Assessing the Stability of Marine Recreational Resource Values Across Space and Time

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Chapter 1. Introduction

Benefit transfer generally refers to the practice of applying estimates of economic values obtained from one or more original valuation studies in one context to the evaluation of economic values in other context by *adaptively transferring* available information from existing primary studies. Following Desvousges, Naughton, and Parsons (1992), a place for which original research was conducted is called a "*study site*" and a place to which estimates of economic values from original research are transferred is called a "*policy site*." As a less costly and time consuming method of obtaining estimates of non-market values for various outdoor recreation activities, the primary goal of benefit transfer practice is to estimate economic benefits with an acceptable degree of accuracy for one context (a policy site) by adaptively transferring benefit estimates from some other context (a study site) when it is too costly or takes too much time to conduct a primary valuation study.

Benefit transfer provides a means by which economic values of an outdoor recreation activity at an unstudied policy site can be estimated using information available from a study site(s). For instance, economic values of marine recreational fishing in a particular state or region could be estimated by adaptively transferring estimates of economic values of marine recreational fishing from the original study conducted in another state or region after adjusting to new circumstances, especially to different characteristics of population and fishing sites. Although this study focuses on transferring estimates of non-market values of marine recreational fishing, benefit transfer techniques could be broadly applied in a number of other circumstances. It may also be applied in screening farming policies, evaluating environmental policies (e.g., U.S. EPA's (1997) assessment of the Clean Air Act), defining the extent of the market affected by a proposed policy, initial screening of natural resource damage assessment, and determining whether original research is warranted (Rosenberger and Loomis 2003).

One purpose of this research is to combine and analyze all relevant Marine Recreational Fishing Statistics Survey (MRFSS) data collected from 1994 to present. The data is comprised of two geographically identical datasets for the Southeast region (1997, 2000), five identical datasets for the Northeast region (1994, 1996, 1997, 1999, 2000), and one dataset for the Pacific region (1998). The data covers the states of Louisiana to Maine on the Atlantic/Gulf Coast, and California, Washington and Oregon on the Pacific Coast. The data will represent fishing activity from 1994 to 2000.

Economic measures of value have been estimated with data from the 1994 data collection effort in the Northeast and the 1997 data collection effort in the Southeast. However, models have not been estimated for the remaining years - representing six datasets. For consistency, econometric models for this analysis are based on previous studies involving the MRFSS data. Modifications to methods are made to update the existing methods when necessary. Nested random utility models are estimated for each data set. Welfare measures are developed from these models.

Another purpose of this research is to assess the feasibility of benefit transfer with MRFSS data. We test the stability of welfare measures across time by comparing welfare measures from the same region over different years. We test the stability of welfare measures across space by comparing welfare measures from different regions over the same year. Testing for stability of estimates involves pooling data and estimating models across time and space. These models are compared to models that are estimated using only data from a particular year and region.

The report proceeds as follows. We first conduct a survey of the current literature on benefit transfer. Next we provide a theoretical framework for our application of benefits transfer. We then present a simulation study on the practical implication of benefit function transfers in random utility models. Using the results of the analysis of the 1994-2000 MRFSS data, an assessment of the *validity and reliability* of benefit transfer methods in estimating non-market values of marine recreational fishing is conducted. The assessment of the feasibility of benefits transfer across time and space with specific case studies from the MRFSS is presented. We conclude with recommendations to the National Marine Fisheries Service in the areas of benefits transfer and the need for regular data collection, and recommendations for how often a large-scale recreational survey should be conducted.

Chapter 2. Introduction to Benefits Transfer Methodologies

Even before any development of formal terminology or systematic procedures and protocols and definitely before any rigorous testing of the validity and reliability of the method, the practice of benefit transfer became popular in the economic analysis of the consequences of environmental regulations in the United States in the mid 1980s. The U.S. Army Corps of Engineers, the U.S. Bureau of Reclamation, and the U.S. Forest Service identified a need for estimates of economic values for various recreation activities for the purpose of formal project evaluation and planning.

The U.S. Water Resources Council first published unit day value estimates for various recreation activities to evaluate water-related projects in 1973, and updated these recreation value estimates in 1979 and 1983. The U.S. Forest Service also began publishing Resources Planning Act (RPA) values for recreation in 1980 as per person per activity day estimates driven by the Renewable Resources Planning Act of 1974 which requires an assessment of the supply of and demand for renewable resources on the Nation's forests and rangelands along with a formal analysis of the costs and benefits associated with the USDA Forest Service's programs. These requirements of an assessment of renewable resources and a program analysis create the need for accurate and reliable measures of non-market values of outdoor recreation activities. Both the U.S. Water Resources Council's unit day recreation values and the U.S. Forest Service's RPA recreation values were based primarily on average willingness to pay estimates for various outdoor recreation activities derived from past empirical estimates along with expert judgment and political screening.

A major development in benefit transfer occurred in 1992 with the publication of a special section on benefit transfer in the journal *Water Resources Research*. This special section collectively provides an extensive critique on benefit transfer methods by defining theories, identifying needs, suggesting protocols, and presenting new approaches. Brookshire and Neill (1992) provide an introduction and overview of this special section on benefit transfer, focusing on conceptual and empirical issues. They address some fundamental issues regarding benefit transfer, including limitations and the need for protocol development. They also point out that the most critical limitation of benefit transfer practice: benefit transfer can only be as accurate as the original estimates of economic benefits. Existing problems associated with original non-market valuation studies will be only magnified in the application of benefit transfer. One seemingly common conclusion from the papers in this special issue seems that benefit transfer is valid only under well-defined conditions although authors do not argue about the possibility of the practice. Prior to this special section, most benefit transfer applications used a *value transfer* method that either directly or adaptively (e.g., day use values adjusting for population) transfers point estimate(s) or a central tendency measure of original study estimates. Loomis (1992) proposes a *benefit function transfer* method that transfers entire demand, benefit, or willingness-to-pay functions as a more rigorous and robust method of benefit transfer compared with the simple transfer of point estimate(s) or a measure of central tendency.

A number of formal studies have been investigating the application of non-market valuation methods (travel cost method, hedonic pricing method, and contingent valuation method) and the validity of the various benefit transfer approaches (value transfer and function transfer) since the publication of a special section on benefit transfer in 1992. Many studies empirically try to evaluate the validity and reliability of benefit transfer applications in various contexts by either adopting a benefit function transfer method or comparing it with a value transfer method (Parson and Kealy 1994; Loomis et al. 1995; Downing and Ozuna 1996; Feather and Hellerstein 1997; Kirchhoff, Colby, and LaFrance 1997; Brouwer and Spaninks 1999; Piper and Martin 2001; Smith, Van Houtven, and Pattanayak 2002). The application of a benefit function transfer method always seems to perform better than a simple value transfer method, resulting in an empirically more robust benefit transfer estimates for the policy site in terms of the validity and reliability. O'Doherty (1995), Desvousges, Johnson, and Banzhaf (1998), Bergstrom and De Civita (1999), Brouwer (2000), and Rosenberger and Loomis (2001, 2003) provide comprehensive overviews of the current status of benefit transfer as a potentially cost and time efficient non-market valuation technique.

An Overview of Benefit Transfer Methodology

Benefit transfer is a practical methodology in evaluating the economic consequences of environmental policies and programs with underlying assumption that economic benefits and costs associated with a particular environmental commodity or change could be extrapolated from existing valuation studies of similar context. In possibly many circumstances, primary research may not be justified or plausible due to budget constraint or time limitation necessitating the application of benefit transfer method. However, this low cost and less time-consuming alternative method for non-market valuation may only be valid and reliable under special circumstances. In addition, there are also several important limitations associated with the application of benefit transfer even when these special circumstances are satisfied. Before a thorough discussion of general procedures in performing and validating benefit transfer methods, we need to carefully address the circumstances under which benefit transfer methods can be successfully performed, potential advantages, and potential limitations.

Necessary Conditions for Successful Benefit Transfer

In order to perform effective and efficient benefit transfer when primary research for a "policy site" is not a plausible option, there exit some conditions that should be satisfied (Desvousges, Naughton, and Parsons 1992; Rosenberger and Loomis 2001).

First, the policy site (the area to which estimated benefits are transferred) context should be thoroughly defined. The extent, magnitude, and quantification of expected impacts from the proposed policy action on the policy site or its resources should be identified along with the extent and magnitude of the population that will be affected by the expected changes in the characteristics of the policy site. The availability of the current data in the policy site and further data needs for benefit transfer application should be identified, including the type of measurement (unit, average, or marginal value), the kind of value measured (use, nonuse, or total value), and the degree of certainty surrounding the transferred data (the accuracy and precision of transferred data).

