Valuing Recreational Fishing Opportunities While Catching Unobserved Characteristics

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[1]E-mail: jennifer.murdock@yale.edu. The most recent version of this paper is available for download at http://pantheon.yale.edu/ murdockj. This research would not have been possible without the help of my advisors Christopher Timmins and Steven Berry and the generosity of Triangle Economic Research in allowing me the use of the Wisconsin Fishing and Outdoor Recreation Survey data. Special thanks go to Patrick Bayer and the other participants at the Yale Prospectus Workshop in Microeconomics for their useful and thoughtful comments. Finally, I am indebted to Doug MacNair, Bill Desvousges, and Reed Johnson and my other former colleagues at Triangle Economic Research who got me excited about recreational fishing.

1 Introduction

Recreation demand models provide valuable insights and information that support both management decisions and litigation in cases of damage to natural resources. Resource management is particularly important for Wisconsin, the topic of this study, where anglers constitute 30 percent of the state's population, devote 15 million days each year to fishing Wisconsin's waters, and spend 1 billion dollars on fishing related travel and gear. The Wisconsin Department of Natural Resources has a budget of over 70 million dollars to manage fish and wildlife resources each year. Another important use of recreation demand models is to estimate welfare losses from damages to recreation locations in support of litigation or settlement negotiations related to Natural Resource Damage Assessments (NRDA). Under current law, parties responsible for injuring a natural resource, through an oil spill or hazardous substance release, are responsible for compensating the public for these damages.[1] While NRDA cases are fairly common, most receive little national news coverage. One ongoing and high profile case is the Upper Hudson River in New York where General Electric is a potentially responsible party for damages to the river from the release of large quantities of toxic substances that have resulted in fishing bans. In these cases, a model of recreation behavior can identify improvements that may offset the damages or in some cases provide a monetary damage estimate for which the responsible party or parties writes a check.

While the discrete choice framework has been used extensively in recreation demand modeling, researchers typically assume that they observe all relevant characteristics of the recreation locations or allow for the possibility of unobserved characteristics in a very restrictive manner. Given the nature of recreation demand data, the assumption that all important characteristics are observed is not easily justified.

This paper presents a new model of recreation demand that allows for unobserved characteristics based on a model presented in Berry, Levinsohn, and Pakes (1995). The consequences of ignoring unobserved characteristics rather than using the proposed model are explored with Monte Carlo simulations. When some characteristics of recreation locations are not observed, simulations show that ignoring these unobserved characteristics leads to less reliable parameter estimates and biased standard errors that dramatically overstate precision. The true parameter estimates are often no where near being included in the confidence bounds generated by these unreliable parameter estimates and biased standard errors. When some characteristics are unobserved, these simulations find that the proposed estimator has a variance one half to one quarter that of the standard estimator.

The proposed approach also provides estimates of the combined impact of unobserved characteristics for each location that can be used to obtain unbiased estimates of welfare

[1]NRDA began in 1980 with the Comprehensive Environmental Response, Compensation, and Liability Act (CERCLA). In 1990, the Oil Pollution Act (OPA) provides legislation specific to oil discharged into navigable waters or adjoining shorelines. Both the National Oceanic and Atmospheric Administration (NOAA) and the Department of the Interior (DOI) created "final rules" in 1996 and 1994 respectively, which in practice guide the way that a NRDA is conducted (15 CFR Part 990, 61 Fed. Reg. 440-510 [1996]; 43 CFR Part 11, 59 Fed. Reg. 14262-14288 [1994]). Under NRDA rules, trustees of natural resources may seek damages for the costs of restoring, replacing, or obtaining the equivalent of the injured resource as well as compensation for services lost in the interim until services are restored at the injured site.

gains or losses from changes in the observed characteristics of recreation locations. Excluding unobserved characteristics from the welfare calculations biases the welfare estimates downward at locations that have desirable unobserved characteristics and biases the welfare estimates upward at locations that have undesirable unobserved characteristics.

The empirical application uses a unique data set that I assisted in collecting. It includes a 1998 summer survey of a panel of 788 Wisconsin anglers, data on the characteristics of fishing locations, and measures of the travel distances between each angler's residence and each fishing site. Results show that unobserved characteristics are important and account for about half the differences across sites. Controlling for unobserved site characteristics substantially affects welfare estimates for a range of policy experiments including changes in land-use, management policies, and fish stocking programs. Allowing for additional taste heterogeneity across anglers by including interaction terms and by allowing tastes to vary randomly across anglers does not change the result that controlling for unobserved characteristics is important.

The models and techniques developed to estimate demand for recreation, a non-marketed good, extend to other fields using discrete choice models to estimate demand for ordinary marketed goods when not all product characteristics are observed. The results may be particularly relevant for goods that have a strong spatial component and many alternatives such as restaurants.

2 Unobserved Site Characteristics

The large number of recreation locations, changes in characteristics from year to year, and the cost of collecting detailed information make unobserved characteristics an undeniable reality in recreation demand modeling. Further, some characteristics, such as aesthetic appeal or secludedness, are hard to measure even if considerable resources are available for frequent on-site data collection.

The problem of unobserved characteristics is not limited to recreation demand applications. Hausman and Wise (1978) note "Only some portion of the factors that determine individual decisions are observed and measured. There remain unobserved attributes of the alternatives in the choice sets available to decision makers as well as unobserved attributes of the decision makers themselves." In modelling consumers' discrete choices among automobiles Berry, Levinsohn, and Pakes (1995) also recognize this problem "In the automobile example, ξ [the unobserved automobile characteristics] reflects the difficult to quantify aspects of style, prestige, reputation, and past experience that affect the demand for different products, as well as the effects of quantifiable characteristics of the car that we simply do not have in our data."

To date, all recreation demand studies known to the author, with the exception of Hausman, Leonard, and McFadden (1995), have assumed that there are no unobserved attributes or have allowed for them in a highly restrictive manner. In other applications researchers have tested for the presence of unobserved characteristics by estimating models with and without a full set of alternative specific constants and conducting a likelihood ratio test. However, this test has not been employed in recreation demand studies where unobserved characteristics are likely to be important. The likely reason is that there has not been a method for comprehensively dealing with unobserved characteristics if they are found to be important, which also allows the researcher to recover parameters crucial for policy experiments.

The recreation demand literature has acknowledged the presence of unobserved characteristics in a limited way by either including a small set of fixed effects, including a full set of fixed effects but not estimating any parameters on characteristics that only vary across sites, or treating unobserved characteristics as a specific form of measurement error. Each approach is discussed next and their limitations, which are not shared with the proposed approach, are highlighted.

The inclusion of a dummy variable for a site the researcher believes is unique or group specific constants for related sites is not uncommon, but this practice recognizes the presence of unobserved site characteristics in a very limited way.[2] The inclusion of group specific constants would be sufficient if the unobserved characteristics are exactly the same for all alternatives in the group and no particular site is unique. The inclusion of a small set of alternative specific constants would be sufficient if the selected sites are the only ones with unobserved characteristics. However, the inclusion of a few dummy variables is ad hoc and given the lack of quality site attribute data that typifies recreation demand studies the assumption that they capture all of the unobserved site characteristics is bold.

Using a two-year panel of Alaska anglers, Hausman, Leonard, and McFadden (1995) estimate a discrete choice model including a full set of alternative specific constants. Using a full set of alternative specific constants precludes the simultaneous inclusion of characteristics that only vary across sites because they would not be identified. However, this does not create an identification problem in their application because all of their variables either vary over sites and individuals or over sites and time and they assume that the alternative specific effects are constant over time. However, this approach is useful only on the rare occasion where there are no variables of interest that vary only across alternatives and when the panel data is long enough such that there is meaningful variation over time.[3]

Morey and Waldman (1998) indirectly acknowledge unobserved site characteristics by redefining them as measurement error in the fish catch rate. They argue that if some sites have more visits than predicted by observed characteristics and geographic location, it must be because the observed catch rate is below the true (unobserved) catch rate. Hence they propose estimating the true catch rate within the model by combining the average catch information and what is effectively an alternative specific constant. The more observations used to calculate the average catch rate, the more it influences the alternative specific

[2] Schuhmann (1998) includes a group specific constant for sites located on a sound and the outerbanks to control for the unobserved differences between these and ocean fishing locations in North Carolina. Parsons, Jakus, and Tomasi (1999) include a single alternative specific constant with the explanation "This site is much more developed than, and we felt it was different in character from the other sites in the choice set. The dummy variable is intended to capture this distinction." Jakus, Downing, Bevelhimer, and Fly (1997) include alternative specific constants for two sites with the largest shares of trips. Parsons and Needelman (1992) include a group specific constant for sites in northern Wisconsin with the explanation "Lakes in the north are more often in a wilderness setting and many are in a state or national forest with the protection of natural amenities that such a designation provides."

[3] Given the costs of following a panel over time, a long panel is rare in recreation demand data sets. In fact Hausman et. al. use data obtained from telephone interviews where respondents list the recreation trips they have taken in the past two years, which is likely to suffer from serious recall problems.

constant, which they call the true catch rate. If there are unobserved site characteristics in addition to those related to measurement error in catch, then Morey and Waldman's conclusion that the catch parameters will be attenuated (biased towards zero) does not necessarily hold. Also, this approach does not allow estimation of parameters on any other characteristics that vary only across sites, aside from a single fish catch measure. This substantially limits the potential applications of Morey and Waldman's model to cases where there is only one characteristic varying across alternatives and no unobserved characteristics aside from the measurement error in the single observed characteristic.[4]

3 Model

This section presents a model of angler choice that generalizes the familiar and commonly used discrete choice model to allow sites to have unobserved characteristics. Following Berry, Levinsohn, and Pakes (1995), specify the utility index for an individual i from visiting site j on choice occasion t as

$$U_{ijt} = X_j\beta_i + Z_iX_j\gamma + D_{ij}\rho + \xi_j + \varepsilon_{ijt} \tag{1}$$

The first term includes the observed characteristics of sites. The second term includes any interaction terms between individual characteristics and site characteristics. The third term is the travel distance or travel cost from an angler's residence to the fishing location and any interactions between the distance variable and individual characteristics. The fourth term, ξ_j, captures the unobserved characteristics of the alternatives. In a standard discrete choice model this term is assumed zero, which corresponds to no unobserved site characteristics. The ε_{ijt} error is the usual iid draw from an extreme value distribution with its scale normalized to one. This distributional assumption on the error, which represents idiosyncratic tastes of an individual on a particular choice occasion, leads to the familiar logit model and the associated probabilities of choosing each alternative.[5]

Fixing the taste parameters, β_i in Equation 1, to be constant across individuals produces the familiar conditional logit model. Retaining the i subscript yields the random parameters logit (also called a mixed logit or heterogeneous logit).[6]

The proposed model can allow for heterogeneous tastes across anglers in at least two ways and for at least two reasons. If different types of anglers have different preferences for site characteristics, interaction terms between individual characteristics and site characteristics can capture these differences. This method is useful if one of the goals is to determine how the gains or losses from a policy experiment are distributed among anglers. Another approach (which can be used in conjunction with the previous approach), is to allow preferences for site characteristics to vary randomly across anglers according to some distribution. This random parameters approach allows for additional taste heterogeneity, but does not

[4] Train, McFadden, and Johnson (2000) in a comment on Morey and Waldman's paper, strongly criticize the assumption that there are no other unobserved site characteristics.

[5] See McFadden (1974, 1981) or for a more accessible description Ben-Akiva and Lerman (1985).

[6] See Hausman and Wise (1978) for the introduction of the heterogeneous probit, which is similar to the logit case aside from a change in the distributional assumption on the random error term ε_{ijt} to normal rather than extreme value.

link a particular angler to a particular taste (or distaste) for a characteristic. Barring a large number of observations per individual, it is not possible to estimate individual-specific taste parameters. Specifying a distribution of tastes and estimating the parameters of that distribution substantially reduces the number of parameters that must be recovered. The taste parameters are assumed constant for a particular individual over their choice occasions. This random parameters approach makes sense if tastes vary across individuals in a manner not well linked to individual characteristics observed by the researcher.

Aside from allowing for more taste heterogeneity, a common argument in favor of the random parameters model is to weaken the IIA (independence of irrelevant alternatives) property. Concerns about IIA also motivate the use of the nested logit model, which can be interpreted as a special case of the random parameters model where a random parameter is placed on a nest-specific dummy that marks all of the included sites. The IIA property, which follows directly from the structure of the discrete choice model and the implied probabilities, limits how individuals will substitute among sites. In particular, individuals will reallocate their trips when a site is removed in a manner proportional with the share of trips they had been allocating to each site.[7] This can cause some strange patterns of substitution when some locations are better substitutes for each other than other locations. For example, suppose a lake suffers a devastating winterkill (extreme drop in dissolved oxygen while the lake is frozen-over that can extinguish live beneath the ice including fish). If a very similar lake is located nearby, and did not suffer the same fate, we may think that nearly all trips should be diverted to it. However, the IIA property means that instead trips are redistributed among all the sites in a manner proportional with the share of trips each individual is predicted to devote to each site.

Despite the fact that relaxing the IIA property often motivates the use of a nested or random parameters logit, this property can be relaxed without using these specification. Allowing for heterogeneity among individuals through the inclusion of interaction terms between individual characteristics and site characteristics and the inclusions of travel distance variables also weakens the IIA property. Returning to the above example, allowing for individual heterogeneity implies that individuals who live nearby and who have strong tastes for the characteristics of the winterkill lake will have a high probability of visiting both this lake and its very similar neighbor. Hence a model recognizing this taste heterogeneity would predict that such individuals would shift a high fraction trips to the neighboring lake. The importance of terms that vary across individuals and sites, such as interaction terms and travel distances, can be even more dramatically illustrated by imagining that they are absent. Suppose that the two neighboring lakes discussed above are in northern Wisconsin (far from population centers) and each attracts the same number of anglers as a polluted river running straight through Milwaukee (a major city in southern Wisconsin). If interaction terms and travel distance measures are absent, the IIA property implies that when the winterkill occurs the same number of visits are diverted to the polluted river in Milwaukee as to the neighboring lake. This ridiculous prediction clearly illustrates the importance of allowing for heterogeneity across individuals to weakening the IIA property.

Both simple interaction terms and the random parameters specification can achieve

[7]See Ben-Akiva and Lerman (1985) especially pages 51-55 and 108-111 for a clear discussion of the IIA property and some illustrations including the well-known "red bus/blue bus" example.

more realistic patterns of substitution by relaxing the IIA property. Because the random parameters specification can potentially allow for more heterogeneity, it can capture even more general patterns of substitution. However, this property will still hold at the level of a particular individual, but in this weakened form it does not lead to the strange implications discussed above.

