attributes. In this model, releasing pollock is as undesirable as it is to keep cod or haddock. Also, cod and haddock have very similar values. Though more stringent regulations were imposed on haddock recently and could have artificially inflated the resource values temporarily, historically haddock has been slightly less desirable than cod and the likelihood of the value of haddock being equal to the value of cod is relatively slim. Even if the value of haddock per pound were equal to the value of cod per pound, the difference in mean fish sizes would imply a higher valuation for one cod relative to one haddock.

Diminishing Marginal Utility of Catch

Table 30 lists the results for the NIC (Model 4) utility model. Model 3 (LIC) results are shown again for ease of comparison. Model 4 has a higher likelihood ratio score for identical degrees of freedom. The trends in estimated parameters for catch are similar to those in Model 3, but catch variables are weighted more heavily relative to other attributes in Model 4 than in Model 3, and the opt-out coefficient is much smaller in Model 4. Accounting for diminishing marginal utilities reduces the magnitude of the opt-out parameter, which is more sensible because fishing attributes should have higher importance-weights than the opt-out alternative. The parameter estimates for trip length and trip cost are not affected. Model 4 is as statistically significant but more theoretically appropriate and will be used in all subsequent analyses.

The welfare estimates for Model 4 are calculated at the average value of the hypothetical catch vector, 4.5 fish. Though there are slight differences, the confidence intervals of the WTP estimates for Model 4 overlap with those of Model 3. Differences between the average WTP estimates are not significant despite marked dissimilarity in the construction of the theoretical model.

Table 30. *Marginal Utility of Catch Analysis: LIC vs. NIC Utility Functions*

Variable	Model 3 (LIC)	Model 4 (NIC)
# Cod kept	0.0648 (0.00470)***	—
# Cod released	0.00938 (0.00526)*	—
# Haddock kept	0.0645 (0.00472)***	—
# Haddock released	-0.00377 (0.00588)	—
# Pollock kept	0.0260 (0.00796)***	—
# Pollock released	-0.0694 (0.00983)***	—
$\sqrt{(\text{Cod kept})}$	—	0.296 (0.0199)***
$\sqrt{(\text{Cod released})}$	—	0.0567 (0.0201)***
$\sqrt{(\text{Haddock kept})}$	—	0.257 (0.0189)***
$\sqrt{(\text{Haddock released})}$	—	-0.0119 (0.0210)
$\sqrt{(\text{Pollock kept})}$	—	0.126 (0.0312)***
$\sqrt{(\text{Pollock released})}$	—	-0.211 (0.0344)***
Trip length × For-Hire	0.188 (0.0181)***	0.183 (0.0180)***
(Trip length)^2 × For-Hire	-0.00987 (0.00128)***	-0.00978 (0.00128)***
Trip cost	-0.00549 (0.000128)***	-0.00550 (0.000130)***
Opt-out	-0.710 (0.0604)***	-0.199 (0.0812)**
LR (χ^2)	4,869.0***	4,898.5***
No. Obs.	39,151	39,151
No. Groups	1,214	1,214
MWTP Cod	$11.79	$12.67
	(9.88, 13.55)	(10.77, 14.42)
MWTP Haddock	$11.75	$11.00
	(9.92, 13.37)	(9.30, 12.53)
MWTP Pollock	$4.74	$5.39
	(2.02, 7.74)	(2.92, 8.16)

$^{***}p < .01$, $^{**}p < .05$, $^{*}p < .10$.

First-Hand Knowledge of Fishery

Results for the models separating the survey sample into groups based on recent species experience are shown in Table 31. The keep parameters in Model 5, the subsample with direct experience, are higher for cod and haddock than in Model 6, but the release values are higher in Model 6. The opposite is true for pollock. These results are intuitive because anglers without direct experience of the species are more likely to value the species equally and possibly place more emphasis on releasing fish (as is common in sport fisheries). Also, the opt-out coefficient is almost double in Model 5 what it is in Model 6, indicating greater preference for fishing than other activities.

Table 31. *Species Familiarity Comparison: Direct vs. No Experience*

Variable	Model 5 (Exp.)	Model 6 (No Exp.)
$\sqrt{\text{(Cod kept)}}$	0.317 (0.0236)***	0.254 (0.0372)***
$\sqrt{\text{(Cod released)}}$	0.0471 (0.0236)**	0.0967 (0.0384)**
$\sqrt{\text{(Haddock kept)}}$	0.273 (0.0229)***	0.221 (0.0346)***
$\sqrt{\text{(Haddock released)}}$	-0.0176 (0.0251)	0.000886 (0.0396)
$\sqrt{\text{(Pollock kept)}}$	0.103 (0.0374)***	0.149 (0.0579)***
$\sqrt{\text{(Pollock released)}}$	-0.220 (0.0411)***	-0.148 (0.0644)**
Trip length × For-Hire	0.160 (0.0226)***	0.170 (0.0303)***
$(\text{Trip length})^2$ × For-Hire	-0.00796 (0.00157)***	-0.0103 (0.00225)***
Trip cost	-0.00554 (0.000153)***	-0.00543 (0.000254)***
Opt-out	-0.559 (0.100)***	0.291 (0.143)**
LR (χ^2)	4,358.8***	935.37***
No. Obs.	27,803	11,348
No. Groups	840	374
MWTP Cod	$13.45	$11.01
	(11.22, 15.52)	(7.42, 14.36)
MWTP Haddock	$11.61	$9.60
	(9.59, 13.44)	(6.47, 12.45)
MWTP Pollock	$4.37	$6.47
	(1.44, 7.68)	(1.80, 11.73)

***p < .01, **p < .05, *p < .10.

Though there is some overlap in WTP values, the confidence intervals for Model 5 are much tighter than in Model 6. This most likely reflects the uncertainty or ambiguity in assigning values to these fish for anglers who are not intimately familiar with the fishery. The values for anglers with direct experience are also higher for cod and haddock and lower for pollock than for anglers without recent direct species experience.

Shore Versus For-Hire

Table 32 lists the results for Model 7, shore anglers only, and Model 8, for-hire anglers only. Though a large number of shore anglers reported having fished for at least one of the three species in recent years, shore fishing appears to be the primary mode for these anglers given the deflated catch parameters in Model 7 compared to Model 8. The variation in preferences is due to little evidence of persistent mode switching in the survey sample and ecological preferences of the fish. WTP confidence intervals are much broader in Model 7, reflecting value uncertainty due to limited interactions with the fish.

Table 32. *Mode Comparison: Shore Angler vs. For-hire Angler Responses*

Variable	Model 7 (Shore)	Model 8 (For-hire)
$\sqrt{(\text{Cod kept})}$	0.190 (0.0702)***	0.304 (0.0208)***
$\sqrt{(\text{Cod released})}$	0.0911 (0.0774)	0.0650 (0.0209)***
$\sqrt{(\text{Haddock kept})}$	0.169 (0.0586)***	0.269 (0.0202)***
$\sqrt{(\text{Haddock released})}$	-0.0377 (0.0756)	-0.00579 (0.0220)
$\sqrt{(\text{Pollock kept})}$	0.107 (0.120)	0.105 (0.328)***
$\sqrt{(\text{Pollock released})}$	-0.172 (0.127)	-0.181 (0.0364)***
Trip length	—	0.124 (0.0212)***
(Trip length)^2	—	-0.00608 (0.00146)***
Trip cost	-0.00571 (0.00116)***	-0.00540 (0.000132)***
Opt-out	-0.249 (0.218)	-0.341 (0.0917)***
LR (χ^2)	113.8***	4818.7***
No. Obs.	3,045	36,106
No. Groups	134	1,080
MWTP Cod	\$7.86	\$13.29
	(1.88, 15.59)	(11.26, 15.16)
MWTP Haddock	\$6.96	\$11.75
	(2.35, 12.56)	(9.90, 13.40)
MWTP Pollock	\$4.41	\$4.59
	(-5.27, 15.48)	(1.94, 7.57)

***p < .01, **p < .05, *p < .10.

Alternative Econometric Specifications

Tables 33 and 34 list the results for the scale heterogeneity models. Larger scale parameters indicate smaller variances and steeper probability functions, which implies that the utility functions are better defined (see Figure 3 from Chapter 2). The scale parameter in the single-source heterogeneity model for direct experience, Model 9 (Table

33), is both statistically significant and positive. Anglers with direct experience have better defined utility functions with smaller variances, which is consistent with Model 5 (direct experience group model in Table 31). The direct experience scale parameter retains its magnitude and significance in the multiple-source heterogeneity model, Model 12 (Table 34), indicating significant differences in angler behavior, and the effect dominates all other scaled heterogeneity effects.

Table 33. *Scale Heterogeneity Models: Single Heterogeneity Source*

Variable	Model 9	Model 10	Model 11
$\sqrt{(\text{Cod kept})}$	0.202 (0.0154)***	0.296 (0.0199)***	0.306 (0.0206)***
$\sqrt{(\text{Cod released})}$	0.0368 (0.0136)***	0.0565 (0.0201)***	0.0700 (0.0208)***
$\sqrt{(\text{Haddock kept})}$	0.174 (0.0145)***	0.256 (0.0190)***	0.270 (0.0200)***
$\sqrt{(\text{Haddock released})}$	-0.00881 (0.0142)	-0.0120 (0.0210)	-0.00586 (0.0219)
$\sqrt{(\text{Pollock kept})}$	0.0763 (0.0214)***	0.126 (0.0312)***	0.107 (0.0325)***
$\sqrt{(\text{Pollock released})}$	-0.140 (0.0240)***	-0.211 (0.0345)***	-0.181 (0.0361)***
Trip length × For-Hire	0.114 (0.0134)***	0.183 (0.0180)***	0.127 (0.0206)***
(Trip length)^2 × For-Hire	-0.00595 (0.000914)***	-0.00978 (0.00128)***	-0.00625 (0.00143)***
Trip cost	-0.00364 (0.000169)***	-0.00550 (0.000130)***	-0.00546 (0.000132)***
Opt-out	-0.204 (0.0555)***	-0.199 (0.0812)**	-0.341 (0.0893)**
γ(Direct Experience)	0.503 (0.0440)***	—	—
γ(# Trips)	—	0.000130 (0.000489)	—
γ(Shore)	—	—	-1.502 (0.0594)**
LR (χ^2)	164.14***	0.1	50.38***
No. Obs.	39,151	39,151	39,151
No. Groups†	12,203	12,203	12,203
MWTP Cod	$13.04	$12.67	$13.23
	(11.10, 14.85)	(10.77, 14.42)	(11.38, 15.08)
MWTP Haddock	$11.25	$11.01	$11.66
	(9.48, 12.82)	(9.31, 12.53)	(9.85, 13.46)
MWTP Pollock	$4.93	$5.40	$4.60
	(2.35, 7.76)	(2.92, 8.16)	(1.84, 7.36)

*** $p < .01$, ** $p < .05$, * $p < .10$.
† No. Groups indicates the number of unique observations for the scale parameter variables.

The scale parameter for avidity in Model 10 (Table 33), as measured by number of trips reported for the previous fishing season, is not statistically significant and the single-source heterogeneity model is not well-estimated; however, it is statistically significant in Model 12 (Table 34), the multi-source heterogeneity model. The sign of the avidity parameter is negative, indicating that more avid anglers have greater variance in behavior and individual trips are less likely to affect the overall utility of their fishing season. For highly avid anglers, this parameter dominates all other heterogeneity effects. Less avid anglers probably consider the attributes of an individual trip more carefully because individual trips have more weight on the total utility of a fishing season.

Table 34. *Scale Heterogeneity Models: Multiple Heterogeneity Sources*

Variable	**Model 12**
$\sqrt{(\text{Cod kept})}$	0.201 (0.0227)***
$\sqrt{(\text{Cod released})}$	0.0431 (0.0137)***
$\sqrt{(\text{Haddock kept})}$	0.172 (0.0206)***
$\sqrt{(\text{Haddock released})}$	-0.00357 (0.0139)
$\sqrt{(\text{Pollock kept})}$	0.0627 (0.0217)***
$\sqrt{(\text{Pollock released})}$	-0.116 (0.0255)***
Trip length × For-Hire	0.0833 (0.0150)***
$(\text{Trip length})^2$ × For-Hire	-0.00409 (0.000971)***
Trip cost	-0.00346 (0.000335)***
Opt-out	-0.266 (0.0615)***
γ(Direct Experience)	0.517 (0.0460)***
γ(# Trips)	-0.00200 (0.000703)***
γ(Shore)	-0.517 (0.102)***
γ(Income)	0.0357 (0.00814)***
γ(Age)	-0.102 (0.0222)***
γ(Non-White)	-0.183 (0.0627)***
LR (χ^2)	270.83***
No. Obs.	39,151
No. Groups†	12,203
MWTP Cod	$13.65
	(11.80, 15.51)
MWTP Haddock	$11.72
	(9.90, 13.54)
MWTP Pollock	$4.27
	(1.50, 7.05)

***p < .01, **p < .05, *p < .10.
† No. Groups indicates the number of unique observations for the scale parameter variables.

Model 11 (Table 33) includes a scale parameter for shore anglers. The coefficient is negative and statistically significant and consistent with Table 32, indicating that the utility of shore anglers is not as well formed for this fishery compared to anglers from other modes. Coincidentally, the parameter for shore anglers is equal to but opposite in sign of the scale parameter for direct experience in Model 12 (Table 34), meaning that direct experience nullifies the shore effect (and vice versa).

Model 12 (Table 34) also captures heterogeneity in respondent demographics. Older anglers have smaller scale parameters. The apparent increase in randomness for this subpopulation could be attributed to decision-making processes dictated by factors not included in this study, such as the opportunity to socialize or general enjoyment of the outdoors. Non-white anglers also have more randomness, but this result may be biased by the skewed ethnic distribution of the dataset because only 7% of the respondents reported being non-white. Higher income levels are correlated with more defined utility functions. Higher income anglers are more certain of their choices because their option sets are broader and are more experienced with a wider range of alternatives.

Figures 4 through 7 illustrate the effect of the scale parameters on choice probabilities, with specific demonstrations of age, income, and avidity effects in Figures 5 through 7. The graphs exhibit signs of fixed-point theorem with expected error of zero.

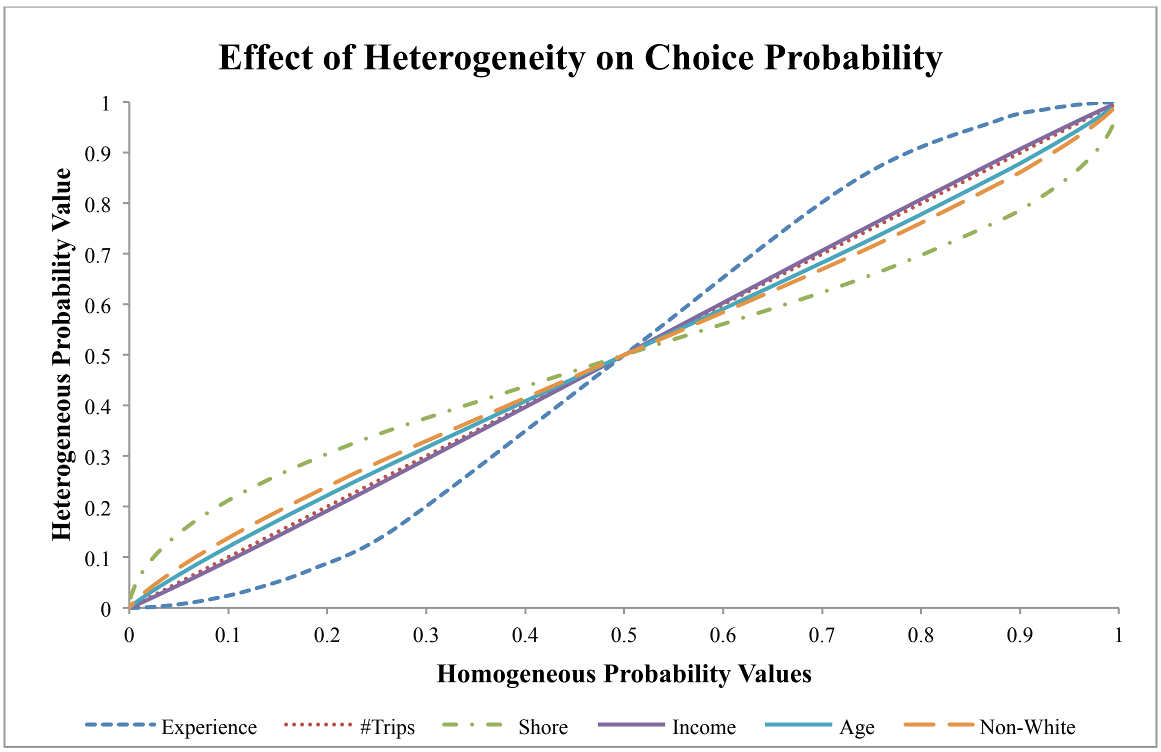


Figure 4. Changes in choice probability after accounting for sources of heterogeneity.

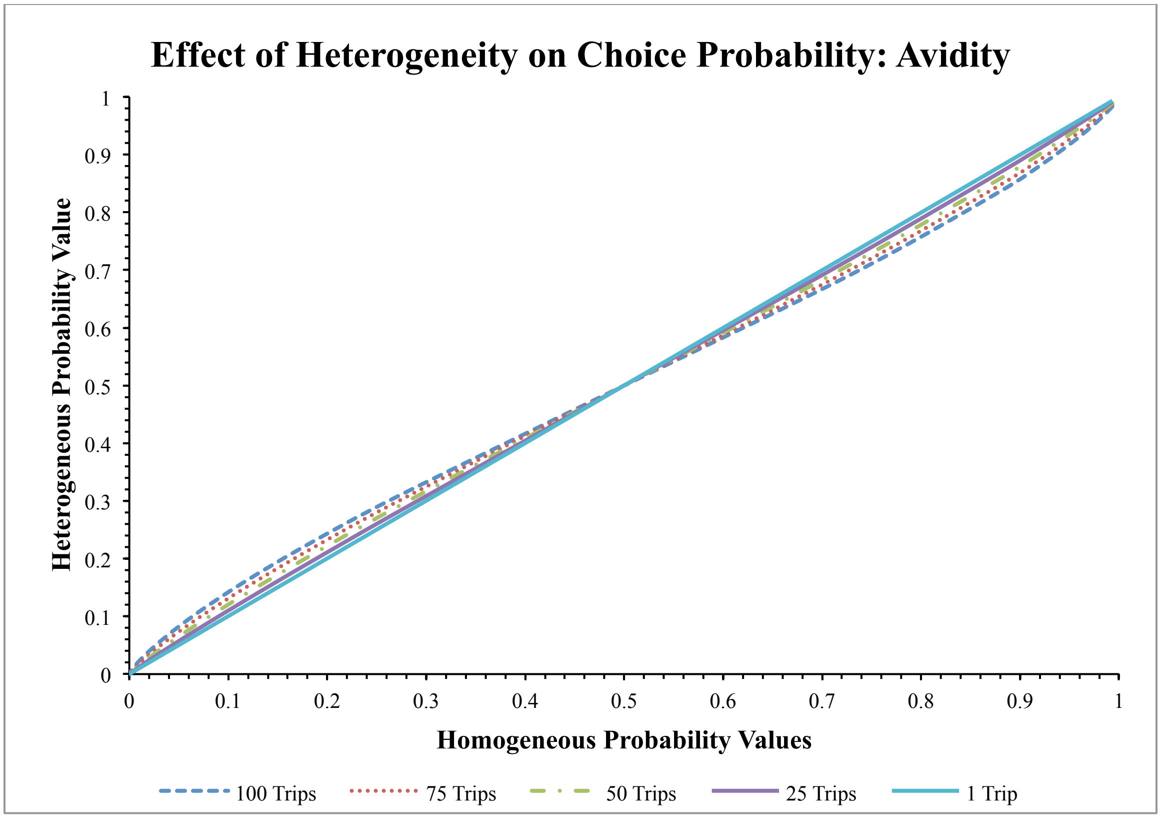


Figure 5. Changes in choice probability after accounting for angler avidity (# trips).

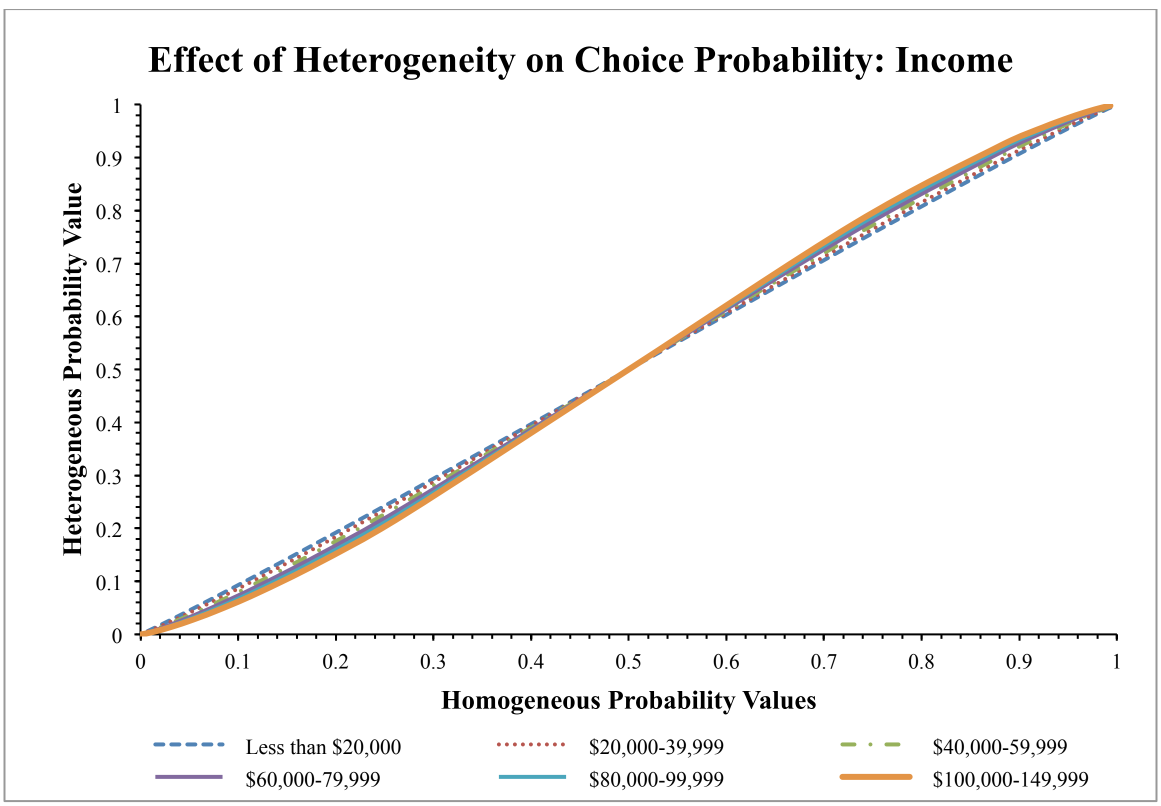


Figure 6. Changes in choice probability after accounting for respondent income.