Second, the study site (the area for which primary research has been implemented) should meet certain conditions for successful benefit transfer. It is necessary that original studies transferred should be based on adequate data, sound economic method, and correct empirical technique (Freeman 1984). The statistical relationships between economic benefits (or costs) and both socio-economic characteristics of the affected population and physical or environmental characteristics of the study site should be contained in the original valuation study. In addition, an adequate number of original studies on a particular recreation activity for similar sites would enable to us to carry out more reliable statistical inferences concerning the transferability of estimated values from study sites to the policy site context.

Finally, the study site(s) and the policy site should exhibit an adequate level of similarity in terms of the environmental commodity and its market, the nature of an environmental change, and the characteristics of the affected population and site. The conditions and quality of the recreation activity analyzed should be similar, including intensity, duration, and skill requirement. Unless enough information on own and substitute prices is available, the markets for the study site and the policy site should be similar. The quality and quantity of the change in the environmental resources at the study site should be similar to those of the expected change in the environmental resources at the policy site, including the measurability and the sources of the change. Other important characteristics that should be considered include site and population characteristics. The similarities of socio-economic profiles of the affected populations and the characteristics of the environmental resources of interest between the study site and the policy site would be an important requirement for a successful application of benefit transfer. Benefit transfer applications would work more effectively and efficiently when the attributes of the environmental commodity and its market, the nature of the environmental change, the characteristics of the affected population and site display an adequate level of similarity between the study site and the policy site.

The above information requirements to implement effective and efficient benefit transfer applications are not always satisfied in the reporting of data and estimation results from primary studies since most original research was not conducted for the future purpose of transferring estimated economic benefits or costs to the policy site of similar context. Therefore, implicit costs of performing benefit transfer with incomplete information should be deliberately accounted for as well as the potential benefits of additional information from possibly expensive primary research.

Potential Advantages of Benefit Transfer

Benefit transfer is a typically less costly and quicker method of estimating economic benefits of various recreation activities compared with conducting an original non-market valuation study as mentioned above. A careful application of benefit transfer methods could have some other advantages in addition to the obvious *cost and time* advantages (Desvousges, Johnson, and Banzhaf 1998).

First, the application of benefit transfer *systematically calculates costs and/or benefits in a way that is consistent with economic theory* by recognizing the behavioral linkages that occur in non-market recreation activities. Transferred benefits for the policy site are constructed primarily based on the benefit estimates from the original research's non-market valuation technique that systematically incorporates these behavioral linkages into the modeling of various recreation activities.

Second, benefit transfer methodologies can help us *organize policy issues and provide a logical framework for non-market valuation* while remaining flexible. As new policy issues are identified or new original studies become available, benefit transfer methods can readily allow additional branches to be attached to the existing linkages by organizing the calculations of benefits in a chain of behavioral linkages. In the practice of benefit transfer methods, a new aspect of recreation benefits (or environmental costs) or better original studies as they become available could be easily incorporated into the existing chain of linkages by identifying and using the appropriate variables for measuring each behavioral linkage.

Finally, the benefit transfer method could be used as a *screening technique* to determine whether more detailed, primary valuation research should be conducted. The availability of relatively easy and quick estimates of aggregate recreational values through the application of a benefit transfer method allows for preliminary research that can identify benefits and costs associated with a primary valuation study. As the accuracy and precision of transferred benefit estimates for the policy site increase, the need for expensive and time-consuming primary research will necessarily decrease.

Potential Limitations of Benefit Transfer

The most fundamental problem of benefit transfer methods stems from the limitations of the original valuation study. Brookshire and Neill (1992) point out that benefit transfer estimates cannot be more reliable than the original study estimates upon which it is based, and the problems associated with the original non-market valuation study will only be magnified in benefit transfer applications. The accuracy of transferred benefit estimates for the policy site is inevitably determined by the accuracy of the benefit estimates from the original valuation study, and any existing problem associated with the original research would be necessarily intensified through the process of benefit transfer. Several studies (Boyle and Bergstrom 1992; Desvousges, Naughton, and Parsons 1992; Navrud and Pruckner 1997; Desvousges, Johnson, and Banzhaf 1998; Bergstrom and De Civita 1999; Azqueta and Touza 2000; Brouwer 2000; Rosenberger and Loomis 2001) collectively provide a comprehensive summary on potential problems encountered in the application of benefit transfer methods.

First, a group of factors affecting the *overall quality of the original study* would be considered as major limitations on the success of benefit transfer practice. As pointed out by many researchers, the quality of the original study significantly affects the quality of benefit transfer procedure: the "garbage in, garbage out" factor (Rosenberger and Loomis 2001). The crucial, underlying assumption to validate benefit transfer practice is that the estimated values from the existing study site are the true values of the environmental resources being evaluated. In addition to this basic assumption, benefit transfer practitioners should make a number of assumptions and professional judgments in applying benefit transfer methods using various non-market valuation studies: "*There is no simple, acceptable way to mechanically transfer a model. Just as the chief ingredient in model construction is judgment, it is also the most important ingredient in transferring benefits*" (McConnell 1992).

These assumptions and professional judgments regarding many aspects of benefit transfer procedure may introduce greater subjectivity and uncertainty into the analysis. An important question to be addressed is whether the added subjectivity and uncertainty surrounding benefit transfer methods are acceptable, and resulting benefit transfer estimates still provide informative results. Inadequate reporting of original valuation studies could prevent us from adjusting estimated non-market values to the new environment of the policy site because most primary valuation studies are not designed for the future application in the benefit transfer process. Although there are no clear guidelines for evaluating existing valuation studies, both Desvousges, Naughton, and Parsons (1992) and Boyle and Bergstrom (1992) suggest some criteria to select among available original valuation studies in applying benefit transfer methods. Their criteria include adequate data, sound economic method, and correct empirical technique; similarities between the study site and the policy site in terms of non-market commodity defined, environmental change valued, and relevant market and population affected; description of non-market value as a function of socio-economic and site characteristics; and proper assignment of property rights leading to the same theoretically appropriate welfare measures at both study and policy sites.

Second, an important limitation can arise from the *availability of original valuation studies*. Finding suitable valuation studies that correspond to the policy site context, especially with regard to site characteristics and available substitutes, could be difficult because many existing studies are single-site studies with no substitutes and variation in site characteristics and, or available multi-site studies may not include sites comparable to the policy site. For some recreation activities, only a small number of original valuation studies may exist although this issue can be improved through time as more original non-market valuation studies based on primary data are implemented, providing a greater pool of non-market value estimates upon which benefit transfer application could be based. As more original valuation studies are conducted, these studies could be made more easily accessible to the researchers conducting benefit transfer studies by establishing a nationwide, or worldwide if possible, database system of both published and unpublished non-market valuation studies containing data sets, estimation techniques, and actual estimated values.

Third, several *methodological issues* should be addressed as possible limitations of benefit transfer. Different research and statistical methods used across existing valuation studies could lead to significant differences in estimated values. In estimating non-market values associated with various recreation activities, original valuation studies may apply revealed (stated) preference techniques which indirectly (directly) estimate consumer surplus (willingness to pay). Revealed preference techniques rely on the *weak complementarity* (no non-use values) assumption between recreation activity and market goods necessary to participate in the activity, meaning that environmental amenity has no effect on the welfare of the individual unless market goods required for recreation experience are purchased. Stated preference techniques rely on the constructed *hypothetical markets* through which people's willingness to pay for environmental resources or recreation opportunities are derived.

The most popular revealed preference technique is the travel cost method which uses the variable costs of a recreation activity (e.g., travel, lodging, entrance fees, equipment rentals, and/or travel time) as a proxy for the price of non-market recreation activity to derive a demand function. The contingent valuation method, which is the most popular stated preferences technique, directly asks people their maximum willingness to pay or minimum compensation required for a recreation opportunity or changes in a recreation experience in a hypothetical market. Original valuation studies may estimate different types of non-market values (use values and/or non-use values) using different methodologies (travel cost method and/or contingent valuation method) with different definitions of a relevant market (the size of affected population and the availability of substitutes), making the comparison of various existing studies more difficult and problematic.

Fourth, the *degree of correspondence* between the study site and the policy site can affect the efficiency and effectiveness of benefit transfer methods. Benefit transfer could produce inaccurate benefit estimates due to the lack of similarities between the study site and the policy site in terms of site and population specific characteristics; site quality, the degree of quality change, site location, and socio-economic characteristics of affected population. Some of the original studies may estimate different non-market values of particular recreation activities at unique recreation sites under unique circumstances, leading to quite different estimated values. Different temporal and spatial dimensions of the study site and the policy site could affect the stability of data and value estimates over time and across locations. Since existing valuation studies usually occur at different points in time and/or location, the extent of the relevant market could be quite different even for the same recreation activity.

