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Deriving Species-Specific Benefits Measures for Expected Catch Improvements in a Random Utility Framework

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Abstract *A random utility model of site choice is applied to marine recreational fishing trips in North Carolina. Expectations of catch rates of different species groups are estimated using a Poisson specification. A likelihood ratio test is employed to separate the expected catch of red drum (Scianops ocellatus) from a larger species group. Per trip measures of compensating variation are measured for two alternative specifications of an improvement in red drum catch, and the catch of other species groups. Willingness-to-pay measures are reported by fishing mode according to target species. Anglers targeting a particular species have higher willingness-to-pay than anglers targeting a different species, and anglers with any target have higher willingness-to-pay than anglers with no target.*

Key words Expected catch improvements, random utility, species-specific willingness-to-pay.

Introduction

The recent collapse of many New England ground fisheries has focused attention on the problem of over-fishing.[1] This problem is not limited to fisheries in the northeast. Many stocks in the southern United States are currently over-utilized. Of the twenty-nine major marine fish species harvested in North Carolina, seventeen are considered stressed or over-fished. Regulations designed to enhance these stocks will likely be implemented at the species level. In addition to evaluating the economic effects of stock-enhancing policies, there are economic effects imposed by harvest reallocations between competing users (*e.g.*, commercial vs. recreational disputes). These too will require economic analyses of policy-induced effects at the species level (Easley 1992).

To estimate the costs and benefits to recreational fishing of single-species stock enhancement and reallocation activities, a species-specific welfare measure is needed. Past studies of marine recreational fishing behavior have focused on valuing an improvement in the catch of all species groups or species aggregates. Most of these studies have focused on the valuation of environmental damage from pollution, and the benefits of water quality improvements from pollution controls. See for example Kaoru (1988), and Kaoru, Smith, and Liu (1995). Studies by Bockstael, McConnell, and Strand (1989) and Milon (1988) have analyzed the value of catch improvements

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[1] See NMFS 1991.

for different species groups. However, the degree of species aggregation employed in these works probably makes them insufficient for accurate analysis of individual species policies for which many regulations continue to be promulgated.

Two broad categories of economic models have been used to value recreation related benefits. These are recreation demand models, which include travel cost models and discrete choice models, and hypothetical or contingent valuation methods.[2] Travel cost models of recreation demand are based on the idea that the price of a recreational experience is represented at least in part by the costs incurred in accessing the recreation site. One such model, the random utility model, views the choice of recreation site as a function of the utility derived from the alternative sites. A trip utility function is specified to be a function of site attributes, and can be estimated using data on individual trips and site characteristics. The estimated utility function can then be used to measure the compensating variation from a change in one of the site attributes. For recreational fishing trips, a characteristic likely to influence site choice is the expected catch rate of different species. The formation and modeling of these expectations for use in a random utility specification are important issues that have not received much attention in the literature. As there are dozens of different species which can potentially be caught on a given fishing trip, having the expected catch of each species as a site characteristic is not a realistic option for deriving species-specific measures. However, we can value the benefits of an improvement in individual species catch by separating the policy-relevant species from the other aggregates.

In this paper we estimate the benefits of improving the recreational catch of red drum (*Scianops ocellatus*) in North Carolina. We further demonstrate two alternatives for estimating effects of changes in stock on expected catch rates—a critical link to the change in recreation demand. North Carolina is currently the only state which allows a relatively large commercial harvest of red drum. It has been argued that the red drum are more valuable as a recreational fish, and that measures should be taken to shift the current allocation of the stock towards the recreational fishing sector. This analysis will allow us to begin to measure the benefits which may accrue to recreational anglers if such reallocation measures are implemented. In addition to red drum, benefits measures for an improvement in four species aggregates are derived. Expected catch of the different species is modeled as following a Poisson process. A random utility model of site choice is estimated as a function of (predicted) expected catch rates (including those of red drum), travel costs, and other site characteristics.

Random Utility Model of Site Choice

Discrete choice or random utility models of recreation behavior focus on the choice among alternative sites for a given recreational trip. These models are well established in the literature.[3] For a given trip choice occasion, the choice among alternative sites is a mutually exclusive decision. The site choice decision is modeled as a function of the utility derived from the alternative sites. It is assumed that the site chosen is the one that yields the maximum utility to the individual, which is a function of site and individual characteristics. Some of these characteristics are observable while others are known only to the individual. Hence from the perspective of the researcher, there is a random component to utility. Hanemann (1982) has shown

[2] For an overview of these methods and a brief history of the valuation of recreation experiences see Bockstael, McConnell, and Strand (1989), and Bockstael, Hanemann, and Kling (1987).

[3] See Bockstael, Hanemann, and Kling (1987), Bockstael, McConnell, and Strand (1989), and Kaoru, Smith, and Liu (1995) for good examples.

that by assuming that the observable and random components of utility enter the utility function in a linear fashion, and that the random component follows a type 1 extreme value distribution, the utility function can be estimated using a simple logit model. To formalize this model, assume that the *k*th angler's indirect utility from a visit to site *i*, V_{ik}, is given by

$$V_{ik} = V(Z_{ik}) + \varepsilon_{ik} \tag{1}$$

where Z_{ik} is a vector of all observable variables which affect the utility derived from a visit to site *i* by angler *k*, and ε_{ik} is the random component of utility known only to angler *k*.

Angler *k* visits site *i* if

$$V(Z_{ik}) + \varepsilon_{ik} > V(Z_{jk}) + \varepsilon_{jk} \quad \text{for all } j \neq i \tag{2}$$

By assuming that the error terms are independent and identically distributed as type 1 extreme value random variables, the probability that an individual will visit site *i* on a given trip, π_i, can be estimated using a simple multinomial logit

$$\pi_{ik} = \exp(V_{ik}) / \sum \exp(V_{jk}) \tag{3}$$

Given an estimate of the indirect utility function in equation (1), the benefits of an improvement in the quality of one of the site characteristics can be estimated as the per trip compensating variation for the logit model

$$CV_k = (1/\beta)\left[\ln \textstyle\sum_i \exp(V_{ik}^0) - \ln \sum_i \exp(V_{ik}^1)\right] \tag{4}$$

where β is the coefficient on access price in the indirect utility function, and V^0 and V^1 represent utility before and after the quality change.

Model Specification and Data

We will estimate the indirect utility function outlined above separately for boat and shore mode recreational fishing trips in North Carolina by employing the common assumption that the indirect utility from a visit to a given site is a linear function of access costs and site quality. Access costs are measured as direct travel costs plus the opportunity cost of travel time. The quality of each site is measured using site-specific characteristics, which include estimates of the expected catch rate of different species groups. These predictions of expected catch—also estimated separately for boat and shore fishing—are generated using unique catch models for each of the four species groups and the red drum.

As our goal is to quantify benefits from a species-specific stock enhancement, we require that the value of an improvement in the catch of a single species be derived. In North Carolina, dozens of different species are regularly caught by recreational anglers. Having such a large number of catch rates as choice variables would be very difficult to successfully model, and is probably an unrealistic view of the way site choice decisions are made. It is more realistic to assume that anglers view potential catch in broad categories of fish according to where and how they can be caught. We therefore aggregate most species into broad groups and consider the species of interest for policy analysis (red drum) separately. Smaller fish will be grouped according to whether they are caught at the bottom or at the surface of the water, and larger fish will comprise another category. See table 1.