Theory provides no guidance on how tastes are distributed in a population. Equation 1 imposes some structure on the taste variation by allowing for varying parameters on the site characteristics but not the interactions between individual and site characteristics. This structure assumes that while there is unobserved heterogeneity that makes each individual's taste for a characteristic unique, all individuals with the same observed characteristics will have their tastes bumped up or down, depending on the sign of the interaction parameter, by the same amount. Further, tastes are allowed to vary over the distance or travel costs measures only through interactions with observable characteristics (ρ is not given an i subscript). While these assumptions are not required for identification, they place some structure on the problem while still allowing for quite general patterns of taste heterogeneity.

For the random parameters specification, the familiar normal distribution is assumed, which is common with many other applications of random parameters models. Given this assumed distribution of tastes, we can rewrite the taste parameters as the sum of a mean and a random deviation. Denote the standard deviation σ and vectors of random draws from the standard normal distribution μ_i.

$$U_{ijt} = X_j\bar{\beta} + X_j\sigma_\beta\mu_i + Z_iX_j\gamma + D_{ij}\rho + \xi_j + \varepsilon_{ijt} \tag{2}$$

The proposed random parameters specification involves estimating both a mean and standard deviation of anglers' tastes, which raises the issue of how both parameters are identified. Identification issues arise because the probability that an individual visits a site with a high level of an attractive characteristic is increased when either the mean or the standard deviation of the taste distribution is increased.[8] The spatial distribution of individuals and alternatives typical of recreation demand data provides a strong source of identification. Both the mean and standard deviation of the taste distribution are needed to explain the trip taking behavior of individuals living near to and far from a particular fishing location. Consider again a site with a high level of an attractive characteristic. A high mean taste implies that some individuals will travel far to visit the site while nearly all those living nearby should visit it. On the other hand, a high standard deviation implies that some individuals will travel far and some living nearby will visit also, but fair number of those living nearby will choose not to visit the location. Hence, both the mean and the standard deviation are required to explain the visitation patterns of individuals who each face different trade-offs depending on where they live in the choice set.

Since we have little prior information about how tastes vary across anglers, both a random parameters specification, which also includes interaction terms, and an interaction-

[8]Increasing the mean taste obviously increases the probability that sites with high levels of the characteristic are chosen and the argument for the standard deviation is only slightly more subtle. Increasing the standard deviation makes it more likely that an individual will have an extremely strong taste for this characteristic, which increases the probability that this site will yield the highest utility index, hence increasing its probability of visitation.

only specification are considered. The random parameters specification has the advantage of allowing for more taste heterogeneity, however its estimation is more computationally costly and it requires the researcher to make assumptions about an unknown distribution of tastes which can affect model implications.

It is useful in thinking about estimation to combine the terms in the utility index that vary only across alternatives and rewrite it as

$$U_{ijt} = \delta_j + X_j \sigma_\beta \mu_i + Z_i X_j \gamma + D_{ij}\rho + \varepsilon_{ijt} \tag{3}$$

Here δ_j includes both the mean tastes for the observed characteristics that vary only across alternatives and the unobserved characteristics. Both the δ_j's and the mean taste parameters ($\bar{\beta}$), along with σ_β, γ, and ρ, are identified and recoverable. The δ_j's are identified by the relative popularity of sites once we control for geographic location and the distribution of tastes (aside from $\bar{\beta}$). The mean tastes ($\bar{\beta}$) are identified through the variation in the δ_j's and the X_j's.

4 Calculation of Welfare

The ultimate use of a recreation demand model is to determine how individuals' welfare changes when recreation locations are either improved or degraded. This section presents methods for calculating unbiased measures of compensating variation that account for the presence of unobserved characteristics.

The compensating variation (CV) associated with a change of site characteristics measures the amount of compensation required to equate the expected maximum utility a person receives in the altered state (state 1) to that of the original state (state 0). The CV can be expressed as

$$CV_i = \frac{-1}{\beta_{Pi}} \left[ln \left(\sum_{j=1}^{J} e^{V_{ij}^1} \right) - ln \left(\sum_{j=1}^{J} e^{V_{ij}^0} \right) \right] \tag{4}$$

where

$$V_{ij} = \delta_j + X_j \sigma_\beta \mu_i + Z_i X_j \gamma + D_{ij}\rho$$

The parameters on either the travel distance measures or the travel cost, β_{Pi}, determine the units of measurement for the estimated CV, which will either be one-way miles of travel or dollars.

Unobserved site characteristics impact welfare estimates by affecting the estimated taste parameters ($\bar{\beta}$, σ_β, γ, and ρ) and through their direct role in the calculation of CV as a portion of δ_j. The impact of the unobserved characteristics on the taste parameters are explored in the next section through the use of Monte Carlo simulation and the direct role they play in the calculation of the CV is considered next.

Inspection of the functional form of the CV calculation, given in Equation 4, reveals the impact of ignoring unobserved site characteristics. Because the deterministic portion of the utility index (V_{ij}) is exponentiated, the CV will be understated for improvements at sites with above average unobserved characteristics and overstated for improvements at sites with below average unobserved characteristics. Further, the CV will more dramatically overstate or understate gains at sites that have favorable values of the observed characteristics. The highly nonlinear form of the CV formula causes it to be quite sensitive to the exclusion of site characteristics from the calculation. As a result, excluding characteristics that affect choice will bias the estimated CV. The degree of bias depends on the popularity of the site based on both observed and unobserved characteristics and the relative importance of unobserved characteristics in influencing choice.[9]

Policy experiments that focus on a single site or a relatively small group of sites are more likely to be seriously affected by this bias. However, if the impact of unobserved characteristics is considerable the results of most policy experiments will be substantially biased. This source of bias can easily cause projects to be ranked incorrectly. When some site characteristics are not observed it no longer necessarily holds that the truly best site (most popular) will have the highest utility index, because this index is calculated using only a subset of the relevant characteristics. This means that even "scaling restoration", which finds an improvement that can compensate for an environmental damage rather than finding the monetary loss, cannot circumvent this bias. This approach would alleviate concerns about bias if both the estimated damage and estimated gains from the proposed improvement were similarly biased. However, if the model cannot even rank a set of proposed projects, this method will not address the problem.

The welfare calculation can be adjusted to account for unobserved characteristics by using the parameters obtained from the proposed model. Simply rewriting the deterministic portion of utility that enters the CV calculation by breaking δ into its components reveals that the unobserved characteristics are included. We can think of subtracting the observed characteristics from the estimated deltas ($\xi_j = \delta_j - X_j\bar{\beta}$) to obtain an estimate of the unobserved site characteristics.

$$V_{ij} = X_j\bar{\beta} + \xi_j + X_j\sigma_\beta\mu_i + Z_iX_j\gamma + D_{ij}\rho \tag{5}$$

Thus the proposed approach provides estimates of the unobserved characteristics that can be including in the calculation of welfare to obtain unbiased estimates.

5 Monte Carlo Simulations: Consequences of Ignoring Unobserved Site Characteristics

Monte Carlo simulations show how the behavioral model is affected when we do not observe all factors affecting individuals' choices. Lee (1982) considers the effects of omitted variables in the multinomial logit model but from the onset assumes that a full set of alternative specific constants are included in the model, which does not directly address the problem at hand. By designing a Monte Carlo experiment that captures the important features

[9]Models that ignore unobserved site characteristics must explain the observed variation in behavior by giving much more weight to the random (or error) component of utility. In the extreme case where there are no observed differences in sites and all behavior is explained by the random component, the expected welfare gains from an improvement would be equal across all sites.

of typical recreation demand data, we can learn about the likely impacts of unobserved characteristics in this context.

Simulated data are preferred to actual data because we know the parameters and exact model used to generate the data, whereas this is unknown for the real data. Hence, we can attribute any differences and biases to the presence of unobserved site characteristics and not to other sources such as incorrect model specification or measurement error in observed characteristics. One potential disadvantage of using simulated data, is that the results may be sensitive to the design of the Monte Carlo experiment, which could limit the relevance of the results. The careful construction of the simulated data, described next, addresses this concern.

5.0.1 Designing the Experiment

To make the results of the Monte Carlo simulation meaningful, it is designed to approximate the patterns observed in the real data. The fishing locations are taken directly from actual data and the anglers' residences retain their geographic location.[10] Hence, the simulation utilizes the real travel distances, measured in one-way miles, from each residential location to each fishing site.

Aside from the spatial distribution of fishing sites and anglers and the implied travel distances, the other characteristics of the fishing sites are simulated. Each site is endowed with six characteristics. The characteristics are simulated by independently drawing each one from a uniform distribution over the range from negative 1 to positive 1 (U[-1,1]). This design implies that each site characteristic has a mean and median of approximately zero and is uncorrelated with the other site characteristics and travel distance. The fact that the site characteristics have mean zero is without loss of generality because only the relative ranking of sites implied by the utility index affects choice, not the absolute level of the index. This design ensures that any effects stem from the simple omission of relevant site characteristics and not from a correlation between the included and omitted variables (endogeneity).

From each of the 338 unique residential locations of the Wisconsin anglers (defined as a zip code), 2,000 simulated trips originate. A large number of trips is required to ensure that each of the alternatives receives at least one visit. Excluding sites not visited biases the results because such sites are not random, but rather are selected based on their site characteristics. Consistent with a standard discrete choice model, on each trip the individual chooses the site that gives the highest level of utility. The utility index is a linear function of the site characteristics, the travel distances, and an additive error with an extreme value distribution:

$$U_{ijt} = X_j\beta + D_{ij}\beta_D + \varepsilon_{ijt} \tag{6}$$

where i indexes the individual, t the trip, and j the fishing location. X includes the site characteristics and D the travel distance to each site from each residential location.

[10] To make the simulation manageable, 185 of the 569 actual single-day fishing sites are used. These include all sites visited more than twice and exclude sites in counties where no anglers live and ones in the same geographic location as another included site.

To complete the specification and simulate choices requires setting the parameters. Parameters are chosen to match the relative importance of the site characteristics, the travel distance, and the random error to the actual data. To begin we set the scale of the error to one, in accordance with convention.[11] A simple conditional logit model that includes a full set of alternative specific constants is estimated using the actual data to determine what percent of the variation in the utility index is explained by travel distance, site characteristics, and the error. Travel distance is found to be extremely important and it explains approximately 87 percent of the variation in the utility index, the combined site characteristics explain approximately 9 percent, and the random error term approximately 4 percent. The following specification of the utility index weights the travel distance, combined site characteristics, and the error approximately the same as the actual data.

$$U_{ijt} = 0.25x_{1j} + 0.75x_{2j} + 1.25x_{3j} + 1.5x_{4j} + 1.75x_{5j} + 2.0x_{6j} - 0.1D_{ij} + \varepsilon_{ijt} \tag{7}$$

where x_1, x_2, x_3, x_4, x_5, and x_6 are a set of independent site characteristics, D_{ij} is one-way travel distance measured in miles, and ε_{ijt} is a draw from an extreme value distribution with scale one. Including multiple site characteristics and giving them different parameters allows experiments where the level of importance of the omitted ("unobserved") variables differs. The combined importance of the six characteristics mirrors the actual data, however the choice of including exactly six characteristics and allocating their relative importance in the above manner is one of many possibilities.

The experiment described above is repeated many times, each time drawing a new set of site characteristics and simulating a new set of choices.[12] By looking across these different draws of data, we can compare the performance of the proposed model that accounts for unobserved characteristics with the standard conditional logit model.

5.0.2 Effects of Unobserved Site Characteristics on Parameter Estimates

This section considers a set of models where the role of unobserved site characteristics ranges from minor to important. The simulations show that ignoring unobserved site characteristics can cause unreliable parameter estimates that destabilize the welfare estimates and cause dangerously misleading standard errors.

A wide range of hypothetical policy experiments could be evaluated and their welfare estimates compared across models. A key determinant of any welfare estimate is the relative importance of the changing site characteristic(s) to the price (travel distance) parameter, which measures the marginal benefit from an incremental unit of the characteristic at the chosen site. Using the ratio of the site characteristic to travel distance parameters to examine the impact of unobserved site characteristics on welfare estimates focuses attention on the estimated parameters' role in welfare estimates and hence provides results that do

[11] This scaling convention is without loss of generality. Once the scale of the error term is set its role in affecting choice is determined by the size of the parameters placed on the site characteristics and travel distance.

[12] The experiment is repeated 134 times. The large size of the simulation and the variety of levels of unobserved site characteristics considered limit the number of repetitions possible with finite time and computing resources. However, this number of simulations is more than enough to clearly identify the trend.

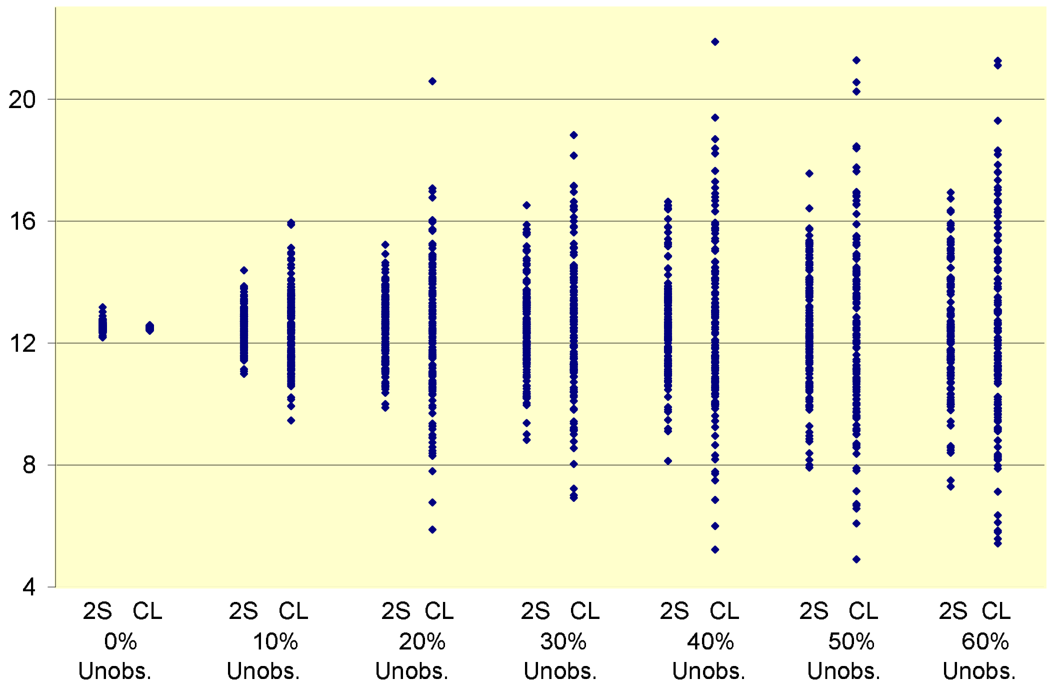

Figure 1: Comparison of Parameter Ratio for the Proposed Approach (2S) and the Conditional Logit (CL)

not depend on specific policy experiments. In fact, the magnitude of the estimated welfare gain from an improvement at all sites is quite nearly proportional to the ratio of that characteristic's parameter to the travel distance parameter. The focus is on the impact that the estimated taste parameters have on the welfare estimates and not the direct bias of the welfare estimates caused by omitting variables from the welfare calculation itself. This second source, discussed in the previous section, is straightforward and does not require simulations to understand or illustrate.