Finally, Bergstrom and De Civita (1999) demonstrate potential sources of *measurement error* in value estimates generated by benefit transfer methods; commodity, population characteristics, welfare change, physio-economic linkage, and estimation procedure and judgment. Failure to identify and adequately measure relevant environmental commodities and available substitutes and complements at the policy and study sites could lead to inaccurate benefit transfer. Errors associated with identifying and measuring socio-economic characteristics (e.g., age, education, income, religion, cultural aspects, and family status) of the study and policy sites could introduce measurement error if the characteristics of the affected populations at the study site and the policy site are different. The theoretical inconsistency of welfare change measures across the study site and the policy site could introduce measurement error in transferring value estimates from the study site. Differences in the linkages between the physical world and the economic behavior and values across the study site and the policy site, coupled with errors in identifying and measuring these linkages, are also important sources of measurement error that could lead to inaccurate benefit transfer. Errors associated with statistical estimation procedures and subjective professional judgments in adaptively transferring estimated values from the original studies are important sources of measurement error although they are necessary components of non-market valuation and benefit transfer studies. Any measurement error inherent in the value estimates from the original study will certainly end up being transferred to the policy site as a result of benefit transfer, potentially magnified by measurement errors of benefit transfer estimates mentioned above.

The potential limitations listed above could lead to the biased value estimates transferred to the policy site and decrease the robustness of the benefit transfer procedure. Although the original value estimates are approximations themselves and therefore subject to many sources of errors, potential limitations involved in the benefit transfer process itself should be minimized, given inherent errors in the original study, to enhance the robustness of benefit transfer methods.

Validity and Reliability of Benefit Transfer

Since benefit transfer methodologies are based on the adaptive use of value estimates from a number of existing non-market valuation studies, the validity and reliability of benefit estimates transferred from the study site to the policy site are fundamental elements of the credibility and successfulness of benefit transfer practices. Although theoretical and empirical aspects of the validity and reliability of the transferred value estimates could be evaluated together, the primary focus will be on the empirical aspect of the validity and reliability issues regarding benefit transfer methodologies.

The *reliability* of estimated non-market values derived from various valuation techniques, including benefit transfer method, is related with the *variance* of estimated monetary values (Azqueta and Touza 2000). When the variance of estimated non-market values is large, these estimated benefit measures will be considered as unreliable. The reliability of benefit estimates does not guarantee the validity of these estimates since reliability is a necessary, not sufficient, condition for validity which requires unbiasedness of value estimates. Loomis (1989) points out that reliable (small variance) value estimates could be biased upward or downward; however, reliability may indicate that reported value estimates are consistent, and reflect a substantial deterministic component of respondent's behavior.

O'Doherty (1995) indicates that we can interpret some tests of the reliability of benefit estimates as an assessment of the feasibility of benefit transfer methods under circumstances that may guarantee successful benefit transfer. The *split half sample tests* of reliability use identical physical and population characteristics of the study and policy sites to evaluate the similarity of results from two samples of the same population taken at the same time. If an estimated measure of the central tendency (mean or median) and the demand or benefit function across the two samples are acceptably similar, the feasibility of a hypothetical benefit transfer in these most desirable circumstances may be guaranteed. The *test-retest method* of assessing the temporal reliability compares results from two samples of the same population taken at different times. By evaluating the similarity of benefit estimates across the two samples taken at different times, the test-retest method may also provide an assessment of the feasibility and temporal stability of benefit transfer. The reliability of benefit estimates from various non-market valuation techniques depends on different factors due to the uniqueness of each valuation technique. For example, the choice of a functional form along with the specification of the independent variables determines the reliability of value estimates from both travel cost method (Kling 1988) and hedonic price method (Cropper, Deck, and McConnell 1988), while the scenario recognition, sample size, and the robustness of statistical techniques determine the reliability of value estimates from the contingent valuation method (Mitchell and Carson 1989; Hanemann 1994).

The concept of the *validity* of using value estimates obtained from benefit transfer process as an alternative for conducting original valuation research for the policy site is related with the absence of *biases* in value estimates (Azqueta and Touza 2000). Validity can guarantee the similarity between what needs to be valued (economic values at the policy site) and what is actually valued (estimated economic values transferred from the value estimates at the study site). Since it is impossible to directly examine the equality between the true values and benefit transfer estimates at the policy site to test the validity of benefit transfer methods, we can indirectly assess the validity by using some reference measures with which benefit transfer estimates can be compared. Azqueta and Touza (2000) describe three types of validity concepts depending on the type of reference measures used: criterion validity, theoretical validity, and convergent validity. *Criterion validity* tests use some particular reference measures that may be considered as valid criteria to compare with value estimates. For instance, hypothetical contingent valuation estimates can be compared with outcomes of simulated or actual markets for the same good. *Theoretical validity* focuses on the theoretical determinants of value estimates from various non-market valuation techniques by examining the consistency of estimation results with underlying economic theories. *Convergent validity*, the most popular validity concept in evaluating estimated values using benefit transfer methods, is based on the comparison of benefit transfer estimates with the original estimated values developed for the policy site.

Convergent validity tests generally begin with the examination of the degree of statistical equality of estimated coefficients of the primary study and transferred benefit functions, both for the policy site. An implicit assumption of this test of coefficient equivalence is that if the estimated coefficients of two benefit functions are statistically equivalent, benefit estimates derived from these functions will also be statistically equivalent (Downing and Ozuna 1996). When nonlinearities are present in the willingness to pay or demand functions, these nonlinearities could introduce the case where statistically equivalent benefit functions yield statistically different welfare measures. Due to possible divergence introduced by nonlinearities in benefit functions, convergent validity tests usually involve a second step of examining statistical similarities between value estimates obtained from benefit transfer methods and primary research. Convergent validity tests performed in the existing studies (Loomis 1992; Parsons and Kealy 1994; Loomis et al. 1995; Downing and Ozuna 1995; Kirchhoff, Colby, and LaFrance 1997; Brouwer and Spaninks 1999; Rosenberger and Loomis 2001) suggest that function transfer is more defensible than value transfer, implying the importance of systematic adjustment of study site values for differences in the characteristics of affected population and site between the study site and the policy site.

Feasibility of Benefit Transfer

The increasing demand for valid and reliable benefit transfer methodologies is derived from the increasing demand for non-market valuation studies by the local, state and federal governments (e.g., public policy and project evaluations) and the courts (e.g., natural resource damage assessment claims) in the United States (O'Doherty 1995). Since there are constraints on time and budget in conducting full-scale non-market valuation studies, speedy and inexpensive yet acceptably accurate alternative method of estimating non-market values are necessary. The required level of accuracy and scrutiny would be diverse depending on the circumstances of decision settings (Brookshire 1992; Desvousges, Dunford, and Mathews 1992). When the estimated benefits are considerably greater than the estimated costs associated with a proposed policy or program, the level of scrutiny required may not be very high. However, it would be more appropriate and safer to adopt very conservative measures of benefit estimates against the upper confidence limit of cost estimates (O'Doherty 1995). The evaluation of the feasibility of benefit transfer methodologies is primarily based on the assessment of the degree of required accuracy and scrutiny along with the degree of optimism in the application of benefit transfer.

Brookshire (1992) proposes a spectrum of minimum level of accuracy required based on different purposes of performing benefit transfer illustrated in Figure 2.1. To simply gain relevant knowledge or some insights in an attempt to identify markets or important behavioral linkages, a relatively low level of accuracy would be sufficient for benefit transfer applications. Benefit transfer could be a useful screening tool as preliminary research for guiding the design of an original valuation study. In this setting, we may not require as much accuracy as needed in the direct application of benefit transfer methods in the policy analysis. To assist policy makers in the decision process for various environmental policies and programs, a higher level of accuracy should be guaranteed although benefit cost analysis may only need to determine whether expected benefits are greater than expected costs without any need for establishing an exact magnitude of benefits and costs. In the context of natural resource damage assessment (NRDA) litigation and some public policy formulation, an actual magnitude of non-market values is required with the highest standard of accuracy to determine compensable damages in NRDA cases and identify externality costs for environmental policies (Pigouvian tax) designed to equate marginal social costs and benefits. The degree of minimum accuracy required is closely related to the cost of making an inaccurate decision based on benefit transfer results. As social costs of inappropriate policy decisions based on inaccurate benefit transfer estimates increase, the level of accuracy required for benefit transfer estimates would necessarily increase. If the costs associated with a wrong decision appear to be too high or irreversible, an original valuation study may be better conducted since the possible benefits of using benefit transfer estimates may not be big enough to cover potential costs.