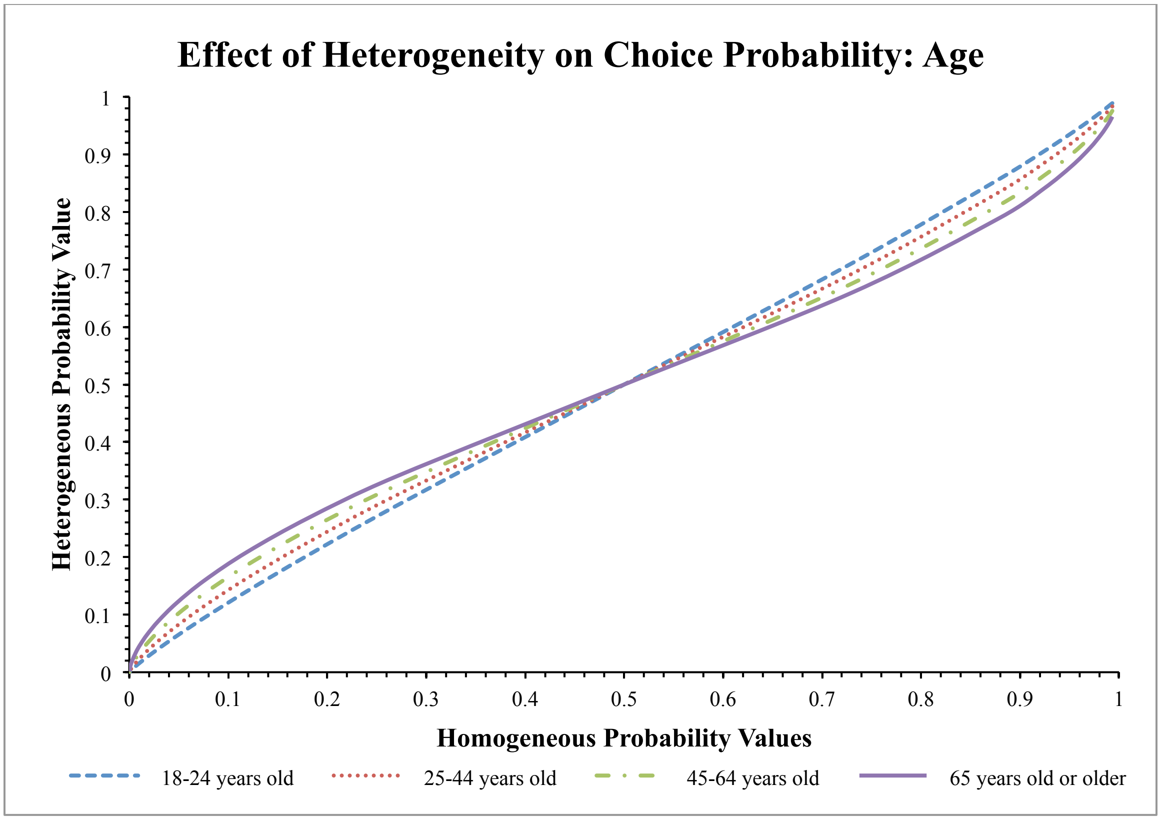


Figure 7. Changes in choice probability after accounting for respondent age.

The MWTP confidence intervals computed for Model 12 overlap with those of the non-heterogeneous model (Model 4). The mean and confidence intervals for cod and haddock are slightly higher in Model 12 than in Model 4, but the opposite is true for pollock. Though scale parameters do not affect MWTP, the estimated parameters for the heterogeneous model are not equal to the parameters of the homogeneous model.

<u>*Conclusion*</u>

Stated preference models of recreational angling in the literature vary greatly despite having many commonalities in attributes and theory. This chapter addressed several different specifications using CE data collected for a recreational meat fishery in New England. Nonlinear utility specifications allow for diminishing marginal utility of catch and fit this particular dataset better than linear utility specifications. Anglers with first-hand encounters of the species in the survey have more defined utility functions, as seen in the greater divergence in parameter estimates for the three species, which affects outcomes of demand analysis. Additionally, the confidence intervals on MWTP for anglers with direct experience are much tighter and have higher mean values than for anglers who have not targeted or caught any of the three species in recent history. Though anglers appear to assign value to all fish species, studies incorporating results from all survey respondents will likely have lower and more variable valuation estimates than those that exclusively target anglers with relevant fishery experience. This result is also supported by the comparison of models using shore anglers versus boat anglers. For this fishery, shore anglers are unlikely to encounter the species and there is a definite divergence in both estimated parameters and welfare measures between the two modes. Including respondents from irrelevant modes or those with no direct species experience is problematic for policy analysis as the meaning of obtained values is indeterminate.

Scale heterogeneity models show that there are structured differences between respondent types based on mode, experience, avidity, and socioeconomic demographics. Whereas the MWTP results for additional fish kept in the heterogeneous models are not statistically different from those for homogeneous models, the parameter estimates are significantly affected by differences in angler avidity, species familiarity, mode, and socioeconomic demographics. Researchers interested only in MWTP estimates can use homogeneous models without significant loss of information; however, any analyses requiring knowledge of demand changes or attribute sensitivity require the use of heterogeneous models. Accounting for sources of heterogeneity will significantly affect assessments of recreational fisheries.

Recreational values of species caught primarily to supplement dinner tables should be approximately equal to market values for those fish (Wheeler & Damania, 2001). Price comparisons of recreational values and ex-vessel prices are commonplace, but incorrect. Ideally, price comparisons should be made between recreational values and retail values for these fish because anglers are assumed to purchase fish for the dinner table if an inadequate number of fish are caught during the trip; however, retail data for these fish are not available. A recent FAO document estimated that the wholesale price for cod in the US is approximately 67% of the retail price (Gudmundsson, Asche, & Nielsen, 2006). The wholesale value of cod based on the NMFS *Fishery Market News* reports is generally $1-$3 per pound. At an average recreational catch size of 7 lbs., the approximate retail value per fish kept would be $11-$31 using the FAO conversion, which is slightly higher than the ranges obtained using the stated preference data for an average-sized catch basket. Haddock, which hovers around $1.50 per pound wholesale, and pollock, at $0.50 per pound, are both approximately $7 for the average fish (3 lbs. and 10 lbs., respectively). The recreational values computed using the stated preference data are much higher for haddock ($7-$12/fish), suggesting additional recreational values associated with haddock fishing or the perception that haddock is scarcer than cod.

This chapter also addressed the theoretical debate in the literature regarding the inclusion or exclusion of regulatory attributes in modeling stated preference discrete choice data. Though stated preference surveys present anglers with information regarding potential management scenarios, fisheries management tools should not be included directly as explanatory variables in models of fishing behavior. The inclusion of such variables in stated preference models produces perplexing and inexplicable results. Bag limits and size limits result in nonsensical parameter estimates or may appear to be misinterpreted by respondents as quality variables in these regressions. Regulations (minimum size limits and bag limits) bind to at random as a consequence of the uncertainties in trip catch and should therefore enter the angler's utility function indirectly through changes in the distribution of catch. Additionally, most anglers consider keep, total catch, cost, trip length, and weather to be the most important aspects of recreational fishing.

Traditional stated preference models do not address fishing preferences properly by including regulations in empirical specifications because the supporting theory is not realistic. Stated preference modelers should consider that few revealed preference studies of recreational angling explicitly incorporate management terms despite the presence of variations in regulations and follow those examples more closely. Admittedly, some applications do not have sufficient variation in regulations across observed time periods to generate solid revealed preferences, and revealed preference data does not allow for observations of new and proposed regulations; however, these facts alone do not necessarily support the explicit inclusion of regulations in stated preference models. Chapter 4 outlines a bioeconomic method for estimating the impact of regulation changes without requiring a RUM that explicitly includes management variables.

Chapter 4: Policy Analysis Through Catch Simulation

Introduction

Policy makers continually strive to understand the effects of fisheries management on recreational angling because consequences are often difficult to measure. Whereas commercial fisheries in the United States are subject to mandatory data reporting and even compulsory observation, recreational fisheries data are often scarce due to reliance on limited collections of voluntary information. Furthermore, recreational angling involves choosing among alternatives with random outcomes. Attributes of recreational angling activities, chiefly catching fish, are known only after the fishing trip has occurred. Inherent differences in angler skills and environmental factors may influence results, but catch is mostly random. The stochastic nature of the catch on recreational angling trips complicates economic analyses of fishery regulations.

Traditionally, recreational fisheries have been managed using a combination of season or area closures, bag (creel) limits, and minimum size limits. These policy tools are designed to control catch, but also may induce changes in angler behavior; however, quantifying the results can be challenging. Because catch is random, bag and size restrictions are not necessarily binding on every trip. Anglers may not be affected by regulations on their fishing trips because that depends entirely on the fish that are caught, which increases the difficulty of measuring the economic impacts of regulation changes.

Four possible angler responses to changes in regulation levels exist. Anglers may not be affected by regulations either before or after the change. For example, let the bag limit be 10 fish in March and 15 fish in August. An angler who only catches 7 fish during trips in both March and August is never affected by bag limits and the bag limit has no impact on the angler's behavior. A second possibility is that anglers are always affected by regulations. If the angler always catches 20 fish, the angler would be affected by the decreased restrictions and respond accordingly. It is also possible that anglers are affected by only one set of regulations and not the other. An angler that catches 7 fish in March and 20 fish in August might feel that regulations became more stringent because the bag limit did not bind in March (no effect), but did bind in August (affected trip outcomes). On the other hand, an angler that catches 12 fish in March and August would perceive that regulations were more relaxed because the regulations were binding on the first trip but not the second. Outcomes of regulation changes depend on the degree to which regulations bind and the impact of binding regulations on angler behavior.

Many economists have tried to assess the economic impact of regulatory changes in recreational fishing (Aas et al., 2000; Gentner & Lowther, 2002; Gillis & Ditton, 2002; Hicks, 2002; Layman, Boyce, & Criddle, 1996; Lew & Seung, 2010; Massey, Newbold, & Gentner, 2006; McConnell, Strand, & Blake-Hedges, 1995; Oh et al., 2005; Paulrud & Laitila, 2004; Olaussen & Skonhoft, 2008; Ruliffson & Homans, 1999; Schuhmann, 1998; Scrogin, Boyle, Parsons, & Plantinga, 2004; Whitehead & Haab, 1999; Woodward & Griffin, 2003). Stated preference methods are popular due to inherent flexibilities. Researchers are free to pick any number of attributes and attribute level combinations, and data deficiencies are easily addressed. Angler preferences and behaviors for virtually any application can be analyzed using a wide range of attribute levels. In stated preference studies, the economic loss (or gain) from a change in regulations is typically estimated using the relative value of a marginal change in regulations from an all-encompassing conjoint model (Aas et al., 2000; Gentner & Lowther, 2002; Gillis &

Ditton, 2002; Hicks, 2002; Lew & Seung, 2010; Massey et al., 2006; Oh et al., 2005; Paulrud & Laitila, 2004); however, as demonstrated in the previous chapter, complications often arise when estimating coefficients for bag and size limits. The role of regulation in angler choices among alternatives should be limited to the impact on the primary services valued by anglers. When anglers care about the number of fish they keep, a bag limit would affect their choices if it were constraining. If anglers care about the catch only, then bag and size limits would have no impact.

Revealed preference methods may not be feasible for estimating the impacts of regulations on behavior because policy variations may not be available in historic data. However, a few studies have addressed both policy changes and randomness in catch using revealed preference RUMs (McConnell et al., 1995; Schuhmann, 1998; Whitehead & Haab, 1999). McConnell et al. (1995) postulated that regulations affect angler utility indirectly. Rather than directly including policy changes in the RUM, they suggested that bag limits affect the distribution of catch. The angler's expected mean catch is altered through changes in the shape of the distribution curve imposed by different regulations, thereby altering angler behavior under the RUM. The economic impact of a change in bag limits can be obtained by evaluating the RUM for different distributions of catch.

This chapter expands the framework outlined in McConnell et al. (1995) to include minimum size regulations. The effects of fisheries management on random catch are examined. Consequences of policy changes are analyzed using simulations of fish catch; however, unlike other catch simulation studies, the model used in this chapter is based on a RUM from a stated preference survey and incorporates age-class biomass predictions from stock assessment tools.

Randomness, Catch, and the Utility Function

Typically, applications of RUMs assume that attributes are known with certainty. Fishery applications are problematic because catch outcomes are random and not known with certainty *ex ante*. This problem was first confronted by Bockstael and Opaluch (1983) and Opaluch and Bockstael (1984), who assumed commercial fishermen would know the parameters of a distribution of returns. To account for the randomness, McConnell et al. (1995) and Schuhmann (1998) used expected catch in their RUMs and computed welfare measures based on changes in the underlying distribution. An adaptation of this concept is presented as an alternative to traditional stated preference measures for regulation valuation.

The traditional RUM assumes that angler utility is a function of income, trip attributes, and costs, and is well described in the literature. For illustration purposes, let utility be a known function ($V(\cdot)$) of fish caught c, observable trip attributes z, and some unobservable characteristics, ε:

$$U = V(c, z) + \varepsilon. \tag{4.1}$$

Because anglers are making decisions *ex ante* and catch is random, the actual utility function is based on an expected level of catch:

$$U = V(E[c], z) + \varepsilon, \tag{4.2}$$

where catch follows some probability distribution function that varies based on stock abundance, fishing mode, gear, bait, weather, temperature, angler experience, total harvest, and other environmental factors. This function is a utility of expectations and not an expectation of utilities. Expected utility ($E[U(x)]$) is the utility of an economic agent facing uncertainty, whereas a utility of expectations ($U(E[x])$) implies the agent's beliefs or assumptions about a future or possible good. The two are not necessarily equal; the relationship between E[U(x)] and U(E[x]) depends on the risk preferences of the individual. From Jensen's inequality, E[U(x)] ≤ U(E[x]) for concave functions (risk-averse behavior). The reverse is true for convex functions (risk-seeking behavior). When anglers are risk neutral, the relationship between E[U(x)] and U(E[x]) is linear, and maximizing expected utility is equivalent to maximizing expected catch.

Though many studies of fishermen assume that fishermen behave rationally by maximizing expected utility (Wilen, Smith, Lockwood, & Botsford, 2002), there is evidence that fishing behavior is inconsistent with expected utility theory (Eggert & Martinsson, 2004; Holland, 2008). Many anglers are risk seeking (Eggert & Lokina, 2007; Smith, 2000) and appear to maximize expected value rather than expected utility (Salas & Charles, 2007). For the purposes of this study, anglers targeting cod and like species are assumed to be risk neutral, though the framework accommodates risk-seeking behavior (expected value maximization).

Assuming that catch is important to anglers, Equation 4.2 does not explain regulatory impacts unless bag limits are always binding, in which case the marginal value of a change in the number of fish caught is equal to the value of a marginal change in the bag limit. Bag limits and minimum size limits have no discernible effect on either the catch distribution or other trip elements if the regulations are not perceived to be binding, which is the case for most recreational fisheries. Additionally, an angler's utility for fish kept may not equal the utility of a fish released. For meat fisheries, the value of a fish kept is much higher than that of a fish released. In competitive sport fisheries, catch is not kept and the angler's utility is based solely on the number of fish released. In most cases there will be some value associated with both keep and release, though the dominant term in the utility function will depend on the fishery type. Dividing the total catch of fish from Equation 4.2 into the number of fish kept k and the number of fish released r gives

$$\mathrm{U} = \mathrm{V}(\mathrm{E}[k], \mathrm{E}[r], z) + \varepsilon. \tag{4.3}$$

Figure 8 demonstrates how bag limits affect the distributions for the number of fish kept and released, but not catch. Keep is defined as the lower end of the catch distribution truncated by the bag limit, and the upper end of the catch distribution defines the distribution of fish released.

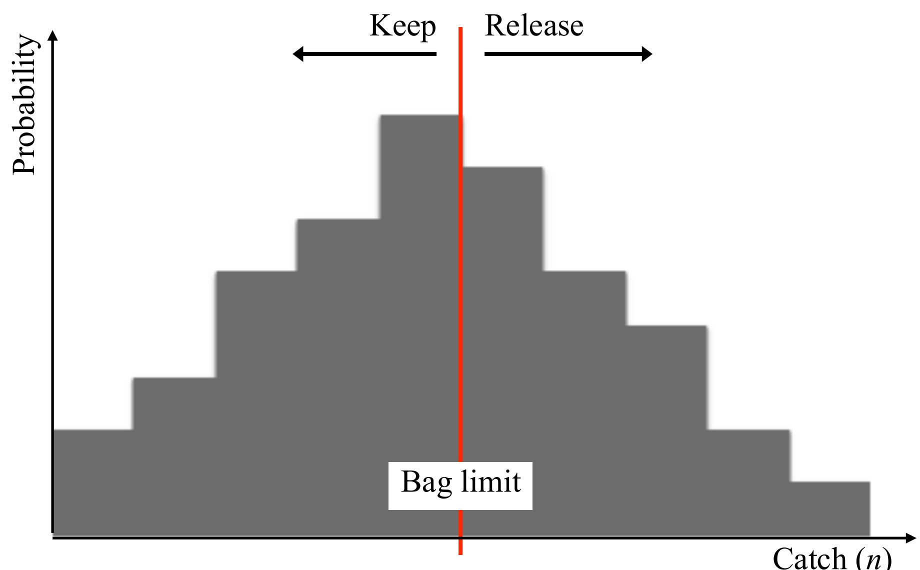


Figure 8. Impact of bag limits on catch number distribution.

Formally, let the number of fish caught be represented by random variables n, and the distribution of catch numbers be characterized by the probability distribution function $f(n)$ and cumulative distribution function $F(n)$. Then the total expected catch is

$$\mathrm{E}[c_n] = \int_0^N n f(n)\, dn = \mathrm{E}[k_n] + \mathrm{E}[r_n]. \tag{4.4}$$

Technically, the distribution of catch is discrete, $\sum_{n=0}^{N} f(n)$, but a continuous function will be used for mathematical simplicity without any loss of generality. The sum of expected keep and expected release must equal the expected catch. If expected keep decreases, expected release must increase and vice versa. The expected number of fish kept given a bag limit B is

$$\begin{aligned}\mathrm{E}[k_n] &= \mathrm{E}[k|n < B]\,\Pr(n < B) + \mathrm{E}[k|n \geq B]\,\Pr(n \geq B)\\ &= \mathrm{E}[c_n|n < B]\,\Pr(n < B) + B\,\Pr(n \geq B)\end{aligned} \tag{4.5}$$

When the number of fish caught is greater than the bag limit, keep equals the bag limit. Equation 4.5 can be rewritten as

$$\begin{aligned}\mathrm{E}[k_n] &= \frac{\int_0^B f(n)\cdot n\,dn}{F(B)} \cdot \int_0^B f(n)\,dn + B\int_B^N f(n)\,dn\\ &= \int_0^B nf(n)\,dn + B\int_B^N f(n)\,dn.\end{aligned} \tag{4.6}$$

Using Leibniz's integral rule, the impact of bag limits on expected keep is thus

$$\frac{\partial}{\partial B}(\mathrm{E}[k_n]) = Bf(B) + \int_B^N f(n)\,dn - Bf(B) = \int_B^N f(n)\,dn. \tag{4.7}$$

Bag limits have little effect on expected keep when B is large because Pr(n > B) is small and catch becomes the constraining factor.

For release, the expected number of fish is simply

$$\mathrm{E}[r_n] = \mathrm{E}[c_n] - \mathrm{E}[k_n] = \int_B^N nf(n)\,dn - B\int_B^N f(n)\,dn. \tag{4.8}$$

The impact of bag limits on expected release is

$$\frac{\partial}{\partial B}(\mathrm{E}[r_n]) = -\int_B^N f(n)\,dn, \tag{4.9}$$

which demonstrates that bag limits have no effect on expected catch:

$$\frac{\partial}{\partial B}(\mathrm{E}[c_n]) = \frac{\partial}{\partial B}(\mathrm{E}[k_n]+\mathrm{E}[r_n]) = 0. \tag{4.10}$$

A similar illustration can be used for minimum size limits. Let catch size be a continuous random variable s with probability density function $g(s)$ and cumulative distribution function $G(s)$. The probability that catch exceeds the minimum size limit is

$$\Pr(s > MS) = \int_{MS}^{\infty} g(s)ds. \tag{4.11}$$

This notation assumes anglers know regulation levels with certainty. If anglers are unsure of the regulations, then *MS* is defined by a pdf as opposed to an exact point limit:

$$\Pr(s > MS) = \int_0^{\infty}\left(\int_s^{\infty} g_s(\theta)d\theta\right)g_{MS}(s)ds. \tag{4.12}$$

If *s* and *MS* are correlated, then a bivariate pdf is required:

$$\Pr(s > MS) = \int_0^{\infty}\int_s^{\infty} g_{s,MS}d\theta ds. \tag{4.13}$$

A minimum size regulation defines the size of fish kept by imposing a truncation of the lower end of the catch size distribution (see Figure 9).