For a selected parameter ratio Figure 1 compares the distribution generated by many repetitions of the simulation experiment across model specifications and levels of importance of the unobserved characteristics. The selected ratio is the parameter on the third characteristic (x_3) to travel distance. The third characteristic is selected for the comparison because it is never omitted and has a middling level of importance relative to the other site characteristics.[13] Figure 1 shows that when all of the characteristics are observed, the conditional logit model produces an estimate very close to the absolute value of the true ratio, 12.5 (1.25/0.10), for all of the draws of simulated data. The proposed approach produces a ratio that on average is 12.5 but varies a bit more across draws of data. However, as we move rightward in Figure 1 and some characteristics are unobserved, the proposed approach clearly outperforms the conditional logit model that ignores the unobserved site characteristics.

[13]The results do not change if you consider the ratio for the other characteristics.

Table 1:
Misleading Standard Errors of Conditional Logit
when Unobserved Characteristics are Present

% Site Characteristics Unobserved	Actual Variation in Parameter Ratio sd	Perceived Variation in Parameter Ratio mean sd	min sd	max sd
0	0.03	0.03	0.03	0.04
10	1.21	0.04	0.03	0.06
20	2.36	0.07	0.05	0.10
30	2.82	0.08	0.06	0.12
40	3.05	0.08	0.06	0.13
50	3.84	0.09	0.06	0.15
60	3.97	0.10	0.06	0.15

While both models produce ratio estimates that are on average equal to the true ratio, the proposed approach produces less variable, more reliable, estimates. For example, when 20 percent of the site characteristics are unobserved the standard deviation across draws of simulated data is 2.27 for the conditional logit model, which is substantially larger than the standard deviation of 1.16 for the proposed approach.[14] As the importance of unobserved site characteristics grows the variability in the estimated ratio grows in both models, but the proposed approach continues to produce more reliable estimate of this ratio, and hence welfare measures, compared to the conditional logit model.

While Figure 1 shows that less variable estimates are obtained from the proposed approach, the standards errors of the conditional logit would like to convince us otherwise. In the presence of unobserved site characteristics the conditional logit model produces standard errors that are biased downward and hence overstate the precision of the estimates. Table 1 compares the actual variation in the estimated parameter ratio (β_{x3}/β_D) across simulations with the amount of variation indicated by the estimated standard errors of the conditional logit model. The contrast is striking. While the estimated ratios bounce around substantially, the standard errors remain small. The maximum standard deviation in any of the simulations is not much bigger than the mean indicating that even when the conditional logit produces estimates very far from the true parameters, the standard errors are small. For example, when 20 percent of the site characteristics are unobserved, the standard errors reflect at most 4.2 percent ($\frac{0.10}{2.36} * 100$) of the actual uncertainty in the estimates.

Unreliable parameter estimates and biased standard errors are a dangerous combination. This can lead to incorrect hypothesis tests about the sign and relative magnitude of parameters. For example, when 20 percent of site characteristics are unobserved the parameter on the first characteristic is incorrectly found to be negative and statistically significant (at standard levels) 12 percent of the time. When 50 percent of characteristics are unobserved, we would draw this incorrect conclusion 33 percent of the time. Similarly, characteristics are often ranked incorrectly and these differences are almost always found to be statistically significant given the small standard errors.

[14] A model with 20 percent of the site characteristics unobserved is obtained by excluding the fourth characteristic (x_4) from estimation (20 % $= \frac{1.5}{0.25+0.75+1.25+1.5+1.75+2.0} * 100$).

This Monte Carlo analysis provides evidence that unobserved site characteristics cause the parameter and welfare estimates of the conditional logit model to be unreliable relative to those obtained from the proposed approach. Simulations show that ignoring unobserved characteristics causes the variance of the parameter estimates to increase substantially. It can be double, triple, or even quadruple the variance of the proposed estimator. Further, the standard errors of the conditional logit are biased downward when unobserved site characteristics are present. These findings combined with the direct bias of welfare estimates caused by omitting characteristics from the welfare calculations, provide strong support for using the proposed approach even when the fraction of characteristics that are not observed is relatively small.

6 Estimation

The proposed models are estimated using maximum likelihood and simulated maximum likelihood techniques. The log-likelihood function measures the probability that the sample makes the observed choices and can be written as

$$L = \sum_{i=1}^{N}\sum_{t=1}^{T}\sum_{j=1}^{J} y_{ijt} ln(P_{it}(j)) \tag{8}$$

where

$$y_{ijt} = \begin{cases} 1 & \text{if site j chosen by individual i on trip t} \\ 0 & \text{otherwise} \end{cases}$$

$$P_{it}(j) = \frac{exp(V_{ij})}{\sum_{k=1}^{J} exp(V_{ik})}$$

$$V_{ij} = \delta_j + X_j \sigma_\beta \mu_i + X_{ij}\bar{\gamma} + X_{ij}\sigma_\gamma \nu_i$$

Model estimation proceeds in two stages. The first stage maximizes the likelihood given in Equation 8, which includes a full set of alternative specific constants, δ, and any terms that vary across individuals (or choice occasions) and alternatives, which includes σ_β, γ, and ρ.[15] This stage produces estimates of the vector δ and parameters on travel distance and interaction terms between individual and site characteristics as well as the standard deviation parameters on the taste distributions.

In the second stage δ is regressed on the site characteristics to obtain estimates of the $\bar{\beta}$ parameters. The error term in this regression includes all of the site characteristics that are not observed.

Estimating a random parameters version of the two-stage model is more computationally challenging, but feasible. It is well-known that the probabilities associated with the random

[15] BLP provide an algorithm to obtain the δ's and to recover the parameter estimates using aggregate (share level) data. However, it is rare for recreational fishing data to contain information on the total visitation to each site by the population, but it is common to have micro level data linking a particular individual to a visit at a particular site. Given micro level data the δ's can be easily recovered by including a full set of alternative specific constants. If information on the total number of trips to each site by the population were available, it could be included and would greatly improve efficiency.

parameters logit do not have a closed form solution and involve multidimensional integration over the taste distribution.[16] This specification can be estimated using standard simulation techniques to obtain consistent parameter estimates. The number of taste draws is set to 5,000 to minimize the simulation error, which enters non-linearly causing a bias that diminishes with increases in the number of simulation draws.

Given the large number of alternative fishing locations in these data and many other recreation demand data sets, a model including a full set of alternative specific constants can be difficult to estimate using standard gradient methods within maximum likelihood estimation. In this application, this problem arises with the random parameters specification but not any of the fixed coefficient specifications, which do not require simulation techniques for estimation. To address this issue, an alternative method of obtaining estimates of the δ's is proposed. This method utilizes a contraction mapping presented in Berry, Levinsohn, and Pakes (1995). The contraction mapping makes use of the fact that for a given set of parameters the maximum likelihood δ's are those that exactly equate the predicted market share of each site to the actual market share. This means that when a full set of alternative specific constants are included the predicted number of trips must equal the actual number of trips for each site. A proof that the the maximum likelihood estimates of the δ's have this property is provided in the Appendix. The contraction mapping is the following

$$\delta_{new} = \delta_{old} + ln(s) - ln(s(X, \delta; \Theta)) \tag{9}$$

where the new estimate of δ is a function of the previous guess and the differences between the actual and predicted share of trips, which depends on the δ's and the parameters of the model.[17] With each iteration this mapping has the property that the subsequent delta is always closer to the truth and that each iteration results in a smaller change in the δ's than the last. The maximum likelihood estimation searches over all the parameters aside from the δ's. For each iteration of the estimation, the δ's that equate the actual and predicted shares for the given parameters are found and the likelihood and gradients are constructed and returned. This limits the number of parameters the maximum likelihood routine must search over and makes estimation of a random parameters model tractable.

7 Data and Empirical Specification

The 1998 Wisconsin Fishing and Outdoor Recreation Survey (WFORS) is the primary source of data used in this analysis.[18] This survey begins with a telephone interview and is completed with the return of four monthly booklets where individuals record all of their fishing and outdoor recreation activities for the summer of 1998.

In May of 1998 a random digit dial telephone survey of households in Wisconsin produced a sample of 1,275 anglers willing to participate in a detailed mail survey about fishing and outdoor recreation in the state of Wisconsin. Of the anglers completing the telephone

[16] See Hajivassiliou (1993).

[17] The share is simply the number of trips to the site divided by the total trips.

[18] Triangle Economic Research, Durham N.C. in conjunction with the Wisconsin Department of Natural Resources designed the 1998 WFORS and the Wisconsin Survey Research Lab administered the survey.

interview, 81.0 percent agreed to participate in the mail survey, which is a good response rate.

The sample is stratified according to the Wisconsin Department of Natural Resources (WDNR) districts and the data include sampling weights. Over 84 percent of the population of Wisconsin lives within the sampled area. Individuals received fishing and outdoor recreation booklets at the beginning of June, July, August, and September in 1998 to record the details of each day's fishing and outdoor recreation activities. Individuals complete a fishing summary form for each day spent fishing. A total of 862 anglers completed 7,294 fishing summaries. Information contained in the fishing summary allows identification of the fishing location visited. The exact fishing location is known and coded for 99.6 percent of the fishing summaries, which illustrates the high quality of the information provided by the survey participants.

Information provided in the booklets indicates whether the fishing day is part of a longer recreational trip including an overnight stay (a multiple day trip) or a single day trip for fishing. The single and multiple day trips exhibit some similarities, but differ in several dimensions including the distance traveled from the individuals' permanent residence and the geographic location of the fishing locations. On average individuals traveled 19.4 miles (one-way) to reach their destination for single day trips compared to 148.0 miles (one-way) for multiple day fishing trips. Multiple day trips introduce many difficulties for a travel cost approach, which ultimately relies on observing how individuals trade-off travel distance with the attributes of fishing locations. One important difficulty is that there are substantial unobserved costs of taking a multiple day trip that will vary a lot across alternative fishing locations, including lodging and food, which are likely to be large relative to the cost of travel. A substantial number of the multiple day trips are to an individual's vacation home or cabin, which obviously makes the marginal cost of visiting locations near the cabin much cheaper than other locations. The decision to purchase the cabin (not the decision to visit the cabin once purchased) is the relevant one for determining the value of the fishing opportunities. Another important problem is how to distribute the total travel cost among the various sites visited during a multiple day trip. Given these difficulties, this analysis will focus on the single day trips. Alternative models that include the choice among sites on single day trips and also include the choice of taking a multiple day trip (but not modeling the choice among sites for these trips), along with the choice not to fish on a given day do not produce dramatically different results, but do increase the size of the problem and the computational burden. Both the choice to take multiple day trips and not to fish on a given day are reintroduced in another paper that considers the link between successive decisions made by an individual angler over the course of the season.

Figures 2 and 3 illustrate the spatial distribution of anglers and their single day trips at the county level. For their single day trips, the figures show that anglers tend to choose fishing locations near their primary residence but outside of major cities like Green Bay and Milwaukee. Figure 3 shows a heavy concentration of residences in northeast because of the stratification scheme, which over sampled this area relative to the rest of the state.

The sample includes those individuals who participated in the entire WFORS and consistently provided high quality information throughout the survey period. Of the 1,036

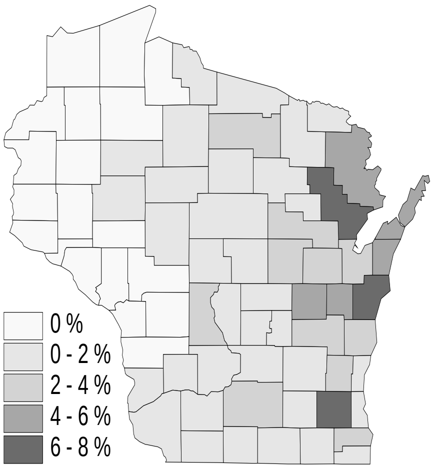

Figure 2: Distribution of Single Day Trips

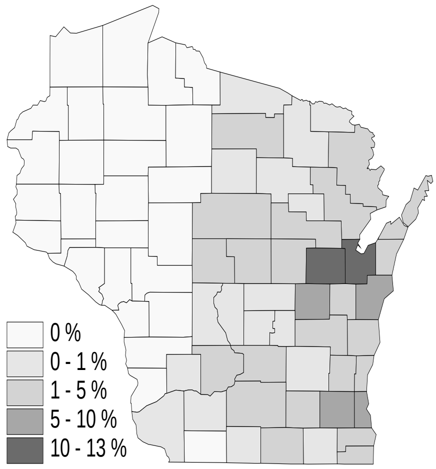

Figure 3: Distribution of Residences

individuals that returned monthly fishing booklets, the sample includes 788 anglers.[19] Table 2 summarizes the available demographic, socioeconomic, and other relevant household information for these anglers.

Of the included 788 anglers, 668 report taking at least one fishing trip during the survey period. These anglers report 6,005 fishing days for which they provided detailed information. Of these fishing days, 61.8 percent are associated with single day fishing trips and 38.2 percent with multiple day trips.

7.1 Site Definition and Travel Distance

Ideally, the fishing sites should be defined in a way consistent with what each angler actually considers when deciding where to fish. Given that we only observe the outcome of the angler's decision and not their thought process, sites are defined in a way that recognizes natural boundaries, conforms with existing data sources, and provides a specific location for the measurement of travel distance.