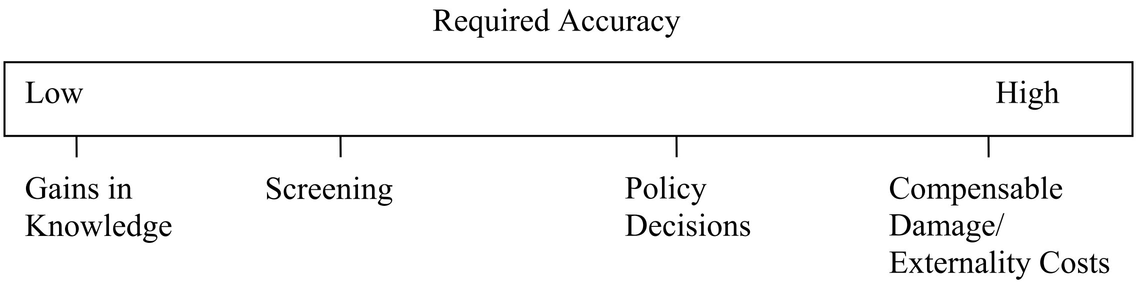

Figure 2.1. A Continuum of Decision Settings from Least Required to Most Required Accuracy
Source: Brookshire (1992)

A continuum of valuation scrutiny in a NRDA context introduced by Desvouges, Dunford, and Mathews (1992) is illustrated in Figure 2.2. Their scrutiny continuum includes initial screening assessment of natural resource damages, negotiated settlement, and litigation. Taking a more optimistic attitude toward the potential advantages of benefit transfer in an NRDA context, they report that the use of benefit transfer estimates is strongly supported for an initial screening assessment or negotiated settlement, which involves relatively little scrutiny, among the participants of their group discussion on the use of benefit transfer estimates in NRDA cases. Because of the higher level of scrutiny required possibly due to the high level of potential gains and losses involved (e.g., Exxon Valdez and Arthur Kill oil spills), the adoption of benefit transfer estimates when litigation is involved would be very difficult to be defensible in a court.

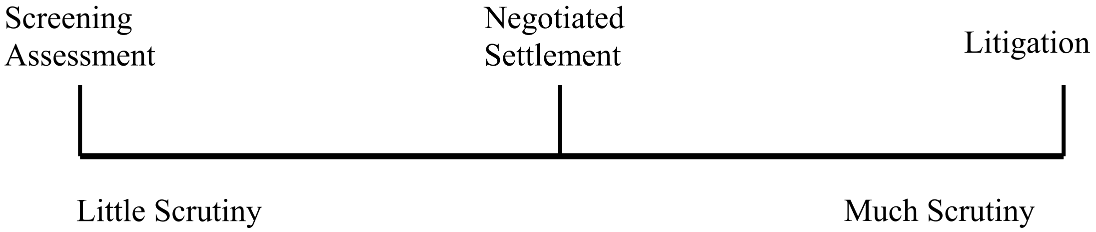

Figure 2.2. A Continuum of Valuation Scrutiny in a NRDA Context
Source: Desvouges, Dunford, and Mathews (1992)

Based on the two continuums of required accuracy and scrutiny level illustrated in Figure 2.1 and 2.2 respectively along with the degree of optimism in the use of benefit transfer, O'Doherty (1995) provides a guideline on the recommended use of benefit transfer (Figure 2.3). He defines the optimism of researchers based on their subjective attitudes toward the credibility of benefit transfer methodologies, not on the fulfillment of necessary conditions for successful benefit transfer application mentioned earlier. Using the necessary conditions for effective and efficient benefit transfer practice to define the degree of optimism would provide a more practical and perhaps more reliable guideline on the recommended use of benefit transfer. Where the level of required accuracy and scrutiny is low along with high degree of optimism in the use of benefit transfer due to a fulfillment of necessary conditions for successful benefit transfer, the application of benefit transfer is most highly recommended. However, the application of benefit transfer methods is never recommended where the level of required accuracy and scrutiny is high along with low degree of optimism in the use of benefit transfer because not many of the necessary conditions for successful benefit transfer are met. In other situations, a rather cautious approach is recommended in deciding whether to use benefit transfer or not.

Required Accuracy/Scrutiny

Degree of Optimism in the Use of Benefit Transfer

	Low	High
Low	Unsure, possibly tempered use	**Don’t use**
High	**Use**	Tempered Use

Figure 2.3. The Recommended Use of Benefit Transfer
Source: O’Doherty (1995)

Chapter 3. Benefit Transfer Methods and Procedure

Two major types of the benefit transfer method will be considered depending on whether estimated benefit measure(s) or an adapted benefit function is transferred from the study site to the policy site. Although there may be different protocols in terms of detailed processes for these two types of benefit transfer, a general guideline in applying either of these methods should be developed before the discussion of details involved with each method.

Methods

There are two broadly defined approaches of conducting benefit transfer: *value transfer* and *function transfer*. Two broad approaches and their variations (Rosenberger and Loomis 2001) are illustrated in Figure 3.1. Value transfer methods include the transfer of a single benefit estimate from the study site and a measure of central tendency, mean or median value for example, for several benefit estimates from the study site(s). Function transfer methods include the transfer of a demand or benefit function from the study site or a meta-regression analysis benefit function derived from existing original studies although the primary purpose of a meta analysis may not be a transfer of benefit function.

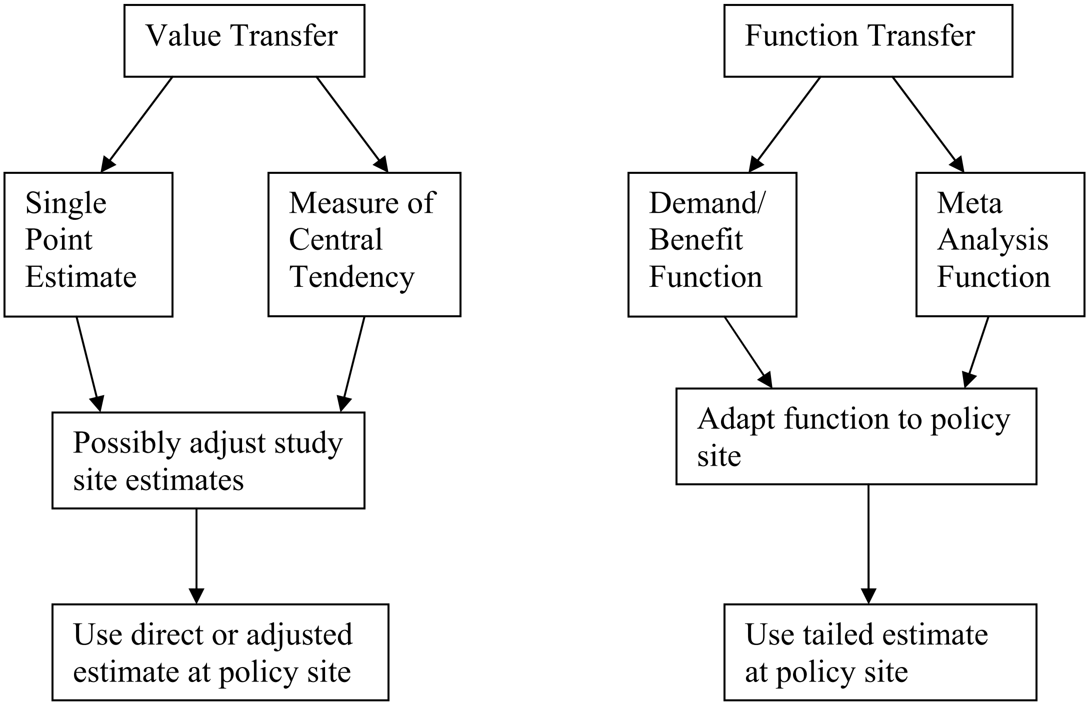

Figure 3.1. Benefit Transfer Approaches
Source: Rosenberger and Loomis (2001)

The simplest, but possibly the most inaccurate, technique of benefit transfer is to estimate a total benefit of environmental commodity at the policy site by simply multiplying a single mean unit value or an average of several mean unit values from study site estimate(s) by the affected population at the policy site. The underlying assumption with these value transfer methods is that the change in welfare for an average individual at the study site would be equivalent to the change in welfare for an average individual at the policy site. If the characteristics of site and relevant population or the nature of environmental change is different, the direct transfer of benefit estimates could be misleading unless benefit transfer estimates are calibrated to reflect these differences. Study site estimates could be adjusted to differences between the study site and the policy site before transferring to the policy site based on expert judgments. Value transfer methods are only recommended where the benefit function for the study site or the values of independent variables for the policy site are not available (Azqueta and Touza 2000).