Table 1
Species Groups

Species Group	Species Included
Red drum	Red drum
Other drum family	Croakers, chubbyu, banded drum, black drum, sand drum, spotted drum, star drum, high-hat, jackknife, kingfish, perch, seatrout, spot, weakfish
Surface fish	Bluefish, barracuda, cobia, dolphinfish, mackerel family (includes tuna)
Bottom fish	Flounder family, cod family, snapper, grouper, jack, grunt, seabass, porgy, wreckfish
Bigger fish	Billfish family, swordfish, tarpon family, sharks, skates, rays, dogfish

For this study, we use the Marine Recreational Fisheries Statistics Survey (MRFSS) intercept data for North Carolina from the period 1987–90.[4] The MRFSS data was collected by on-site interview of recreational anglers who were intercepted after their trip had taken place at 261 different locations across eleven coastal North Carolina counties.[5] Information was collected about aspects of the current trip such as mode of fishing, species targeted, and quantity and type of fish caught. Angler characteristics such as county of residence, age, gender, and fishing experience were also collected. We use this survey data for two distinct estimations. First, the full set of 25,532 complete interviews from the period 1987–89 are used to generate catch equations.[6] The results of this estimation (which is outlined in the next section) are then used to predict expected catch rates to serve as input into the estimation of the boat and shore utility functions for a sample of 2,892 single-day anglers from 1990. The value of improving the expected catch of red drum is then derived for the 1990 sample.

Random utility models employing on-site interview data face the issue of estimation with a choice-based or endogenously stratified sample. Because the probability of an individual being chosen into the sample may depend on which site was chosen, resulting parameter estimates may be biased. The correction for such bias, detailed by Train (pp. 48–49) and Pudney (pp. 102–5), requires including a set of alternative specific constants in the utility function, and weighting these constants using population site visitation shares. Unfortunately, population site share estimates are often not available, as is the case here. However, when the on-site sampling distribution coincides with the distribution of individuals across sites, the sampling can be considered exogenous, and is likely to produce consistent model estimates without weighting (for discussion, see Manski and Lerman 1977). Sampling conducted in the Marine Recreational Fisheries Statistics Survey was undertaken according to estimates of expected fishing activity at the sites. Hence, more anglers are inter-

[4] The MRFS survey was principally designed as a creel survey. The objective of the survey was "to determine finfish catch, effort, participation, fishing mode, area of fishing, state and county residence, number of trips, and biological data (weight and length by species)" (NCDMF 1994). Because we are limited to the set of variables collected for these purposes, the data may be less than optimal for use in policy analysis. We acknowledge this potential limitation, and note that it may affect the accuracy of empirical results.

[5] Interviews conducted by the North Carolina Division of Marine Fisheries. Total interviews completed for the years 1987–90 are 7,700, 8,215, 10,950, and 10,862 respectively. See Marine Recreational Fisheries Statistics Survey (1994) for further detail.

[6] Incomplete interviews were removed from the sample.

cepted at sites that are characterized as receiving higher than average visits; hence the proportion of anglers interviewed at the sites roughly approximates the proportion of anglers selecting the sites. We therefore do not consider the choice-based sampling bias to have a serious effect on our estimation.

For estimation of both the catch equations and the utility function, intercept points are aggregated to the county level. This aggregation may introduce bias into the estimation of the utility function coefficients. To account for this potential bias, we follow the suggestion in Ben-Akiva and Lerman (1985) and use the log of the number of intercept points as a quality variable in the indirect utility function.[7] For boat fishing, this is the number of launch points identified in the MRFSS data, and for shore fishing it is the number of intercept points where shore fishing is an alternative.[8] Two pairs of counties (Tyrell/Dare and Craven/Pamlico) are aggregated together due to limited data availability for one site in each pair. The counties aggregated together border one another, and offer similar fishing opportunities. Thus there are nine counties over which choice will be modeled. These are Dare/Tyrell, Hyde, Beaufort, Pamlico/Craven, Carteret, Onslow, Pender, New Hanover, and Brunswick. See figure 1.

The geography of the North Carolina coast is such that different types of fishing opportunities are available in different counties. Counties which include the barrier island chain known as the Outer Banks (Dare, Hyde, and Carteret) offer both ocean and sound fishing opportunities. Craven/Pamlico and Beaufort counties offer only sound fishing opportunities, and the remaining four sites (Onslow, Pender, New Hanover, and Brunswick counties) primarily offer ocean fishing opportunities.[9] To account for these differences, we employ two dummy variables in the site choice model, so that the utility derived from a site is a function of the type of fishing opportunities available at that site. Hence, in addition to travel costs, we have five expected catch rates, two site type dummy variables, and the site-specific log of the number of intercept points as quality variables in our site choice model.

We can therefore write an explicit form for equation (1), the indirect utility from a recreational fishing trip to site *i* for angler *k*, as:

$$\begin{aligned} V_{ik} = {} & \alpha_1(\textit{travel cost}_{ik}) + \alpha_2(\textit{expected catch red drum}_{ik}) \\ & + \alpha_3(\textit{expected catch other drum}_{ik}) + \alpha_4(\textit{expected catch surface fish}_{ik}) \\ & + \alpha_5(\textit{expected catch bottom fish}_{ik}) + \alpha_6(\textit{expected catch bigger fish}_{ik}) \\ & + \alpha_7(\textit{outerbanks dummy}_i) + \alpha_8(\textit{sound site dummy}_i) \\ & + \alpha_9(\textit{log of intercept points}_i) \end{aligned} \tag{5}$$

where, $\textit{travel cost}_{ik}$ = (0.41)(round-trip distance in miles to site *i* by angler *k*) +

[7] This aggregation of sites may compromise the accuracy of subsequent estimation. McFadden (1978) demonstrates that the bias resulting from the aggregation of individual sites is that introduced by omitting two log terms from the estimation of the site utility function: the number of individual sites in each aggregate, and a measure of the heterogeneity of individual sites in each aggregate. Parsons and Needelman (1992) test the empirical implications of aggregation bias by comparing models that include these terms with those that do not. Their results show that in the context of predicting behavior, an aggregate model that includes a correction for size, such as the one used in this work, is a reasonable approximation to a complete information model, but is less reliable in terms of approximating benefits. We lack a measure of heterogeneity. As the individual intercept points in each North Carolina county are proximate and offer very similar fishing opportunities in the majority of cases, we feel that the bias introduced by omitting the heterogeneity term is minimal.

[8] We note that site definitions based on launch points for boat trips do not account for the possibility that an angler may travel by boat to another county. The MRFSS data do not permit a more accurate modeling of sites as locations on the water where the fishing actually took place.

[9] The Albemarle Sound is located to the north of Tyrell and Dare counties, and the Pamlico Sound is to the east of Dare, Hyde, Beaufort, and Pamlico, and to the northeast of Carteret county.