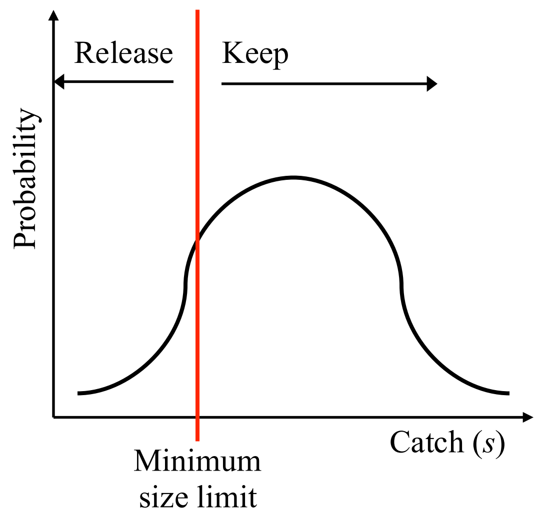


Figure 9. Impact of minimum size limits on catch size distribution.

Keep, for a single fish based on size, is defined as

$$k_s = \begin{bmatrix} 1\ if\ catch\ size \geq MS \\ 0\ otherwise \end{bmatrix}. \tag{4.14}$$

The expected keep based on size is thus

$$\mathrm{E}[k_s] = 1 \cdot \Pr(s \geq MS) + 0 \cdot \Pr(s < MS) = \int_{MS}^{\infty} g(s)ds. \tag{4.15}$$

The impact of a minimum size on expected keep is

$$\frac{\partial}{\partial MS}(\mathrm{E}[k_s]) = \frac{\partial}{\partial MS}\left(\Pr(s \geq MS)\right) = -g(MS). \tag{4.16}$$

Similarly, release in terms of fish size is defined as

$$r_s = \begin{bmatrix} 1\ if\ catch\ size < MS \\ 0\ otherwise \end{bmatrix} \tag{4.17}$$

and expected release is

$$\mathrm{E}[r_s] = 1 \cdot \Pr(s < MS) + 0 \cdot \Pr(s \geq MS) = \int_0^{\mathrm{MS}} g(s)ds. \tag{4.18}$$

The impact of a minimum size on expected release is

$$\frac{\partial}{\partial MS}(\mathrm{E}[r_s]) = \frac{\partial}{\partial MS}\big(\Pr(s < MS)\big) = g(MS). \tag{4.19}$$

Again, the minimum size has no effect on the expected catch:

$$\frac{\partial}{\partial MS}(\mathrm{E}[c_s]) = \frac{\partial}{\partial MS}(\mathrm{E}[k_s] + \mathrm{E}[r_s]) = 0. \tag{4.20}$$

Assuming catch size and catch number are independent distributions, expected keep and release can be described as

$$\begin{aligned} \mathrm{E}[k] &= \mathrm{E}[k_s]\mathrm{E}[k_n] = \int_{MS}^{\infty} g(s)ds \left(\int_0^B nf(n)\,dn + B\int_B^N f(n)\,dn\right) \\ \mathrm{E}[r] &= \mathrm{E}[r_s]\mathrm{E}[r_n] = \int_0^{MS} g(s)ds \left(\int_B^N nf(n)\,dn - B\int_B^N f(n)\,dn\right). \end{aligned} \tag{4.21}$$

The effect that changes in bag limits have on an angler's utility is

$$\frac{\partial}{\partial B}\mathrm{U}(\cdot) = \frac{\partial}{\partial B}V(\mathrm{E}[k], \mathrm{E}[r], z). \tag{4.22}$$

For a simple linear-in-parameters, linear-in-catch specification such as

$$V = \beta_1\mathrm{E}[k] + \beta_2\mathrm{E}[r] + \beta_3 z, \tag{4.23}$$

Equation 4.22 becomes

$$\begin{aligned} \frac{\partial}{\partial B}U(\cdot) &= \beta_1\mathrm{E}[k_s]\frac{\partial}{\partial B}\mathrm{E}[k_n] + \beta_2\mathrm{E}[r_s]\frac{\partial}{\partial B}\mathrm{E}[r_n] \\ &= \int_B^N f(n)\,dn\left(\beta_1\int_{MS}^{\infty} g(s)ds - \beta_2\int_0^{MS} g(s)ds\right). \end{aligned} \tag{4.24}$$

Similarly, the impact of a change in minimum size limits on an angler's utility is

$$\begin{aligned} \frac{\partial}{\partial MS}U(\cdot) &= \beta_1\mathrm{E}[k_n]\frac{\partial}{\partial MS}\mathrm{E}[k_s] + \beta_2\mathrm{E}[r_n]\frac{\partial}{\partial MS}\mathrm{E}[r_s] \\ &= \begin{pmatrix} B(\beta_1 - \beta_2)\int_B^N f(n)\,dn \\ +\beta_1\int_0^B nf(n)\,dn - \beta_2\int_B^N nf(n)\,dn \end{pmatrix} g(MS). \end{aligned} \tag{4.25}$$

Incorporating expected keep and release in the angler's utility function expresses the stochastic nature of catch and still allows quantitative measurements of changes in regulations to affect utility. This model assumes that the distributions for catch size and number are independent, perhaps oversimplifying true stock dynamics. These fish typically school by size in specific locations based on the ecological requirements of each life-cycle stage. Additionally, the model does not explicitly incorporate high-grading, the practice of selectively harvesting fish by discarding non-optimal catch even when the fish can be legally kept. A model with high-grading requires inter-temporal correlations in the utility function allowing anglers to compare new fish with previous catch, which is beyond the scope of this study.

Aggregate Outcomes (Short-run and Long-run Modeling)

The framework outlined in the previous section addresses the effect of regulation changes on angler behaviors for individual trips but describes aggregate outcomes for the fishery poorly. Simply expanding out the utility function for N fishing trips might capture some welfare effects but provides little information regarding effort shifts. Additionally, historic data are inadequate for determining the impact of regulations on angler behavior in the New England groundfisheries. To address both insufficiencies, a simulation process is used to quantify the effects of different regulatory and biological scenarios on effort and angler welfare. This study extends works like Schuhmann (1998) and Woodward and Griffin (2003) by examining minimum size limits using age-class distributions of fish. Rather than assuming distributions of catch, the simulations apply parameters from stock assessment projections and historic catch records. The stated preference analyses from Chapter 3 form the basis of the behavioral model used in the simulation exercises, which demonstrates an alternative use of stated preference surveys and compensates for lack of historic data. Additionally, the computations of total effort and consumer welfare offer more insight than mean expected compensating variation per trip for changes in catch.

The simulation has global fishery controls and individual trip level calculations. During actual fishing trips, anglers bait lines, throw the lines into the water, and pull out the lines when fish bite. A fishing line that captures a fish is known as a successful cast. An angler interested in keeping fish to supplement the dinner table continues this fishing process until either the bag limit is reached or the opportunity cost of time exceeds the value of the fishing trip. The simulation replicates angler behavior by mimicking this fishing process and then aggregates across trips to compute global outcomes.

For each trip, the simulation randomly assigns each computer angler a value for the maximum possible number of successful casts from a distribution. Because the average number of successful casts thrown per hour cannot be derived using available information, historic catch data serves as the proxy for fishing success per trip. MRFSS data from the last five years are used to generate catch distributions for each species, from which random numbers are drawn. Each simulated angler is assigned one draw from the catch distribution as the total number of successful casts. The program then assigns a size to each successful cast based the biomass projections for each age-class-length of fish overlaid with an appropriate age-class catchability factor. The resulting parameters form the basis for determining the size of fish in each angler's catch basket.

The program simulates the fishing action by "catching" fish, or retrieving the pre-assigned fish from the successful casts. The computer angler sorts "caught" fish into keep and release buckets. This process is continued until either the bag limit binds or all successful casts for the trip are used. The probability that the computer angler takes the simulated trip is determined at the end of the iteration using the total number of fish kept and released on the trip. RUMs estimated using the stated preference survey determine simulated angler behavior. If the trip is considered acceptable ($\Pr(choice = U_{trip}) > 50\%$), the value of the trip is calculated using the MWTP derived from the stated preference survey analysis and added to the total welfare measure for the fishery. The algorithm for one trip iteration is outlined in Figure 10.

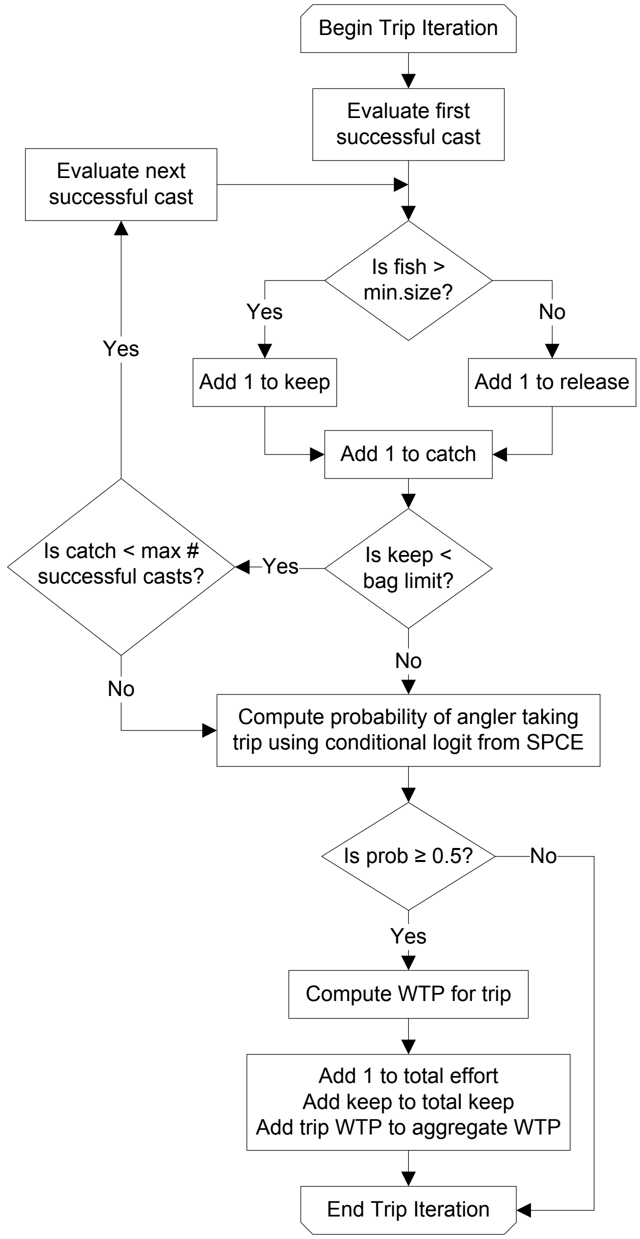


Figure 10. Trip algorithm (single simulation iteration).

The computer simulates trips until either the total allowed recreational harvest is reached (season closure) or the allotted number of iterations has been taken (see Figure 11). The biological model that determines the total allowed recreational harvest in the simulation is the model policy makers use to set annual catch limits (ACLs), and the ACLs used in the simulation are actual projections for future years. The total number of iterations allowed for each simulation is based on the average estimated effort for the most recent five years for the study area, which is approximately 500,000 fishing trips.

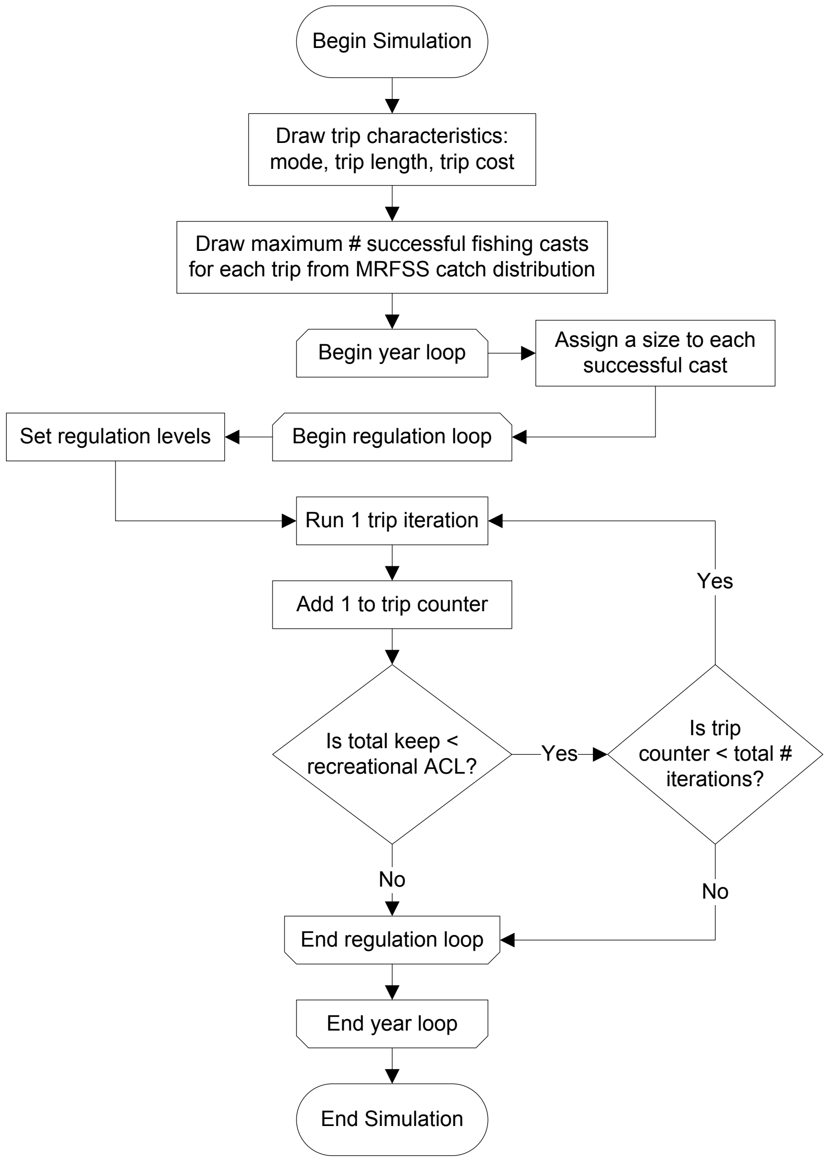


Figure 11. Simulation algorithm (global controls).

The simulation process replicates the mix of angler modes, trip types, and fishing experiences that could actually occur for future policy scenarios. To ensure comparability of results, the program randomizes all trip-specific variables (trip length, trip cost, mode, successful casts) only once across all scenarios in all years. Fish size is assigned for all successful casts once per year based on the biological model. Because the biological parameters are specific to each year, fish sizes must be drawn independently for each year to capture changes in age-class distributions.

Trips are always taken in the same order with the same characteristics but under different regulatory and biological restrictions. Forcing most of the simulation system to be invariant enables explicit comparisons of changes in the distribution of keep and release under various biological, bag limit, and size restriction combinations. To illustrate this concept, consider the following example. Trip 53 is always the 53rd trip in the simulation. The fishing mode will always be party boat, and the maximum number of successful casts for cod is five fish for all simulations in all years on trip 53. The sizes of the cod caught in 2011 for trip 53 will always be 19", 23", 26", 29", and 12", in that order, but the cod in 2012 for trip 53 are 18", 20", 22", 16", and 17" in size because of changes in the underlying age-class distribution. A minimum size limit of 20" and a five fish bag limit in 2011 will result in three kept fish and two discards on trip 53, but only two fish are kept in 2012.

Most bioeconomic models incorporate some type of catchability factor that translates raw biomass into landed fish. Typically, catchability is defined as the proportion of stock removed by one unit of fishing effort, or

$$C/E = sqN \tag{4.26}$$

where C represents catch, E is fishing effort, s is a constant related to a particular type of fishing gear, q is the catchability coefficient, and N is the population size. Differences in environmental preferences and biological behaviors require separate catchability estimates for each age class to accurately model probable recreational catch; however, the total population size and effort for each age class are not known with certainty so q cannot be solved for explicitly. For the purposes of this simulation, the catchability coefficient is important only for obtaining the distribution of catch in terms of fish size for each ecological scenario.

An approximation based on historic catch is generated without explicitly solving for the catchability coefficient, assuming that s is identical across all fishing gears and modes. Historic total and recreational catch are known for each age-class because this information is compiled for stock assessments and virtual population analysis (VPA). Tables detailing catch by age-class and total biomass estimates for each stock are available in several stock assessment reports by the NEFSC (Mayo, Shepherd, O'Brien, Col, & Traver, 2009; Northeast Fisheries Science Center, 2008a, 2008b). Rewriting Equation 4.23, the ratio of the two types of catch for a representative age-class is

$$\frac{C_{rec}}{C_{total}} = \frac{E_{rec}s_{rec}q_{rec}N}{E_{total}s_{total}q_{total}N} = \frac{E_{rec}s_{rec}q_{rec}}{E_{total}s_{total}q_{total}}. \quad (4.27)$$

Assuming that effort is static, the catchability ratios from Equation 4.22 can be used to recalibrate the biomass estimates and generate a new age-class distribution that approximates some measure of angler success. The biomass distribution is multiplied by the catchability ratios to obtain a new distribution reflecting probable angler successes for catching fish of particular sizes in each year. All simulated catch sizes are assigned to successful casts using this method.

Angler utility is affected by catch size as in other bioeconomic simulation studies (Woodward & Griffin, 2003); however, this model assumes that fish size is only important for determining whether or not the fish can be legally kept. Because size regulations are published in inches, age-class distributions are converted into lengths using age-length distribution keys for each species. The age-length keys are specific to ecological conditions and generated using advanced biological modeling algorithms.

Several assumptions are made for simplicity and to reduce computing time. Each trip is computed for one representative angler who has no repeat trips (or for whom the repeated trips have identical preference structures). Because the New England groundfishery is a meat fishery, it is assumed that all simulated anglers will fish until the legal bag limit is reached and no more. This does not mean that every simulated angler gets the bag limit; trips are allowed to have different numbers of catch, but the simulation limits high-grading. To prevent simulated anglers from high-grading, trips are terminated early (before all successful casts are used) if the trip has an unusually high number of legal-sized fish. For example, if there are seven possible successful casts on a trip with cod sizes 26”, 27”, 28”, 25”, 26.5”, 29”, and 12”, in that order, and the regulations restricts keep to three cod 22” or larger, then the program terminates the trip after the 28” fish. This simulated angler would only keep three fish instead of catching seven fish and discarding four. The termination mimics for-hire fishing behavior in this fishery because most for-hire captains cease fishing activities once the legal limit has been reached. Though there are cases where captains allow fishing beyond the legal limit and sell off excess catch, such practices are the exception rather than the norm in this fishery. The assumption of no high-grading introduces minimal error because analysis of historic release data shows that less than 1% of recreational anglers surveyed in the past 10 years reported high-grading for haddock, and fewer than 3% reported high-grading for cod.

Anglers are also assumed to view the biological condition of the fishery as identical throughout the year and have no temporal discounting as was assumed in the study by Woodward and Griffin (2003). Management efforts are restricted to changes in bag and size limits, and it is assumed that the policies are uniform across all modes.

Though the system will induce early termination (season closure) if ACLs are exceeded, none of the scenarios reached that limit. This study does not consider advanced management tools such as sectors and transferable quotas.

The biological and management conditions of the fishery are assumed to be static over the course of a year and spatially identical, which is inconsistent with seasonal age-class adjustments due to spawning and migration patterns. The assumption is necessary to generate enough information to accurately predict the number of fish by size. Locational delineation was not possible because the biological models were not estimated on a very fine spatial scale.

The simulations are not as sophisticated or as dynamic as the General Bioeconomic Fisheries Simulation Model (GBFSM) developed by Woodward and Griffin (2003) as there is no biofeedback loop where changes in catch one year affect the stock biomass in the following year. The recreational landings of cod and haddock are usually not high enough to affect species biomass (personal communication, S. Steinback, March 18, 2011), so this deviation from reality is insignificant; however, the ACLs specified in the biological model for each year affect projections of future biomass, so it is assumed that the fishery always achieves the ACL. Because the recreational allocation of ACLs are not reached in any of the scenarios, the simulation implicitly assumes that the commercial sector will always over-harvest, causing the fishery to reach the total ACL in each year. Short-run economic outcomes are easily estimated for each year using the simulation, and, assuming that the ACLs are binding, long-run economic outcomes can be obtained by combining simulation outputs across years.