For function transfer methods, the entire demand or benefit function estimated for the study site is used along with the relevant data at the policy site for independent variables in the estimated demand or benefit function. The adjusted policy site estimates can be derived from plugging mean values of the independent variables at the policy site into the benefit function estimated from the study site. Benefit transfer estimates obtained from transferring the demand or benefit function tend to be less biased due to more systematic adjustment of study site values to account for the differences in the characteristics of site and population. As a more complicated approach of function transfer, the meta-analysis function technique estimates a regression equation of benefit estimates from existing studies with site and population characteristics and valuation methods as independent variables in a meta-analysis regression equation. Benefit estimates for the policy site can be derived by inserting the values of explanatory variables including summary statistics of population and site characteristics, the type of recreation activity, the geographic location, and detailed valuation methods into the meta-analysis benefit function. Meta-analysis benefit function transfer can help us better understand the influence of valuation methodologies (revealed or stated preference methods) and other study-specific factors (detailed strategies for each valuation method) on the benefit estimates by systematically using existing valuation studies for benefit transfer. However, many variables need to be standardized for consistency, especially variables that represent different qualities since original valuation studies were not designed to be pooled together for meta-analysis.

Procedure

A contribution to the development of a generally acceptable protocol or systematic procedure for the application of benefit transfer methods is a major goal of many previous studies on benefit transfer and this study too; although it appears that an appropriate benefit transfer procedure may be dependent, at least to some extent, on the specific circumstances being considered. Several studies (Boyle and Bergstrom 1992; Kask and Shogren 1994; Desvousges, Johnson, and Banzhaf 1998; Azqueta and Touza 2000; Brouwer 2000; Rosenberger and Loomis 2001, 2003) suggest basic steps that can be followed in conducting benefit transfer as an attempt to develop a protocol. To summarize benefit transfer procedures recommended by previous studies, five fundamental steps in conducting benefit transfer methods are illustrated in Figure 3.2.

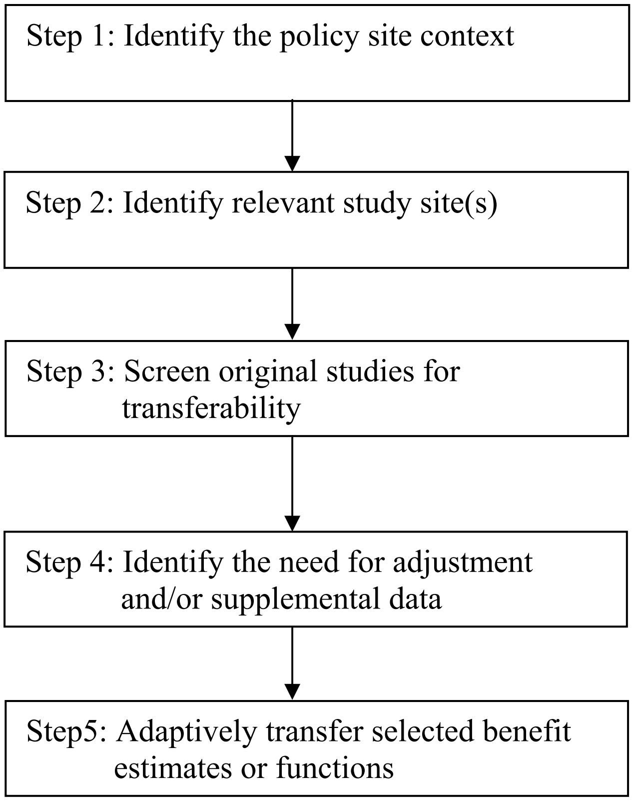

Figure 3.2. Basic Steps in Benefit Transfer Procedure

The first step is to *identify the context of the policy site* to which benefit estimates will be transferred. The definition of the environmental commodity and its potential change to be evaluated should be carefully specified. Socio-economic characteristics (such as income, education, age, and sex) of the affected population and site characteristics (such as location, accessibility conditions, and site quality) that can influence benefit estimates are analyzed in this first step. Kask and Shogren (1994) recommend that the purpose and the level of required accuracy of benefit transfer estimates should be determined in this step.

The second step is to *identify relevant study site(s)* from which benefit estimates might be transferred. A thorough literature search should be conducted by reviewing relevant journal articles, working papers, books, unpublished government reports, conference papers, and doctoral theses to locate and gather the most appropriate valuation studies that can be potentially applied to the policy site. As the number and diversity of non-market valuation studies along with the popularity of benefit transfer methods increase, internet-based large databases of non-market valuation studies become available. These databases provide data on benefit estimates and estimation methodologies so that the applicability of existing valuation studies in benefit transfer can be evaluated systematically. The Environmental Valuation Reference Inventory (EVRI) by the Environment Canada and the environmental valuation database (ENVALUE) by the Environmental Protection Agency of New South Wales are two general environmental valuation databases currently available. The Beneficial Use Values Database (BUVD) of water resource by the University of California, Davis and the sport fishing values database by the U.S. Fish and Wildlife Service are more specialized databases available.

The third step is to *screen original studies for transferability*. Potential study site estimates obtained from the literature search conducted in the previous step should be evaluated to determine whether they are transferable or not. Both Boyle and Bergstrom (1992) and Desvousges, Naughton, and Parsons (1992) suggest several criteria to select among available valuation studies for benefit transfer based on the equivalence of the environmental commodity and its change valued, the characteristics of site and affected population, and theoretical welfare measure between the study site and the policy site. Along with the equivalence of these aspects, the adequacy of the data, economic method, and empirical technique used in the existing valuation studies should also be evaluated.

The fourth step is to *identify the need for adjustment and/or supplemental data*. It is most likely that the characteristics of site and affected population and the nature of environmental change at study and policy sites would be unequal, necessitating the adjustment process for study site values to better reflect the policy site attributes. A systematic adjustment of study site values or value functions could provide more accurate benefit transfer estimates for the policy site. Usually in function transfer methods, some primary data collection may be necessary to gather summary data for the policy site. The relevant data on some independent variables in the demand or benefit function used by primary research may not be available for the policy site.

The final step is to *adaptively transfer selected benefit estimates or functions*. This is the stage where the actual benefit transfer occurs. Benefit transfer could be performed as value transfer (the transfer of a single point estimate or average value) or function transfer (the transfer of the entire demand/benefit function or meta-analysis benefit function) with or without systematic adjustment process using the information obtained from the original valuation studies. These direct or adjusted benefit transfer estimates for the policy site could be aggregated by multiplying them by the number of relevant units to yield a measure of total benefit or cost at the policy site.

Modeling Benefit Transfer

Rosenberger and Loomis (2003) comprehensively illustrate the process of modeling benefit transfer methods, value transfer and function transfer. Following their notations, V_S (V_{Si}) and V_P (V_{Pj}) represent the estimated and needed measures of environmental values for the study site (study site i) and the policy site (policy site j) respectively. In an attempt to derive estimates of V_{Pj} for the policy site j by adaptively transferring estimates of V_{Si} from the study site i, study site values become transfer values (V_{Ti}) when applied to the policy site *j* (V_{Si} => V_{Ti}). Modeling benefit transfer methods involves the fundamental question of how the estimates of V_{Si} can be used to estimate V_{Pj}.

Value Transfer Methods

In the direct, or possibly adaptive, application of a single or several benefit estimates, there are two approaches (Figure 4) in conducting value transfer, including point estimate transfer and average value transfer.

Point Estimate Transfer. Point estimate transfer is based on typically a single or possibly range of point estimates obtained from relevant primary studies. The measure(s) of study site value (V_{Si}) under the context of the study site *i* (Q_{Si}) can be used to estimate policy site value (V_{Pj}) under the context of the policy site *j* (Q_{Pj}):

(3.1) $V_{Pj}|Q_{Pj} = V_{Si}|Q_{Si}$.

When a range of benefit estimates is transferred, a confidence interval for transferred point estimates can be constructed if the standard error of the benefit estimate is available. It must be noted that all benefit measure estimates transferred should be expressed in a comparable index such as consumer surplus per activity day per person.