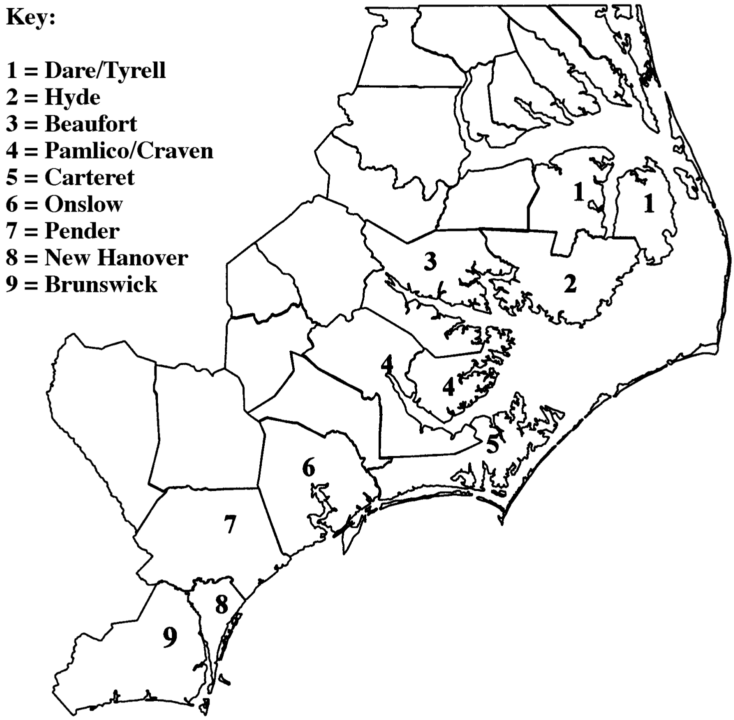

Figure 1. Coastal Counties in North Carolina

(0.66)(hourly wage)(hours driving time);[10] *outerbanks dummy*$_i$ = 1 for Dare, Hyde, and Carteret counties (sites 1, 2, and 5), and = 0 otherwise; *sound site dummy*$_i$ = 1 for Craven/Pamlico and Beaufort counties (sites 3 and 4), and = 0 otherwise; *log of intercept points*$_i$ = log of the number of intercept points at site *i* as reported in the MRFSS data set. The derivation of expected catch rates is outlined in the following section.

Modeling Expected Catch as a Poisson Process

Since anglers are intercepted after their trip has taken place, information about the catch expected prior to the trip is not obtained. To accurately model the site choice

[10] The explicit travel costs were estimated at $0.41 per mile which was the cost of operating a motor vehicle in 1990 reported in the Statistical Abstract of the United States (U.S. Bureau of the Census 1991). We note that this value includes both fixed and variable costs, and therefore may be different from the marginal cost of vehicle operation. Driving time was calculated assuming 45 miles per hour average speed. The software package HYWAYS/BYWAYS was used to generate travel distances. Starting points were the city recognized by the software nearest to the midpoint of the angler's home county. Endpoints were the city of destination for the actual site visited, and the average distance to sites in each aggregate for the alternatives not selected. To estimate the hourly wage, county level per capita income (U.S. Bureau of the Census, County and City Data Book 1994) were divided by 2,080 for employed individuals (individual wage and income data were not available from respondents, however respondents did indicate if they were employed). Two-thirds of the wage and zero were used as approximations of the opportunity cost of time for employed and unemployed individuals respectively.

decision, we must therefore form a proxy for expected catch. We can assume that different anglers will have different expectations about the catch of different species. Individual characteristics such as fishing experience, age, familiarity with the site, choice of target, and gear used will all likely influence expected catch. Further, the expected catch of different species will likely vary across seasons. Modeling actual catch as a function of these variables will form a reasonable proxy for expected catch. Moreover, by allowing angler attributes to influence expected catch, we allow for the quality variable to be random and may therefore classify welfare gains from catch improvements according to the same set of individual characteristics that influence expectations.

Actual catch on a given trip can be considered the realization of a random process with mean equal to the expected catch. The realized value of catch per trip must take on integer values greater than or equal to zero. We model expected catch per trip with a Poisson to satisfy both of these conditions and thereby allow for a better data fit than other specifications such as OLS.[11] The Poisson regression model stipulates that each observation y_j is drawn from a Poisson distribution with parameter λ_j, which is related to regressors x_j. The primary equation of the model is

$$\text{prob}(Q_j = y_j) = e^{-\lambda}\lambda_j^{y_j} / y_j!, \quad y_j = 0, 1, 2, 3, \ldots \tag{6}$$

The formulation for λ_j is

$$\ln \lambda_j = \beta' x_j \tag{7}$$

so that

$$\lambda_j = e^{\beta' x_j} \tag{8}$$

Using the MRFSS data from the period 1987–89, we estimate the following catch model via maximum likelihood separately for boat and shore fishing[12] for each of the four species groups plus red drum

$$\begin{aligned} Q_{ik}^{a} = \exp\,[&\beta_0 + \beta_1(target_k^a) + \beta_2(historical\ stock_i^a) + \beta_3(seasonal\ trend^a) \\ &+ \beta_4(experience_k) + \beta_5(hours\ fished_{ik}) + \beta_6(mode_k)] \end{aligned} \tag{9}$$

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[11] See McConnell, Strand, and Blake-Hedges (1995), and Kaoru, Smith, and Liu (1995). The Poisson model has been criticized because of its implicit assumption that the variance of the dependent variable equals its mean (Greene 1993). When the true variance of the dependent variable exceeds the mean, the data are said to suffer from the problem of "overdispersion," which may result in estimates being downwardly biased (but consistent). Tests for overdispersion were conducted on the catch data used here (see Greene 1993, pp. 679), and it was found that overdispersion may, in fact, be present in the data. As a check of the degree to which the estimation results are robust to the Poisson specification, catch models were re-estimated using a negative binomial specification, which does not impose the restriction mentioned above. It was found that on a site-by-site basis, the predicted catch rates generated by the Poisson were closer to actual catch rates than those generated by the negative binomial for 57.6% of the catch regressions. In these cases, the difference in the site mean expected catch rate provided by the Poisson was statistically different from that provided by the negative binomial. For 25.9% of the catch regressions, there was no statistical difference between the mean predicted values from the two models. Hence using a negative binomial specification would not improve the predictive power of the model.

[12] Sample sizes used in the Poisson catch regressions are 17,741 and 7,791 for boat and shore fishing respectively. Note that although realized catch is often zero for nontargeted species, by estimating each catch equation for each angler, we allow for generated expected catch rates of nontargeted species to be greater than zero.

where, Q_{ik}^{a} = the number of fish of species group a caught by angler k at site i; $target_k^a$ = 1 if angler k is targeting species a, and = 0 otherwise;[13] $historical\ stock_i^a$ = a proxy for the existence of species a at site i; $seasonal\ trend^a$ = a measure of the seasonal variation in the catch of species a; $experience_{ik}$ = a measure of the fishing experience of angler k; $hours\ fished_{ik}$ = length of trip in hours; $mode_k$ = 1 for boat fishing from a private boat, and = 0 from a charter boat, $mode_k$ = 1 for shore fishing from a beach or bank, and = 0 from a man-made structure such as a bridge, jetty, or pier.