Data

Angler behavior is simulated using a RUM derived from the stated preference survey. The previous chapter characterized the angler maximization problem for Northeast groundfish using several different specifications of the utility model. This chapter continues the evaluation of three of those models using simulations. The first model examined in this chapter is the LIC utility model from Chapter 3 (Equation 3.9, Model 3) where utility $U(\cdot)$ for angler n and trip j is represented by

$$\begin{aligned} U_{jn} = {} & \beta_1\left(\mathrm{E}[cod\ kept]_{jn}\right) + \beta_2\left(\mathrm{E}[cod\ released]_{jn}\right) \\ & +\beta_3\left(\mathrm{E}[haddock\ kept]_{jn}\right) + \beta_4\left(\mathrm{E}[haddock\ released]_{jn}\right) \\ & +\beta_5\left[(tr.leng.)_{jn} \times for\text{-}hire_n\right] + \beta_6\left[(tr.leng.)_{jn} \times for\text{-}hire_n\right]^2 \\ & +\beta_7\left(opt\text{-}out\right)_{jn} + \beta_8(trip\ cost)_{jn} + \varepsilon_{jn}. \end{aligned} \tag{4.28}$$

Because biological prediction models are not available for pollock, Equation 4.28 only includes two species from the market basket in Chapter 3. Atlantic pollock biology is generally not well understood, and recreational data for the species is limited. The second model modifies the NIC utility function from Chapter 3 (Equation 3.10, Model 4):

$$\begin{aligned} U_{jn} = {} & \beta_1\sqrt{\mathrm{E}[cod\ kept]_{jn}} + \beta_2\sqrt{\mathrm{E}[cod\ released]_{jn}} \\ & +\beta_3\sqrt{\mathrm{E}[haddock\ kept]_{jn}} + \beta_4\sqrt{\mathrm{E}[haddock\ released]_{jn}} \\ & +\beta_5\left[(tr.leng.)_{jn} \times for\text{-}hire_n\right] + \beta_6\left[(tr.leng.)^2_{jn} \times for\text{-}hire_n\right] \\ & +\beta_7\left(Opt\text{-}out\right)_{jn} + \beta_8(Trip\ Cost)_{jn} + \varepsilon_{jn}. \end{aligned} \tag{4.29}$$

The third model in the simulations has the same specification as Equation 4.23 but is estimated using only anglers with self-reported targeting or catch of cod and haddock (fishery users), which is analogous to Model 5 in Chapter 3, and will be referred to hereafter as the nonlinear-in-catch users (NICU) model. Trip costs used in the behavior models are drawn from the same distribution used to create the attribute levels in the CE survey. The trip length distribution is based on the frequency of trip types (4 hour, 8 hour, 12 hour) advertised by various online vendors servicing the study area.

Species targeting and target switching are included implicitly in the simulations. Because the model includes a basket of fish from two species, anglers are allowed to target either species using the tradeoffs determined by the stated preference survey. The program does not explicitly separate anglers targeting cod from those targeting haddock, but some simulated anglers expect to catch more cod than haddock, whereas others expect to catch more haddock than cod. For example, a computer angler who expects to catch 15 cod and 2 haddock is classified as targeting cod, whereas a computer angler with an expected catch of 87 haddock and 2 cod is classified as targeting haddock.

Anglers are also allowed to exit and enter the fishery at will because there are no explicit controls requiring mandatory participation. Though the opt-out choice in the stated preference survey was "Do something else, but not saltwater fishing," there are no freshwater substitutes for cod. Therefore, it is appropriate to assume that simulated anglers that choose not to take the saltwater fishing trip are not fishing at all, and the simulation outputs can be assumed to be a measure of participation. Such information is useful for determining total economic impacts as measured via input/output (I/O) models because non-participating anglers that cease all fishing activities affect regional economic transfers whereas non-participating anglers that participate in other fisheries do not.

The estimated parameters for the two-species RUMs listed in Table 35 are significantly different from the three-species model results in Chapter 3. The coefficients for fish caught are smaller in the LIC model than the results in Chapter 3, but the coefficients for trip length and trip cost are larger. The reverse is observed in the NIC and

NICU models, indicating that the two models compensate for the lack of one fish species in different ways. Despite differences in the angler behavior model, MWTP confidence intervals overlap with those of Chapter 3. The confidence intervals for the NIC and NICU specifications are computed at the average catch of 4.5, but simulated total trip values are calculated using marginal values for each fish.

Table 35. *Conditional Logit Parameters Used in Simulation Exercises*

Variable	LIC	NIC	NICU
Cod kept	0.0636 (0.00461)***	—	—
Cod released	0.0210 (0.00482)***	—	—
Haddock kept	0.0613 (0.00464)***	—	—
Haddock released	0.00849 (0.00538)	—	—
$\sqrt{(\text{Cod kept})}$	—	0.302 (0.0197)***	0.318 (0.0233)***
$\sqrt{(\text{Cod released})}$	—	0.111 (0.0146)***	0.120 (0.0173)***
$\sqrt{(\text{Haddock kept})}$	—	0.247 (0.0186)***	0.258 (0.0224)***
$\sqrt{(\text{Haddock released})}$	—	0.0397 (0.0155)***	0.0537 (0.0185)***
Trip length × For-Hire	0.156 (0.0176)***	0.158 (0.0176)***	0.130 (0.0219)***
(Trip length)^2 × For-Hire	-0.00740 (0.00124)***	-0.00786 (0.00124)***	-0.00556 (0.000152)***
Trip cost	-0.00557 (0.000130)***	-0.00559 (0.000130)***	-0.00563 (0.000153)***
Opt-out	-0.730 (0.0603)***	-0.153 (0.0792)*	-0.504 (0.0981)***
LR (χ^2)	4,817.18***	4,860.1***	4326.6***
No. Obs.†	39,151	39,151	27,803
No. Groups†	1,214	1,214	840
MWTP Cod (keep)	\$11.41	\$12.71	\$13.31
	(9.72, 13.11)	(11.00, 14.43)	(11.28, 15.33)
MWTP Cod (release)	\$3.76	\$4.68	\$5.00
	(2.07, 5.45)	(3.48, 5.88)	(3.58, 6.41)
MWTP Haddock (keep)	\$10.99	\$10.42	\$10.79
	(9.26, 12.72)	(8.79, 12.06)	(8.85, 12.73)
MWTP Haddock (release)	\$1.52	\$1.67	\$2.25
	(-0.36,3.41)	(0.40, 2.94)	(0.74, 3.75)

$^{***}p < .01$, $^{**}p < .05$, $^{*}p < .10$.

† No. Obs. is the total number of items (alternatives) and No. Groups is the number of clusters used in computing the standard errors (N), which equals the number of individuals

The biological data for the simulation comes from AGEPRO V3.1, courtesy of Paul Nitschke and Scott Steinback of NEFSC. The software program projects biomass by species for any number of future years. Output includes total biomass, spawning biomass, recruitment biomass, and harvest biomass for different age-class specifications. Biomass estimates become more uncertain as the projection year moves away from the last year of historical data available; thus, simulations in this study are limited to five years into the future (2011-2015 at the time of writing). The simulation analysis is also restricted to GOM stocks and anglers presuming to fish in the GOM (see Figure 12) because projected biomass data are only available for the Gulf of Maine (GOM) cod and haddock stocks. Detailed estimates of future biomass for pollock are not available at this time.

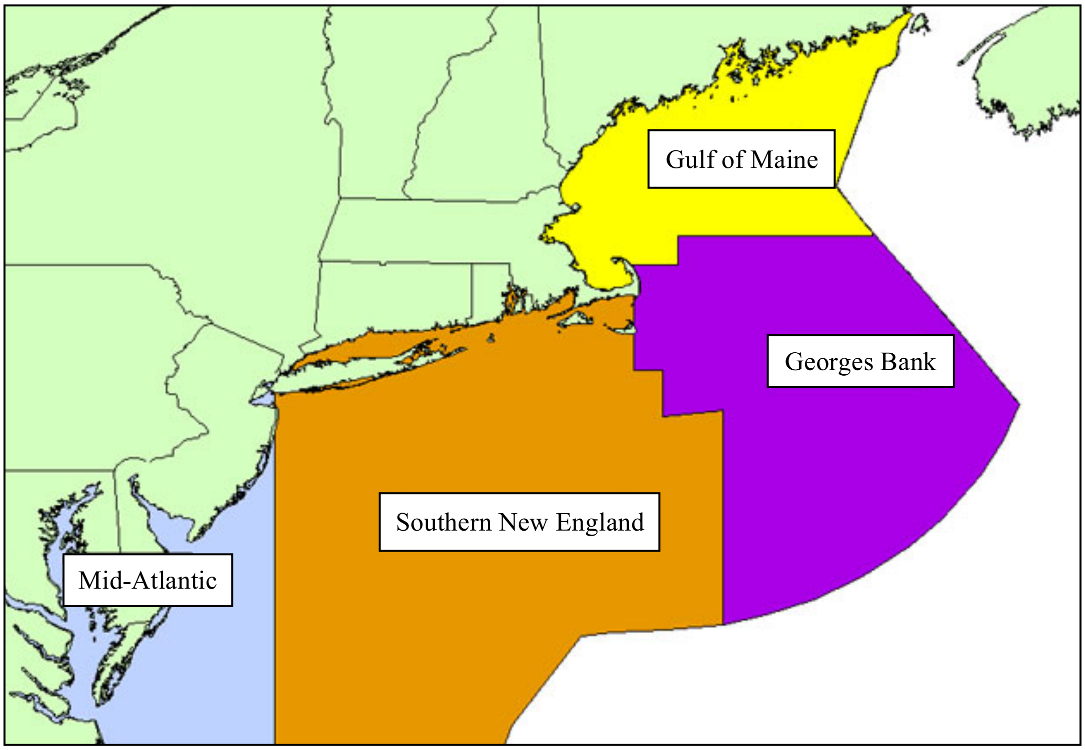


Figure 12. Map of stock regions (NMFS Northeast Regional Office, 2008).

The total number of iterations used in the simulations is derived from estimates of historic effort. Proportions of the total number of anglers in each mode for Maine, New Hampshire, and Massachusetts were aggregated to compute a possible total number of relevant trips in the region; however, the 500,000 iterations used in the simulations is probably higher than actual effort because Massachusetts fishing trips are split between Georges Bank and the GOM. Anglers south of Chatham, MA, are likely to be fishing in Georges Bank rather than in the GOM. Additionally, the proportional standard error (PSE) associated with effort estimates for species targeting in specific geographic areas is very high. The simulation results should be considered conservative estimates because actual effort levels are almost certainly lower.

The proxy distribution for the total number of successful casts was derived from historic data. Total trip catch from the most recent five years of MRFSS data for cod and haddock from Maine, New Hampshire, and Massachusetts were entered into EasyFitXL, a distribution fitting software package. This particular software package was used because it is one of the few programs that generates random numbers for discrete distributions. EasyFitXL identified the best-fitting discrete distributions for combined keep and release data from MRFSS. For cod, a geometric distribution with p = 0.14901 was most appropriate. A geometric distribution is the probability distribution of the number of Bernoulli trials needed to get one success. Given a probability of success X on each trial of p, the probability of the k^{th} trial being the first success is

$$\Pr(X = k) = (1 - p)^k p. \qquad (4.30)$$

A logarithmic distribution with p = 0.89251 was fit to the haddock catch data. The probability mass function of a logarithmic distribution is

$$f(k) = \frac{-1}{\ln(1-p)} \frac{p^k}{k}. \qquad (4.31)$$

EasyFitXL generated strings of random draws following the fitted discrete distributions. The expected trip catch numbers (successful casts) for each species were then randomly assigned to each simulated angler resulting in double randomization. All trip characteristics, including costs, mode, trip length, expected catch, and catch size, were assigned only once for all simulations to ensure comparability.

Trip Results

The economic impacts of different regulations on individual trips are predicted using the theoretical framework outlined earlier in this chapter. The expected keep and release for each species under different regulatory scenarios is calculated using the distributions for catch size and catch number for each year. Regulations truncate the catch distribution at different points, resulting in changes to expected keep and release (see Table 53 and Table 54 in Appendix C). The WTP per trip for a representative angler, computed using the expected keep and release values, are shown in Tables 36 through 38.

Table 36. *Theoretical (Expected) Mean WTP per Trip for LIC Model*

Regulations	2011	2012	2013	2014	2015
20 C ≥ 20", any H ≥ 14"	$113.54	$104.11	$103.50	$103.29	$103.38
10 C ≥ 20", any H ≥ 18"	$105.12	$104.67	$104.54	$104.49	$104.48
15 C ≥ 22", any H ≥ 16"	$109.44	$108.87	$108.85	$108.68	$108.68
15 C ≥ 22", any H ≥ 18"	$108.92	$108.18	$108.06	$107.91	$107.91
10 C ≥ 22", any H ≥ 16"	$104.21	$103.70	$103.68	$103.54	$103.53
10 C ≥ 22", any H ≥ 18"	$103.68	$103.01	$102.89	$102.76	$102.76
10 C ≥ 22", 15 H ≥ 20"	$100.43	$98.84	$98.36	$98.20	$98.20
2 C ≥ 22", any H ≥ 18"	$78.76	$78.40	$78.28	$78.25	$78.25
10 C ≥ 23", any H ≥ 18"	$102.86	$102.10	$101.97	$101.80	$101.79
2 C ≥ 23", any H ≥ 18"	$78.47	$78.09	$77.96	$77.92	$77.92
10 C ≥ 24", any H ≥ 16"	$102.12	$101.25	$101.24	$100.98	$100.98
10 C ≥ 24", any H ≥ 18"	$101.59	$100.56	$100.45	$100.21	$100.20
10 C ≥ 24", any H ≥ 19"	$101.18	$100.01	$99.81	$99.59	$99.59
8 C ≥ 24", 10 H ≥ 18"	$93.73	$92.80	$92.71	$92.49	$92.49
2 C ≥ 24", any H ≥ 18"	$78.04	$77.56	$77.44	$77.37	$77.37
10 C ≥ 26", any H ≥ 18"	$99.28	$97.92	$97.80	$97.45	$97.44
10 C ≥ 26", any H ≥ 20"	$97.98	$95.63	$95.13	$94.75	$94.74
8 C ≥ 26", 10 H ≥ 20"	$90.51	$88.42	$87.99	$87.64	$87.63
5 C ≥ 26", 10 H ≥ 21"	$81.64	$79.55	$78.95	$78.57	$78.66

Table 37. *Theoretical (Expected) Mean WTP per Trip for NIC Model*

Regulations	2011	2012	2013	2014	2015
20 C ≥ 20", any H ≥ 14"	\$234.43	\$229.21	\$228.59	\$228.49	\$228.59
10 C ≥ 20", any H ≥ 18"	\$233.76	\$233.65	\$233.65	\$233.61	\$233.61
15 C ≥ 22", any H ≥ 16"	\$234.71	\$234.62	\$234.66	\$234.60	\$234.60
15 C ≥ 22", any H ≥ 18"	\$235.20	\$235.09	\$235.09	\$235.04	\$235.04
10 C ≥ 22", any H ≥ 16"	\$232.47	\$232.23	\$232.26	\$232.16	\$232.16
10 C ≥ 22", any H ≥ 18"	\$232.96	\$232.70	\$232.70	\$232.60	\$232.60
10 C ≥ 22", 15 H ≥ 20"	\$231.64	\$230.52	\$230.13	\$230.03	\$230.03
2 C ≥ 22", any H ≥ 18"	\$201.40	\$201.08	\$201.08	\$200.97	\$200.96
10 C ≥ 23", any H ≥ 18"	\$232.45	\$232.10	\$232.09	\$231.96	\$231.96
2 C ≥ 23", any H ≥ 18"	\$200.80	\$200.40	\$200.40	\$200.25	\$200.25
10 C ≥ 24", any H ≥ 16"	\$231.10	\$230.52	\$230.57	\$230.36	\$230.36
10 C ≥ 24", any H ≥ 18"	\$231.59	\$230.99	\$231.00	\$230.80	\$230.80
10 C ≥ 24", any H ≥ 19"	\$231.61	\$230.93	\$230.89	\$230.70	\$230.69
8 C ≥ 24", 10 H ≥ 18"	\$226.64	\$225.81	\$225.74	\$225.53	\$225.53
2 C ≥ 24", any H ≥ 18"	\$199.85	\$199.23	\$199.24	\$199.04	\$199.03
10 C ≥ 26", any H ≥ 18"	\$229.80	\$228.82	\$228.83	\$228.49	\$228.48
10 C ≥ 26", any H ≥ 20"	\$229.59	\$227.97	\$227.65	\$227.31	\$227.30
8 C ≥ 26", 10 H ≥ 20"	\$223.74	\$221.75	\$221.34	\$220.98	\$220.97
5 C ≥ 26", 10 H ≥ 21"	\$213.85	\$211.47	\$210.85	\$210.36	\$210.45

Table 38. *Theoretical (Expected) Mean WTP per Trip for NICU Model*

Regulations	2011	2012	2013	2014	2015
20 C ≥ 20", any H ≥ 14"	\$244.93	\$241.52	\$240.94	\$240.85	\$240.95
10 C ≥ 20", any H ≥ 18"	\$244.92	\$244.89	\$244.93	\$244.89	\$244.88
15 C ≥ 22", any H ≥ 16"	\$245.44	\$245.39	\$245.45	\$245.39	\$245.39
15 C ≥ 22", any H ≥ 18"	\$246.29	\$246.28	\$246.33	\$246.28	\$246.27
10 C ≥ 22", any H ≥ 16"	\$243.27	\$243.05	\$243.10	\$243.00	\$242.99
10 C ≥ 22", any H ≥ 18"	\$244.12	\$243.93	\$243.98	\$243.88	\$243.87
10 C ≥ 22", 15 H ≥ 20"	\$243.58	\$242.62	\$242.29	\$242.19	\$242.19
2 C ≥ 22", any H ≥ 18"	\$211.63	\$211.37	\$211.42	\$211.30	\$211.29
10 C ≥ 23", any H ≥ 18"	\$243.60	\$243.33	\$243.37	\$243.24	\$243.23
2 C ≥ 23", any H ≥ 18"	\$211.00	\$210.67	\$210.71	\$210.56	\$210.55
10 C ≥ 24", any H ≥ 16"	\$241.88	\$241.33	\$241.39	\$241.18	\$241.17
10 C ≥ 24", any H ≥ 18"	\$242.74	\$242.21	\$242.27	\$242.06	\$242.05
10 C ≥ 24", any H ≥ 19"	\$242.93	\$242.34	\$242.36	\$242.15	\$242.15
8 C ≥ 24", 10 H ≥ 18"	\$238.70	\$237.88	\$237.82	\$237.61	\$237.61
2 C ≥ 24", any H ≥ 18"	\$210.02	\$209.45	\$209.52	\$209.30	\$209.29
10 C ≥ 26", any H ≥ 18"	\$240.93	\$240.00	\$240.06	\$239.71	\$239.70
10 C ≥ 26", any H ≥ 20"	\$241.13	\$239.73	\$239.50	\$239.15	\$239.15
8 C ≥ 26", 10 H ≥ 20"	\$235.90	\$233.98	\$233.62	\$233.25	\$233.24
5 C ≥ 26", 10 H ≥ 21"	\$225.94	\$223.63	\$223.05	\$222.56	\$222.65

The average change in mean WTP per trip (averaged across all years) for moving from the current regulations (keep 10 cod larger than 22”; keep any haddock larger than 18”) to one of the other regulatory scenarios is listed in Table 39. Overall, the NIC and NICU models exhibit smaller responses in mean WTP per trip to changes in regulations than the LIC model.

Table 39. *Difference in Mean WTP per Trip from Changing Current Regulations*

Regulations	LIC	NIC	NICU
20 C ≥ 20", any H ≥ 14"	$2.55	-$2.85	-$2.12
10 C ≥ 20", any H ≥ 18"	$1.64	$0.95	$0.95
15 C ≥ 22", any H ≥ 16"	$5.89	$1.93	$1.46
15 C ≥ 22", any H ≥ 18"	$5.17	$2.38	$2.33
10 C ≥ 22", any H ≥ 16"	$0.71	-$0.46	-$0.88
10 C ≥ 22", any H ≥ 18"	$0.00	$0.00	$0.00
10 C ≥ 22", 15 H ≥ 20"	-$4.21	-$2.24	-$1.38
2 C ≥ 22", any H ≥ 18"	-$24.63	-$31.61	-$32.55
10 C ≥ 23", any H ≥ 18"	-$0.92	-$0.60	-$0.60
2 C ≥ 23", any H ≥ 18"	-$24.95	-$32.29	-$33.26
10 C ≥ 24", any H ≥ 16"	-$1.71	-$2.13	-$2.57
10 C ≥ 24", any H ≥ 18"	-$2.42	-$1.67	-$1.69
10 C ≥ 24", any H ≥ 19"	-$2.99	-$1.75	-$1.57
8 C ≥ 24", 10 H ≥ 18"	-$10.18	-$6.86	-$6.03
2 C ≥ 24", any H ≥ 18"	-$25.46	-$33.43	-$34.44
10 C ≥ 26", any H ≥ 18"	-$5.04	-$3.83	-$3.88
10 C ≥ 26", any H ≥ 20"	-$7.37	-$4.75	-$4.22
8 C ≥ 26", 10 H ≥ 20"	-$14.58	-$10.96	-$9.96
5 C ≥ 26", 10 H ≥ 21"	-$23.55	-$21.32	-$20.39

To illustrate the effect of regulations more explicitly, Figures 13 through 18 depict the response in mean WTP per trip to changes in bag and size limits, *ceteris paribus*. The impact of minimum size regulations on mean WTP per trip, shown in Figures 13 through 15, have a smaller effect on the mean WTP per trip for cod than haddock, but the opposite is true for bag limits. Mean WTP declines much more rapidly in the LIC model than in the NIC or NICU models in response to increases in minimum size limits, but mean WTP plateaus slower in the in the LIC model in response to bag limits than the NIC or NICU models (Figures 16 through 18).The bag limit graphs plateau because the probability that bag limits bind changes abruptly. When bag limits are low, the probability that the bag limit binds is high, causing large changes in mean WTP. When bag limits are high, the probability that the bag limit binds is low, resulting in small mean WTP responses. When bag limits are very high, the probability of bag limits binding becomes negligible and mean WTP stays the same.