The WFORS provides detailed information on each angler's fishing destination, including the name of the water body, the nearest town, and the location of a sticker on a large 3 foot by 3 foot Wisconsin map marking the fishing location for each trip. The sticker location is recorded by dividing the state into quadrangles based on the U.S. Geological Survey (USGS) 7.5-minute quadrangle system.[20] Detailed information on each angler's fishing location permits a disaggregate approach to site definition. A disaggregate approach allows a more accurate measure of the distance from an angler's home to each site (compared to measuring the distance to the center of some larger aggregate site such as a county or

[19]Those 861 anglers that returned all four of the booklets (June-September) are retained. Of these individuals, 28 are dropped because they did not complete the catch summary form or did not report the date for 25 percent or more of their fishing days. An additional 45 individuals who did not complete the demographic questions are dropped.

[20]Wisconsin contains 1,154 quadrangles and each is roughly seven miles long and five miles wide.

Table 2:
Angler Characteristics

	Mean	Median	Min	Max
Male	0.70	1	0	1
Age	43.84	41	18	90
White (ethnicity)	0.98	1	0	1
Personal Income	36928	35000	5000	120000
Works full-time	0.76	1	0	1
Works part-time	0.09	0	0	1
Not employed	0.15	0	0	1
Hours work / week[1]	45.04	45	20	85
Some high school	0.06	0	0	1
High school degree	0.40	0	0	1
Some college	0.33	0	0	1
College degree (4 year)	0.11	0	0	1
Some graduate work	0.04	0	0	1
Graduate degree	0.06	0	0	1
Age when started fishing	9.59	8	1	55
Number of adults in HH	2.12	2	1	6
Number of anglers in HH	1.54	1	1	5
Number of children in HH	0.92	0	0	5
Household Income	51408	45000	5000	300000
HH owns a boat	0.47	0	0	1
HH owns a fishing pond	0.03	0	0	1
Primary residence is waterfront	0.07	0	0	1
HH owns a vacation home	0.17	0	0	1
Vacation home is waterfront	0.53	1	0	1

[1] Among those individuals working at least 20 hours per week.

Table 3:
Distribution of Anglers over Sites

Number of Anglers	Number of Sites		Number of Trips	
Visiting Site	Number	Cum. %	Number	Cum. %
1	615	57.9	1460	20.1
2	190	75.7	1052	34.6
3-5	161	90.9	1589	56.4
6-10	69	97.4	1402	75.7
11-20	18	99.1	747	86.0
21-42	10	100.0	1016	100.0
TOTAL	1063	–	7266	–

region), is consistent with the level of detail available in most of the additional data sources, and is likely to avoid the biases that arise when heterogeneous sites are aggregated.[21]

Sites are defined using information on the water body name and quadrangle. Each inland lake visited by an angler constitutes a separate fishing site. Lake Michigan, Green Bay, Lake Winnebago, and all rivers and streams are further divided because of their large size or long length. Fishing sites on these water bodies are defined by partitioning the water bodies into quadrangles. According to this definition, there are 1,063 different sites visited including 503 unique quadrangles and 808 unique water bodies.

Given this narrow definition of sites most sites are visited by very few anglers, as shown in Table 3. A majority of sites, 57.9 percent, are visited by only one angler and 75.7 percent of sites are visited by two or fewer anglers. However, the majority of trips, 55 percent, are to sites that are visited by 4 or more anglers while only 20.1 percent of trips are to sites visited by only one angler. Thinking in terms of visits rather than anglers, a sizable fraction of the sites receive only a single visit, 33.0 percent. Nearly half the sites are visited two or fewer times.

7.2 Travel Cost

Days of recreational fishing do not have a market price, which means that a price must be constructed using an assumption of weak complementarity, which links fishing days with marketed goods (see Bockstael and McConnell (1999) and Freeman (1993)). Weak complementarity holds when the individual places no value on the non-marketed good unless they consume some of the marketed goods. To clarify, in this application the marketed goods include the fuel and automobile maintenance required to travel to a fishing location, the angler's time, and any entrance fee associated with a site. Weak complementarity implies that if an angler does not visit a site (does not consume the marketed goods) then their utility is invariant to changes in the quality of that site.[22] The weak complementarity

[21] See Parsons and Needelman (1992) and Feather (1994) for discussion of these biases.

[22] This excludes so-called "non-use values", which are independent of any observable behavior and measure the value individuals may place on the quality of a location they will never visit. Non-use values (also called passive-use or existence values) received a lot of attention during the economic debate following the 1989 *Exxon Valdez* oil spill in Alaska. See Solow et al (1993), Desvousges et al (1993), and Randall (1993) for

assumption combined with the assumption that individuals get no utility or disutility from driving, allows us to treat the travel cost as the price of a day of fishing.

Fishing involves many expenditures in addition to travel costs such as the cost of gear (including rods, lures, tackle box, cooler, waders, etc.), the cost of obtaining a fishing license ($14.00 per year in Wisconsin in 1998), the cost of owning and operating a boat, and the cost of owning and maintaining a water-front vacation home. Excluding expenditures other than the travel costs limits the scope of the policy experiments that can be considered. Without these expenditures, we cannot make statements about the total value of fishing or the value of a day of fishing.[23] However, we can value a change in the attributes of some sites even without information on equipment, lodging and the other expenses mentioned above so long as we assume that anglers would not vary these expenditures because of proposed changes in the attributes at some sites. So long as the unobserved additional costs of a fishing day do not affect the choice among sites and only the choice of whether or not to fish on a particular day, then policy experiments where some site attributes are changed are valid. This type of policy experiment rather than the total value of fishing is of interest to fishery managers and barring a massive damage to fishery resources is of interest in most NRDA cases.

The constructed travel cost includes three components: the mileage cost, the access fee for visiting a site, and the opportunity cost of the driving time. The mileage cost is calculated using the round trip travel distance to each site as computed by PC Miler at a rate of 10.7 cents per mile, which is the estimate of the average operating costs for gas, oil, maintenance, and tires according to the American Automobile Association in in the 1998 edition of "Your Driving Costs". In general, there are no access fees for the lakes and rivers of Wisconsin so these are ignored.

The typical approach for determining the opportunity cost of driving time is to use some fraction of the individual's wage rate as the hourly opportunity cost of time. See Feather and Shaw (1999) for a review of the literature. Use of the wage rate assumes that the labor market is in equilibrium; in other words, employees are not willing to trade pay cuts (increases) for decreases (increases) in the hours they work per week. Further, this approach does not provide guidance on how to handle students, the unemployed, and retired individuals. Alternatives, such as hedonic wage equations where wages are regressed on observed individual characteristics and disequilibrium labor market models where the shadow wage is calculated are discussed in Smith et al. (1983) and Feather and Shaw (1999). Both alternatives allow non-zero opportunity cost of time measures for those that are not employed.

This paper constructs travel costs using the after tax wage rate of individuals that are working full or part-time and the predicted wage rate for those individuals that are not working using a hedonic regression as the measure of the opportunity cost of travel time. The

a discussion. This application considers the value of attributes of fishing locations that are less likely to inspire non-use values so they are ignored.

[23]Stavins (1997) estimates the total value of fishing by using aggregate data about fishing license sales for a cross section of states over a 15 year period. Theoretically he is able to estimate the total value of fishing by using a household production model and the fact that a fishing license is an essential input in the production of a day of fishing. However, the variation in licensing fees across states and over time may not be able to trace out most of the demand curve and lead to heavy reliance on functional form assumptions.

Table 4:
Hedonic Wage Regression

Independent Variables	Parameter Estimate	Standard Error
Two adults	0.75	0.66
Three or more adults	1.94	0.77
One child	0.63	0.57
Two children	0.86	0.56
Three or more children	1.00	0.66
Male	1.54	0.45
Age	0.54	0.11
Age squared	-0.0051	0.001
High school diploma	0.12	1.09
Some college	1.70	1.09
College degree	2.44	1.19
Some graduate work	4.39	1.38
Graduate degree	7.07	1.30
White	1.30	1.66
Median household income of zip code (in $10,000)	1.33	0.28
Constant	-9.90	3.12
Adjusted R-squared	0.23	
Number of Observations	696	

Note: Dependent variable is the wage rate after tax.

socioeconomic and demographic variables available for use in the hedonic wage regression include age, gender, level of education, ethnicity, and household structure (number of adults and number of children). Information from the 1990 Census at the zip code level is also included.Table 4 reports the results of the hedonic wage regression, which is estimated using a sample of those working full or part time (at least 20 hours per week). The overall fit as measured by an adjusted R-squared statistic of 0.23 is comparable to other studies including hedonic wage regressions (see Feather (1999)). All of the included variables have the expected sign. For the majority of individuals (73.6 percent) the actual after tax wage rate is used as a measure of the opportunity cost of an hour of travel time for the remaining 26.3 percent, the predicted wage is used.

The travel cost is calculated by summing the mileage costs and the time costs. The mean travel cost per mile is 39 cents and the median is 36 cents. The minimum for any individual is 11 cents per mile and the maximum is $2.76.While it difficult to determine the validity of these estimates, it is useful to compare them with some other per mile measures to see if they are reasonable. These numbers compare well with the per mile compensation of truck drivers which is currently 35 cents per mile for the Wisconsin area.[24]

Given the bold assumptions required to construct a measure of travel cost, and par-

[24] Estimate based on the per mile compensation for a driver with one year experience as advertised by the Heartland Express trucking company on their website www.heartlandexpress.com posted June 2001.

ticularly those related to the opportunity cost of travel time, this paper includes both a measure of travel cost and an alternative. The alternative approach simply measures the cost of visiting a site in round trip miles. Within the behavioral model, the taste parameter for travel distance is allowed to vary across individuals depending on variables including the estimated opportunity cost of their time and their employment status. This approach does not attempt to disentangle the marginal utility of money from the true travel cost measured in dollar units, but it does allow individuals' reactions to travel distance to vary based on factors that should in theory affect their travel costs. Thus the gains or losses from policy experiments can either be measured in terms of dollars or miles of travel distance. While a measure in dollars in general would be more useful, measurement in miles of travel is sufficient for ranking potential projects or finding an improvement that will offset a damage when both can be evaluated within the same model.

7.3 Fish Catch Rates

Sites vary in the types of fish available and the size of the populations. While the WDNR collects information on the quality of fisheries, it is not comprehensive or regularly updated. This necessitates an approach that combines the available information from the WDNR with fish catch information collected in the WFORS to create measures of fishery quality for all included fishing sites.

The ideal data would include species-specific measures of fish population collected during the summer of 1998 for all fishing sites. However, Wisconsin's tremendous number of fishing locations, including over 15,000 inland lakes, 12,600 rivers and streams that extend for over 43,000 miles, and 650 miles of Great Lakes shoreline, makes the collection of detailed, comprehensive, and current data impossible within any reasonable budget.

Good exogenous data on fishery quality is usually absent from angling data, which has led researchers to develop several different approaches for incorporating the reported catch of the individuals also included in the behavioral model. Two common approaches using the catch reported by survey participants either form an average over some geographic regions or predict catch assuming a Poisson or related process. See McConnell et al (1995) for a review of the literature. Whether catch is some geographic average or predicted for each angler affects the interpretation of the parameters in the behavioral model. With the averaging approach, all anglers face the same measures of catch. This site specific measure should be strongly related the fish population, which is what resource managers consider.[25] The estimated parameters on catch in the behavioral model reflect the tastes the angler has for an improvement in the average catch rate (or fish population).

However, some researchers argue that the behavioral model calls for a measure of what each particular angler expects to catch. See McConnell et al (1995), Kaoru et al (1995), and Schuhmann (1998). Skills, gear, and information certainly vary across anglers and impact the rate at which fish are caught, but these individual variables are clearly endogenous and determined by underlying tastes. Anglers that have a strong preference for a particular

[25]Often fishery managers consider the catch rate directly in assessing the quality of a fishery given the difficulty and expense of directly monitoring fish populations. See Eggold (1998) and Johnson and Carpenter (1994).

species are likely to develop skills, purchase gear, and collect information that allow them to improve their angling success. This approach entangles the anglers taste for a species with their catch rate provided that anglers seek to improve their catch rate for species that they like.

The behavioral model developed in this paper allows the tastes for species to vary rather than creating measures of catch that vary across anglers. By allowing for variation in tastes, the model recognizes that anglers with different skill, gear, information, and preferences will react differently to changes in the quality of fisheries. This clarifies the interpretation of the catch parameters as the taste for an improvement in the objective quality of a fishery.[26]

Much of the literature has aggregated species to form a single measure of catch or catch proxy or has looked only at one species. Schuhmann (1998), Parsons and Hauber (1998), Morey, Shaw, and Rowe (1991), and Parsons and Needelman (1992) allow for some differentiation across species. The detailed data available for this study allows catch to be identified separately for nine different species, which include salmon (coho and chinook), trout (brook, brown, and rainbow), walleye, largemouth bass, smallmouth bass, temperate basses (white bass and white perch), panfish (yellow perch, bluegill, crappie, and sunfish), northern pike, and musky, which is the most disaggregate approach to date in the literature. The diversity of fishing opportunities in Wisconsin, the species-specific nature of fishery management programs, and the specificity of anglers' tastes regarding species necessitates a disaggregate approach for this application.[27]

The goal is to develop a measure of catch for each site and each type of fish that best reflects the objective quality of the fishery. The primary sources of data are the WFORS and the WDNR data available for inland lakes. The WFORS fish catch data have the advantage of being detailed and comprehensive (at least for the sites actually visited by the survey participants). Table 5 shows the species groups, the number of fishing days where anglers report success (defined as a day with a positive catch), and the catch rates per hour for those days. For each day spent fishing, individuals record the number and species of the fish they personally caught and the time spent fishing. The survey provides catch information for nearly 28,000 hours of fishing effort.

The catch rates are an average over the June-September season. One important consid-

[26] However, along with all approaches that rely on catch rates as the measure of fish population rather than a direct biological measure, which while preferred is seldom available, this approach cannot truly disentangle aggregate tastes for a fish species from the catch rate. Species that are less popular targets will have lower catch rates compared with species that are more popular because many anglers not targeting the fish will catch zero. In theory, this problem could be addressed by using the catch rate when a species is targeted. However, most sites are visited only once or twice which means that for most species there is no observed targeted catch rate. The fact that fishery managers often make policy decisions based on average catch rates makes this measure of fish population meaningful for policy experiments despite the fact it incorporates aggregate tastes.