As a proxy for the historical existence of the stock of each species at each site, we use the average catch per hour of the species at each site over the entire sample period.[14] As a measure of the fishing experience of each angler, we use as explanatory variables the number of marine fishing trips in North Carolina in the past two months as reported by the angler, plus the age and square of age of the angler, and a dummy variable for whether or not the angler lives in the county in which he or she is fishing. Because of the potential for endogeneity between hours fished and catch per trip, we use an estimate of hours fished for each angler. Hours fished is predicted via OLS, where the regressors are the entire list of explanatory variables in the Poisson equations.[15]

The measure of seasonal variation in catch of each species is given by:

$$seasonal\ trend = \sum_{l=1}^{5}\left[\hat{\delta}_l \sin\left(\frac{2\pi lt}{365}\right) + \hat{\gamma}_l \cos\left(\frac{2\pi lt}{365}\right)\right]. \tag{10}$$

The five sine and cosine pairs in equation (10) represent orthogonal functions which scale the year to occur between 0 and 2π to trace out any seasonal trend in the catch data. The index here denotes the number of times the calendar year has been partitioned to trace out seasonal variation.[16]

As there are two modes and five species, there are ten estimates of equation (9). These equations will serve to generate predictions of expected catch for the 1990 sub-sample so that the utility function in equation (5) can be estimated.

Results

The results from the Poisson regressions are given in tables 2 and 3. The reported Poisson coefficients can be interpreted as logarithmic elasticities. That is, each coefficient indicates the percentage change in expected catch per trip given a one unit change in the dependent variable. The signs of the estimated coefficients in the ten catch equations are mostly as expected. Notice that the target dummy variables are positive and significant in nine of the ten regressions. This result indicates that anglers tar-

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[13] By including a single dummy variable in each catch equation, an indication of which species are complementary in terms of catch comes through examination of predicted catch for nontargeted species. An alternative, not estimated here, would be to include a larger set of target dummy variables in each catch equation.

[14] Using an average catch rate as an explanatory variable in an equation modeling catch per trip certainly introduces the potential for endogeneity. However, the sample sizes used are sufficiently large to make the contribution of each individual observation to the RHS average insignificant.

[15] There are four target dummy variables in the hours equation as "no target" is an option. There are also five stock proxy variables. The condition that the number of regressors in the instrument equation be larger than the number in the primary equation is therefore satisfied.

[16] Here, t takes on values between 1 and 365 denoting the day of the year. The ten coefficients in equation (10) are estimated simultaneously with the β's in each catch equation. Hence there are ten distinct parameter estimates which are used together to generate the seasonal trend. Taken individually these estimates provide no intuitive interpretation and are therefore not reported. The estimates are however employed in the generation of each angler's expected catch. See Thurman and Knoeber (1994).

Table 2
Results of Poisson Regressions for Boat Fishing (N = 17,741)

	Coefficient				
Variable	Red Drum	Other Drum	Surface Fish	Bottom Fish	Bigger Fish
Constant	–7.01**	–6.45**	–1.68**	–6.81**	–5.59**
	(1.47)	(0.35)	(0.33)	(0.20)	(1.47)
Target dummy	3.30**	1.16**	0.67**	1.22**	2.01**
	(0.16)	(0.02)	(0.01)	(0.02)	(0.10)
Stock proxy	27.24**	3.15**	11.01**	4.25**	55.15**
	(3.15)	(0.13)	(0.33)	(0.09)	(2.28)
Trips in past 2 months	0.03*	0.01**	0.02**	–0.005**	0.04**
	(0.01)	(0.002)	(0.001)	(0.002)	(0.01)
Same county	–0.30*	–0.43**	–0.07**	0.13**	–0.35**
	(0.17)	(0.03)	(0.02)	(0.02)	(0.08)
Age	0.03**	0.01**	0.002**	0.002**	0.01**
	(0.006)	(0.001)	(0.0008)	(0.0007)	(0.003)
Age squared	–0.00006	0.00008**	0.0001**	0.0002**	–0.0001**
	(0.00008)	(0.00001)	(0.0000104)	(0.000009)	(0.00004)
Predicted hours fished	–0.12	–0.17**	–0.20**	0.89**	–0.51**
	(0.24)	(0.04)	(0.02)	(0.03)	(0.11)
Private boat dummy	1.78**	3.99**	–1.45**	1.23**	–0.41+
	(0.52)	(0.14)	(0.04)	(0.06)	(0.22)

Notes: Standard error in parentheses. * indicates significance at the 5% level, ** indicates significance at the 1% level, + indicates significance at the 10% level.

Table 3
Results of Poisson Regressions for Shore Fishing (N = 7,791)

	Coefficient				
Variable	Red Drum	Other Drum	Surface Fish	Bottom Fish	Bigger Fish
Constant	–5.50**	–0.86**	–5.25**	0.01	–4.06**
	(1.34)	(0.18)	(0.18)	(0.18)	(0.32)
Target dummy	1.11**	1.13**	0.21**	1.20**	0.45
	(0.22)	(0.02)	(0.05)	(0.04)	(0.36)
Stock proxy	265.48**	2.81**	5.94**	5.05**	42.90**
	(38.51)	(0.06)	(0.36)	(0.25)	(2.66)
Trips in past 2 months	0.05**	0.02**	–0.007**	0.02**	0.009
	(0.01)	(0.002)	(0.002)	(0.003)	(0.006)
Same county	–0.34	–0.97**	0.65**	–0.52**	0.02
	(0.35)	(0.05)	(0.06)	(0.06)	(0.12)
Age	–0.005	0.02**	–0.001	0.02**	0.01**
	(0.02)	(0.0009)	(0.002)	(0.002)	(0.004)
Age squared	0.0001	–0.0001**	0.00007**	–0.0001**	–0.0002**
	(0.0002)	(0.00001)	(0.00002)	(0.00002)	(0.00005)
Predicted hours fished	–0.52*	–0.48**	0.75**	–0.53**	0.007
	(0.24)	(0.03)	(0.04)	(0.04)	(0.07)
Beach/bank dummy	0.60**	–1.11**	0.44**	–0.36**	0.008
	(0.21)	(0.03)	(0.18)	(0.04)	(0.08)

Notes: Standard error in parentheses. * indicates significance at the 5% level, ** indicates significance at the 1% level, + indicates significance at the 10% level.

geting a particular species are much more likely to catch that species. It is interesting to note that targeting appears to contribute more to success for the drum and bottom fish species, which feed off of the bottom. The stock proxy variables are significant in all cases. This result implies higher predicted catch rates at sites where anglers are, on average, successful, holding individual angler characteristics constant.

It appears that the experience of the angler as indicated by the number of North Carolina marine fishing trips in the past two months is a good indicator of expected catch. Notice that this coefficient is positive and significant in seven of the ten cases. However, the “same county” variable did not reflect this result, being negative in seven of the ten cases. Another unexpected result was the negative signs on the “predicted hours” variable. An interpretation is that a lengthier trip reduces total catch per trip. Towards explaining this counterintuitive result, it is interesting to note that the hours variable is negative and significant in the same seven cases as the “same county” variable. We would expect that anglers who reside in the county where they fish take shorter than average fishing trips as the necessary travel is minimal, and frequency of visits can be substituted for length. An explanation for the signs on the “same county” coefficients may therefore help explain the “predicted hours” result. Resident anglers may be more active, and experienced anglers who are not satisfied with participating in common fishing trips where easy-to-catch species are targeted in popular locations. These anglers may take shorter trips, use more specialized gear, visit infrequently utilized locations, or target difficult-to-catch species; hence contributing to their lower expected catch rates.[17]

It is interesting to note that within the shore fishing mode, fishing from a man-made structure appears to be better suited for catching bottom fish and fish in the drum family, while shore fishing is more appropriate (or more popular) for red drum fishing and catching fish in the surface fish category. Fishing from a private boat appears to significantly influence the catch of all fish in the drum family as well as bottom fish, while charter boats are more successful with surface fish and big game fish. This may indicate that the charter boat trip is a more popular means of targeting these latter species.