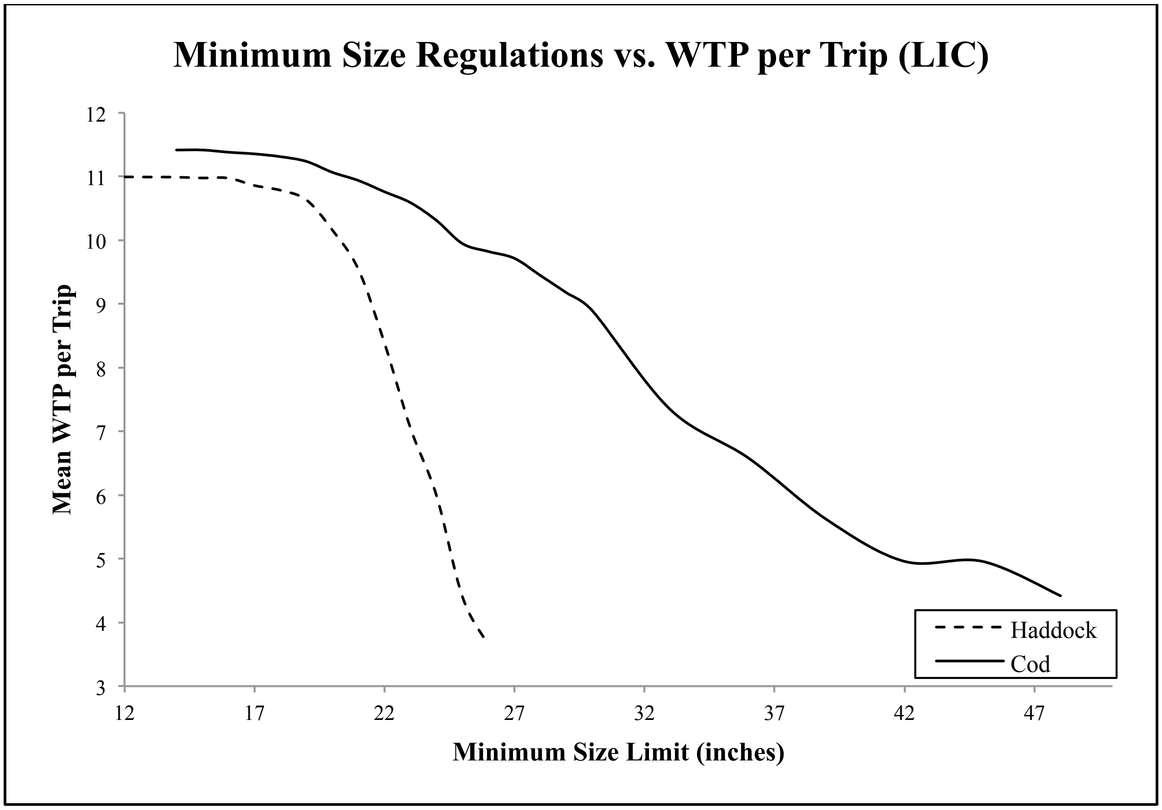


Figure 13. Effect of minimum size regulations on WTP per trip, *ceteris paribus* (LIC).

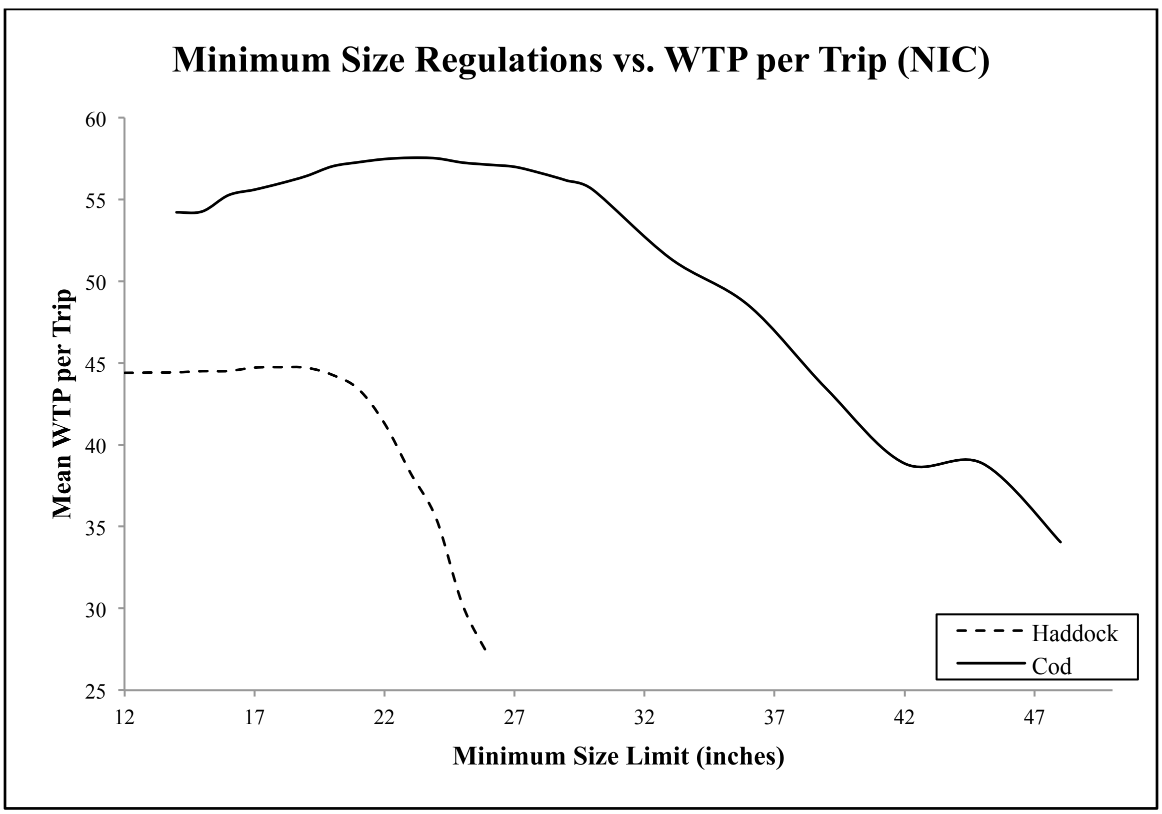


Figure 14. Effect of minimum size regulations on WTP per trip, *ceteris paribus* (NIC).

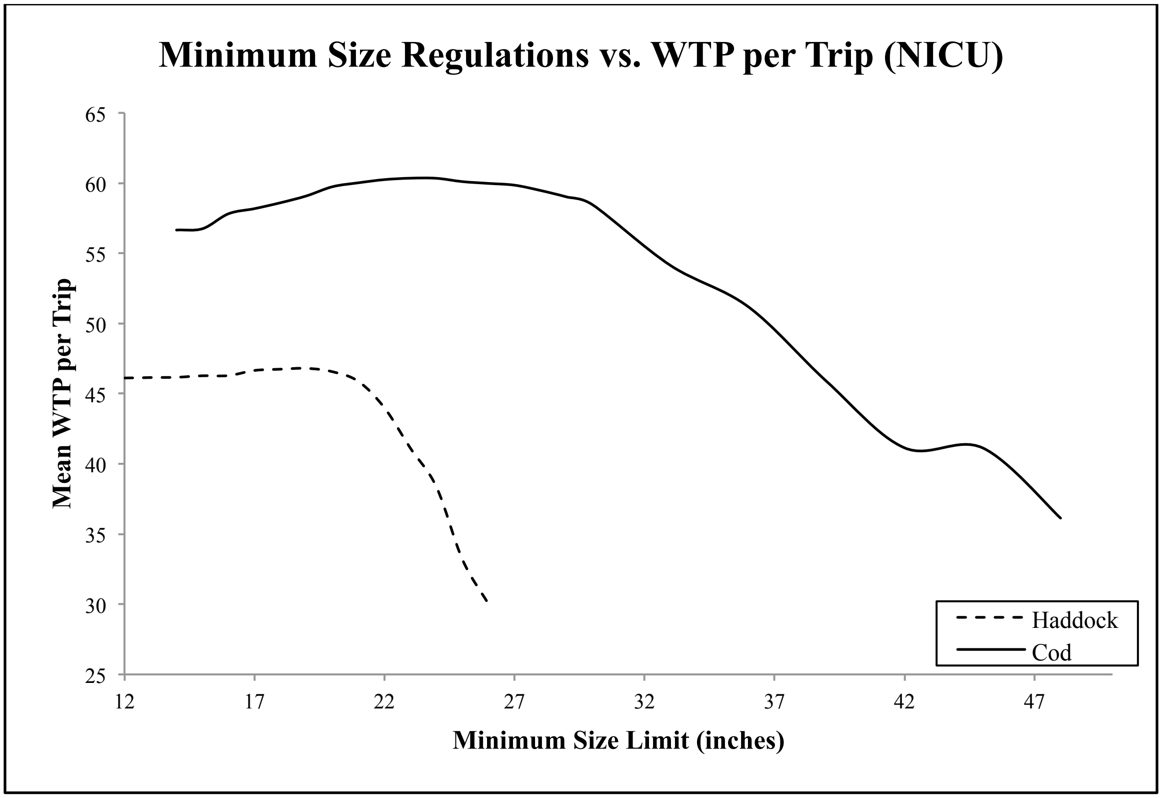


Figure 15. Effect of minimum size regulations on WTP per trip, *ceteris paribus* (NICU).

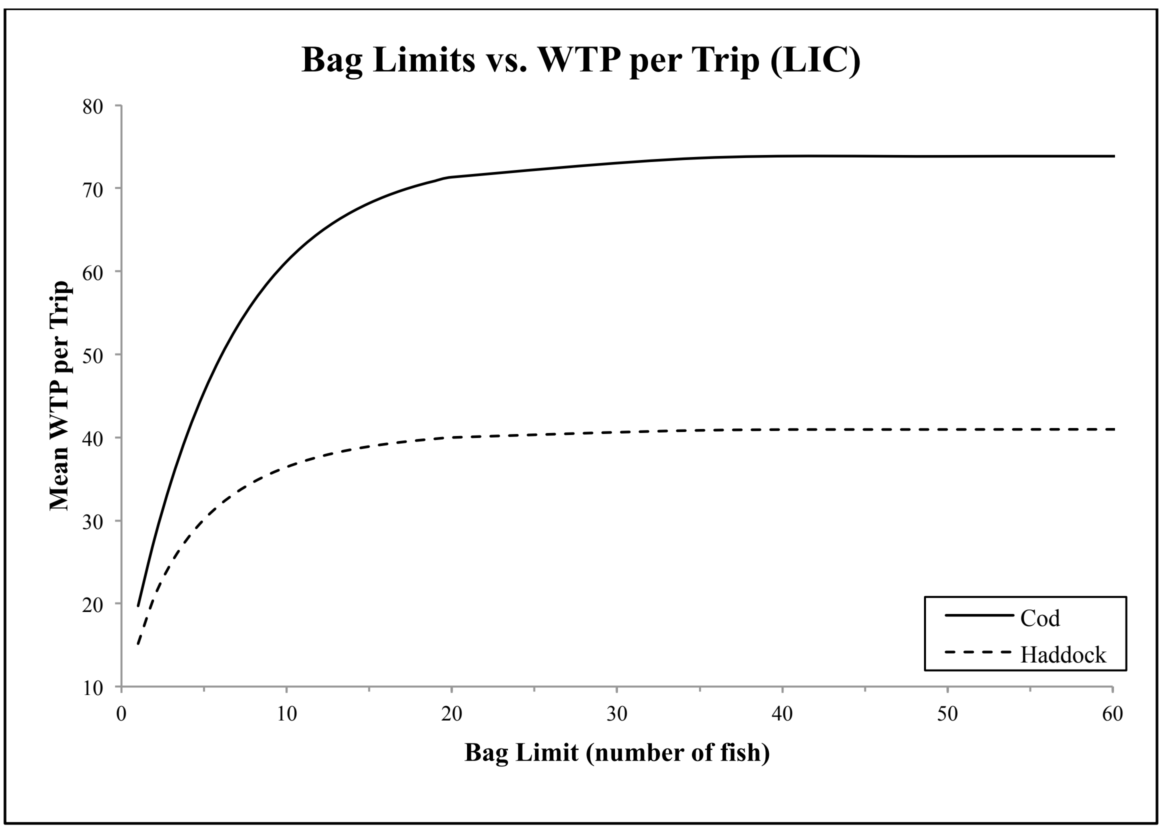


Figure 16. Effect of bag limits on WTP per trip, *ceteris paribus* (LIC model).

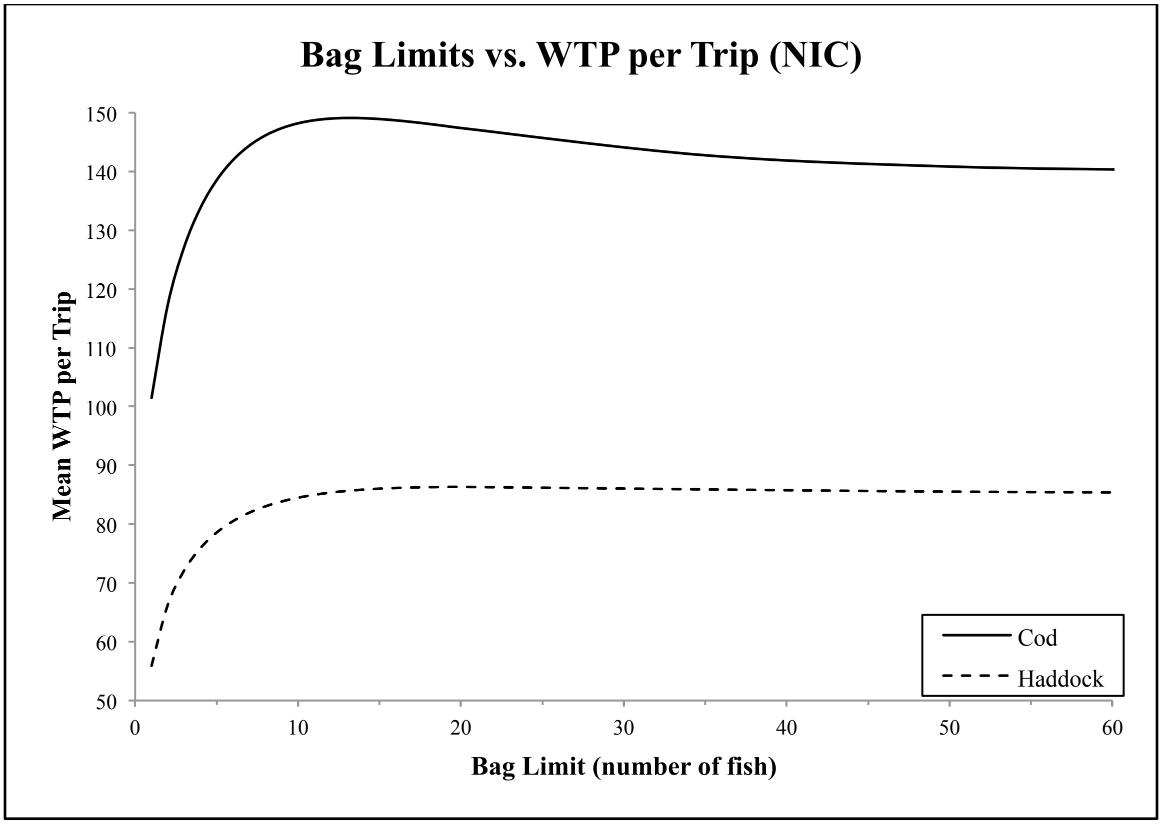


Figure 17. Effect of bag limits on WTP per trip, *ceteris paribus* (NIC model).

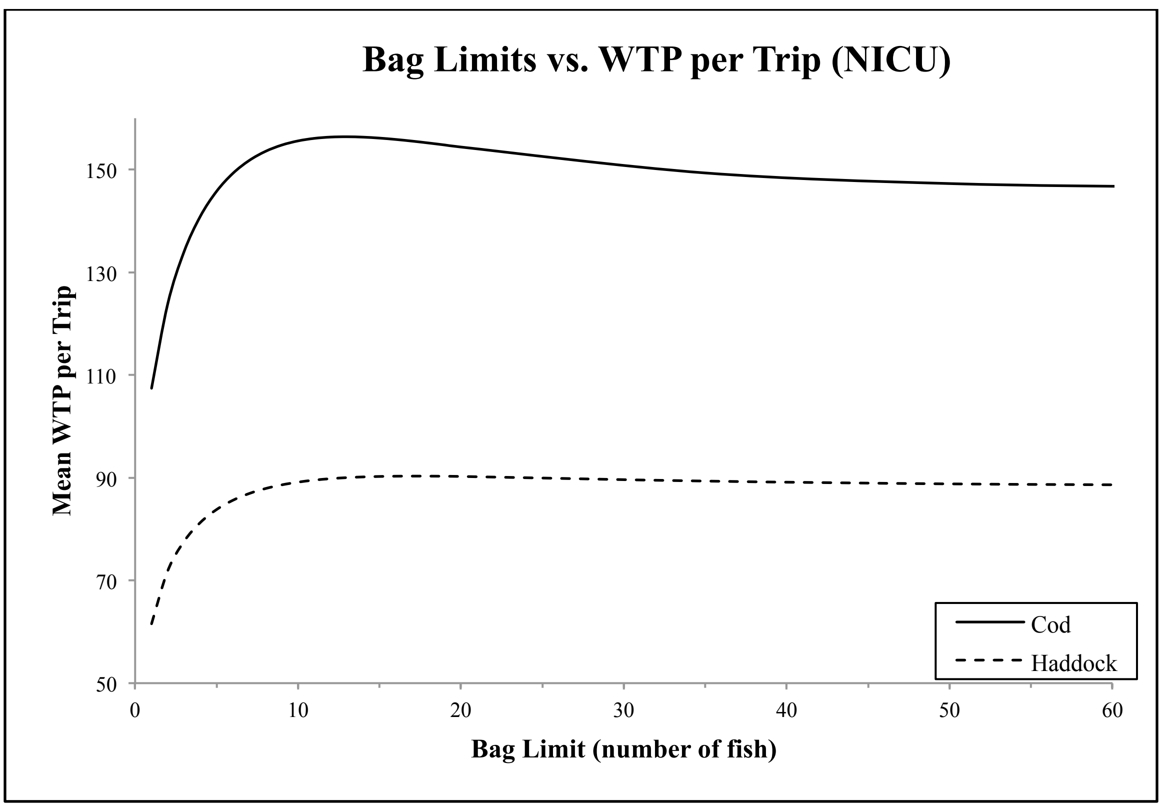


Figure 18. Effect of bag limits on WTP per trip, *ceteris paribus* (NICU model).

Simulation Results

Eighteen different bag limit and minimum size limit combinations are presented for the five biological scenarios. The simulation results for each year represent short-run outcomes in the fishery under different management parameters. Differences in age-class distributions between scenario years are reflected in the number of fish kept and released per trip for the same regulation levels, and consequently in WTP values. Simulation results are listed in the tables below and in Tables 55 through 57 in Appendix C.

Simulated Mean WTP per Trip

Table 40 shows the simulated mean WTP per trip for each behavioral model averaged across all scenario years. The mean WTP per trip values shown here are smaller than in Tables 36 through 38 because the no-highgrading algorithm imposed an artificial truncation on the MRFSS catch distribution causing simulated keep and release values to differ from the theoretical expected keep and release values. Were simulated anglers allowed to high-grade, as occurs in other recreational fisheries, the simulation results would align more closely with the theoretical derivation because the distribution of catch used to calculate the theoretical values would be the same as the distribution used in the simulations; however, the simulation truncates catch when the bag limit is reached instead of simply dividing the catch distribution into keep and release. Consequently, actual simulated catch numbers are not equal to draws from the MRFSS historic catch distribution, and the theoretical values are higher than the simulation numbers.

Table 40. *Average Simulated Mean WTP per Trip*

Regulations	LIC	NIC	NICU
20 C ≥ 20", any H ≥ 14"	$96.17	$116.66	$122.85
10 C ≥ 20", any H ≥ 18"	$84.26	$109.70	$115.67
15 C ≥ 22", any H ≥ 16"	$90.35	$114.38	$120.55
15 C ≥ 22", any H ≥ 18"	$89.40	$113.92	$120.17
10 C ≥ 22", any H ≥ 16"	$84.09	$110.33	$116.29
10 C ≥ 22", any H ≥ 18"	$78.14	$105.92	$111.03
10 C ≥ 22", 15 H ≥ 20"	$79.04	$108.04	$114.16
2 C ≥ 22", any H ≥ 18"	$49.06	$72.36	$75.76
10 C ≥ 23", any H ≥ 18"	$77.82	$105.87	$110.98
2 C ≥ 23", any H ≥ 18"	$49.11	$72.55	$75.97
10 C ≥ 24", any H ≥ 16"	$83.18	$110.09	$116.07
10 C ≥ 24", any H ≥ 18"	$82.22	$109.62	$115.68
10 C ≥ 24", any H ≥ 19"	$81.12	$109.04	$115.17
8 C ≥ 24", 10 H ≥ 18"	$71.85	$103.78	$109.54
2 C ≥ 24", any H ≥ 18"	$49.20	$72.88	$76.33
10 C ≥ 26", any H ≥ 18"	$80.72	$108.77	$114.82
10 C ≥ 26", any H ≥ 20"	$78.97	$107.83	$114.00
5 C ≥ 26", 10 H ≥ 21"	$60.35	$93.60	$99.09

The estimates are roughly equivalent to the price of an offshore party boat or head boat trip in the Gulf of Maine and, in general, decrease as regulations become more restrictive. The mean WTP per trip for the LIC model is lower than for the NIC and NICU specifications and exhibits the greatest variation between the regulation extremes.

Simulated Results for the Short-run (1 year)

Tables 41 through 43 show the simulation results averaged across scenario years for the three behavioral models in the short-run (1 year). Recreational angling values, as measured by total WTP, differ by more than 20% between the simulated regulation extremes. Nonlinear-in-catch welfare estimates are higher than the linear-in-catch welfare estimates because the initial marginal utility of catch is greater in the nonlinear case. The nonlinear models also have higher effort estimates than the linear model and smaller responses to regulation changes. Because users are likely to place more value on the fishery than non-users, the total WTP using the NICU model is higher than the NICU model despite lower effort projections.