Average Value Transfer. Average value transfer is based on the measure of central tendency of benefit estimates obtained from relevant primary studies. This approach is similar to point estimate transfer except for the use of an average or other measure of central tendency from relevant and applicable valuation studies. A mean, median, or other measure of central tendency based on all or a subset of relevant study site benefit estimates is used as a benefit transfer estimate for the policy site. This approach is defined as

(3.2) $V_{Pj}|Q_{Pj} = \overline{Vs} \,|\, \overline{Qs}$

where $V_{Pj}|Q_{Pj}$ is a policy site value under the policy site context, $\overline{Vs}$ is a measure of central tendency for all or a subset of study site measures under these study sites' contexts ($\overline{Qs}$). Again, all benefit estimate measures used to calculate a measure of central tendency should be adjusted to a common unit relevant to the policy site. The average value could be affected by large or small "outlier" benefit estimates, implying that a subset of estimates, not all of them, would be a better measure of central tendency especially when there is a small number of benefit estimates.

Validity Test. To test the validity of benefit transfer estimates, we need to know the "true value" of the environmental commodity at the policy site. The only practical way of conducting a validity test is to compare benefit transfer estimates to value estimates obtained from a primary study at the policy site. That is, we actually assume that original study estimates at the policy site are true values, and test the validity of applying benefit transfer estimates, obtained from original study site studies, to the policy site by comparing these two estimates, original and benefit transfer estimates both for the policy site. If the error associated with benefit transfer is define as

(3.3) $V_{Ti} = V_{Pj} + \delta_{ij}$

where V_{Ti} is the transferred value from the study site *i*, V_{Pj} is the true value derived from a primary study at the policy site *j*, and δ_{ij} is the benefit transfer error. The benefit transfer error can be expressed as the percentage difference between the true value and the transferred value:

(3.4) $\%\delta_{ij} = |\{(V_{Ti} - V_{Pj})/V_{Pj}\}| * 100.$

The magnitude of benefit transfer error will depend primarily on the degree of the correspondence between the study site and the policy site attributes, including site and population characteristics and the environmental commodity and its change valued.

Function Transfer Methods

Function transfer methods involve the transfer of functions or statistical models that relate benefit measures with the study site characteristics, including demand or benefit functions and meta-regression analysis function. Function transfer methods are generally considered as providing more accurate estimates than value transfer methods because benefit functions are tailed to better reflect the characteristics and conditions of the policy site.

Demand or Benefit Function Transfer. Demand or benefit function transfer is based on the assumption that the estimate of study site value (V_{Si}) can be expressed as a function of the study site characteristics (Q_{Si}) and other independent variables (X_{Si}):

(3.5) $V_{Si} = f(Q_{Si}, X_{Si}).$

By requiring a high degree of correspondence between the study site and the policy site, value transfer methods are less sensitive and robust to significant differences between the study site and the policy site. The transfer of the entire demand or benefit function to the policy site should increase the accuracy of benefit transfer estimates because the demand or benefit function could be tailored to specific characteristics of the policy site such as location, physical features, climate, and socio-economic variables. The demand or benefit function transfer is defined as

(3.6) $V_{Pj} = f_{S|P}(Q_{Pj}, X_{Pj})$

where the needed measure of the policy site (V_{Pj}) is derived from the study site demand or benefit function ($f_{S|P}(.)$) adapted to the characteristics of the policy site (Q_{Pj} and X_{Pj}). The demand or benefit function at the study site should be adjusted to the difference between study site variables (Q_{Si} and X_{Si}) and policy site variables (Q_{Pj} and X_{Pj}) before transferring to the policy site since it may be the case that study site and policy site variables are not directly comparable. For example, a study site variable may be a continuous variable (years of education), but the same variable for the policy site could be discrete (categorical based on the degree acquired). In this case, the effect (regression coefficient from the study site function) of this variable on benefit measure obtained from the study site should be adjusted to reflect different format of the same socio-economic variable, education. By adjusting the study site function to the policy site context, demand or benefit function transfer provides a tailored benefit estimate for the policy site although the statistical relationships (regression coefficients) between the benefit estimate and relevant independent variables at the study and policy sites are assumed to be the same.

Meta-Regression Analysis Benefit Function Transfer. Meta-regression analysis benefit function transfer is based on the statistical summarization of relationships between benefit estimates and the detailed characteristics of valuation studies. Meta-regression analysis typically uses summary statistics data from the existing valuation studies such as benefit estimates, the characteristics of population and site, environmental commodity, and detailed valuation methodologies. Meta-regression analysis benefit function transfer is defined as

$$(3.7) \quad V_S = f(Q_S, \overline{X}_S, M_S) \qquad \text{(Meta-regression function)}$$

$$(3.8) \quad V_{Pj} = f_{S|P}(Q_{Pj}, \overline{X}_{Pj}, M_{Pj}) \qquad \text{(Transferred function)}$$

where Q is a vector of quantity/quality variables, X is a vector of socio-economic variables and site characteristics (e.g., income, age, education, and gender; geographic location, accessibility, and site quality), and M is a vector of methodological variables (e.g., valuation method, modeling format, and functional form). Benefit transfer estimates tailored to the policy site can also be obtained by adapting the meta-regression analysis function to specific characteristics of the policy site. Meta-regression coefficients should also be adjusted ($f_{S|P}$) to reflect the difference in the variable format between the study sites and the policy site, let alone possible adjustment of the same variable from multiple study sites to format difference to make it more amenable to meta-regression analysis. Meta-analysis can statistically explain the variation of benefit estimates across many valuation studies with different valuation method, survey mode, geographic location, time, and other study-specific features. These methodological and study-specific attributes are not independent variables in individual valuation studies; therefore, meta-regression analysis may only be able to identify individual effects of these variables on benefit estimates.

Validity Test. Existing empirical studies trying to compare the validity of value transfer estimates and function transfer estimates using equation (4), suggest that function transfer methods are more accurate than value transfer methods (Loomis 1992; Parsons and Kealy 1994; Loomis et al. 1995; Downing and Ozuna 1995; Kirchhoff, Colby, and LaFrance 1997; Brouwer and Spaninks 1999; Rosenberger and Loomis 2001). The improved accuracy of function transfer estimates is mainly due to the ability of adapting benefit functions to specific attributes of the policy site. Some studies suggest that meta-regression analysis transfer performs better than demand or benefit function transfer (Brouwer and Spaninks 1999; Rosenberger and Phipps 2001; VandenBerg, Poe, and Powell 2001). Rosenberger and Phipps (2001) demonstrate that study-specific site characteristics that are invariant within an individual study, but vary across different studies, can explain much of the error associated with benefit transfer methods; however, these effects could be controlled for with the increased accuracy of benefit transfer estimates if meta-regression analysis benefit function transfer is performed.

Benefit Transfer Application: Marine Recreational Fishing

The importance of and need for efficient and effective management programs for recreational fisheries as a renewable resource has been recognized to accomplish an economically and biologically sustainable level of harvest (i.e., catch and keep). According to the NMFS, in 2001 there were 15 to 17 million marine recreational anglers, taking over 86 million fishing trips and harvesting over 189 million fish weighing almost 266 million pounds (over 254 million fish were caught and released) in 2001. Marine recreational fishing has a significant economic impact on coastal areas, non-coastal areas where market goods related with this activity are produced, and available fish stocks. To develop fishery management plans and evaluate the impacts of resulting regulations on marine recreational anglers and fisheries, the NMFS collects data on the number and socio-economic characteristics of participants; total number of fishing trips; and the number, size, and weight of recreational harvest through its MRFSS.

Benefit transfer techniques could be applied using the MRFSS data to estimate non-market recreational benefits associated with marine recreational fishing in the coastal areas of the United States. As mentioned earlier, an appropriate protocol or systematic adjustment process in applying benefit transfer methods could be dependent on the context being considered although a general guideline may exist. As an empirical attempt to evaluate the validity of benefit transfer estimates in the context of marine recreational fishing, two primary non-market valuation studies, with a high level of correspondence in many aspects, are considered to be potential candidates of both the study site and the policy site.

Case Studies

Both Hicks et al. (1999) and Haab, Whitehead, and McConnell (2001) attempt to estimate economic values associated with access to fishing sites (willingness to pay for the opportunity of marine recreational fishing in a particular area) and the quality of marine recreational fishing (willingness to pay for the better opportunity of catching fish) using the data from the MRFSS combined with the Add-On MRFSS Economic Survey (AMES) collected in 1994 in Northeast and 1997 in the Southeast, respectively. Some important features of both valuation studies are summarized in Table3.1.