RUM Estimation

The coefficients in tables 2 and 3 are used to generate a proxy for the expected catch rate for each of the five species groups for each angler at each site for a sample of single-day fishing trips in 1990. From the 10,862 interviews conducted in 1990, we have a sample of 2,892 single-day trips to which we will fit the utility function in equation (5).[18] This estimation is carried out separately for boat and shore anglers, with 2,153 and 739 obser-

[17] These hypotheses are at least partially supported by the data. For the 1987–89 samples, anglers who were fishing in the same county that they reside made up 17.45% of the boat fishing sample, and 12.86% of the shore fishing sample. On average, these anglers reportedly took more than three times as many fishing trips in the past twelve months as nonresident anglers, and had an average trip length that was approximately 1 hour shorter. These anglers had higher than average incidence of targeting bottom dwelling species (including the drums), and were less likely to target surface fish and bigger fish.

[18] We limit our sample to single-day anglers to avoid any bias from including multipurpose trips, which may be more likely for trips that last more than one day. In the 1990 survey, anglers were questioned about length of their trip in days. However, some of the data recorded was nonsensical as anglers from as far away as California were reporting single day trips to North Carolina. It is more probable that these anglers either were taking a multiday trip, or had a trip purpose that included activity other than fishing. We therefore limit our sample to anglers who could access at least one of the sites in 3.5 hours or less. For shore fishing, we therefore limit our sample to anglers with a round-trip travel distance of less than or equal to 315 miles. For boat fishing, we limit our sample to anglers with a round-trip distance of less than or equal to 270 miles, because some time is spent traveling by boat.

vations respectively. Note that the expected catch rates generated for these anglers use the instrumental variable model prediction of hours fished, as well as the ten individual seasonal trend coefficients from equation (10). We assume that all anglers in the sample have all sites in their choice set. In our sample, there were no single-day shore anglers intercepted in Beaufort County (site 3), hence the set of alternative sites for shore anglers consists of the remaining eight counties.

Table 4 contains summary statistics for variables in the expected catch model when applied to the 1990 data. Table 5 contains the predictions of expected catch per trip for these anglers at each site, as well as the average actual catch rate. Notice that for the 1990 data, both the actual catch rates and the generated expected catch rates were quite low for red drum. These rates also exhibited very little variation across anglers and sites. The site choice model relies on differences in quality characteristics across alternatives to estimate the underlying utility function. The lack of

Table 4
Summary Statistics for Catch Equation
Variables for 1990 Single-Day Sub-Samples

	Mean	
Variable	Boat Mode	Shore Mode
Red drum target dummy	0.004	0.04
	(0.06)	(0.18)
Other drum target dummy	0.11	0.08
	(0.31)	(0.27)
Surface target dummy	0.28	0.19
	(0.45)	(0.39)
Bottom target dummy	0.10	0.06
	(0.30)	(0.23)
Bigger target dummy	0.009	0
	(0.09)	(0)
Red drum stock proxy [a]	0.003	0.004
	(0.005)	(0.002)
Other drum stock proxy [a]	0.16	0.22
	(0.09)	(0.13)
Surface stock proxy [a]	0.13	0.13
	(0.04)	(0.04)
Bottom stock proxy [a]	0.17	0.15
	(0.09)	(0.06)
Bigger stock proxy [a]	0.01	0.03
	(0.01)	(0.02)
Trips in past 2 months	4.85	5.41
	(6.02)	(7.48)
Same county [a]	0.43	0.39
	(0.49)	(0.49)
Age	37.82	39.14
	(14.56)	(16.05)
Age squared	1,642.59	1,789.15
	(1,195.50)	(1,390.24)
Predicted hours fished	4.92	3.94
	(0.77)	(0.79)
Private boat/beach bank dummy	0.88	0.42
	(0.33)	(0.49)

Note: Standard deviations in parentheses.
[a] Average over all sites.

Table 5
Mean (Predicted) Expected Catch Per Trip for 1990 Single-Day Sub-Samples

	Mean (Predicted) Expected Catch									
	Boat					Shore				
Site	Red Drum	Other Drum	Surface Fish	Bottom Fish	Bigger Fish	Red Drum	Other Drum	Surface Fish	Bottom Fish	Bigger Fish
1	0.017	0.595	1.685	0.682	0.051	0.017	0.879	0.584	0.584	0.140
2	0.019	1.794	0.307	0.852	0.042	0.127	0.738	0.272	0.417	0.037
3	0.118	1.052	0.343	0.590	0.036	NA	NA	NA	NA	NA
4	0.022	1.176	0.554	1.177	0.056	0.007	3.670	0.657	0.344	0.024
5	0.017	0.681	1.137	0.929	0.059	0.020	1.171	0.395	0.681	0.034
6	0.017	1.146	0.453	1.731	0.043	0.013	1.969	0.279	1.273	0.030
7	0.018	0.715	1.205	1.552	0.063	0.010	2.974	0.702	0.596	0.044
8	0.019	0.686	1.196	1.105	0.054	0.022	1.189	0.475	1.159	0.060
9	0.018	0.944	0.822	1.400	0.219	0.015	3.185	0.373	0.489	0.264
Average [a]	0.018	0.824	1.095	1.047	0.061	0.029	1.216	0.468	0.683	0.085
Actual [b]	0.008	0.892	1.481	1.234	0.020	0.026	0.915	0.678	0.713	0.053

[a] Average predicted expected catch is a share weighted average expected catch per trip over all sites using actual portions of sample visiting each site.
[b] Actual catch is average catch per trip over all sites using actual sample reported catch per trip.

variation across sites inhibited the tendency of red drum catch rates to be a significant determinant of site choice. Despite this result, it may be incorrect to assume that catch of red drum does not contribute to the utility derived from a recreational fishing trip. If we assume that most anglers derive satisfaction from the act of catching fish—whether or not that fish is the primary target—any fish may contribute to utility.[19] However, lack of variation for a species that is infrequently targeted and caught may prevent the site choice model from revealing its contribution to utility.

To compensate for this common data shortcoming, we add the expected catch of red drum to the expected catch of other drum to yield the expected catch of all drum, and enter the sum as a quality characteristic in the site choice model. The compensating variation from an improvement in red drum catch can then be estimated by increasing only the red drum portion of total drum catch. This specification imposes the restriction that the coefficient on red drum, α_2, in the indirect utility function given by equation (5), is equal to the coefficient on other drum, α_3. Using a likelihood ratio test for the restricted and unrestricted models we cannot reject the hypothesis that the red drum and other drum coefficients are equal at the 1% level for both the boat and shore fishing models. We therefore estimate the indirect utility function in equation (5) using four expected catch rates (the three species groups and the summed drum) in addition to the two dummy variables and the log of the number of intercept points. The estimated coefficients are given in table 6. The actual and predicted shares for each of the sites in the choice set are listed in table A1 in the appendix.

Using the estimated probabilities of visiting each site given by equation (3), we generate the probability weighted average expected catch per trip of each species for

[19] This will especially be true for species that "put up a good fight" and are therefore enjoyable to catch. The red drum are reportedly very strong fighters, and provide the type of fishing action that sport anglers enjoy (Goldstein 1986).