Table 41. *Short-run Simulation Results for LIC (Averaged Across Scenario Years)*

Regulations	Effort	For-hire Effort	Total WTP (in $1000s)	Cod Keep	Haddock Keep
20 C ≥ 20", any H ≥ 14"	332,694	297,686	29,946	1,582,754	1,019,669
10 C ≥ 20", any H ≥ 18"	328,252	297,467	26,005	1,293,151	968,280
15 C ≥ 22", any H ≥ 16"	329,679	297,546	27,912	1,379,355	1,003,076
15 C ≥ 22", any H ≥ 18"	329,037	297,515	27,569	1,377,738	969,773
10 C ≥ 22", any H ≥ 16"	327,542	297,417	25,884	1,217,924	999,165
10 C ≥ 22", any H ≥ 18"	326,893	297,387	25,543	1,216,340	965,988
10 C ≥ 22", 15 H ≥ 20"	325,158	297,324	24,200	1,212,535	842,278
2 C ≥ 22", any H ≥ 18"	311,925	294,274	15,302	415,577	933,590
10 C ≥ 23", any H ≥ 18"	326,491	297,371	25,409	1,195,206	965,129
2 C ≥ 23", any H ≥ 18"	312,025	294,282	15,323	414,437	933,738
10 C ≥ 24", any H ≥ 16"	326,475	297,368	25,517	1,160,422	996,694
10 C ≥ 24", any H ≥ 18"	325,811	297,338	25,176	1,158,870	963,619
10 C ≥ 24", any H ≥ 19"	324,997	297,306	24,784	1,157,178	925,538
8 C ≥ 24", 10 H ≥ 18"	322,427	297,146	21,900	1,052,783	789,009
2 C ≥ 24", any H ≥ 18"	312,162	294,287	15,359	412,407	933,923
10 C ≥ 26", any H ≥ 18"	324,374	297,132	24,616	1,072,476	960,425
10 C ≥ 26", any H ≥ 20"	323,077	297,082	23,999	1,070,080	900,378
5 C ≥ 26", 10 H ≥ 21"	315,985	296,168	18,117	775,739	718,946

Table 42. *Short-run Simulation Results for NIC (Averaged Across Scenario Years)*

Regulations	Effort	For-hire Effort	Total WTP (in $1000s)	Cod Keep	Haddock Keep
20 C ≥ 20", any H ≥ 14"	369,984	302,087	41,595	1,751,047	1,108,469
10 C ≥ 20", any H ≥ 18"	367,516	302,027	38,875	1,467,583	1,060,140
15 C ≥ 22", any H ≥ 16"	368,901	302,098	40,666	1,546,763	1,094,242
15 C ≥ 22", any H ≥ 18"	368,319	302,098	40,438	1,545,379	1,059,371
10 C ≥ 22", any H ≥ 16"	367,850	302,051	39,131	1,385,337	1,093,648
10 C ≥ 22", any H ≥ 18"	367,247	302,051	38,900	1,383,774	1,058,785
10 C ≥ 22", 15 H ≥ 20"	365,976	302,037	38,128	1,380,785	935,612
2 C ≥ 22", any H ≥ 18"	350,940	300,648	25,393	476,766	1,031,391
10 C ≥ 23", any H ≥ 18"	367,034	302,057	38,857	1,359,807	1,058,258
2 C ≥ 23", any H ≥ 18"	351,100	300,648	25,472	475,095	1,031,793
10 C ≥ 24", any H ≥ 16"	367,145	302,064	38,969	1,319,375	1,091,895
10 C ≥ 24", any H ≥ 18"	366,541	302,064	38,739	1,317,907	1,057,125
10 C ≥ 24", any H ≥ 19"	365,812	302,063	38,455	1,316,109	1,016,744
8 C ≥ 24", 10 H ≥ 18"	364,826	301,980	36,525	1,209,470	893,813
2 C ≥ 24", any H ≥ 18"	351,246	300,648	25,602	471,829	1,032,327
10 C ≥ 26", any H ≥ 18"	365,003	301,994	38,285	1,216,376	1,054,631
10 C ≥ 26", any H ≥ 20"	363,852	301,988	37,831	1,213,723	990,568
5 C ≥ 26", 10 H ≥ 21"	358,292	301,738	32,381	892,091	816,677

Table 43. *Short-run Simulation Results for NICU (Averaged Across Scenario Years)*

Regulations	Effort	For-hire Effort	Total WTP (in $1000s)	Cod Keep	Haddock Keep
20 C ≥ 20", any H ≥ 14"	371,870	301,290	43,745	1,765,050	1,115,547
10 C ≥ 20", any H ≥ 18"	369,626	301,239	40,964	1,481,631	1,067,558
15 C ≥ 22", any H ≥ 16"	370,901	301,299	42,818	1,559,850	1,101,473
15 C ≥ 22", any H ≥ 18"	370,414	301,299	42,625	1,558,823	1,066,641
10 C ≥ 22", any H ≥ 16"	369,936	301,261	41,216	1,398,326	1,101,078
10 C ≥ 22", any H ≥ 18"	369,423	301,261	41,019	1,397,046	1,066,248
10 C ≥ 22", 15 H ≥ 20"	368,301	301,255	40,282	1,394,460	943,064
2 C ≥ 22", any H ≥ 18"	352,538	299,672	26,708	480,159	1,040,196
10 C ≥ 23", any H ≥ 18"	369,202	301,267	40,976	1,372,679	1,065,671
2 C ≥ 23", any H ≥ 18"	352,723	299,672	26,795	478,496	1,040,548
10 C ≥ 24", any H ≥ 16"	369,250	301,274	41,058	1,331,458	1,099,434
10 C ≥ 24", any H ≥ 18"	368,739	301,274	40,862	1,330,276	1,064,638
10 C ≥ 24", any H ≥ 19"	368,089	301,274	40,610	1,328,713	1,024,159
8 C ≥ 24", 10 H ≥ 18"	367,053	301,198	38,540	1,221,057	901,079
2 C ≥ 24", any H ≥ 18"	352,908	299,672	26,938	475,208	1,041,104
10 C ≥ 26", any H ≥ 18"	367,140	301,181	40,393	1,227,251	1,062,038
10 C ≥ 26", any H ≥ 20"	366,150	301,181	39,992	1,225,092	997,991
5 C ≥ 26", 10 H ≥ 21"	360,806	300,925	34,302	901,296	824,636

Though keep, and consequently total WTP, decreases markedly with tighter restrictions, effort declines are relatively small. Moving from the most lenient to the most severe simulated regulatory scenario decreases total effort by less than 10%. Moreover, effort in the for-hire sector, which represents the larges portion of recreational angling in this fishery, barely fluctuates. These findings are consistent with anecdotal evidence that many anglers continue to fish regardless and historical data from for-hire trips that show very little change in number of participants per trip over the past 15 years despite changes to regulations for both cod and haddock. Modest movements in total effort may also be due to target switching behaviors, which deflate effort reductions for the fishery overall.

Simulated Results for the Long-run (5 years)

Assuming that the ACL is reached each year by the commercial fishery, long-run outcomes can be obtained by adding up short-run outcomes from the simulation. Tables 44 through 46 show the long-run simulation results for the three behavioral models if regulations were to remain constant for the next five years. Effort, and more specifically for-hire effort, remains fairly consistent across regulation scenarios; however, large losses (gains), as much as \$50 million, can be seen in consumer welfare. Additionally, the simulation results show significant consequences for biomass over the five-year period from changes in regulations. Recreational harvest mortality can change by more than 5 million fish over the five-year period if policy makers adopt more stringent regulations.

Table 44. *Long-run Simulation Results for LIC*

Regulations	Effort	For-hire Effort	Total WTP (in \$1000s)	Cod Keep	Haddock Keep
20 C ≥ 20", any H ≥ 14"	1,663,468	1,488,429	149,729	7,913,772	5,098,343
10 C ≥ 20", any H ≥ 18"	1,641,260	1,487,333	130,026	6,465,754	4,841,398
15 C ≥ 22", any H ≥ 16"	1,648,396	1,487,731	139,559	6,896,777	5,015,381
15 C ≥ 22", any H ≥ 18"	1,645,186	1,487,577	137,845	6,888,692	4,848,865
10 C ≥ 22", any H ≥ 16"	1,637,711	1,487,087	129,420	6,089,622	4,995,825
10 C ≥ 22", any H ≥ 18"	1,634,465	1,486,933	127,714	6,081,698	4,829,942
10 C ≥ 22", 15 H ≥ 20"	1,625,792	1,486,621	121,000	6,062,676	4,211,391
2 C ≥ 22", any H ≥ 18"	1,559,627	1,471,370	76,511	2,077,883	4,667,950
10 C ≥ 23", any H ≥ 18"	1,632,456	1,486,854	127,046	5,976,029	4,825,646
2 C ≥ 23", any H ≥ 18"	1,560,123	1,471,408	76,617	2,072,187	4,668,692
10 C ≥ 24", any H ≥ 16"	1,632,376	1,486,841	127,583	5,802,112	4,983,472
10 C ≥ 24", any H ≥ 18"	1,629,053	1,486,691	125,879	5,794,350	4,818,093
10 C ≥ 24", any H ≥ 19"	1,624,985	1,486,531	123,919	5,785,891	4,627,692
8 C ≥ 24", 10 H ≥ 18"	1,612,136	1,485,732	109,498	5,263,917	3,945,046
2 C ≥ 24", any H ≥ 18"	1,560,812	1,471,434	76,797	2,062,036	4,669,613
10 C ≥ 26", any H ≥ 18"	1,621,872	1,485,662	123,082	5,362,380	4,802,127
10 C ≥ 26", any H ≥ 20"	1,615,384	1,485,408	119,997	5,350,402	4,501,892
5 C ≥ 26", 10 H ≥ 21"	1,579,926	1,480,838	90,583	3,878,693	3,594,728

Table 45. *Long-run Simulation Results for NIC*

Regulations	Effort	For-hire Effort	Total WTP (in \$1000s)	Cod Keep	Haddock Keep
20 C ≥ 20", any H ≥ 14"	1,849,922	1,510,436	207,973	8,755,235	5,542,343
10 C ≥ 20", any H ≥ 18"	1,837,581	1,510,135	194,375	7,337,914	5,300,699
15 C ≥ 22", any H ≥ 16"	1,844,507	1,510,492	203,330	7,733,817	5,471,209
15 C ≥ 22", any H ≥ 18"	1,841,594	1,510,492	202,189	7,726,896	5,296,854
10 C ≥ 22", any H ≥ 16"	1,839,250	1,510,257	195,656	6,926,683	5,468,238
10 C ≥ 22", any H ≥ 18"	1,836,233	1,510,257	194,500	6,918,870	5,293,924
10 C ≥ 22", 15 H ≥ 20"	1,829,882	1,510,185	190,639	6,903,925	4,678,062
2 C ≥ 22", any H ≥ 18"	1,754,701	1,503,240	126,964	2,383,831	5,156,957
10 C ≥ 23", any H ≥ 18"	1,835,169	1,510,285	194,283	6,799,035	5,291,292
2 C ≥ 23", any H ≥ 18"	1,755,500	1,503,240	127,362	2,375,475	5,158,966
10 C ≥ 24", any H ≥ 16"	1,835,726	1,510,322	194,843	6,596,874	5,459,477
10 C ≥ 24", any H ≥ 18"	1,832,705	1,510,322	193,695	6,589,533	5,285,626
10 C ≥ 24", any H ≥ 19"	1,829,058	1,510,313	192,276	6,580,545	5,083,720
8 C ≥ 24", 10 H ≥ 18"	1,824,130	1,509,899	182,627	6,047,352	4,469,064
2 C ≥ 24", any H ≥ 18"	1,756,229	1,503,240	128,009	2,359,144	5,161,636
10 C ≥ 26", any H ≥ 18"	1,825,014	1,509,971	191,426	6,081,879	5,273,155
10 C ≥ 26", any H ≥ 20"	1,819,261	1,509,942	189,156	6,068,614	4,952,841
5 C ≥ 26", 10 H ≥ 21"	1,791,458	1,508,688	161,905	4,460,456	4,083,384

Table 46. *Long-run Simulation Results for NICU*

Regulations	Effort	For-hire Effort	Total WTP (in $1000s)	Cod Keep	Haddock Keep
20 C ≥ 20", any H ≥ 14"	1,859,350	1,506,449	218,725	8,825,249	5,577,737
10 C ≥ 20", any H ≥ 18"	1,848,128	1,506,195	204,822	7,408,154	5,337,791
15 C ≥ 22", any H ≥ 16"	1,854,503	1,506,497	214,091	7,799,248	5,507,366
15 C ≥ 22", any H ≥ 18"	1,852,070	1,506,497	213,126	7,794,113	5,333,203
10 C ≥ 22", any H ≥ 16"	1,849,679	1,506,307	206,079	6,991,630	5,505,388
10 C ≥ 22", any H ≥ 18"	1,847,116	1,506,307	205,093	6,985,232	5,331,239
10 C ≥ 22", 15 H ≥ 20"	1,841,505	1,506,273	201,410	6,972,298	4,715,321
2 C ≥ 22", any H ≥ 18"	1,762,688	1,498,360	133,538	2,400,796	5,200,980
10 C ≥ 23", any H ≥ 18"	1,846,009	1,506,333	204,878	6,863,395	5,328,355
2 C ≥ 23", any H ≥ 18"	1,763,617	1,498,360	133,977	2,392,478	5,202,738
10 C ≥ 24", any H ≥ 16"	1,846,251	1,506,369	205,290	6,657,292	5,497,168
10 C ≥ 24", any H ≥ 18"	1,843,694	1,506,369	204,310	6,651,381	5,323,188
10 C ≥ 24", any H ≥ 19"	1,840,443	1,506,369	203,048	6,643,563	5,120,796
8 C ≥ 24", 10 H ≥ 18"	1,835,266	1,505,989	192,698	6,105,283	4,505,395
2 C ≥ 24", any H ≥ 18"	1,764,540	1,498,360	134,692	2,376,040	5,205,522
10 C ≥ 26", any H ≥ 18"	1,835,702	1,505,907	201,967	6,136,256	5,310,192
10 C ≥ 26", any H ≥ 20"	1,830,752	1,505,903	199,962	6,125,461	4,989,953
5 C ≥ 26", 10 H ≥ 21"	1,804,028	1,504,623	171,509	4,506,481	4,123,179

Meta-Analyses of Simulations

Meta-analyses of the simulations are detailed in Tables 47 through 52. The term *meta-analysis* is used to denote a linear regression of the simulation outputs, which could be considered an assessment of the response surfaces with regards to changes in regulation levels. The data include the outcomes of the simulation scenarios listed in previous tables as well as an additional 100 to 200 simulation scenarios that cover the intermediate regulation levels. The meta-analysis models include all simulation parameters except a fixed effect for year 2015 to avoid multicollinearity.

In the effort meta-analyses (Table 47), coefficients for bag limits are positive and negative for minimum size limits, as predicted. Increasing minimum size limits should discourage anglers from fishing while increases in bag limits should increase effort. Effort responds most to cod regulations in the LIC model whereas the NIC and NICU models weight haddock minimum size limits more heavily. Haddock bag limits have very little impact on the fishery in all of the models though the coefficient is significant.

Table 47. *OLS Linear Meta-analysis Results for Effort*

Variable	LIC	NIC	NICU
Cod bag limit	614.177***	143.966***	197.311***
	(23.854)	(48.239)	(47.513)
Cod minimum size	-425.173***	-349.991***	-370.715***
	(38.437)	(66.049)	(43.094)
Haddock bag limit	14.347***	21.915***	7.228***
	(4.089)	(5.080)	(2.702)
Haddock minimum size	-220.621***	-537.808***	-499.498***
	(94.093)	(87.482)	(67.102)
Year 2011	650.014**	530.954*	499.498*
	(263.993)	(287.463)	(67.102)
Year 2012	164.512	138.462	142.233
	(265.470)	(290.036)	(244.019)
Year 2013	158.861	137.769	141.133
	(265.470)	(290.036)	(244.019)
Year 2014	7.605	4.27	5.1
	(265.470)	(290.036)	(244.019)
Constant	332,588.8***	381,018.1***	383,417.1***
	(2,208.676)	(2,855.674)	(1,895.566)
R-squared	0.888	0.833	0.756
Adjusted R-squared	0.884	0.822	0.744
F-statistic†	205.72***	75.95***	55.03***
No. Obs.	216	131	150

***p < .01, **p < .05, *p < .10.

† F(8, 207) for LIC, F(8, 122) for NIC, F(8, 141) for NICU

The meta-analyses for total WTP (Table 48) show that changes in cod bag limits have the greatest impact on total WTP. Because the value of fish kept is higher than the value of fish released, changes in bag limits should have a larger effect on total WTP than changes in minimum size limits. The coefficient for cod minimum size limits is larger than the coefficient for haddock minimum size limits in the LIC model, but the reverse is true in the NIC and NICU models. Cod minimum size limits are more important in the NICU model than in the NIC model, which can be attributed to differences in the perception of species importance between the survey sample groupings. Though the coefficients for haddock bag limits are statistically significant, the magnitude is small relative to other regulation variables.

Table 48. *OLS Linear Meta-analysis Results for Total WTP (in $1000s)*

Variable	LIC	NIC	NICU
Cod bag limit	526.344***	255.426***	334.952***
	(16.159)	(38.236)	(36.346)
Cod minimum size	-111.447***	-91.782*	-154.493***
	(26.039)	(52.353)	(32.966)
Haddock bag limit	15.510***	21.941***	11.475***
	(2.770)	(4.027)	(2.067)
Haddock minimum size	-62.944	-197.734***	-186.708***
	(63.743)	(69.341)	(51.331)
Year 2011	249.361	150.490	97.735
	(178.840)	(227.853)	(186.821)
Year 2012	69.203	44.062	35.774
	(179.840)	(229.892)	(186.668)
Year 2013	67.199	43.722	35.037
	(179.840)	(229.892)	(186.668)
Year 2014	3.684	2.004	1.609
	(179.840)	(229.892)	(186.668)
Constant	21,941.34***	40,070.21***	43,275.18***
	(1,496.248)	(2,263.503)	(1,450.056)
R-squared	0.900	0.830	0.777
Adjusted R-squared	0.896	0.818	0.765
F-statistic†	231.55***	74.23***	61.55***
No. Obs.	216	131	150

***p < .01, **p < .05, *p < .10.

† F(8, 207) for LIC, F(8, 122) for NIC, F(8, 141) for NICU

Tables 49 through 52 show the results of the meta-analyses for catch. Increasing cod bag limits increases the number of cod kept, and increasing the minimum size decreases the number of cod kept, as expected. Cod bag limits are more important in determining the number of cod kept in the LIC model whereas cod minimum size limits are more important in the NIC and NICU models due to the nonlinear catch structure in the latter models. The number of cod released is most affected by the minimum size of cod in all models. Cod bag limits have only minor impacts on the number of cod released. In general, haddock regulations have little effect on the number of cod kept or released.

Table 49. *OLS Linear Meta-analysis Results for Cod Keep (in 1000s fish)*

Variable	LIC	NIC	NICU
Cod bag limit	43.792***	32.307***	32.790***
	(1.405)	(3.928)	(1.625)
Cod minimum size	-23.256***	-40.172***	-44.147***
	(2.265)	(5.378)	(1.474)
Haddock bag limit	-0.519**	-0.655	0.325***
	(0.241)	(0.414)	(0.092)
Haddock minimum size	8.634	3.056	-3.830*
	(5.545)	(7.123)	(2.295)
Year 2011	25.862*	37.425	37.358***
	(15.557)	(23.407)	(8.354)
Year 2012	6.984	9.709	9.430
	(15.644)	(23.617)	(8.347)
Year 2013	7.114	9.982	9.769
	(15.644)	(23.617)	(8.347)
Year 2014	0.247	0.306	0.263
	(15.644)	(23.617)	(8.347)
Constant	1,160.78***	1,940.251***	2,072.618***
	(130.158)	(232.529)	(64.844)
R-squared	0.886	0.888	0.957
Adjusted R-squared	0.882	0.881	0.954
F-statistic†	201.85***	120.98***	388.16***
No. Obs.	216	131	150

***$p < .01$, **$p < .05$, *$p < .10$.

† F(8, 207) for LIC, F(8, 122) for NIC, F(8, 141) for NICU

Table 50. *OLS Linear Meta-analysis Results for Cod Release (in 1000s fish)*

Variable	LIC	NIC	NICU
Cod bag limit	4.060***	2.160***	6.251***
	(0.452)	(1.260)	(1.255)
Cod minimum size	48.442***	58.058***	57.183***
	(0.729)	(1.726)	(1.138)
Haddock bag limit	0.722***	1.587***	0.026
	(0.078)	(0.133)	(0.071)
Haddock minimum size	-2.266	-4.375*	-1.549
	(1.784)	(2.286)	(1.772)
Year 2011	-38.863***	-56.494***	-59.696***
	(5.006)	(7.513)	(6.450)
Year 2012	-9.542*	-13.790*	-14.503**
	(5.034)	(7.580)	(6.445)
Year 2013	-9.776*	-14.281*	-15.106**
	(5.034)	(7.580)	(6.445)
Year 2014	-0.310	-0.407	-0.395
	(5.034)	(7.580)	(6.445)
Constant	-875.563***	-1,020.847***	-971.652***
	(41.886)	(74.636)	(50.063)
R-squared	0.960	0.967	0.955
Adjusted R-squared	0.959	0.965	0.952
F-statistic†	621.90***	445.79***	372.54***
No. Obs.	216	131	150

***$p < .01$, **$p < .05$, *$p < .10$.

† F(8, 207) for LIC, F(8, 122) for NIC, F(8, 141) for NICU

Haddock minimum size limits are most important for determining the number of haddock kept in all models. Increasing haddock minimum size decreases the number of haddock kept, as expected. Haddock bag limits have little effect on the number of haddock kept. Increases in the minimum size of cod decreases the number of haddock kept in the LIC and NICU models. This indicates that cod fishing is complementary to haddock fishing and anglers are less likely to fish for haddock when cod regulations are stricter. The coefficient for cod minimum size limits is positive in the NIC model, which can be attributed to preference differences between the NIC and NICU models. Increases in cod bag limits results in slight increases in the number of haddock kept, which also suggests complementarity in fishing.