[27] There are substantial differences in how anglers view different fish species. For example, the musky is a highly esteemed fish noted for its elusive behavior. According to the WDNR, it takes the average angler over 50 hours to land a musky larger than 30 inches. In contrast, the yellow perch is easily caught (it is not uncommon for an angler to catch 30 or more panfish on a single trip) and is recommended for young children. In addition some fish species are noted for their delicious taste, including panfish, walleye, salmon, trout, catfish, and northern pike, while others are seldom eaten. Fish of different species also differ substantially in their average size and ability to fight once on the line.

Table 5:
Fish Species Caught

Species Group	Number Trips Catch > 0	Percent Trips Catch > 0	Catch Rate when Catch > 0
Panfish	2864	42.5	3.83
Walleye	1011	15.0	1.07
Largemouth	913	13.6	1.05
Smallmouth	929	13.8	1.52
Temperate	325	4.8	2.03
Musky	211	3.1	0.50
Northern	796	11.8	0.72
Trout	426	6.3	1.00
Salmon	291	4.3	0.59
Other	493	7.3	1.37
All	5320	79.0	3.20

eration is whether this aggregation over time is valid. Figure 4 shows weekly average catch rates for the nine different types of fish. While there is quite a bit of variation, which is most likely caused by somewhat small sample sizes for particular weeks, there is no clear trend in the catch rate over the summer season.[28] Given the lack of a clear trend and the limits imposed by the sample size, aggregating catch rates over the June-September season seems reasonable.

The WDNR provides information on the fish abundance for virtually all inland lakes. The fish abundance measure takes on the following values ‘abundant’, ‘common’, ‘present’, and ‘not present’ and is included for musky, northern pike, walleye, largemouth bass, smallmouth bass, panfish, trout, catfish, and sturgeon. A strength of these data is that, at least for inland lakes, it is comprehensive and detailed. A major shortcoming is that the bulk of the data was collected in the 1950s and 1960s, which makes it somewhat outdated, and it excludes Lake Michigan, Green Bay, streams, and rivers. It has been periodically updated for some lakes, but there is no information indicating which lakes have been updated or when. Even with these shortcomings, there is certainly useful information in these data that should be exploited.

The simplest approach of averaging over those trips to each site is rejected because the majority of sites are visited only once or twice, which is too few to create a reliable average. To increase the number of observations, and in turn the accuracy of the catch measure, consider including nearby locations with similar observable characteristics.

A localized weighted least squares (WLS) procedure is used to predict the catch rate for each species at each site. The approach is localized in that the regression for a particular site includes only those sites of the same type within a 50 mile radius. This radius provides ample observations, while including only those sites likely to be quite similar to the origin. The different types of water bodies are inland lakes, rivers and streams, and Green Bay and Lake Michigan. This approach takes advantage of the unobserved similarities among sites

[28] Johnson and Carpenter (1994) using a panel of walleye catch data for Lake Mendota, a popular Wisconsin lake near Madison, also find no trend in the catch rate over the June-September season.

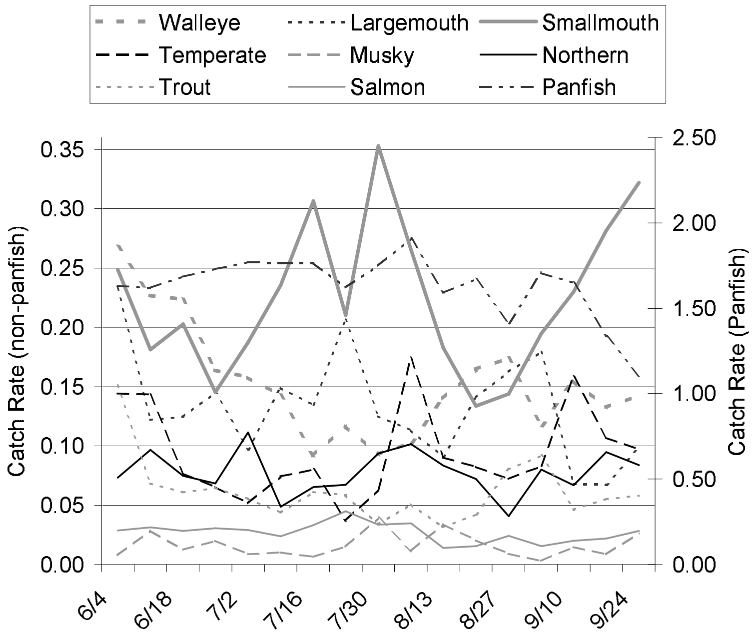

Figure 4: Variation in Catch Rate by Species Over Summer

located geographically near each other and allows incorporation of the WDNR lakes data.

The weights vary according to distance in miles from the origin site, the number of trips to that site, and the physical similarity between the origin and destination site. Within the weight itself these components must be assigned relative importance; otherwise, their importance will be completely arbitrary and based on the units of measurement and variability. The proposed weights assign equal importance to the number of trips, the distance from the origin, and physical similarity. The formula used to construct the weights is given by

$$weight = \left(\sqrt{trips}\right) * \left(\frac{1}{\sqrt{dist}}\right) * \left(\frac{1}{\sqrt{1 + dacres^{\frac{\sqrt{mddist}-1}{mdacres}}}} * \frac{1}{\sqrt{1 + ddepth^{\frac{\sqrt{mddist}-1}{mddepth}}}}\right) \tag{10}$$

where *trips* is the number of trips to the site, *dist* is the distance in miles between the sites, *dacres* is the absolute value of the difference in the surface area of the lakes, and *ddepth* is the absolute value of the difference in the maximum depth of the lakes. The terms *mdacres*, *mddepth*, and *mddist* are the absolute value of the maximum difference in the surface area, depth, and distance respectively. This formula imposes that a site twice as far from the origin should have twice as many trips to receive equal weight.

A WLS regression is estimated for each of the 996 origin sites and for each of the nine different species of fish. The dependent variable is the average catch rate for each site and the independent variables include a constant term and, in the case of inland lakes, a measure

Table 6:
Summary of Localized WLS Regressions

Species	% Regressions with pos. coef. on abundance	% Regressions with t-stat > 2 on abundance	Average R-squared
Walleye	100.0	87.8	.18
Smallmouth	94.2	34.2	.04
Musky	99.4	73.3	.19
Northern	58.9	20.6	.02
Trout	90.0	48.9	.07

of abundance from the WDNR data.[29] With only a constant term included, the WLS regression produces a simple weighted average. For inland lakes a single variable measuring fish abundance is constructed for each species by combining the dummy variables that measure whether a species is listed as abundant, common, present, or not present.[30] Using a single measure of abundance rather than a group of fish abundance dummies prevents such dummies from becoming site specific constants in those regressions where only a single site has a particular abundance rating for a particular species. Including site specific constants defeats the purpose of including other sites in the regression.

Table 6 summarizes the fit of the localized WLS regressions for the inland lakes. For those species not reported in Table 6 and non-inland lakes, a simple weighted average is used as the catch measure. Considering that there is only one included explanatory variable, the overall explanatory power of the models is acceptable as measured by the average R-squared statistic.

7.4 Additional Site Characteristics

Recreational facilities vary across fishing sites. Some sites offer a full range of services and facilities while others are in isolated locations with limited access and no facilities. Multiple data sources including the 1998 Delorme Atlas and Gazetteer, Hot Spot Fishing Maps, *County Parks of Wisconsin* (Wisconsin Trails, 1996), State Parks Visitors Guide (WDNR, 1998), Municipal Parks Listings (Parks and Recreation Departments, 1998), Wisconsin Great Lakes Public Access Guide (WDNR and Department of Tourism 1993), telephone conversations with local parks and recreation authorities, and the facilities observed by survey participants produce the measures of available facilities. These data taken together contain information on the availability of the following recreational facilities: boat launches, paved boat launches, parking lots, rest rooms, fishing piers, boat docks, picnic areas, and

[29] Possible regressors include the relative abundance measures from the WDNR inland lakes data and functions of the latitude and longitude of each site. Preliminary regressions show that including a second order polynomial function of latitude and longitude (including interaction terms) adds little to the fit of the regression and hence these terms are dropped.

[30] The WDNR does not provide any numerical values for these categories. However, a single measure is constructed that is 0 for not present, 1 for present, 2 for common, and 8 for abundant. Using the full sample of inland lake sites, this restriction is not rejected for any of the species according to an F-test comparing this specification to the unrestricted case that includes a dummy variable for each abundance category.

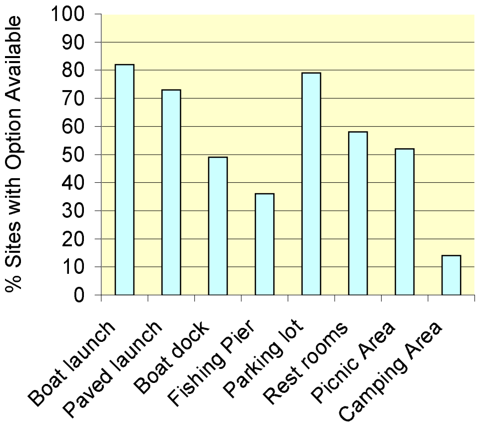

Figure 5: Recreational Facilities

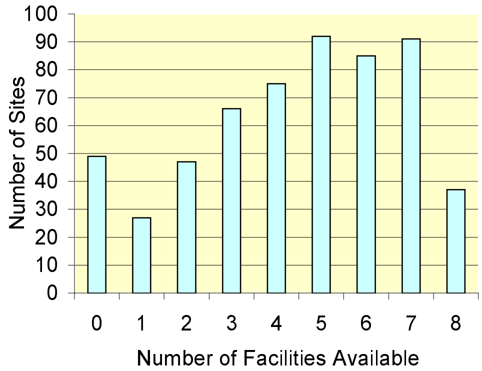

Figure 6: Distribution of Facilities

camping facilities.

While some information about the quantity of these facilities at a fishing site is available (for example, the number of boat launches), the approach taken here is to treat these facilities as options. For example, the variables measuring whether an angler visiting the site has the option to used a paved boat launch or fish from a pier. This approach produces preference parameters that are more easily interpreted than an approach that measures the number of facilities at a site. Larger and more heavily used fishing sites have a greater number of facility units, but this does not necessarily mean that these sites have "more" facilities from the perspective of a user compared to a smaller less used location with fewer facility units. Some anglers may place a small (or even negative) value on the availability of certain facilities. For example, anglers that do not own a boat and engage in shore fishing may prefer a location without boat launching facilities since boaters may interfere with the shore fishing experience. Figure 5 shows the fraction of sites with each of the facilities available and Figure 6 shows that the number of these facilities available at each site varies widely.

The continued residential and industrial development of the shorelands of Wisconsin's lakes and streams has raised concerns about the impacts increasing development has on the water bodies and the recreationists that visit them. The WDNR reports that the number of dwellings on shoreland increased 216 percent between 1965 and 1995. The WDNR also conducted an aerial survey of 235 lakes in northern Wisconsin and found that while 60 percent of these lakes were undeveloped in 1965 only 40 percent were undeveloped in 1995.[31]

While shoreland development is a complicated issue with many impacts and concerned parties, this study can consider the cost of continued shoreland development to the recreational anglers that fish these waters by including variables measuring shoreland development in the behavioral model. Ideally the model would include measures of the fraction of

[31]See the Wisconsin Lakes Partnership, WDNR, and University of Wisconsin 1999 slide presentation "Margin of Error? Human Influence on Wisconsin Shores"at http://cleanwater.uwex.edu/pubs/margin/index.htm for more information.

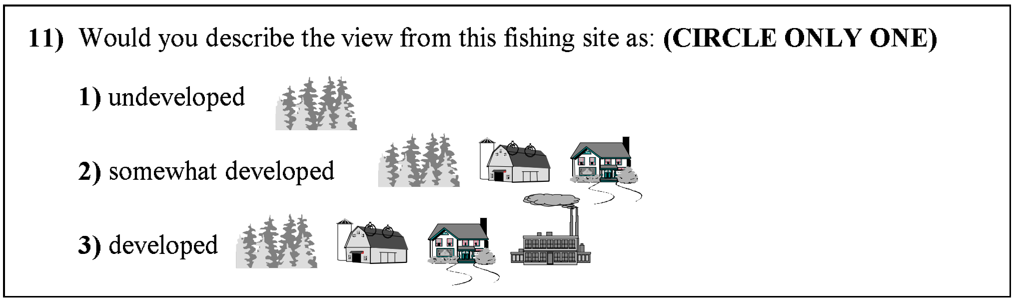

Figure 7: Survey Question on Level of Development

shoreland devoted to residential development, commercial development, industrial development, natural forest or wetlands, and the proximity of any development to the shoreline. However no comprehensive, current, and detailed data on the development of shoreland are available. Some measures of shoreland use can be constructed by using the 1998 Delorme Atlas and Gazetteer, the observations of the WFORS participants, and the designation of forest land, wildlife areas, and parks.

The Delorme Atlas and Gazetteer shows virtually all lakes and rivers in Wisconsin and clearly marks urban areas. The designation of urban areas is made by the USGS and includes areas with a population greater than 50,000 in 1998. By inspection of these maps, lakes or river segments with at least a portion of their shoreland designated as urban are identified. Of the 569 sites in the analysis sample, the shoreland of 17.9 percent includes urban areas. Shoreland at 11.9 percent of inland lakes, 25.8 percent of river sites, 44.8 percent of Lake Michigan or Green Bay sites includes urban areas.

The WFORS participants report the status of shoreland development for each fishing site visited. Figure 7 shows the question as it appeared in the fishing summary completed after each fishing day. The sites are placed into three categories: those where no one reports development, those where some anglers report residential and/or farm development, and those where some anglers report industrial development in addition to residential and/or farm development. Of the 569 sites, 15.6 percent are classified as undeveloped. Of the developed sites, approximately half have residential and/or farm development while the other half have some industrial development as well.

A considerable portion of Wisconsin's land is designated as national forest, state forest (or park), county forest, or wildlife areas. Inspection of maps allows the identification of lakes and river segments that fall within a forest or park boundary. A sizable percentage, 23.4, of the fishing sites are within these areas, with 4.8 percent in national forests, 7.6 percent in state forests or parks, 5.4 percent in county forests, and 5.6 percent in a wildlife area or refuge. Including variables measuring the land designation in the behavioral model allows policy experiments that consider the value of these lands to anglers.