Table 6
Indirect Utility Function Coefficient Estimates

	Coefficient	
Variable	Boat Fishing (N = 2,153)	Shore Fishing (N = 739)
Travel cost	–0.063 **	–0.063 **
	(0.002)	(0.004)
Total drum catch	0.517 **	0.074
	(0.087)	(0.072)
Surface fish catch	0.567 **	1.158 **
	(0.016)	(0.351)
Bottom fish catch	0.650 **	–1.49 *
	(0.115)	(0.611)
Bigger fish catch	1.511 $^{+}$	5.94 **
	(0.883)	(2.135)
Outerbanks dummy	3.582 **	3.843 **
	(0.199)	(0.351)
Sound only dummy	–1.577 **	–3.718 **
	(0.169)	(0.776)
Log of intercept points	0.055	0.393 **
	(0.112)	(0.128)

Notes: Standard error in parentheses. Both models significant at the 1% level. * indicates significance at the 5% level, ** indicates significance at the 1% level, $^{+}$ indicates significance at the 10% level.

each angler.[20] These values are reported by target in tables 7 and 8. As is expected given the significance of the target dummy variables in the catch regressions, the expected catch rates for each species are highest for anglers specifically targeting that species. Also note that the expected catch rates for red drum, bottom fish, and surface fish are generally higher for boat anglers than for shore anglers.

Value of an Improvement in Catch Rates

The utility function coefficients in table 6 can be used to estimate the compensating variation for an improvement in expected catch per trip. The process by which expected catch is adjusted for valuation is a critical component of any welfare analysis of stock enhancement. That is, the adjustment of expected catch to changes in stock is the link by which a policy change affects behavior and changes value. Given our specification for expected catch, there are two alternatives for quantifying this improvement which have been employed in the literature. We illustrate them both here; not to demonstrate which alternative is appropriate, but rather to illustrate that differences in subsequent welfare estimates may result depending upon which approach is chosen, so that more attention can be allocated to this issue in future research. First, following McConnell, Strand, and Blake-Hedges (1995), we can increase the “historical catch”

[20] That is, each angler, k, has an expected catch per trip for each species, a, at each site, i, as given by Q^a_{ik} in equation (9). Each angler also has a probability of visiting each site, π_{ik}, given by equation (3). For each angler, the probability weighted average expected catch of a particular species is therefore given by: $\Sigma_i \pi_i Q_i$.

Table 7
Mean Probability Weighted Expected Catch Per Trip By Target: Boat Mode

		$\left(\sum_i \pi_i Q_i\right)/n$				
Target	Anglers (n)	Red Drum	Other Drum	Surface Fish	Bottom Fish	Bigger Fish
Red drum	9	0.98	0.81	0.90	0.66	0.04
		(1.02)	(0.37)	(0.42)	(0.40)	(0.03)
Other drum	240	0.02	2.30	0.61	1.20	0.04
		(0.02)	(0.97)	(0.38)	(0.75)	(0.04)
Surface fish	607	0.01	0.49	1.93	0.81	0.05
		(0.01)	(0.25)	(1.40)	(0.47)	(0.04)
Bottom fish	221	0.01	0.64	0.92	2.65	0.07
		(0.01)	(0.26)	(0.55)	(2.00)	(0.04)
Bigger fish	19	0.005	0.28	1.82	0.66	0.30
		(0.005)	(0.27)	(1.14)	(0.24)	(0.25)
No target	1,057	0.01	0.55	0.86	0.97	0.05
		(0.01)	(0.33)	(0.67)	(0.56)	(0.04)
Total	2,153	0.02	0.74	1.15	1.12	0.05
		(0.09)	(0.70)	(1.04)	(0.98)	(0.05)

Note: Standard deviation in parentheses.

component of expected catch. Recall that this variable was used as a proxy for the presence of stock at a given site. This specification for a catch improvement is logical if we assume that following a stock enhancement, expected catch would increase only to the extent that the stock component of expected catch increases. That is, if anglers adjust their expectations of catch to changes in information about the size of stock. This implies that for a ϕ percent improvement in stock, the new expected catch of a given species by angler k at site i, which we will define as Q'_{ik}, is given by

$$\begin{aligned} Q'_{ik} &= \exp\{\beta_0 + \beta_1(\mathit{target}_k^a) + \beta_2[(1+\phi)\cdot \mathit{historical\ stock}_i] \\ &+ \beta_3(\mathit{seasonal\ trend}) + \beta_4(\mathit{experience}_k) + \beta_5(\mathit{hours\ fished}_{ik}) + \beta_6(\mathit{mode}_k)\}. \end{aligned} \tag{11}$$

Alternatively, we can value a ϕ percent increase in the expected catch. This approach, which is more commonly employed in the literature, assumes that anglers adjust their expectations of catch upward by a certain percentage in response to information about catch. This specification implies that the new expected catch of a given species by angler j at site i, Q^*_{ik}, is given by:

$$\begin{aligned} Q^*_{ik} &= (1+\phi)\exp[\beta_0 + \beta_1(\mathit{target}_k^a) + \beta_2(\mathit{historical\ stock}_i) + \beta_3(\mathit{seasonal\ trend}) \\ &+ \beta_4(\mathit{experience}_k) + \beta_5(\mathit{hours\ fished}_{ik}) + \beta_6(\mathit{mode}_k)] = (1+\phi)Q_{ik} \end{aligned} \tag{12}$$

For a common value of ϕ, equations (11) and (12) can be rearranged to show that the two alternative are equivalent if:

$$\beta_2(\mathit{historical\ stock}) = \log(1+\phi)/\phi. \tag{13}$$

Notice that as we value larger improvements the RHS of equation (13) becomes smaller, so it becomes more likely that the historical catch improvement measure, Q', will be larger than the expected catch alternative, Q^*. Also note that we should

Table 8
Mean Probability Weighted Expected Catch Per Trip By Target: Shore Mode

		$\left(\sum_i \pi_i Q_i\right) / n$				
Target	Anglers (n)	Red Drum	Other Drum	Surface Fish	Bottom Fish	Bigger Fish
Red drum	26	0.05	0.26	0.42	0.23	0.20
		(0.06)	(0.33)	(0.35)	(0.23)	(0.13)
Other drum	58	0.02	3.50	0.42	0.59	0.08
		(0.03)	(2.94)	(0.24)	(0.25)	(0.11)
Surface fish	140	0.01	0.45	1.43	0.28	0.10
		(0.02)	(0.36)	(0.87)	(0.13)	(0.09)
Bottom fish	41	0.02	0.66	0.59	1.44	0.06
		(0.02)	(0.63)	(0.39)	(0.56)	(0.05)
Bigger fish	0	NA	NA	NA	NA	NA
No target	474	0.02	0.91	0.42	0.56	0.07
		(0.04)	(0.63)	(0.31)	(0.21)	(0.07)
Total	739	0.02	0.99	0.62	0.55	0.08
		(0.04)	(1.25)	(0.61)	(0.34)	(0.09)

Note: Standard deviation in parentheses.

see a larger willingness-to-pay when using the Q' measure to quantify improvements for species where the contribution of historical catch to expected catch is large.[21] This result indicates that using the historical catch measure to value species whose stocks have been declining may result in lower benefit measures than those derived using the expected catch alternative. From a policy perspective, if one of the alternative catch improvement measures is a more accurate reflection of angler's true decision making, then a welfare analysis using the alternative results in misevaluation.[22]