Using the same data source although in different years and regions, two studies attempt to evaluate the identical recreation activity, single-day fishing trips for saltwater sport fishing, which enables us to focus benefit transfer application on the clearly defined context. Hicks et al. (1999) examines economic values associated with marine recreational fishing in the Northeast region (ten coastal states from Virginia through Maine) of the United States in 1994, while Haab, Whitehead, and McConnell (2001) examines the same values in the Southeast region (seven coastal states from North Carolina through Louisiana) in 1997. Mode/species-site choice models of marine recreational fishing behavior are estimated with a two-stage nested random utility model (NRUM) assuming that the angler first determines one of 15 possible mode-species combinations from three fishing modes (private/rental boat fishing, charter/party boat fishing, and shore fishing) and five target species (big game, small game, bottom fish, flat fish, or no/other target), and then chooses a specific fishing site conditional on the choice of mode-species combination. The choice structure of two-stage decision process is illustrated in Figure 3.3. The only difference in the choice structure is the number of alternative county-level fishing zones in the second stage of decision process; 63 and 70 alternative zone sites in the Northeast and Southeast studies respectively.

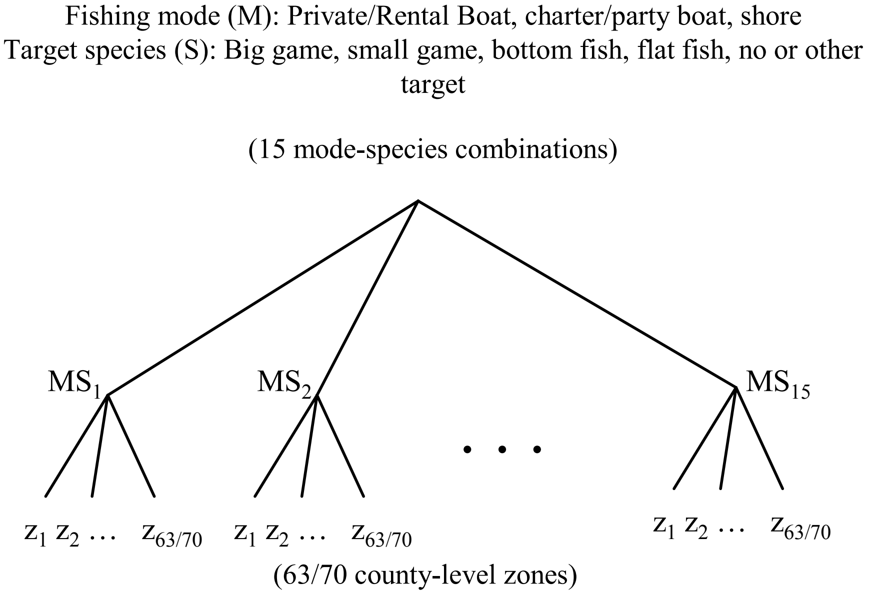

Figure 3.3. The Choice Structure in NRUM

The explanatory variables used in the estimation of a two-stage nested random utility model include travel cost (explicit and opportunity costs), travel time (if no opportunity cost is involved), the number of interview sites in an aggregated county-level zone (correction for aggregation), and historic harvest rate per trip by species, mode, and site (fish quality measure). One noticeable difference in their modeling is in the specification of mode-species specific inclusive values that will determine the probability of choosing mode-species combination. Haab, Whitehead, and McConnell (2001) allows the inclusive value parameter to differ between four targeted species (big game, small game, bottom fish, and flat fish) and other non-targeted species by recognizing possibly different substitution patterns, while Hicks et al. (1999) uses a single inclusive value parameter for all targeted and non-targeted species.

Welfare measures estimated in both studies are economic values associated with access to fisheries and the quality of fishing experience. They estimate the value of access to fisheries in each state as the mean willingness to pay per trip for site access to a particular state across waves assuming all fishing sites in the state and/or wave are closed under current choices of all anglers. As a proxy for the quality of fishing experience in each site, they use average harvest (catch and keep) per trip by averaging actual kept catches by wave, fishing mode, target species, and site over the past five-year period (historic harvest rate). To measure the marginal willingness to pay for a one-unit increase in historic harvest rate by state and species, historic harvest rate per trip is increased by one unit at all fishing sites in a particular state and species for all anglers. The standard welfare measure from a nested logit random utility model that is linear in travel cost compares the expected maximum utility obtainable under two different policy regimes and then converts that to a money metric by normalizing with the marginal utility of income. The willingness to pay (WTP) for a change in policy situation from 0 to 1 is:

(3.9) $\mathrm{WTP} = (V^0 - V^1) / \beta_1$

where V^0 and V^1 are the expected maximum utility levels under each policy regimes and β_1 is the coefficient of travel cost variable (negative marginal utility of income).

Plans for Benefit Transfer Practice

Function transfer method will be conducted with two primary studies (Hicks et al. 1999; Haab, Whitehead, and McConnell 2001) described in the previous section by transferring the entire benefit function, equation (3.9), assuming that necessary conditions for successful benefit transfer application are mostly satisfied. A comparison of critical attributes of two studies in Table 3.1 definitely implies that the two studies are very similar, almost identical in many features. Any state included in the Northeast (NE) and Southeast (SE) studies could be either the study site or the policy site in practicing benefit method.

<u>Scenario 1: Inter-region Transfer</u>. With inter-region transfer we transfer either the Northeast or Southeast region's welfare function to any state (the policy site) in another region by plugging this state's, along with the other states included together, values for independent variables into the welfare function transferred from the study site ($\mathrm{WTP}_{S,\,NE}$ or $\mathrm{WTP}_{S,\,SE}$). If the Southeast (the study site) region's welfare function is transferred to a particular state in the Northeast region (the policy site), inter-region benefit function transfer, by adopting equations (3.3), (3.5), and (3.6), can be described as

(3.10) $\mathrm{WTP}_{S,\,SE} = (V^0_{SE} - V^1_{SE}) / \beta_1^{SE}$ (Study site value function)

(3.11) $\mathrm{WTP}_{P,\,NE} = (V^0_{SE|NE} - V^1_{SE|NE}) / \beta_1^{SE|NE}$ (Transferred function)

(3.12) $\delta_{SE, NE} = WTP_{T, SE} - \mathit{WTP}_{P, NE}$ (Convergent validity)

where $WTP_{S, SE}$ is the estiated value from the study site (SE), $WTP_{P, NE}$ is the needed measure for the policy site (NE's state), and $\mathit{WTP}_{T, NE}$ is the assumed "true value" derived from the primary study at the policy site. The expected maximum utility levels (V^0 and V^1) under each policy regimes and the coefficient (β_1) of travel cost variable at the study site (SE) could be adjusted to the characteristics of the policy site (NE). As a convergent validity test of benefit transfer estimates ($\mathit{WTP}_{T, SE}$), they can be compared with the value estimates ($\mathit{WTP}_{P, NE}$) that already exist at the policy site and are assumed to be true values we are trying to estimate by applying a benefit transfer method (equation (3.12)).

Scenario 2: Pooled Data Transfer. First, the data sets from the Southeast and Northeast regions are combined together, which enables us to estimate welfare measures using a bigger data set with extended choice set definition. We can estimate a two-stage nested random utility model using the same choice structure as existing case studies and resulting welfare measures (values of site access and fishing quality improvement) for all 17 states and any sub-region of interest (e.g., NE, SE, Gulf Coast, South Atlantic, Mid-Atlantic, and New England regions). Second, the data set for the policy site (a particular state or region) is subtracted from the pooled data set to construct the data set for the study site, and then we can derive study site estimates of welfare measures using the same methods as in the first step. Third, the entire welfare function at the study site (pooled without the policy site) is transferred to the policy site with its values for the independent variables resulting in benefit transfer estimates of welfare measures for the omitted policy site. Finally, tests of convergent validity could be performed by comparing these transferred welfare estimates with the assumed "true values" obtained from the first step using all of the pooled data. Pooled data benefit function transfer can be defined as

(3.13) $WTP_{S, Pool\text{-}1} = (V^0_{Pool\text{-}1} - V^1_{Pool\text{-}1}) \,/\, \beta_1^{Pool\text{-}1}$ (Study site value function)

(3.14) $WTP_P = (V^0_{Pool\text{-}1|P} - V^1_{Pool\text{-}1|P}) \,/\, \beta_1^{Pool\text{-}1|P}$ (Transferred function)

(3.15) $\delta_{Pool\text{-}1, P} = WTP_{T, Pool\text{-}1} - \mathit{WTP}_{P, Pooled}$ (Convergent validity)

where $WTP_{S, Pool\text{-}1}$ is the estimated value for the study site (pooled sites except for the policy site), WTP_P is the needed measure for the policy site (omitted from pooled data), and $\mathit{WTP}_{P, Pooled}$ is the assumed "true value" for the policy site derived from pooled (including the policy site) data estimation process. The expected maximum utility levels (V^0 and V^1) under each policy regimes and the coefficient (β_1) of travel cost variable at the study site (pooled sites except for the policy site) may have to be adjusted to the characteristics of the policy site. Convergent validity of benefit transfer estimates ($WTP_{T, Pool\text{-}1}$) can be evaluated by comparing these values with estimated values ($\mathit{WTP}_{P, Pooled}$) that are obtained from the pooled estimation (including the policy site) and are assumed to be the true values we are trying to estimate by applying a benefit transfer method (equation (3.15)).