The WDNR issues fishing regulations each year as part of their efforts to manage the fishery resources in Wisconsin.[32] The fishing regulation booklet describes the general fishing regulations by species and lists all the water bodies with special fishing regulations and

[32] *Guide to Wisconsin Hook and Line Fishing Regulations 1998-99* issued by the WDNR.

management policies.

The fishing regulation booklet identifies quality fishing locations, locations where motor trolling is permitted, and urban fishing waters. Indicator variables mark sites that have these special designations. WDNR identifies 10.7 percent of fishing sites as having quality fishing opportunities, where quality relates to the size of the fish inhabiting the waters. In general, motor trolling is not permitted in Wisconsin's waters.[33] However, special regulations allow motor trolling in 19.5 percent of the sites. Urban fishing waters are small lakes or ponds (less than 25 acres) that are intensively and cooperatively managed with a municipality. WDNR designates 7 of the sites (1.2 percent) as urban waters. Regulations also restrict the speed boats can travel on small lakes. Specifically motorboats may not travel faster than "slow-no-wake" on lakes 50 acres or smaller. This regulation affects 17.2 percent of the sites.

The WDNR also manages waters by regulating which fish anglers can take from specific locations. The most prevalent are minimum length limits and bag limits. To comply with minimum length limits anglers must immediately release fish shorter than a specified length. To comply with bag limits anglers must stop fishing for a species once a set number a that species are caught and kept (not released). For each species there is a state wide minimum length (for some species it is zero) and bag limit (for some species it is unlimited). For musky, walleye, northern pike, largemouth bass, and smallmouth bass there are a non-negligible number of sites with special regulations relating to the minimum length, bag limit, or both. The bag limit and minimum length regulations are quite intricate and complicated, making them difficult to quantify and include in the behavioral model, hence they are excluded. Preliminary models that included a large number of dummy variables to try to capture the regulations found that overall they did not explain much about angler choice.

The physical characteristics of the fishing sites include the type, size, and depth of each water body. The majority of the sites are inland lakes, 63.6 percent, a considerable fraction are river sites, 31.3 percent, and the remaining are on Lake Michigan or Green Bay. The WDNR collects considerably more data on inland lakes compared with rivers and streams. The lack of data for rivers and streams can be partly attributed to their more rapidly changing nature across seasons and within seasons depending on rainfall. The WDNR records the size of inland lakes, which varies considerably ranging from a 1 acre pond to Lake Winnebago with 137,708 acres of surface area. The distribution is extremely skewed with many very small and moderately small lakes and a few very large lakes. The surface area is included in the behavioral model with a log transformation to allow the difference between a 1 acre and a 100 acre lake to be greater than a 50,001 acre and 50,100 acre lake.

8 Results

Both the proposed approach that accounts for unobserved characteristics and the traditional approach are estimated allowing for varying levels of taste heterogeneity among anglers. Across the alternative specifications of taste heterogeneity, controlling for unobserved characteristics is found to substantially affect welfare estimates from some illustrative policy

[33] Motor trolling involves trailing a lure or bait from a moving vessel (motor boat or sail boat).

experiments.

The first comparison is similar to that considered in the Monte Carlo simulation, but with the actual data replacing the simulated data. This simple specification includes no interaction terms between individual and site characteristics and allows for taste heterogeneity only through the extreme value distributed error. A likelihood ratio test clearly rejects the hypothesis that all site characteristics are observed.[34] Results show that the simple conditional logit model produces both parameter and welfare estimates that differ substantially from the corresponding model that allows for unobserved characteristics. The first two columns of Table 9 in the Appendix show the parameter estimates.

Comparing the parameter ratio for site characteristics to travel distance between these two models, indicates that they give quite different levels of importance to a number of characteristics. Recall that this ratio measures the marginal utility of an additional unit of the characteristic at a visited site. For example, in the two-stage model the ratio of the walleye catch parameter to travel distance is 0.24 ($= \frac{2.57}{10.75}$). Since distance is measured in hundreds of one-way miles, this means that an angler would be willing to travel 24 additional one-way miles for a one unit increase in the walleye catch rate. If this seems high, remember that the average walleye catch rate is only 0.13 fish per hour (with a maximum at any site of 0.85 fish per hour), which makes a one unit increase substantial.[35] Because the parameter on travel distance does not differ too much between the models (-9.03 versus -10.75) a rough idea of the differences between these two models can be gathered by simply comparing the parameter estimates on the characteristics provided in Table 9. Doing a more careful comparison based on the parameter ratio, we see that compared to the standard model the two-stage approach finds that this ratio is 47 percent larger for the walleye catch rate, 2.6 times as big for musky, 1.9 times as big for management for quality, and 4.4 times more negative for urban shoreland development.

In all cases the standard errors are smaller for the traditional model than the two-stage approach. However, recall that the Monte Carlo simulations indicate that the traditional model produces estimates that are actually more variable (less reliable) than the two-stage approach and that the standard errors are biased downward when unobserved characteristics are present. The results of the two-stage approach support the qualitative arguments made earlier and indicate that unobserved site characteristics are in fact likely to be a substantial factor in these data. The second stage regression of the estimated δ's on the site characteristics has an R-squared of 0.50, which indicates that about 50 percent of the variation can be explained by observed characteristics and 50 percent by unobserved characteristics.[36] Together the results from the simulations and the finding that unobserved characteristics are a real concern in the actual data argue for the parameter estimates produced by the

[34] The log-likelihood of the traditional model is -12,882 compared to -11,198 for the model including a full set of alternative specific constants, which produces a chi-squared test statistic of 3,368 compared to the critical value of 623 for 543 degrees of freedom and significance level 0.01.

[35] It is important to consider the ratio rather than the absolute magnitude of the parameters because we have constrained the scale of the error to be constant (equal to one) across model specifications. This means that the relative importance of the error is determined by the scale of a estimated parameters.

[36] Attributing the remaining 50 percent to unobserved characteristics assumes that there is no measurement error in the estimated δ's. Noise in the δ's would also be included in the error of this second stage regression along with the unobserved site characteristics.

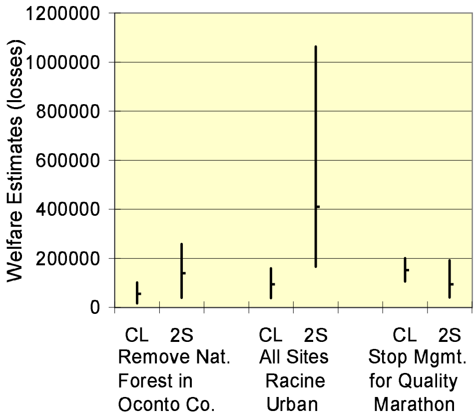

Figure 8: Comparison of Welfare Estimates and 90% Confidence Intervals for Three Policy Experiments

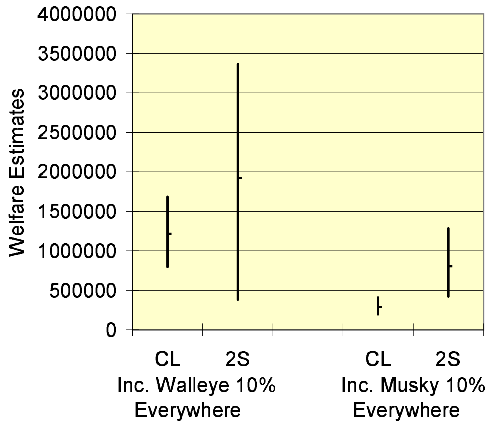

Figure 9: Comparison of Welfare Estimates and 90% Confidence Intervals for Two Policy Experiments

two-stage approach over those of the standard model.

To compare the models based on their welfare predictions consider a set of illustrative policy experiments. These include two experiments that affect land-use: (1) remove the national forest designation from those thirteen fishing locations in Oconto County that are currently within forest boundaries and (2) allow additional development at the five fishing locations in Racine County that currently do not have urban areas on their shoreland. Another experiment stops WDNR management programs for quality fishing at the three locations in Marathon County that currently have this program. The remaining two policy experiments affect a broader geographic area and involve increasing the catch rate for walleye and musky by 10 percent at all locations.

Figures 8 and 9 show the welfare estimates and their 90 percent confidence intervals for both the standard conditional logit model (CL) and the proposed two-stage approach (2S) measured in one-way miles of travel distance for these illustrative policy experiments.[37] The figures show that for the Racine county project and the musky improvement project, the confidence intervals do not overlap, indicating a statistically significant difference. However, much more important that the statistical difference, is the real difference. The two-stage approach obtains an estimate of welfare loss that is 4.3 times larger for the Racine project and an estimate of welfare gain that is 2.8 times larger for the musky project. The estimates from other projects, while not statistically different, are economically different and the

[37]The confidence intervals for the conditional logit model are estimated by taking 1000 draws from the distribution of the parameter vector (as determined by the variance-covariance matrix) and calculating the welfare estimate for each draw. The 5^{th} and 95^{th} percentiles form the lower and upper confidence bounds. The calculation of the confidence intervals for the two-stage model requires accounting for both the error in estimating the alternative specific constants (first stage) and the error in estimating the parameters on the site characteristics (second stage). This is achieved by drawing 50 vectors of alternative specific constants from their distribution (as determined by the variance-covariance matrix) and for each calculating the associated parameters on the site characteristics (OLS). Then for each vector of alternative specific constants, 50 vectors of site characteristic parameters are drawn, which produces a total of 2500 welfare estimates. The 5^{th} and 95^{th} percentiles form the lower and upper confidence bounds.

differences are large enough to potentially influence policy decision. Further, in all cases the confidence bounds associated with the standard model do not include the point estimate produced by the two-stage approach. This indicates that the traditional approach, because of its less reliable parameter estimates and understated standard errors, may be leading us to reject the true welfare estimates, if the proposed two-stage model is closer to the true model.

Estimating the model with travel cost (measured in dollars) does not change the relative importance of the characteristics in a noteworthy manner, but rescales them. A quick conversion of the welfare estimates from miles to dollars is possible by simply reducing the estimates by 16 percent.

In addition to concern about the reliability of the parameter estimates when unobserved characteristics are ignored, is the issue of bias in the welfare estimates. It is difficult to precisely separate the effect of excluding characteristics from the welfare calculation from the effect of unreliable parameter estimates. The non-linearity of the CV formula implies that the marginal impact of a change in the utility index depends on the level of the utility index at a site. This makes the relative importance of the unreliable parameter estimates and the unobserved characteristics in the welfare calculation dependent upon the particular sites and the particular modification chosen for the policy experiment. However, we can calculate welfare for the two-stage model both when the δ terms are included and excluded to get an idea as to how much the welfare estimates are influenced by the correction for unobserved characteristics. This comparison shows that both the variability in the parameter estimates and the inclusion of the unobserved characteristics in the welfare estimates are important in determining the magnitude of the welfare estimates. Policy experiments that involve a smaller number of sites are more likely to be dramatically affected.

Next consider some more realistic models that allow for taste heterogeneity among anglers to see if the large differences we have found between models that control for unobserved characteristics versus those that do not are being driven by the restriction of homogeneous tastes. Taste heterogeneity can be incorporated by including interaction terms between individual characteristics and site characteristics and/or by allowing preferences to be a random draw from a distribution of tastes.

To implement a model that allows tastes for site characteristics to vary across individuals according to their observed characteristics requires specifying which observable individual characteristics interact with which site characteristics. Not having strong prior information about how tastes may vary, I select four individual characteristics likely to be related to fishing preferences: gender, age, income, and boat ownership.[38] The first three are interacted with measures of catch rate, land use, and fishery management policies. It is easy to think of reasons why preferences for the characteristics may vary across these demographic groups. For example, individuals with high income may be more likely go after game fish that require more expensive gear and bait. Boat ownership is interacted with those characteristics likely linked with the ownership of a boat, including lake size, management policies, and boating related facilities. Individuals with boats may be more likely to head out on

[38] Additional demographic variables such as number of children in the household and quadratic and log terms were tried. The included individual characteristics seemed the most important. To keep the number of estimated parameters reasonable, only a selection can be included.

larger lakes where most of the waters are accessible only by boat and speed limits are higher and to choose locations with boating facilities.

When using travel distance as the price metric, it makes sense to think about heterogeneity in preferences related to it. Here, theory provides some guidance. Individuals with higher levels of opportunity cost should be more sensitive to longer travel distances that imply a longer travel time. To capture this, variables related to the opportunity cost of time are interacted with travel distance. This includes dummies measuring whether an individual is unemployed or employed part-time and the individual's wage rate or predicted wage from the hedonic wage equation if the individual is not employed, which is simply denoted "opportunity cost" in the Tables providing the estimation results. Another interaction uses a dummy variable measuring whether the individual's primary residence is located on water-front property. Including this term makes sense if there is some fixed cost of driving to a location. Hence the difference between a site 0 miles away and 1 mile away is not the same as the difference between a site 1 mile away and 2 miles away. Possible fixed costs could include hooking up the boat trailer to the the car and loading and unloading gear (and people) from a vehicle.[39]

The parameter estimates for a simple interaction model and the two-stage interaction model are provided in the third and fourth columns of Table 9 in the Appendix. For the most part, the parameter estimates themselves are somewhat difficult to interpret and compare across models. Many of the interactions about which we have a prior, do have the expected sign. Anglers with boats prefer larger lakes, dislike particularly small lakes (less than 50 acres) where boating regulation are particularly strict, and prefer locations where a paved boat launch is available. Anglers with more experience (proxied for by age) prefer sites managed for big quality fish as do anglers with more income (who have the means to buy the expensive gear and bait required for pulling in the big ones). Anglers that are not employed or work part-time are less adverse to longer travel distances (presumably have a lower opportunity cost of time) and anglers whose primary residence is on water-front property are substantially more adverse to traveling. The sign on the opportunity cost of time as measured by the wage is the opposite of what we would expect. Conditional on being employed full time, individuals with a higher opportunity cost of time are less adverse to traveling additional distance. While this result is puzzling, it does make the use travel distance more attractive than using the constructed travel cost. When using travel cost, we assume that the cost is higher for individuals with a higher opportunity cost of time, which appears to be a questionable assumption (at least, given our measure of opportunity cost).

It is especially useful to make comparisons based on estimates of welfare for the models including interaction terms where it is more difficult to scan the parameter estimates or ratios of these estimates to determine overall effects. The set of illustrative policy experiments includes those considered above along with some additional experiments, including smaller scale improvements of walleye and musky catch rates in Shawano and Oneida counties respectively and changes at single locations including allowing urbanization of the shoreland of Tichigan Lake in Racine county and removing the county forest designation at Andersen Lake in Oconto county. The specific group of policy experiments considered is selected to

[39]Another possible explanation is that individuals who have selected the option of living on water-front property are more likely to be adverse to traveling to other locations for fishing.