For estimation purposes, we consider the case of a 25% improvement ($\phi = 0.25$). The per trip compensating variation for both measures of catch improvements were calculated using equation (4) and the restricted form of equation (5). The mean values are reported by species targeted for each of the five species groups in tables 9–12 below (to give context to these numbers, the reader should refer to the listing of expected catch rates in tables 7 and 8). For the shore-fishing mode, results are not reported for the bottom fish category. This is because the negative coefficient on bottom fish catch in the estimated utility function will generate a negative willingness-to-pay for an improvement. Also, for our sample of shore fishing, there were no anglers targeting species in the bigger fish category.[23]

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[21] For the analysis here, the historical catch variable takes on different values at different sites, so it is difficult to predict the species for which this will be true. It may be informative to note that the historical catch rates were generally low for red drum and species in the bigger fish category.
[22] As we have no information regarding the process by which anglers adjust their expectations to stock or catch changes, we cannot form a hypothesis regarding the nature of such misevaluation for this study. We present estimation results for both measures recognizing that their accuracy is dependent upon the way expectations are formed.
[23] Despite the fact that no shore anglers were specifically targeting species in the bigger fish category, the bigger fish coefficient in the site choice model is quite large, which results in relatively high values for bigger fish catch improvements for all targeters. This result is likely due to the large value on the bigger fish stock proxy variable in the catch regression (see table 3). This indicates that expected catch of species in the bigger fish category will be relatively large at sites where the past catch was high, regardless of target. Species such as sharks, skates, and rays in this category that may be caught incidentally may contribute to catch being less dependent on target.

Table 9
Mean Compensating Variation for a 25% Increase in Historical Catch: Boat Mode

		$ Mean Per Trip Willingness-to-Pay for ($Q' - Q$)				
Target	Anglers (n)	Red Drum	Other Drum	Surface Fish	Bottom Fish	Bigger Fish
Red drum	9	2.98	1.35	4.99	1.55	0.15
		(8.49)	(8.91)	(3.06)	(1.32)	(0.07)
Other drum	240	0.008	4.00	2.85	3.44	0.21
		(0.03)	(3.61)	(2.37)	(2.97)	(0.46)
Surface fish	607	0.004	0.53	10.37	1.83	0.23
		(0.02)	(0.48)	(8.84)	(1.68)	(0.40)
Bottom fish	221	0.002	0.93	5.04	7.39	0.25
		(0.004)	(0.81)	(3.71)	(8.68)	(0.25)
Bigger fish	19	0.001	0.29	10.92	0.89	1.32
		(0.002)	(0.35)	(7.15)	(0.76)	(1.96)
No target	1,057	0.005	0.79	4.18	2.55	0.20
		(0.02)	(0.81)	(3.77)	(2.04)	(0.20)
Total	2,153	0.02	1.08	5.93	2.92	0.23
		(0.55)	(1.83)	(6.31)	(3.75)	(0.45)

Note: Standard deviation of willingness-to-pay in parentheses.

The per trip results illustrate that the willingness-to-pay for an improvement in expected catch may be quite low for species in the drum family and bigger fish category. For red drum, the per trip willingness-to-pay values are especially low for those anglers not targeting red drum. This is due to the fact that the generated expected catch rates are so low that, using either the Q' or Q^* measure, a 25% improvement amounts to catch per trip increasing by only very small fractions of a fish.[24] Those anglers who do target the species have higher estimates of expected catch, and hence the percentage improvement amounts to a larger change in terms of numbers of fish. Willingness-to-pay for the improvement is therefore higher. To make preliminary assessments as to the likely recipients of benefits transfer from stock-enhancing policies on an annual basis, we report the number of anglers targeting each species in tables A2 and A3 in the appendix, where we also report the mean number of marine fishing trips taken in North Carolina in the past twelve months.[25]

The results indicate that boat angler's willingness-to-pay for a red drum catch improvement is considerably larger than that for shore anglers. This is likely due to the large magnitude of the coefficient on drum catch in the utility function for boat anglers relative to that for shore anglers. For both boat and shore anglers, the his-

[24] It is important to note that we are not allowing the target decision to change as catch rates are improved. The higher expected catch per trip may induce some anglers not previously targeting red drum to begin targeting red drum. This would increase the willingness-to-pay for the improvement for these anglers, and hence increase the mean over all anglers. As a result, our benefits estimates may be understated. By treating the target decision as exogenous, we do not incorporate this aspect of trip choice into our modeling. We note that a nested model would permit target substitution and may be a preferable model specification. See Bockstael, McConnell, and Strand (1989), and Parsons and Hauber (1998) for examples.

[25] Clearly there is a strong potential for stock-enhancing policies to result in benefits transfer to anglers who have high expected catch rates and those who make frequent trips. When weighted by annual participation, we note that anglers with smaller than average per trip willingness-to-pay who fish very often (such as the red drum anglers) may receive a disproportionate share of annual benefits.

Table 10
Mean Compensating Variation for a 25% Increase in Historical Catch: Shore Mode

		$ Mean Per Trip Willingness-to-Pay for ($Q' - Q$)				
Target	Anglers (n)	Red Drum	Other Drum	Surface Fish	Bottom Fish	Bigger Fish
Red drum	26	0.04	0.09	1.89	NA	8.60
		(0.06)	(0.06)	(2.22)		(6.43)
Other drum	58	0.006	1.02	2.01	NA	3.52
		(0.01)	(1.34)	(1.47)		(6.11)
Surface fish	140	0.004	0.13	7.77	NA	4.32
		(0.007)	(0.15)	(5.70)		(4.22)
Bottom fish	41	0.008	0.20	2.91	NA	2.58
		(0.008)	(0.26)	(2.48)		(2.53)
Bigger fish	0	NA	NA	NA	NA	NA
No target	474	0.01	0.24	2.00	NA	2.44
		(0.02)	(0.24)	(1.98)		(3.53)
Total	739	0.01	0.27	3.14	NA	3.10
		(0.02)	(0.46)	(3.79)		(4.20)

Note: Standard deviation of willingness-to-pay in parentheses.

Table 11
Mean Compensating Variation for a 25% Increase in Expected Catch: Boat Mode

		$ Mean Per Trip Willingness-to-Pay for ($Q^* - Q$)				
Target	Anglers (n)	Red Drum	Other Drum	Surface Fish	Bottom Fish	Bigger Fish
Red drum	9	2.17	1.68	2.06	1.72	0.24
		(2.54)	(0.50)	(0.96)	(1.03)	(0.15)
Other drum	240	0.05	4.80	1.37	3.13	0.25
		(0.04)	(2.01)	(0.85)	(1.95)	(0.24)
Surface fish	607	0.02	1.03	4.36	2.10	0.32
		(0.02)	(0.53)	(3.16)	(1.22)	(0.22)
Bottom fish	221	0.03	1.33	2.08	6.92	0.40
		(0.02)	(0.52)	(1.24)	(5.27)	(0.24)
Bigger fish	19	0.01	0.58	4.10	1.70	1.84
		(0.01)	(0.57)	(2.56)	(0.61)	(1.58)
No target	1,057	0.03	1.13	1.95	2.52	0.28
		(0.03)	(0.65)	(1.52)	(1.45)	(0.23)
Total	2,153	0.04	1.52	2.60	2.91	0.31
		(0.21)	(1.45)	(2.35)	(2.58)	(0.31)

Note: Standard deviation of willingness-to-pay in parentheses.

torical catch improvement measure, Q', resulted in larger willingness-to-pay than the expected catch measure, Q^*. This result indicates that a 25% increase in historical catch amounts to more than a 25% increase in expected catch. While this may seem counterintuitive, we can use equation (13) to see that it is a plausible result considering the large coefficient on historical catch of red drum in the Poisson catch regressions (see tables 2 and 3). Valuing larger improvements will decrease the RHS of equation (13) and will likely reverse this result.