Table 51. *OLS Linear Meta-analysis Results for Haddock Keep (in 1000s fish)*

Variable	LIC	NIC	NICU
Cod bag limit	0.974***	3.613**	0.858
	(0.246)	(1.820)	(1.439)
Cod minimum size	-2.726***	8.704***	-5.005***
	(0.395)	(3.234)	(1.305)
Haddock bag limit	1.718***	0.254***	1.452***
	(0.0421)	(0.038)	(0.082)
Haddock minimum size	-16.191***	-39.081***	-17.265***
	(0.097)	(3.429)	(2.032)
Year 2011	10.332***	10.034	8.191
	(2.719)	(11.091)	(7.397)
Year 2012	2.616	2.754	2.641
	(2.734)	(11.190)	(7.391)
Year 2013	2.346	2.508	2.390
	(2.734)	(11.190)	(7.391)
Year 2014	0.212	0.215	0.204
	(2.734)	(11.190)	(7.391)
Constant	1,127.576***	1,437.174***	1,354.899***
	(22.747)	(111.744)	(57.410)
R-squared	0.960	0.713	0.830
Adjusted R-squared	0.958	0.694	0.821
F-statistic†	615.35***	37.81***	86.14***
No. Obs.	216	131	150

***p < .01, **p < .05, *p < .10.

† F(8, 207) for LIC, F(8, 122) for NIC, F(8, 141) for NICU

Increasing the minimum size increases the number of haddock released. Haddock bag limits have little effect on the number of haddock released. Increases in effort account for the increase in the number of haddock released when bag limits for cod rise, but the positive relationship between cod minimum size limits and number of haddock released is most likely due to effort switching.

Table 52. *OLS Linear Meta-analysis Results for Haddock Release (in 1000s fish)*

Variable	LIC	NIC	NICU
Cod bag limit	4.580***	2.189***	3.574***
	(1.376)	(0.488)	(0.394)
Cod minimum size	11.333***	3.650***	2.123***
	(2.217)	(0.669)	(0.357)
Haddock bag limit	-1.111***	-0.262***	-0.071***
	(0.236)	(0.051)	(0.022)
Haddock minimum size	164.401***	20.142***	23.352***
	(5.428)	(0.886)	(0.556)
Year 2011	-86.498***	-10.839***	-10.198***
	(15.229)	(2.910)	(2.024)
Year 2012	-22.103	-2.635	-2.556
	(15.314)	(2.936)	(2.022)
Year 2013	-19.72	-2.397	-2.293
	(15.314)	(2.936)	(2.022)
Year 2014	-1.970	-0.212	-0.204
	(15.314)	(2.936)	(2.022)
Constant	-2,652.02***	-392.323***	-439.871***
	(127.410)	(28.912)	(15.706)
R-squared	0.892	0.887	0.946
Adjusted R-squared	0.888	0.880	0.943
F-statistic (8, 207)	214.28***	120.13***	307.43***
No. Obs.	216	131	150

***p < .01, **p < .05, *p < .10.

Conclusion

Understanding the socioeconomic impacts of recreational angling is critical for effectively managing fisheries, particularly when there are commercial interests involved; however, economic analyses of recreational fisheries often rely on strong, unrealistic assumptions. This chapter outlined a theoretical framework for understanding the effect of management changes on angler behavior and welfare by modeling trip catch in stochastic rather than deterministic terms. Additionally, global fishery outcomes were derived using a unique simulation method that overcame data scarcity by combining stated preference survey results with actual stock biomass projections.

The nonlinear models explored in the previous chapter were more stable to changes in regulations than linear models in terms of total recreational fishery values and effort, but effort response was small in all models, which is more consistent with historic and anecdotal evidence of angler behavior. Total WTP and catch decreased with increased regulation stringency, as expected. Haddock bag limits have very little effect on fishery outcomes in terms of effort, total WTP, or catch of either species; however, haddock minimum size regulations affect effort, total WTP, and haddock catch. Effort and catch are highly affected by cod minimum size limits but cod bag limits have a greater effect on total WTP.

The simulation results indicate that local economies remain relatively unaffected by changes in policy. Because effort responses to changes in regulations are small, fishing-related expenditures remain constant and the economic ramifications for providers of recreational angling goods and services are negligible. Additionally, changing minimum size limits are most effective for reducing catch and effort while minimizing impacts related to total consumer fishery values.

Simulation models are limited by algorithmic assumptions but provide insight in situations where real-world data are lacking if all dynamic elements are considered. For recreational fisheries, such models are useful for cost-benefit and allocation analyses because data are often sparse. The framework used in this chapter was specific to two New England groundfish species, so results may not be generalizable; however, combining stated preference methods with biological and regulatory information extends previous work in bioeconomic analyses of recreational angling and provides fisheries managers with additional tools for policy analysis.

Chapter 5: Conclusion

Accurate descriptions of agent behavior are crucial for economic analyses of nonmarket policy options. This dissertation investigates various facets of policy analysis in recreational angling; specifically, whether CEs are well behaved and how CE surveys can be used to understand changes in fish catch and the effect of regulations. The three main phases of the research process, data collection, model specification, and utilizing model outputs, are examined to address these issues.

The first analytical chapter in this dissertation focuses on survey design, which is relevant for many applications outside nonmarket policy evaluation. Survey design is particularly germane for nonmarket applications, though, because data is often nonexistent or lacking variation. Revealed preference methods cannot be used in many cases due to data constraints and researchers must obtain information about economic behavior from other sources. Stated preference methods, and CEs especially, are popular tools for data collection because multiple tradeoffs can be evaluated simultaneously and CEs are relatively simple to construct and analyze. Despite the ease of implementation and interpretation, CEs can approach the limits of human information processing capabilities. This chapter addresses whether increasing cognitive demands induce behavioral responses stemming from task stimuli rather than respondent preferences in choice experiment surveys using five different types of choice experiment instruments: a control, three different questionnaire lengths, and a variation on the number of alternatives in a choice set.

The results of random utility models, heterogeneous scale parameter logit models, and random parameters models for the different task complexity treatments show that survey design does have a significant impact on behavior models and estimated MWTP. MWTP estimates for the number of fish kept were statistically different between treatment groups, with means ranging from $6.83 to $12.08 per fish. The shapes of behavioral models are also affected by CE survey structure. Questionnaire length and error variance have an inverted U-shaped relationship, reaching the plateau around 15 questions. Negative relationships between the number of alternatives and error variance and between information diversity and error variance are observed. Increasing the number of choices in a choice set decreases the respondent error, but this result may be biased because the experiment only included two different levels for the number of alternatives. Increasing the number of different pieces of information a respondent must process increases the error variance in an estimated model. A slight survey fatigue effect is detected but is overshadowed by other forms of task complexity. Testing for individual complexity effects is not the same as testing for multiple complexity effects simultaneously as the estimations appear to be prone to omitted variable bias. For example, order effects were significant in single-source complexity models but not in multi-source complexity models. Conversely, the degree of information overlap is significant in multi-source models but not in single-source models. Homogenous models, or models that ignore possible sources of task complexity, result in statistically smaller parameters than models that account for heterogeneity due to task complexity. Policy analysts should strongly consider cognitive responses to survey design when using CE surveys for behavioral information and valuation estimates.

The second chapter of this dissertation focuses on how choice experiments can be used to understand changes in catch by exploring the attributes included in econometric models, the functional form of included attributes, inclusion of survey respondents, and angler heterogeneity using species-specific random utility models and heterogeneous scale logit models. The results of the analyses show that regulatory attributes cannot be included directly in stated preference behavioral models because the parameter results are nonsensical and inconsistent with observed angler behavior. Nonlinear-in-catch models have comparable MWTP estimates and model fit statistics to linear-in catch models, but are more realistic due to diminishing marginal utility of catch. Inclusion of responses from non-fishery users biases estimated results. Fishery users, identified through self-reported species targeting, have significantly different utility structures and higher MWTP per fish than non-fishery users. Angler age, species experience, and mode have the largest effect on utility structures for models incorporating scale parameters representing heterogeneity in angler characteristics.

The third chapter of this dissertation focuses on modeling changes in regulations in angler utility functions. Because catch is random, regulations will not be binding on every trip. Additionally, regulations have no immediate effect on catch. A framework for understanding angler behavior is developed reflecting these two facts, which are often overlooked by other studies. Expected keep and release per trip are derived using expected catch with distributions truncated by bag limits and minimum size limits. By incorporating expectations of keep and release in the angler's behavior model, analysis of regulatory changes are possible given that a utility of expectations is equivalent to expected utility for a risk neutral angler. This formulation measures changes in the MWTP per trip from regulatory impacts on the underlying distributions for keep and release.

To analyze longer-term effects of regulations, a series of simulations is conducted for different biological and management scenarios. Simulated anglers make trip decisions driven by the behavior model from the stated preference survey results. Characteristics of the trips taken are summarized to produce estimates of total effort, catch, and WTP (total trip values). The simulation results show that effort declines are small, even with very stringent regulations, but total WTP fluctuations are large, reflecting changes in average catch per trip and species composition. Additionally, linear-in-catch models exhibit larger responses to changes in regulations compared to nonlinear-in-catch models. The meta-analysis of the simulations revealed that changes in minimum size restrictions are more effective for reducing effort and total catch, but cod bag limits have a greater effect on total WTP. Haddock bag limits have very little effect on fishery outcomes in terms of effort, total WTP, or catch of either species; however, haddock minimum size regulations affect effort, total WTP, and haddock catch.

The simulation results imply that local economies remain relatively unaffected by changes in regulations. If there is little variation in the total number of trips taken, then there should be little change in consumption of recreational fishing inputs including for-hire services, bait, and tackle. Policy managers should use minimum size limits to alter effort and harvest levels and preserve fishery value.

Appendix A: Brochure and Survey Instrument

Figure 19. Intercept survey brochure, side 1.

WHAT IS THIS SURVEY ABOUT?

Since 1994, NOAA Fisheries has collected economic and human dimensions information from anglers using the marine recreational data collection program. Paired with catch and biomass data, this information forms the core components of most regulatory and allocation decisions. We are occasionally asked to conduct additional studies to supplement existing data.

This year, NOAA Fisheries is gathering more information about your fishing experiences in the Northeast to help us better understand how management policies affect saltwater anglers.

HOW WILL THIS SURVEY BE CONDUCTED?

This survey will be mailed to every saltwater angler that agrees to participate. The survey asks questions about the number of trips they have taken, target species, and the effect of potential management and biological situations on their fishing choices.

HOW WILL THE DATA BE USED?

The results from the survey will be used to assess models of potential effects of various management options on fishing effort and economic impacts.

Additionally, this data will help us assess the value of access, natural resource damages, and measure the benefits of improving fishing quality.

WHY SHOULD I PARTICIPATE?

Your input is important to us. This survey is an opportunity for you to register your opinions about fisheries management and help us improve your recreational fishing experience.

IS THIS SURVEY CONFIDENTIAL?

Participation in this survey is voluntary. All of the information collected is confidential and will be released only as summaries in which no individual's answers can be identified. When you return your completed questionnaire, your name will be deleted from the mailing list and never connected to your answers in any way. Your information will not be sold to any marketing companies and will not be released to other Federal Agencies.

WHAT CAN I DO TO HELP?

Our surveys rely on voluntary cooperation by all saltwater anglers. You can help improve this survey and the results by:

Agreeing to be part of the survey and providing an accurate and current mailing address;

Answering all questions honestly;

Mailing the completed survey back in the enclosed stamped envelope.

THANK YOU!

NOAA Fisheries and our state agency and Commission partners would like to thank the millions of anglers who have participated in our surveys over the last 25 years. We appreciate the valuable contributions you make to support marine fisheries management.

Figure 20. Intercept survey brochure, side 2.

North Atlantic Saltwater Recreational Fishing Survey

Help us improve your recreational fishing experience!

Questions? Call 301.713.2328 x104 or email Sonia.Jarvis@NOAA.gov.

Sponsored by NOAA Fisheries
(National Marine Fisheries Service)
Office of Science and Technology
http://www.st.nmfs.noaa.gov/st5/RecFishEcon.html

This survey is entirely voluntary.
All responses are confidential.

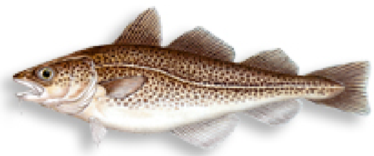
FME Series 01LE12CE01

OMB Control Number 0468-0052 expires 04/30/2011

Figure 21. Survey cover page.

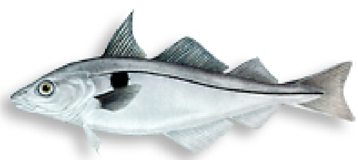
ATLANTIC SALTWATER GROUNDFISH IN THIS SURVEY

ATLANTIC COD



- Also known as "codfish" and "cod"
- Fishing season varies by state
- Average weight is 5-10 pounds
- Average length is 20-30 inches
- Maximum published weight is 211.2 pounds
- Maximum published length is 78.7 inches
- Daily limit varies by state*
- Minimum size in Federal waters is 22 or 24 inches depending on location*

HADDOCK



- Can usually be fished year-round
- Average weight is 2-5 pounds
- Average length is 18-23 inches
- Maximum published weight is 37 pounds
- Maximum published length is 44.1 inches
- No daily limit*
- Minimum size in Federal waters is 18 inches*

POLLOCK

- Also known as "Boston bluefish"
- Can usually be fished year-round
- Average weight is 4-15 pounds
- Average length is 22-26 inches
- Maximum published weight is 70.4 pounds
- Maximum published length is 51.2 inches
- No daily limit*
- No minimum size in Federal waters*

*Regulations as of May 1, 2009

Images © NHFG / Victor Young Illustrations

Questions? Call Sonia Jarvis at 301.713.2328 x104 or email Sonia.Jarvis@NOAA.GOV 2

Figure 22. Information page (page 2 of survey).

SECTION A: YOUR SALTWATER FISHING ACTIVITIES

The questions on <u>this page</u> are about **YOU** and **YOUR** fishing activities.
Do <u>not</u> include any information from other fishing party or household members.
Please <u>print clearly</u> and fill in boxes with a with a ☑ or ☒.

1 Which of the following species have you <u>personally</u> caught or tried to catch in the **last five years**? (Please mark <u>all</u> that apply.)

☐ Cod  ☐ Haddock  ☐ Pollock 

2 Which of the following species have you <u>personally</u> caught or tried to catch **last season**? (Please mark <u>all</u> that apply.)

☐ Cod  ☐ Haddock  ☐ Pollock 

3 What is the **total** number of fishing trips you took last season for **cod**, **pollock**, or **haddock**?

☐☐☐

4 How many fishing trips did you take **last season** for **cod**, **haddock**, or **pollock** on a **private boat**? (A <u>private boat</u> is a boat owned by you, a friend, or an acquaintance where you did <u>not</u> have to pay any rental fees for the vehicle used on the trip.)

☐☐☐

5 How many fishing trips did you take **last season** for **cod**, **haddock**, or **pollock** on a **party boat**, **open boat**, or **head boat**? (A <u>party boat</u>, <u>open boat</u>, or <u>head boat</u> is a boat that takes paying passengers for a defined length of time where the fee paid is <u>per person</u>.)

☐☐☐

6 How many fishing trips did you take **last season** for **cod**, **haddock**, or **pollock** using a **private charter boat**? (A <u>private charter boat</u> is a boat that takes paying passengers for a defined length of time where the fee paid <u>rents the boat and captain</u>, regardless of the number of passengers so long as the legal limit for the vessel is not exceeded.).

☐☐☐

Questions? Call Sonia Jarvis at 301.713.2328 x104 or email Sonia.Jarvis@NOAA.GOV 3

Figure 23. Section A (page 3 of survey).

SECTION B: SALTWATER FISHING TRIPS

Please compare Trip A, Trip B, and Trip C in the table below, then answer questions **1** and **2**.
Compare only the trips on this page. Do not compare these trips to trips on other pages in this survey.
Assume that the trips below are identical in every way except for the features listed in the table.
All regulations remain as they are today unless otherwise noted in the table below.

	TRIP FEATURES	TRIP A	TRIP B	TRIP C
REGULATIONS	**DAILY BAG (TAKE) LIMIT** Number of fish you can legally keep per day.	4 Pollock	10 Cod	Do something other than saltwater fishing.
	MINIMUM SIZE LIMIT Smallest fish you can legally keep of this species.	23 inch Pollock	22 inch Cod	
CATCH	**NUMBER OF LEGAL-SIZE FISH YOU CATCH** These fish are at least legal minimum size. Some fish are released if you catch more than the daily bag limit.	10 Pollock	1 Cod	
	NUMBER OF UNDERSIZED FISH YOU CATCH These fish are below the legal minimum size. All of these fish must be released.	1 Pollock	3 Cod	
	NUMBER OF OTHER FISH YOU KEEP Other fish you catch on this trip that can be legally kept.	3 Cod 6 Haddock	1 Haddock 3 Pollock	
TRIP DETAILS	**TRIP LENGTH** Total time purchased for this trip.	8 Hours	12 Hours	
	TOTAL TRIP COST YOUR share of the fishing trip cost, including bait, ice, fishing equipment, daily license fees, boat rental fees, boat fuel, and round trip transportation costs associated with traveling to and from the fishing location. Travel costs may include vehicle fuel, car rental, tolls, airfare, and parking. This cost does not include the price of food or drink.	$312	$276	

1 I like this trip best:
(Please mark the **ONE** option **YOU** like best with a ☑ or ☒)

TRIP A ☐ TRIP B ☐ TRIP C ☐

2 Please rate the trips listed in the table above. (Circle the number that reflects your opinion best.)

TRIP A	*DISLIKE*	**1**	**2**	**3**	**4**	**5**	**6**	**7**	**8**	**9**	**10**	*LIKE*
TRIP B	*DISLIKE*	**1**	**2**	**3**	**4**	**5**	**6**	**7**	**8**	**9**	**10**	*LIKE*
TRIP C	*DISLIKE*	**1**	**2**	**3**	**4**	**5**	**6**	**7**	**8**	**9**	**10**	*LIKE*

Questions? Call Sonia Jarvis at 301.713.2328 x104 or email Sonia.Jarvis@NOAA.GOV 4

Figure 24. Sample choice experiment (pages 4-27, Base and Length treatments).

SECTION B: SALTWATER FISHING TRIPS

Please compare Trip A, Trip B, Trip C, and Trip D in the table below, then answer questions **1** and **2**.
Compare only the trips on this page. Do not compare these trips to trips on other pages in this survey.
Assume that the trips below are identical in every way except for the features listed in the table.
All regulations remain as they are today unless otherwise noted in the table below.

	TRIP FEATURES	TRIP A	TRIP B	TRIP C	TRIP D
REGULATIONS	**DAILY BAG (TAKE) LIMIT** Number of fish you can legally keep per day.	10 Cod	8 Cod	10 Haddock	Do something other than saltwater fishing.
	MINIMUM SIZE LIMIT Smallest fish you can legally keep of this species.	18 inch Cod	20 inch Cod	12 inch Haddock	
CATCH	**NUMBER OF LEGAL-SIZE FISH YOU CATCH** These fish are at least legal minimum size. Some fish are released if you catch more than the daily bag limit.	10 Cod	6 Cod	1 Haddock	
	NUMBER OF UNDERSIZED FISH YOU CATCH These fish are below the legal minimum size. All of these fish must be released.	1 Cod	3 Cod	1 Haddock	
	NUMBER OF OTHER FISH YOU KEEP Other fish you catch on this trip that can be legally kept.	6 Haddock 3 Pollock	1 Haddock 1 Pollock	3 Cod 6 Pollock	
TRIP DETAILS	**TRIP LENGTH** Total time purchased for this trip.	2 Hours	8 Hours	10 Hours	
	TOTAL TRIP COST YOUR share of the fishing trip cost, including bait, ice, fishing equipment, daily license fees, boat rental fees, boat fuel, and round trip transportation costs associated with traveling to and from the fishing location. Travel costs may include vehicle fuel, car rental, tolls, airfare, and parking. This cost does not include the price of food or drink.	$78	$312	$70	

1 **I like this trip best:**
(Please mark **ONE** option **YOU** like best with a ☑ or ☒)

TRIP A ☐ | TRIP B ☐ | TRIP C ☐ | TRIP D ☐

2 **Please rate the trips listed in the table above.** (Circle the number that reflects your opinion best.)

TRIP A	*DISLIKE*	**1**	**2**	**3**	**4**	**5**	**6**	**7**	**8**	**9**	**10**	*LIKE*
TRIP B	*DISLIKE*	**1**	**2**	**3**	**4**	**5**	**6**	**7**	**8**	**9**	**10**	*LIKE*
TRIP C	*DISLIKE*	**1**	**2**	**3**	**4**	**5**	**6**	**7**	**8**	**9**	**10**	*LIKE*
TRIP D	*DISLIKE*	**1**	**2**	**3**	**4**	**5**	**6**	**7**	**8**	**9**	**10**	*LIKE*

Questions? Call Sonia Jarvis at 301.713.2328 x104 or email Sonia.Jarvis@NOAA.GOV 4

Figure 25. Sample choice experiment (pages 4-11, 3-Alternative treatment).