Table 7:
Illustrative Policy Experiments and
Estimates of Welfare Gains or Losses Across Model Specifications

Policy Experiment[1]	CV Estimate[2] Trad. Homog.	2S Homog.	Trad. Hetero.	2S Hetero.	Trad. RPL	2S RPL
Inc. Walleye Catch by 10% Everywhere (524)	1,214.9	1,924.0	1,242.4	1,592.2	1,107.6	1,932.4
Inc. Musky Catch by 10% Everywhere (452)	286.0	805.6	492.5	855.4	736.8	557.1
Inc. Walleye Catch 10% at All Shawano Co. Sites (13)	9.2	12.7	7.0	9.4	-1.7	2.3
Inc. Musky Catch 10% at All Oneida Co. Sites (28)	28.5	88.3	39.9	85.1	-2.1	-11.2
Remove County Forest Desig. in Oconto Co. (9)	-219.7	-215.8	-202.2	-200.7	-12.6	-143.3
Remove County Forest Desig. Andersen Lake Oconto Co. (1)	-20.3	-30.3	-17.7	-24.7	-2.0	-16.7
Make All Sites in Racine Co. Urban (5)	-94.4	-409.8	-105.6	-413.9	-28.6	-356.4
Make Tichigan Lake in Racine Co. Urban (1)	-36.7	-67.2	-19.5	-61.7	6.5	-57.9
Remove Mgmt. for Quality in Marathon Co. (3)	-151.2	-94.6	-78.9	-86.9	-99.4	-109.1

[1] Number of sites affected by the policy experiment in parentheses.
[2] CV estimates given in terms of total one-way miles in thousands of miles (1,000).

Table 8:
Distribution of Gains Among Anglers

Policy Experiment	Taste Hetero-geniety?	% of Total Gains/Losses to Group: Male (71%)[1]	Age > 42 (54%)	Income $> 35,000$ (34%)	Own Boat (60%)	Water-Front (12%)
Inc. Walleye Catch by 10%	No	69.8	53.3	27.6	64.9	13.8
Everywhere	Yes	93.5	71.8	27.9	74.4	10.1
Inc. Musky Catch by 10%	No	67.9	52.0	36.6	62.6	15.0
Everywhere	Yes	35.2	45.5	28.8	57.1	7.1
Remove Mgmt. for Quality in	No	57.7	27.6	29.9	67.3	5.6
Marathon Co.	Yes	72.1	36.0	47.1	77.8	2.0

[1] Weighted percent of population in category given in parentheses.

show both a range of types of experiments including changing catch rates, land use, and management policies over a range of different locations and scopes.

The first and second columns of Table 7 compare the welfare estimates between the simple and two-stage model when no interaction terms are included and the third and fourth columns compare the models when the interaction are included. Aside from the simplest models that allow for taste heterogeneity only through the additive extreme value error, for which a graphical comparison of the traditional and two-stage approach is provided in Figures 8 and 9, the confidence intervals about the welfare estimates are quite large. Allowing for substantially increased taste heterogeneity, either through interaction terms or random taste parameters, substantially increases the overall model uncertainty and hence the confidence bounds about the welfare estimates. However, this does not necessarily make a comparison of the point estimates uninteresting or uninformative.

In many cases, although not all, allowing for heterogeneous tastes through interaction terms does not substantially alter the welfare estimates, which can be seen by comparing the first and third and the second and forth columns. For most of the policy experiments, large differences are obtained when comparing the welfare estimates of the two stage approach with the traditional approach that ignores the unobserved characteristics (compare column 1 with 2 and column 3 with 4). While only the larger differences observed in columns 1 and 2 are statistically significant, the magnitude of many of the differences cannot be dismissed. In particular, comparing the interaction models that allow for considerable taste heterogeneity, we see that the two-stage approach produces estimates of welfare gains that are 74 percent larger for the state-wide musky improvement program and 2.1 times larger for the Oneida county musky program and estimates of welfare losses that are 3.9 times as large for the urbanization of Racine county lakes experiment and 3.2 times as large for the Tichigan Lake urbanization.

The inclusion of interaction terms allows us to consider the distribution of welfare gains or losses among different demographic groups. Table 8 shows how the gains from three of the policies considered above are distributed among some of the important demographic groups. It shows both the results for the two-stage model estimated with and without the interaction terms to show how the distributional implications change when interaction terms are included. For example, these results show that when we include interaction terms the gains from increasing the walleye catch are disproportionately allocated to older males that own boats. For all of the policy experiments shown, individuals whose primary residence is on waterfront property gather less than their share of the gains. This result makes sense because these individuals have the opportunity to visit a location with a travel distance of zero and a strong fixed-cost of driving is found. Hence, improvements on water bodies other than the one on which the individual is located have less impact.

The random parameters logit (RPL) allows for additional taste heterogeneity, and particularly for tastes that vary in the population according to unobserved individual characteristics. The question arises as to which taste parameters should be allowed additional heterogeneity beyond the interaction terms. This decision is guided by available computer resources, researcher priors, and reasonable expectations as to the degree of model complexity and generality the data can support. Each additional parameter allowed to vary increases the dimension of integration by one. As the number of dimensions grows, the number of simulation draws used to approximate the integral should also increase, which increases computational costs.[40] There is likely to be a considerable amount of taste variation over the species-specific catch rates that is difficult to link to observable individual characteristics. Fish species differ in their ability to fight, their size, their taste, their abundance, and the methods by which they are caught, which makes different species appeal to different anglers. The taste for fish species are allowed to vary randomly across anglers.[41]

Table 10 in the Appendix provides the parameter estimates obtained for an RPL model when conventional interaction terms are either excluded (RPL (1)) or included (RPL (2)) and the corresponding two-stage RPL model (2S RPL). The estimated standard deviations of tastes are both economically and statistically significant for all nine species of fish indicating a substantial degree of unobserved taste heterogeneity. Interestingly, the standard deviation estimates for nearly all the species do not change much if the observed interaction terms between individual and site characteristics are included. Also, the magnitude of the standard deviation is large relative to the interaction terms (see Table 10 and recall the age is measured in hundreds of years and income in hundreds of thousands of dollars). This indicates that much of the taste variation for fish species is of the unobserved variety.

The RPL model estimates taste distributions for the species-specific catch rates that are generally characterized by a negative mean taste and a large standard deviation. This is most apparent if you look at the first column of Table 10, but is also true for the RPL model that includes interaction terms and the two-stage interaction RPL. Such taste distributions obviously have strange implications about behavior (many anglers are willing to travel substantial distances to avoid certain fish), but also can lead to some more subtle but equally strange implications about welfare. The first four policy experiments that involve improving walleye and musky catch rates, illustrate the concern. The tastes for walleye

[40] These costs are considerable even with a dual processor Pentium 4 machine, which I used for estimation.

[41] Alternative specifications were also explored and are included in the discussion below of the sometimes odd implications of the RPL model when used with data having a spatial (geographic) component.

and musky catch are normally distributed and both have a negative mean and a large standard deviation. This, however, does not mean that the welfare effect from an increase in the catch rate is negative, as is clearly illustrated by the positive gains for the first two policy experiments in Table 7 that increase the walleye and musky catch rates.[42] However, the results from the third and fourth policy experiments have the perverse negative sign. The taste distribution for walleye and musky implies that on average individuals living near the group of improved sites will suffer *losses* while individuals living further away will obtain the gains. This can be understood by remembering that travel distance has a considerable negative impact on utility, which means that individuals tend to have relatively high probabilities of choosing close-by sites and low probabilities of choosing faraway sites. On average people living further away can benefit because of this group the only people who had a reasonable probability of ever going to the affected locations are those with a strong positive taste for walleye or musky. The net result in the first two policy scenarios is that the winners outweigh the losers, while the opposite is true for the third and fourth scenarios.[43]

This peculiar result is not limited to the case where the random parameters are on the catch variables. Other specifications explore possible random parameters on the land-use and management variables and find similar distributions. For example, a model with a random parameter on variable measuring the presence of a national forest has a negative mean and large standard deviation. While the welfare estimate from preserving the national forest is positive, those living near the affected forests suffer losses while those living further away enjoy the gains.[44]

Additional research on the distributional assumptions associated with the random parameters models is needed before they can be reliably applied to spatial data such as recreation demand data. If preferences have multiple peaks or are distributed non-symmetrically then the normal distribution or variants of it will be poorly suited for the job. Another possibility is that there may be mass points within the distribution, such as at zero if many individuals do not care either way about a particular characteristic. The results found in this study indicate that random parameters models must be used with caution in recreation demand analyses. While some of the overall welfare estimates produced by this model seem plausible and are comparable with the other models, they all have implausible implications about the distribution of the welfare gains or losses among anglers.

Given the unusual implications of the random parameters model, the preferred specification is one that includes interactions with observable individual characteristics. The interaction model allows for taste heterogeneity, produces plausible welfare estimates, and requires modest time and computer resources for estimation. For this model (and the

[42]In fact, for all the species a similar policy experiment produces welfare estimates that have the same sign as the non-RPL specifications despite the negative mean taste.

[43]The sign of the welfare estimates depends on the distribution of anglers relative to the affected sites. If enough anglers are living nearby, their losses will outweigh the gains of the more distant anglers.

[44]For variables where the expected taste is negative the perverse results go in the opposite direction. For example, a model estimated with a random taste parameter on urban development finds the expected negative mean taste but this is also accompanied by a large standard deviation (over three times the size of the mean estimate). This means that on average individuals living further away benefit from the urbanization of these more distant water bodies.

others) accounting for unobserved characteristics substantially affects model implications, which shows that controlling for unobserved characteristics produces important differences in the bottom line estimates of welfare that drive policy decisions.

9 Conclusion and Extensions

This paper addresses the reality that researchers cannot reasonably expect to observe all of the characteristics that affect anglers' choices among fishing and recreation locations. A new model is introduced to the recreation demand literature that allows researchers to confront this reality while still recovering all of the structural parameters needed for meaningful policy experiments.

Both the results from Monte Carlo simulations and comparisons of welfare estimates among traditional models and the proposed approach indicate that controlling for unobserved characteristics substantially affects the results of policy analyses. Simulations show that ignoring unobserved site characteristics causes the variance of parameter estimates to grow, relative to the proposed approach, and the standard errors to be biased downwards to dramatically overstate precision. These results alone suggest that researchers using discrete choice modeling techniques should be wary of results obtained while not controlling for unobserved characteristics.

In addition, ignoring unobserved characteristics causes welfare estimates to be biased for policies affecting individual locations or groups of fishing locations, because the baseline level of utility for a site used in the welfare calculations will be incorrect and the formula for calculating the compensating variation is quite non-linear. This causes the welfare estimates from changes in observed characteristics to be biased downward for locations that have desirable unobserved characteristics and to be biased upward for locations with undesirable unobserved characteristics. The proposed model provides the researcher with estimates of the unobserved characteristics, which enables unbiased welfare estimates to be obtained. This bias should be a particularly serious concern in applications where the policy experiments involve a single location or a small group of locations.

An illustrative group of policy experiments including changes in land use, management policies, and fish stocking programs shows that the welfare estimates obtained using the proposed approach that controls for unobserved characteristics can be four times as large as those obtained when unobserved characteristics are ignored. The magnitude and direction of these differences depends on a number of factors including the importance of unobserved characteristics at the locations affected by the policy experiment and the difference in the estimated taste parameters for the modified site characteristics. The substantial differences found for the modest group of illustrative policy experiments used in this study indicates that the methodological concerns outlined above are likely to translate into important differences in the welfare estimates that drive policy decisions and determine liability in NRDA cases.

The empirical results have broad implications because the data available for this study contains more detailed and comprehensive information on the characteristics of fishing locations than is typically available for recreation demand studies. Despite this, unobserved characteristics are found to be quite important, explaining up to 50 percent of the differences among sites, and substantially influence policy implications. This makes it quite likely that controlling for unobserved characteristics will be important for a wide range applications. These include applications of discrete choice models to explain individual choice in other fields, where it is also true that researchers will not observe or cannot measure a substantial fraction of the characteristics of the alternatives that enter an individual's decision process.

In extensions to this research, I am exploring the potential link between successive decisions made by an individual in the discrete choice setting. Nearly all existing models assume that an angler makes each choice in a sequence of choices independently. However, it is not uncommon for recreation demand data to include multiple observations of the same individual. In the WFORS data used in this study, the fishing behavior of anglers is observed for 122 days corresponding to the summer months and the average angler spends 8 days fishing and takes 5 single day fishing trips.

In part because anglers face a wide range of fishing alternatives, a fully dynamic model would require very restrictive assumptions to implement. In this extension an alternative model is proposed that links an anglers current decision to their previous trip. The actual catch on each fishing outing serves as the link. The model allows for the possibility that anglers may engage in "variety seeking" behavior similar to that found in the marketing literature (Erdem (1996)). Fishing success for a particular species may lead them to seek something else on their next outing and hence have a diminished taste for that same species. To address the well-known problems of distinguishing true from spurious state dependence (Heckman (1981)), the model allows for substantial taste heterogeneity across anglers, which controls for the fact that anglers may have unobserved tastes that, if unaccounted for, may induce positive correlation among their choices. Variation key for identification is obtained from daily changes in the observed weather for each location and the random shock associated with the actual catch rate. This model allows us to gauge the relative importance of previous experience on current behavior and to determine whether relaxing the assumption of independent sequential decisions substantially alters the model's behavioral predictions.