Table 12
Mean Compensating Variation for a 25% Increase in Expected Catch: Shore Mode

		$ Mean Per Trip Willingness-to-Pay for ($Q^* - Q$)				
Target	Anglers (*n*)	Red Drum	Other Drum	Surface Fish	Bottom Fish	Bigger Fish
Red drum	26	0.02 (0.02)	0.07 (0.09)	1.92 (1.60)	NA	4.64 (3.16)
Other drum	58	0.005 (0.009)	1.08 (0.92)	1.93 (1.12)	NA	1.90 (2.53)
Surface fish	140	0.003 (0.005)	0.14 (0.11)	6.61 (4.03)	NA	2.38 (2.09)
Bottom fish	41	0.006 (0.005)	0.20 (0.19)	2.72 (1.81)	NA	1.43 (1.24)
Bigger fish	0	NA	NA	NA	NA	NA
No target	474	0.007 (0.01)	0.28 (0.18)	1.93 (1.41)	NA	1.62 (1.76)
Total	739	0.006 (0.01)	0.30 (0.37)	2.86 (2.83)	NA	1.88 (2.02)

Note: Standard deviation of willingness-to-pay in parentheses.

The importance of deriving separate values for red drum catch improvements can be seen by comparing the willingness-to-pay values for different species across all anglers (reported in the last row in tables 9–12). It is clear that the value of a red drum stock improvement is less than that for an improvement in other drum species of the same percentage. Analysis of a red drum policy using the average value for all drum would therefore overstate the true benefits of a given stock improvement. Welfare analysis of species-specific stock improvement policies should therefore attempt to estimate a separate value for the relevant species rather than attributing to the species the value of a larger aggregate.

Conclusions

We have shown that there are clear differences in the value of an improvement in the catch of different species across anglers. Because anglers targeting a particular species will have a higher expected catch than those not targeting the species, they will value an improvement in the catch of that species more than anglers not targeting the species. There are also differences in the value of an improvement across fishing modes. Anglers fishing from the shore appear to place a higher value on an improvement in the catch of bigger fish species than boat anglers, while boat anglers place a higher value on surface fish and species in the drum family. For our samples, approximately 15% of boat anglers and 19% of shore anglers were successful in catching at least one fish in the drum family, 20% of boat anglers and 17% of shore anglers caught at least one fish in the surface fish category, and 1.3% of boat anglers and 3.5% of shore anglers caught at least one fish in the bigger fish category. Because these success rates are so similar across modes, it seems unlikely that the differences in willingness-to-pay for an improvement are solely attributable to differences in the probability of catching a particular type of fish.

Within each mode, benefits realized from the improvements in the catch of any species tend to be lower for anglers who do not have a target than the benefits realized by anglers who do have a specific target. This result is due to the fact that the target anglers have higher predicted expected catch rates than the no-target anglers. Because we have estimated a single utility function for all anglers, we cannot directly attribute this result to different preferences for targeters and nontargeters. However, it seems plausible that anglers who do target a species are more "serious" anglers who place a higher value on catching fish, and in some way this preference is manifested in a higher catch. Likewise, anglers who do not have a specific target may have less concern for catch aspects of a recreational fishing trip, and catch fewer fish as a result. We also find that in most cases the range of willingness-to-pay across species groups was lower for the no-target anglers than for those with a target. This may also indicate that for the no-target anglers there is a greater degree of substitutability between species.

We can conclude that the benefits from a species-specific stock-enhancement would accrue mostly to anglers who are targeting that species, and that anglers who target any species will benefit more than those who do not have a target. This result is consistent with speculation first raised by McConnell and Sutinen (1978), and more recently put forth by Anderson (1993), and implies that species-specific or angler-specific stock-enhancing policies may be more effective than more general policies. We have shown that an improvement in the catch of red drum in North Carolina will likely benefit boat anglers more than shore anglers, and within each mode, the highest benefits will accrue to those who are specifically targeting red drum. These results show that to accurately value welfare changes and benefits transfer from stock-enhancing regulations, it will be necessary to identify the characteristics and number of anglers who target the species of concern. Perhaps most importantly, we have shown that there are large differences in the value of an improvement in catch rates of different species and species groups. The importance of this result stems from the fact that stock enhancement policies will be implemented at the species level. Traditional aggregation of species for valuation purposes imposes the value of a larger species group on a subset of that group. This may result in an erroneous estimate of individual species value. In this analysis, fish in the drum family could have been categorized as a bottom fish. A welfare analysis using this more general categorization would attribute the mean willingness-to-pay for all bottom fish to the catch of drum. The results derived here show that this would likely result in an overestimate of the benefits from a drum stock improvement.

We have also shown that the specification for the expected catch improvement may affect the resulting estimate of willingness-to-pay. The mean willingness-to-pay for an improvement in catch of fish in the drum family and bigger fish category was significantly higher when the Q^* expected catch improvement was used. For surface fish and bottom fish the willingness-to-pay per trip was higher using the improvement in historical catch, Q'. Species in the former category exhibited low historical catch rates over the period examined. While we are left with no clear picture as to which of the illustrated alternatives is preferable, this result does indicate that reliance on benefits measures derived using the historical catch specification may undervalue policies designed to enhance depleting stocks. As the definitions of these alternative improvement specifications can be attributed to differences in the way anglers are hypothesized to adjust their expectations of catch following a stock improvement, it is of critical importance for future research to resolve the issue of how information about stock fluctuations is spread to anglers, and how their expectations of catch are subsequently adjusted. More careful modeling of this link between the policy change and subsequent valuation will result in more accurate welfare analysis. Angler surveys designed to elicit values of catch improvements should therefore attempt to address this important facet of participation behavior.

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Appendix

Table A1
Actual and Predicted Site Shares

	Boat Mode		Shore Mode	
Site	Actual Share	Predicted Share	Actual Share	Predicted Share
1	0.2508	0.2241	0.3735	0.3376
2	0.0567	0.0505	0.1055	0.1067
3	0.0033	0.0064	0	NA
4	0.0325	0.0294	0.0027	0.0036
5	0.3247	0.3576	0.2882	0.3287
6	0.1491	0.1280	0.0487	0.0331
7	0.0218	0.0285	0.0419	0.0428
8	0.1087	0.1338	0.1015	0.0833
9	0.0525	0.0418	0.0379	0.0643

Table A2
Mean Trips in Past Twelve Months by Target: Boat Mode

Target	Anglers	Avg. Trips
Red drum	9	80.08
Other drum	240	35.40
Surface fish	607	31.72
Bottom fish	221	30.11
Bigger fish	19	12.05
No target	1,057	19.50
Total	2,153	26.00

Table A3
Mean Trips in Past Twelve Months: Shore Mode

Target	Anglers	Avg. Trips
Red drum	26	114.12
Other drum	58	46.86
Surface fish	140	83.58
Bottom fish	41	51.00
Bigger fish	0	Na
No target	474	27.34
Total	739	43.89