SECTION B: SALTWATER FISHING TRIPS

Please compare Trip A, Trip B, and Trip C in the table below, then answer questions **1** and **2**.
Compare only the trips on this page. Do not compare these trips to trips on other pages in this survey.
Assume that the trips below are identical in every way except for the features listed in the table.
All regulations remain as they are today unless otherwise noted in the table below.

	TRIP FEATURES	TRIP A	TRIP B	TRIP C
REGULATIONS	**DAILY BAG (TAKE) LIMIT** Number of fish you can legally keep per day.	8 Pollock	4 Cod	Do something other than saltwater fishing.
	MINIMUM SIZE LIMIT Smallest fish you can legally keep of this species.	23 inch Pollock	24 inch Cod	
CATCH	**NUMBER OF LEGAL-SIZE FISH YOU CATCH** These fish are at least legal minimum size. Some fish are released if you catch more than the daily bag limit.	1 Pollock	3 Cod	
	NUMBER OF UNDERSIZED FISH YOU CATCH These fish are below the legal minimum size. All of these fish must be released.	3 Pollock	1 Cod	
	NUMBER OF OTHER FISH YOU KEEP Other fish you catch on this trip that can be legally kept.	1 Cod 3 Haddock	6 Haddock 1 Pollock	
COST	**TOTAL TRIP COST** YOUR share of the fishing trip cost, including bait, ice, fishing equipment, daily license fees, boat rental fees, boat fuel, and round trip transportation costs associated with traveling to and from the fishing location. Travel costs may include vehicle fuel, car rental, tolls, airfare, and parking. This cost does not include the price of food or drink.	$15	$120	

1 I like this trip best:
(Please mark the **ONE** option **YOU** like best with a ☑ or ☒)

TRIP A ☐ TRIP B ☐ TRIP C ☐

2 Please rate the trips listed in the table above. (Circle the number that reflects your opinion best.)

TRIP A	*DISLIKE*	**1**	**2**	**3**	**4**	**5**	**6**	**7**	**8**	**9**	**10**	*LIKE*
TRIP B	*DISLIKE*	**1**	**2**	**3**	**4**	**5**	**6**	**7**	**8**	**9**	**10**	*LIKE*
TRIP C	*DISLIKE*	**1**	**2**	**3**	**4**	**5**	**6**	**7**	**8**	**9**	**10**	*LIKE*

Questions? Call Sonia Jarvis at 301.713.2328 x104 or email Sonia.Jarvis@NOAA.GOV 4

Figure 26. Sample choice experiment (pages 4-11, Shore treatment).

SECTION N: ABOUT YOU AND YOUR HOUSEHOLD

1 Are you male or female? ☐ Male ☐ Female

2 How old are you? ☐ 18-24 years ☐ 25-44 years ☐ 45-64 years ☐ 65 years and over

3 What is the highest level of education you have completed? (Please mark only <u>one</u> category.)

- ☐ Some high school
- ☐ High school graduate or GED completion
- ☐ 2-year degree or trade school graduate
- ☐ Attended some college
- ☐ 4-year degree
- ☐ Some graduate school
- ☐ Master's degree (ex: MA, MS, MBA)
- ☐ Professional or doctoral degree

4 What is your ethnic background? (Please mark all that apply.)

- ☐ White
- ☐ Black or African American
- ☐ Hispanic or Latino
- ☐ Asian or Pacific Islander
- ☐ American Indian
- ☐ Other: ___________________________

5 Which of the following categories best describes your **household's total annual income before taxes** in 2008? (Please mark only <u>one</u> category.)

- ☐ Less than $20,000
- ☐ $20,000—39,999
- ☐ $40,000—59,999
- ☐ $60,000—79,999
- ☐ $80,000—99,999
- ☐ $100,000—149,999
- ☐ $150,000—199,999
- ☐ $200,000 or more

6 Was this survey completed by the person to whom it was mailed? ☐ Yes ☐ No

THANK YOU FOR PARTICIPATING!

Your answers to this survey will help us better manage our fisheries. If you have any questions or comments regarding the survey, contact Sonia Jarvis at **301.713.2328 x104** or email **Sonia.Jarvis@NOAA.gov**.

Cover page image courtesy of Kevin Sullivan, New Hampshire Fish and Game Department. Notwithstanding any other provisions of the law; no person is required to respond to, nor shall any person be subject to a penalty for failure to comply with a collection of information subject to the requirement of the Paperwork Reduction Act, unless that collection of information displays a currently valid OMB Control Number. Public reporting burden for this survey is estimated to average 20 minutes per response, including the time for reviewing instructions, searching existing data sources, gathering and maintaining the data needed, and completing and reviewing the collection of information. Send comments regarding this burden estimate or any other aspect of this collection of information, including suggestions for reducing this burden, to ***Sonia Jarvis, NMFS F/ST5, 1315 East West Highway, Silver Spring, MD 20901.***

Figure 27. Survey last page (back cover).

Appendix B: Experimental Designs

Overview of Optimal Experimental Designs

Optimal design methodology has arisen in response to situations where classical tabulated designs (full factorial, fractional factorial, Latin squares, Box-Behnken, etc.) are incapable of adequately addressing the experiment. In this case, the need for a non-classical tabulated design comes from a mixture of model nonlinearity and an odd combination of factor-level requirements. The terms *factor* and *attribute* are used interchangeably in the literature to denote variables of interest. The term *level* refers to fixed values for factors that have more than one value (some factors may have only one value). A researcher desires to use an optimal design because it ensures that the outcome of his experiments, the fitted mathematical function (or model), results in predicted values that are close to the observed raw data. Changes to the experimental design directly affects the degree of control a researcher has on the fit of models, particularly when external restrictions are imposed. As stated by Johnson and Mansfield (2008), “no amount of complex analysis can compensate for a poor survey design that can generate only flawed [CE] data.”

A good experimental design ensures that factor effects are not contaminated by other factor effects or the survey design. The goal is to construct a model-dependent design that enables the estimation of all primary relationships, has capacity for an alternative model, minimizes variation in estimated coefficients, increases sample size where necessary to minimize influences by noisy areas or capture steep changes in the model, replicates sample points as much as possible, and randomizes the sequence of data values during collection (Croarkin & Tobias, 2010). The use of efficiency-maximizing algorithms does not guarantee designs that are mathematically optimal, but the results are commonly accepted as such.

Analyses of experimental designs with factor-level matrices $\mathbf{X}$ are based on $\mathbf{X'X}$, the sum of squares (or information matrix); $(\mathbf{X'X})^{-1}$, the matrix inverse of the sum of squares; and the eigenvalues of the inverse, A. The main attributes of interest when considering the optimality of potential designs are balance and orthogonality. A balanced design is an experimental design where all runs p have the same number of observations, guaranteeing an orthogonal relationship between the intercept and each effect. A run or factor combination is one alternative (vector of attributes) in a choice experiment. Mathematically, all off-diagonal elements in the intercept row and column are zero for $(\mathbf{X'X})^{-1}$. An orthogonal design is one where combinations of levels for specific factors are proportional or equal, and the submatrix of $(\mathbf{X'X})^{-1}$ excluding the intercept row and column is diagonal. A balanced and orthogonal design meets specific efficiency criteria so that, for $\mathbf{N}$ repetitions of the design, $(\mathbf{X'X})^{-1}$ is diagonal and $\mathbf{X'X} = \mathbf{NI}$ where $\mathbf{I}$ is an identity matrix of dimensions equal to the number of runs in the experiment.

There are four main types of efficiency criteria. To achieve D-optimality, the experimental design minimizes the generalized variance of the parameter estimates (measured by the geometric mean of the eigenvalues $|(\mathbf{X'X})^{-1}|^{1/p}$). In A-optimality, the experimental design minimizes the average variance of the parameter estimates (measured by $\mathrm{tr}((\mathbf{X'X})^{-1})/p$). G-optimal experimental designs maximize the maximum variance of the predicted values as measured by the maximum standard error of prediction over the candidate set. V-optimal designs minimize the average variance of the predicted values. A-, D-, and G- efficiency measures are convex functions of A and are usually correlated. All three measures increase when designs tend towards balance and orthogonality. The most common experimental design algorithms are based on maximizing D-efficiency because the algorithms are less computationally expensive than any of the other methods and the ratio of any two designs does not change, whereas some measures (e.g., A-efficiency) may vary depending on the coding scheme.

Only full-factorial designs achieve D-efficiency scores of 100. Full-factorial designs contain every possible combination of attribute and attribute level. It is not possible to administer such surveys in most cases either due to cost or respondent burden. For example, a three alternative, three attribute study with three levels per attribute would require $3^3 \times 3^3 \times 3^3 = 19{,}683$ choice sets for an orthogonal and balanced design (Louviere et al., 2000). Most experimental designs employed in the social sciences are blocked fractional-factorial designs with D-efficiency scores below 100. Subsets of choice experiments are selected from the full-factorial and then arranged into several blocks or survey versions, minimizing respondent burden, but also reducing the ability to identify all combinations of attribute effects.

Researchers may elect to cleanly estimate any combination of main effects and interaction effects. A *main effect* is a change in the outcome variable Y based on a change in one factor x_j. For example, increasing the number of candies consumed by a child from 3 to 10 induces 2 additional tantrums. An *interaction effect* is a non-additive change in the outcome variable Y based on a change in two or more factors x_j and x_k. For example, combinations of water and light levels have different effects on plant height. Desired estimable effects must be explicitly specified in the design search algorithm for fractional-factorial designs; otherwise, other effects may confound the results. The number and type of attribute effects selected determines the design's D-efficiency score and the total number of choice experiments that must be run to achieve the statistical properties desired. In the example above, only 243 choice sets are required to identify all main effects and two-way interaction effects. This number can be divided among several blocks to minimize respondent burden. The three-alternative example survey could be decomposed into 27 versions with 9 questions each, or 9 versions with 27 questions each.

Utility and the Experimental Design

Based on Lancaster's approach to consumer theory (1966, 1971) and random utility theory, the basic assumptions of the CE stated preference technique are that the utility of a good consists of the utility of the attributes characterizing the good and the researcher is only able to observe a component of the consumer's utility function. Classically, the utility function is assumed to be a linear-in-parameters function of product attributes and net income,

$$\mathrm{V}_{ikn}(M_n - P_{ik}, \mathbf{z}_{ik}, \varepsilon_{ikn}) = \boldsymbol{\beta}\mathbf{L} + \varepsilon_{ikn}, \tag{B.1}$$

where V_{ikn} denotes the indirect utility of individual n for choosing alternative i from CE k, M_n is the respondent's annual household income, P_{ik} is the price of alternative i in choice set k, $\mathbf{L} = [M_n - P_{ik}\mathbf{z}_{ik}']'$ is a column vector of regressors, and the row vector $\boldsymbol{\beta} = [\beta_M\ \beta_z]$ represents the portion of the individual's utility function (part-worths) that are attributable to characteristics of the environmental good or policy specified in **L**. The random component or error term ε_{ikn} may include characteristics of the alternative omitted by the researcher, measurement errors, unobserved characteristics of the individual, or the choice context.

The individual n is assumed to choose alternative i from CE k if the utility of that alternative exceeds the utility associated with any other alternative in the choice set $\mathbf{S}_{kn}$, which can be expressed as the probability

$$P_{kn}(i \mid \mathbf{S}_{kn}) = \Pr[V_{ikn}(M_n - P_{ik}, \mathbf{z}_{ik}, \varepsilon_{ikn}) \geq V_{jkn}(M_n - P_{jk}, \mathbf{z}_{jk}, \varepsilon_{jkn})] \quad \forall\, j \neq i \in \mathbf{S}_{kn}. \tag{B.2}$$

Using the linear-in-parameters specification above, the probability function can be rewritten as

$$P_{ikn} = \frac{\exp(\boldsymbol{\beta}[M_n - P_{ik} z_{ik}]')}{1 + \sum_{j=1}^{I} \exp(\boldsymbol{\beta}[M_n - P_{jk} z_{jk}]')}. \tag{B.3}$$

Assuming completely independent choices, the log-likelihood function is thus

$$\ln\{L(\beta)\} = \sum_{n=1}^{N} \sum_{k=1}^{K} \sum_{i=1}^{I} y_{ikn} \ln(P_{ikn}), \tag{B.4}$$

where y_{ikn} is 1 if individual n chooses alternative i in CE k, 0 otherwise.

Optimal experimental designs are selected such that the vectors z_{ik} minimize the variance-covariance matrix of the parameter estimates β, which is asymptotically equivalent to

$$\Sigma = \left(\mathrm{R}'\mathrm{PR}\right)^{-1} = \left[\sum_{n=1}^{N} \sum_{k=1}^{K} \sum_{i=1}^{I} r'_{ikn} \mathrm{P}_{ikn} r_{ikn}\right]^{-1}, \tag{B.5}$$

where $$r_{ikn} = z_{ik} - \sum_{i=1}^{I} z_{ik} P_{ikn}. \tag{B.6}$$

If the assumed β used in constructing the design is correct and there are no external sources of variance, then the variance matrix of the design should be very close, if not exactly, the variance matrix of the resulting analysis. Equation B.5 shows how increasing the number of alternatives, choice sets per respondent, and number of respondents increases the statistical efficiency of the design and minimizes the variance-covariance matrix of β. Further information regarding optimal design and econometric methods can be found in Train (2003) and in Kuhfeld's SAS documentation (Kuhfeld, 2009).

Appendix C: Simulation Results

Table 53. *Expected Keep Using Theoretical Framework*

Regulations	2011	2012	2013	2014	2015
C: 20", 10 fish	5.16	5.12	5.12	5.11	5.11
C: 20", 20 fish	6.00	5.96	5.95	5.94	5.94
C: 22", 10 fish	4.97	4.91	4.91	4.89	4.89
C: 22", 15 fish	5.66	5.58	5.58	5.56	5.56
C: 24", 10 fish	4.70	4.59	4.59	4.55	4.55
C: 24", 8 fish	4.25	4.15	4.15	4.12	4.12
C: 26", 10 fish	4.40	4.24	4.24	4.19	4.19
C: 26", 5 fish	3.04	2.93	2.93	2.90	2.90
C: 26", 8 fish	3.98	3.84	3.84	3.80	3.80
H: 14", no limit	3.72	3.72	3.72	3.72	3.72
H: 16", no limit	3.71	3.71	3.71	3.71	3.71
H: 18", 10 fish	3.19	3.17	3.16	3.16	3.16
H: 18", no limit	3.66	3.64	3.63	3.63	3.63
H: 19", no limit	3.61	3.58	3.56	3.56	3.56
H: 20", 10 fish	3.07	2.96	2.91	2.91	2.91
H: 20", 15 fish	3.32	3.20	3.15	3.15	3.15
H: 20", no limit	3.52	3.40	3.34	3.34	3.34
H: 21", 10 fish	2.89	2.76	2.69	2.68	2.69

Table 54. *Expected Release Using Theoretical Framework*

Regulations	2011	2012	2013	2014	2015
C: 20", 10 fish	1.09	1.12	1.12	1.13	1.13
C: 20", 20 fish	0.25	0.29	0.29	0.30	0.30
C: 22", 10 fish	1.28	1.34	1.34	1.36	1.36
C: 22", 15 fish	0.59	0.66	0.66	0.69	0.69
C: 24", 10 fish	1.55	1.66	1.66	1.69	1.69
C: 24", 8 fish	1.99	2.09	2.09	2.12	2.12
C: 26", 10 fish	1.85	2.00	2.00	2.05	2.05
C: 26", 5 fish	3.21	3.31	3.31	3.35	3.35
C: 26", 8 fish	2.27	2.41	2.41	2.45	2.45
H: 16", no limit	3.72	3.72	3.72	3.72	3.72
H: 18", 10 fish	3.71	3.70	3.70	3.70	3.70
H: 18", no limit	3.71	3.70	3.70	3.70	3.70
H: 19", no limit	3.69	3.68	3.68	3.68	3.68
H: 20", 10 fish	3.67	3.64	3.62	3.62	3.62
H: 20", 15 fish	3.67	3.64	3.62	3.62	3.62
H: 20", no limit	3.67	3.64	3.62	3.62	3.62
H: 21", 10 fish	3.61	3.57	3.55	3.55	3.55
H: 14", no limit	3.72	3.72	3.72	3.72	3.72
H: 18", no limit	3.71	3.70	3.70	3.70	3.70

Table 55. *Simulated Catch Estimates Averaged Across Scenario Years (LIC Model)*

Regulations	Total Cod Kept	Total Cod Released	Total Haddock Kept	Total Haddock Released
20 C ≥ 20", any H ≥ 14"	1,582,754	179,185	1,019,669	4,419
10 C ≥ 20", any H ≥ 18"	1,293,151	144,147	968,280	44,066
15 C ≥ 22", any H ≥ 16"	1,379,355	299,736	1,003,076	14,834
15 C ≥ 22", any H ≥ 18"	1,377,738	298,964	969,773	44,182
10 C ≥ 22", any H ≥ 16"	1,217,924	261,760	999,165	14,739
10 C ≥ 22", any H ≥ 18"	1,216,340	260,989	965,988	43,912
10 C ≥ 22", 15 H ≥ 20"	1,212,535	259,119	842,278	88,187
2 C ≥ 22", any H ≥ 18"	415,577	62,957	933,590	41,898
10 C ≥ 23", any H ≥ 18"	1,195,206	292,128	965,129	43,869
2 C ≥ 23", any H ≥ 18"	414,437	71,611	933,738	41,907
10 C ≥ 24", any H ≥ 16"	1,160,422	345,808	996,694	14,690
10 C ≥ 24", any H ≥ 18"	1,158,870	344,809	963,619	43,768
10 C ≥ 24", any H ≥ 19"	1,157,178	343,625	925,538	76,794
8 C ≥ 24", 10 H ≥ 18"	1,052,783	309,332	789,009	34,875
2 C ≥ 24", any H ≥ 18"	412,407	86,804	933,923	41,909
10 C ≥ 26", any H ≥ 18"	1,072,476	467,591	960,425	43,586
10 C ≥ 26", any H ≥ 20"	1,070,080	465,222	900,378	95,589
5 C ≥ 26", 10 H ≥ 21"	775,739	318,232	718,946	110,249

Table 56. *Simulated Catch Estimates Averaged Across Scenario Years (NIC Model)*

Regulations	Total Cod Kept	Total Cod Released	Total Haddock Kept	Total Haddock Released
20 C ≥ 20", any H ≥ 14"	1,751,047	206,515	1,108,469	5,010
10 C ≥ 20", any H ≥ 18"	1,467,583	172,251	1,060,140	50,988
15 C ≥ 22", any H ≥ 16"	1,546,763	352,564	1,094,242	17,045
15 C ≥ 22", any H ≥ 18"	1,545,379	351,873	1,059,371	50,900
10 C ≥ 22", any H ≥ 16"	1,385,337	314,518	1,093,648	17,027
10 C ≥ 22", any H ≥ 18"	1,383,774	313,775	1,058,785	50,852
10 C ≥ 22", 15 H ≥ 20"	1,380,785	312,330	935,612	104,719
2 C ≥ 22", any H ≥ 18"	476,766	91,742	1,031,391	48,996
10 C ≥ 23", any H ≥ 18"	1,359,807	351,846	1,058,258	50,816
2 C ≥ 23", any H ≥ 18"	475,095	104,734	1,031,793	49,007
10 C ≥ 24", any H ≥ 16"	1,319,375	417,483	1,091,895	16,991
10 C ≥ 24", any H ≥ 18"	1,317,907	416,643	1,057,125	50,745
10 C ≥ 24", any H ≥ 19"	1,316,109	415,590	1,016,744	89,466
8 C ≥ 24", 10 H ≥ 18"	1,209,470	380,738	893,813	42,379
2 C ≥ 24", any H ≥ 18"	471,829	128,025	1,032,327	49,010
10 C ≥ 26", any H ≥ 18"	1,216,376	565,016	1,054,631	50,573
10 C ≥ 26", any H ≥ 20"	1,213,723	563,028	990,568	111,782
5 C ≥ 26", 10 H ≥ 21"	892,091	403,910	816,677	136,224

Table 57. *Simulated Catch Estimates Averaged Across Scenario Years (NICU Model)*

Regulations	Total Cod Kept	Total Cod Released	Total Haddock Kept	Total Haddock Released
20 C ≥ 20", any H ≥ 14"	1,765,050	208,648	1,115,547	5,078
10 C ≥ 20", any H ≥ 18"	1,481,631	174,460	1,067,558	51,657
15 C ≥ 22", any H ≥ 16"	1,559,850	356,539	1,101,473	17,253
15 C ≥ 22", any H ≥ 18"	1,558,823	355,997	1,066,641	51,554
10 C ≥ 22", any H ≥ 16"	1,398,326	318,470	1,101,078	17,242
10 C ≥ 22", any H ≥ 18"	1,397,046	317,867	1,066,248	51,513
10 C ≥ 22", 15 H ≥ 20"	1,394,460	316,590	943,064	106,197
2 C ≥ 22", any H ≥ 18"	480,159	93,665	1,040,196	49,774
10 C ≥ 23", any H ≥ 18"	1,372,679	356,394	1,065,671	51,479
2 C ≥ 23", any H ≥ 18"	478,496	106,887	1,040,548	49,778
10 C ≥ 24", any H ≥ 16"	1,331,458	422,742	1,099,434	17,212
10 C ≥ 24", any H ≥ 18"	1,330,276	422,067	1,064,638	51,420
10 C ≥ 24", any H ≥ 19"	1,328,713	421,148	1,024,159	90,627
8 C ≥ 24", 10 H ≥ 18"	1,221,057	385,903	901,079	43,039
2 C ≥ 24", any H ≥ 18"	475,208	130,615	1,041,104	49,786
10 C ≥ 26", any H ≥ 18"	1,227,251	572,125	1,062,038	51,252
10 C ≥ 26", any H ≥ 20"	1,225,092	570,443	997,991	113,282
5 C ≥ 26", 10 H ≥ 21"	901,296	410,368	824,636	138,672

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