10 Appendix: Results

Table 9:

Models With Heterogeneous Tastes Only Through Interaction Terms

Variables	CL Simple Est. (se)	2S Simple Est. (se)	CL Interact Est. (se)	2S Interact Est. (se)
Panfish	0.61 (0.05)	1.11 (0.18)	0.28 (0.22)	0.55 (0.17)
X male			0.40 (0.13)	0.52 (0.18)
X age			0.75 (0.36)	0.59 (0.58)
X income			-0.82 (0.32)	-0.49 (0.47)
Walleye	1.47 (0.16)	2.57 (0.70)	-0.97 (0.56)	-0.41 (0.65)
X male			1.63 (0.32)	1.98 (0.44)
X age			2.92 (0.91)	2.72 (1.28)
X income			-0.51 (0.77)	-0.89 (1.02)
Largemouth	-2.24 (0.32)	-7.37 (0.90)	-2.55 (1.27)	-5.70 (0.85)
X male			0.26 (0.72)	0.37 (1.07)
X age			-7.33 (2.42)	-9.61 (3.86)
X income			9.77 (1.71)	8.88 (2.54)
Smallmouth	1.15 (0.13)	2.21 (0.49)	-0.73 (0.57)	0.73 (0.46)
X male			0.82 (0.31)	0.96 (0.46)
X age			1.16 (0.91)	0.21 (1.25)
X income			1.73 (0.69)	1.27 (0.92)
Temperate Bass	0.91 (0.34)	-0.48 (1.37)	-1.14 (1.53)	-4.22 (1.29)
X male			0.72 (0.86)	1.44 (1.10)
X age			-0.82 (2.54)	1.55 (3.83)
X income			4.55 (1.89)	4.71 (2.47)
Musky	5.98 (0.92)	18.41 (4.18)	22.27 (3.73)	32.41 (3.88)
X male			-12.84 (2.32)	-14.98 (4.00)
X age			-18.22 (7.22)	-13.80 (11.45)
X income			7.89 (6.33)	3.63 (10.45)
Northern Pike	2.60 (0.47)	1.78 (1.50)	5.32 (2.47)	2.49 (1.42)
X male			-1.98 (1.27)	0.55 (1.85)
X age			2.01 (4.08)	2.42 (6.03)
X income			-8.22 (2.73)	-8.69 (4.20)
Trout	2.34 (0.23)	3.71 (0.65)	-2.35 (1.18)	-0.72 (0.60)
X male			1.94 (0.63)	1.95 (1.10)
X age			4.50 (1.62)	2.29 (3.74)
X income			3.12 (1.49)	5.23 (2.60)
Salmon	6.63 (0.41)	5.86 (2.22)	9.20 (1.88)	5.21 (2.06)
X male			3.57 (1.05)	6.06 (1.46)
X age			-12.13 (2.94)	-7.62 (4.29)
X income			-2.12 (2.35)	-5.43 (3.25)
Acres < 50	-0.25 (0.09)	0.11 (0.27)	-0.16 (0.12)	0.15 (0.25)
X boat			-0.31 (0.15)	-0.20 (0.31)
Ln(Acres)	0.10 (0.01)	0.26 (0.05)	0.05 (0.01)	0.21 (0.05)
X boat			0.06 (0.01)	0.08 (0.02)
Motor Trolling	-0.12 (0.06)	-0.56 (0.26)	-0.07 (0.09)	-0.54 (0.24)
X boat			0.08 (0.09)	-0.02 (0.13)

Table 9: *continued*

Variables	CL Simple Est. (se)	2S Simple Est. (se)	CL Interact Est. (se)	2S Interact Est. (se)
Boat Launch	0.67 (0.09)	0.86 (0.30)	0.70 (0.13)	0.85 (0.28)
X boat			0.16 (0.19)	0.02 (0.55)
Paved Launch	-0.04 (0.06)	0.47 (0.26)	-0.28 (0.10)	0.14 (0.25)
X boat			0.44 (0.15)	0.62 (0.33)
Fishing Pier	0.03 (0.05)	-0.05 (0.19)	0.22 (0.07)	0.04 (0.16)
X boat			-0.14 (0.08)	-0.01 (0.13)
Quality Mgmt.	0.37 (0.07)	0.84 (0.27)	0.95 (0.31)	0.10 (0.26)
X male			-0.10 (0.17)	-0.32 (0.27)
X age			1.16 (0.62)	0.58 (0.89)
X income			2.18 (0.32)	1.67 (0.56)
National Forest	0.38 (0.15)	0.94 (0.38)	-2.33 (0.66)	-0.44 (0.36)
X male			0.89 (0.43)	0.12 (0.75)
X age			3.80 (1.15)	2.87 (2.11)
X income			0.51 (1.13)	-0.52 (2.23)
State Forest	-0.14 (0.07)	0.03 (0.29)	0.64 (0.33)	1.09 (0.28)
X male			-0.51 (0.19)	-0.59 (0.26)
X age			-0.52 (0.52)	-0.49 (0.72)
X income			-0.05 (0.54)	-1.01 (0.72)
County Forest	0.69 (0.10)	1.03 (0.34)	-0.20 (0.44)	0.47 (0.32)
X male			-0.18 (0.24)	-0.47 (0.36)
X age			0.18 (0.78)	0.30 (1.26)
X income			2.99 (0.76)	2.18 (1.10)
Urban Area	-0.14 (0.05)	-0.74 (0.23)	-0.03 (0.22)	-0.39 (0.21)
X male			-0.30 (0.12)	-0.30 (0.16)
X age			-0.80 (0.38)	-1.27 (0.49)
X income			1.07 (0.29)	1.13 (0.38)
Residential Dev.	0.06 (0.08)	-0.39 (0.23)	0.68 (0.37)	0.17 (0.22)
X male			0.88 (0.20)	0.63 (0.58)
X age			-1.43 (0.65)	-1.59 (2.03)
X income			-1.71 (0.52)	-0.89 (1.30)
Industrial Dev.	0.28 (0.05)	-0.19 (0.19)	0.44 (0.21)	-0.79 (0.18)
X male			-0.39 (0.12)	-0.47 (0.20)
X age			1.31 (0.36)	1.14 (0.59)
X income			1.31 (0.30)	1.39 (0.48)
Miles (one-way)	-9.03 (0.09)	-10.75 (0.16)	-9.28 (0.29)	-11.24 (0.41)
X lake-front			-10.66 (0.44)	-10.43 (0.58)
X opp. cost			4.88 (2.06)	7.79 (2.94)
X not employ			1.31 (0.25)	1.10 (0.41)
X part-time			0.67 (0.34)	0.79 (0.59)

Note: Age is measured in hundreds of years, opportunity cost in hundreds of dollars, income in hundreds of thousands of dollars, and miles in hundreds of one-way miles.

Table 10:

Models With Heterogeneous Tastes Through Interaction Terms and Random Parameters

Variables	RPL (1) Est. (se)	RPL (2) Est. (se)	2S RPL Est. (se)
Panfish	-0.07 (0.12)	0.62 (0.53)	0.88 (0.26)
sd	1.83 (0.10)	2.05 (0.10)	1.81 (0.11)
X male		0.33 (0.32)	0.37 (0.25)
X age		-2.23 (0.77)	-1.92 (0.50)
X income		0.05 (0.88)	-0.96 (0.63)
Walleye	-1.86 (0.49)	-1.93 (1.40)	0.88 (1.02)
sd	5.10 (0.36)	4.48 (0.29)	5.15 (0.39)
X male		2.68 (0.70)	4.09 (0.87)
X age		0.34 (2.18)	-3.74 (1.27)
X income		-2.01 (2.05)	-5.23 (1.67)
Largemouth	-3.46 (0.70)	-1.16 (2.55)	-10.45 (1.32)
sd	8.34 (0.66)	7.98 (0.63)	9.18 (0.59)
X male		-0.34 (1.69)	0.04 (1.57)
X age		-12.25 (5.10)	-7.61 (3.49)
X income		10.51 (3.34)	11.93 (3.55)
Smallmouth	-0.88 (0.38)	-1.96 (1.42)	-7.52 (0.71)
sd	4.33 (0.37)	4.23 (0.40)	6.03 (0.36)
X male		0.84 (0.82)	1.69 (0.93)
X age		1.03 (2.67)	4.32 (1.87)
X income		1.43 (1.55)	-0.10 (1.72)
Temperate Bass	-3.36 (0.92)	-9.07 (2.97)	-11.89 (2.00)
sd	10.59 (0.87)	10.65 (0.68)	13.59 (0.85)
X male		-2.02 (1.97)	1.00 (1.84)
X age		-0.08 (4.69)	2.23 (3.87)
X income		16.34 (4.74)	7.14 (4.47)
Musky	-5.96 (3.02)	-10.93 (8.31)	-12.01 (6.03)
sd	21.85 (2.83)	21.11 (2.16)	32.45 (3.00)
X male		-8.06 (5.02)	-14.28 (5.72)
X age		17.30 (15.34)	25.90 (12.53)
X income		16.39 (11.52)	5.55 (13.29)
Northern Pike	-7.45 (1.25)	-1.05 (4.97)	-6.02 (2.02)
sd	16.85 (1.08)	16.98 (1.13)	26.17 (1.19)
X male		2.10 (2.80)	2.48 (3.32)
X age		-3.89 (8.97)	-13.88 (5.88)
X income		-16.37 (7.38)	-24.86 (7.59)
Trout	-5.80 (0.82)	-8.38 (4.06)	-11.33 (0.94)
sd	9.88 (0.40)	13.03 (0.71)	12.89 (0.54)
X male		3.38 (2.26)	4.27 (1.88)
X age		-3.13 (6.71)	-2.40 (3.81)
X income		-1.69 (5.53)	-3.26 (4.13)
Salmon	-10.13 (2.08)	-9.09 (5.97)	-11.20 (3.20)
sd	15.75 (1.35)	16.97 (1.10)	20.01 (0.82)
X male		3.03 (2.60)	6.25 (2.29)
X age		-11.67 (10.39)	-20.97 (4.68)

Table 10: *continued*

Variables	RPL (1) Est. (se)	RPL (2) Est. (se)	2S RPL Est. (se)
X income		7.16 (5.24)	-1.18 (4.53)
Acres < 50	-0.01 (0.09)	0.19 (0.11)	-0.37 (0.39)
X boat		-0.68 (0.14)	-0.26 (0.11)
Ln(Acres)	0.19 (0.02)	0.11 (0.02)	0.08 (0.07)
X boat		0.09 (0.03)	0.21 (0.03)
Motor Trolling	0.10 (0.06)	0.01 (0.08)	-0.55 (0.38)
X boat		-0.01 (0.11)	-0.15 (0.10)
Boat Launch	0.44 (0.07)	0.60 (0.11)	1.05 (0.44)
X boat		0.09 (0.18)	0.02 (0.16)
Paved Launch	-0.14 (0.06)	-0.22 (0.08)	0.04 (0.38)
X boat		0.40 (0.13)	0.49 (0.10)
Fishing Pier	0.21 (0.04)	0.32 (0.05)	0.11 (0.25)
X boat		-0.17 (0.07)	-0.32 (0.06)
Quality Mgmt.	0.36 (0.06)	-0.98 (0.32)	-0.20 (0.40)
X male		-0.17 (0.18)	0.03 (0.19)
X age		1.31 (0.71)	2.28 (0.40)
X income		2.14 (0.27)	1.19 (0.35)
National Forest	0.55 (0.15)	-0.80 (0.59)	-2.00 (0.56)
X male		0.40 (0.43)	0.35 (0.40)
X age		2.83 (1.12)	4.42 (0.65)
X income		-2.82 (0.98)	-2.12 (1.02)
State Forest	0.16 (0.04)	1.20 (0.27)	0.86 (0.43)
X male		-0.30 (0.18)	-0.60 (0.20)
X age		-1.91 (0.41)	-0.98 (0.33)
X income		0.06 (0.40)	-0.12 (0.41)
County Forest	-0.12 (0.10)	-0.64 (0.35)	-0.25 (0.50)
X male		-0.66 (0.21)	-0.65 (0.22)
X age		1.26 (0.65)	2.12 (0.43)
X income		1.89 (0.65)	1.44 (0.64)
Urban Area	-0.10 (0.04)	0.27 (0.18)	-0.19 (0.33)
X male		-0.24 (0.11)	-0.23 (0.12)
X age		-1.48 (0.35)	-1.72 (0.23)
X income		1.20 (0.24)	1.26 (0.22)
Residential Dev.	0.32 (0.08)	0.20 (0.30)	-0.12 (0.34)
X male		1.25 (0.15)	0.37 (0.17)
X age		-0.15 (0.52)	0.55 (0.33)
X income		-2.14 (0.45)	-1.06 (0.45)
Industrial Dev.	0.35 (0.05)	-0.49 (0.18)	-0.62 (0.28)
X male		-0.43 (0.10)	-0.30 (0.11)
X age		1.15 (0.32)	0.79 (0.21)
X income		1.45 (0.28)	0.95 (0.25)
Miles (one-way)	-12.34 (0.12)	-11.27 (0.30)	-12.69 (0.31)
X lake-front		-10.67 (0.43)	-10.45 (0.54)
X opp. cost		7.17 (2.22)	8.31 (2.37)
X not employ		0.54 (0.32)	0.19 (0.37)
X part-time		0.38 (0.35)	0.28 (0.33)

11 Appendix: Contraction Mapping

Contraction mapping obtains δ such that the predicted market shares are exactly equal to observed market shares. Show that when alternative specific constants are included, the likelihood is maximized only when predicted number of trips equals actual number of trips.

$$L = \sum_{i=1}^{N}\sum_{t=1}^{T}\sum_{j=1}^{J} y_{ijt} ln(P_{it}(j))$$

where

$$P_{it}(j) = \frac{exp(V_{ijt})}{\sum_{k=1}^{J} exp(V_{ikt})}$$

$$V_{ijt} = \delta_j + X_j\sigma_\beta\mu_i + X_{ij}\bar{\gamma} + X_{ij}\sigma_\gamma\nu_i$$

$$L = \sum_{i=1}^{N}\sum_{t=1}^{T}\sum_{j=1}^{J} y_{ijt}\left(V_{ijt} - ln\left(\sum_{k=1}^{J} exp(V_{ikt})\right)\right)$$

$$\frac{\partial L}{\partial \delta_1} = \sum_{i=1}^{N}\sum_{t=1}^{T}\left[y_{i1t} - \sum_{j=1}^{J} y_{ijt}\frac{exp(V_{i1t})}{\sum_{k=1}^{J} exp(V_{ikt})}\right]$$

$$\frac{\partial L}{\partial \delta_1} = \sum_{i=1}^{N}\sum_{t=1}^{T}\left[y_{i1t} - \frac{exp(V_{i1t})}{\sum_{k=1}^{J} exp(V_{ikt})}\sum_{j=1}^{J} y_{ijt}\right]$$

$$\frac{\partial L}{\partial \delta_1} = \sum_{i=1}^{N}\sum_{t=1}^{T}[y_{i1t} - P_{it}(1)] = 0$$

$$\sum_{i=1}^{N}\sum_{t=1}^{T} y_{i1t} = \sum_{i=1}^{N}\sum_{t=1}^{T} P_{it}(1)$$

Actual Number of Trips to Site 1 = Predicted Number of Trips to Site 1

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