Table 3.1. Summary of Two Valuation Studies

	Hicks et al. (1999)	*Haab, Whitehead, and McConnell (2001)*
Recreation activity	Saltwater sport fishing: one day trip	Saltwater sport fishing: one day trip
Data	MRFSS: 1994 Northeast (NE)	MRFSS: 1997 Southeast (SE)
States included	VA, MD, DE, NJ, NY, CT, RI, MA, NH, & ME	NC, SC, GA, FL, AL, MS, & LA
Estimation technique	2-stage nested random utility model (NRUM)	2-stage nested random utility model (NRUM)
Welfare measures	WTP for site access to a state across waves[1] (3~6) & for a 1 unit ↑ in historic harvest rate by state and 4 species groups	WTP for site access to a state across waves (2~6) & for a 1 unit ↑ in historic harvest rate by state and 4 species groups
Choice set definition	3 fishing modes-5 target species & 63 county-level zone sites	3 fishing modes-5 target species & 70 county-level zone sites
Independent variables of indirect utility function	Trip cost & time, # interview sites in a aggregate county zone, & site-specific historic harvest per trip for species group	Trip cost & time, # interview sites in a aggregate county zone, & site-specific historic harvest per trip for species group

[1] A wave is a two-month period: Jan/Feb (wave1) ~ Nov/Dec (wave6).

The way you show this table, I was expecting results here. Maybe merge this with another chapter (maybe chapter 5)??

Chapter 4. Function Transfer of Random Utility Models: Using Monte Carlo Analysis to Analyze the Importance of the Scale Parameter

With the notable exceptions of Parsons and Kealy (1992); Downing and Ozuna (1996); and Morrison et al. (2002), investigation of the validity of function transfer in the RUM framework has not been widely researched. These studies offer important evidence concerning function transfer in RUMs but do not differentiate between properties of RUM models as compared to continuous trip demand or bid functions. In this chapter, we show that the usual notions of validity and reliability as applied to continuous trip demand or bid functions are not complete with respect to RUM models. Because in RUM models the estimated behavioral parameters are not identifiable independently of the scale parameter, a function transfer by definition carries with it information about parameter estimates and the error structure of the population at the study site. Because of this, stronger conditions must be met for valid function transfer in RUM's.

Consider an SP study offering hypothetical recreation trips to respondents essentially mimicking the choice problem of RP discrete trip choice. To apply the results of this study for policy analysis requires that the stated preference parameters be applied to some real world baseline (see Morrison et al., for a discussion of defining baselines for passive use welfare measurement using SP models). One approach simultaneously estimates the study and policy models to "calibrate" the parameters from one dataset to another by accounting for differences in the scale parameter (first suggested by Ben-Akiva and Morikawa (1992) and applied to environmental valuation by Adamowicz et al (1994)). The calibrated parameters can then be applied to the policy explanatory variables providing a meaningful baseline for welfare measurement[1].

Another approach, the uncalibrated function transfer, applies the SP parameters directly to the explanatory variables describing the policy region baseline. This latter approach, clearly analogous to the function transfer approach used elsewhere in the literature (see, for example, Kirchoff et al., Loomis (1992), Loomis et al. (1995), and Brouwer and Spaninks (1999)), avoids the effort required to estimate the policy model, but fails to control for differences in the relative scale parameter, and hence differences in the variance of the error structure, across the two datasets.

Even if recreators at the study and policy regions have identical preferences for site characteristics, it may be possible that the unobservable components of their site choice may lead to biased welfare estimates when conducting an uncalibrated function transfer. If this is indeed the case, then successful function transfer in a RUM context will require conditions on both the estimated behavioral parameters and the variance of the error terms across the two regions also be equal.

The Model

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[1] Swait, Louviere, and Williams (1994) have also suggested a sequential approach for transferring SP parameter to RP data. This approach requires that alternative specific constants be recovered from the RP data and then transfer the SP coefficients to the RP data and augmenting only by the RP estimated alternative specific constants.

Consider a simple discrete choice model of recreation demand. An individual is assumed to choose from among a set (S) of feasible recreation sites. This choice is contingent upon a vector of set of site-specific characteristics ($X_{i,1xn}$). For recreational fishing applications, common examples of data used in these models include the cost of accessing a site and quality indicators at the site. These quality indicators may include environmental amenities (water quality and biological catch conditions) and other site amenities (e.g. boat ramps and facilities)[2].

In the model, individuals are assumed to choose the alternative yielding the highest level of utility. This choice depends on the individual's preferences (B_{1xn}), observable site-specific factors that influence that choice ($X_{i,1xn}$), and site-specific factors associated with each alternative that are not observable to the researcher denoted by ε_i. Define an individual's indirect utility function for alternative i as

$$V(\mathbf{B},\mathbf{X}_i,\varepsilon_i) = v(\mathbf{B},\mathbf{X}_i) + \varepsilon_i \tag{4.1}$$

From the individual's perspective, alternative i will be chosen if

$$V(\mathbf{B},\mathbf{X}_i,\varepsilon_i) > V(\mathbf{B},\mathbf{X}_j,\varepsilon_j)\ \forall i, j \in S \tag{4.2}$$

The researcher can't observe all of the components of the individual's choice represented by (4.2). To enable the estimation of the preference parameters, B, we employ the commonly used assumption that the unobservable portions of the individual's indirect utility function are identically and independently distributed as $\text{GEV I}\left(0,\frac{\pi^2}{6(\lambda)^2}\right)$. Employing this assumption and linearizing the observable portion of the indirect utility function allows the probability that an individual chooses alternative i to be written as

$$P(i\,|\,\lambda,\mathbf{B},\mathbf{X}) = \frac{e^{\lambda(\mathbf{X}_i'\mathbf{B})}}{\sum_{j\in S} e^{\lambda(\mathbf{X}_j'\mathbf{B})}}, \tag{4.3}$$

where the scale parameter λ (a scalar value) enters the probability statement and is inversely related to the variance of the error term described in (4.1).

Scale parameters in RUM models

An artifact of equation (4.3) that is commonly understood yet rarely explicitly stated in applications of the random utility model is that the estimated preference

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[2] Additionally, individual and site specific data such as whether the individual has visited the site in the past or other data collected over the respondent (such as preferences or knowledge about sites and site amenities) are typically included in these studies.

parameters can not be identified independently of the scale parameter. Louviere, Henscher, and Williams state

> "The implication of these observations about the behavior of the scale parameter is that it plays a role in choice models that is unique compared to more familiar statistical models such as OLS regression. That is, the model parameters and the characteristics of the error terms are intimately (even inextricably!) related."

In this context, it is important to think carefully about what is meant by function transfer. The intuition behind function transfer methods is similar behavioral parameters at the policy and study site will likely yield valid and reliable benefit estimates. This scale parameter adds another difficulty to benefits transfer due to the variance of the error structure.

For RUM models it is usual to assume for any given sample that all of the errors for the model are drawn from the same distribution. With this assumption in place it is not possible to recover behavioral parameters (B) independent of the scale parameter since the scale parameter is not identifiable. Letting our estimated vector of parameter estimates from a region i be written as $\boldsymbol{\beta}^{i} = \lambda^{i}\mathbf{B}$, notice that the vector of estimated parameters, $\boldsymbol{\beta}^{i}$, is equal to the vector of underlying behavioral parameters, $\mathbf{B}^{i}$, multiplied by the scale factor λ^{i}, a scalar value. Consequently, transferring the estimated coefficients from study to policy regions entangles information on preferences with information on the error term from the estimation.

Adamowicz et al. (1994) have shown that the scale parameter likely differs across studies. The implications of this relationship for the validity of an unscaled function transfer (where we do not account for differences in the scale factor) is that a stronger condition is needed. This condition, $\boldsymbol{\beta}^{S} = \boldsymbol{\beta}^{P}$, implies that $\lambda^{S}\mathbf{B}^{S} = \lambda^{P}\mathbf{B}^{P}$. Notice that this stronger condition does not ensure that the underlying behavioral parameters (B) are equal unless the scale factors are also equal across the two datasets. However, without estimating econometric models at both the study and policy sites, these issues cannot be explored. Consequently, there is an open question as to the performance of unscaled transfer when analysts are unable to estimate the policy region model and, instead, must rely on the study region function.

Monte Carlo Data Generation Process

We use a Monte Carlo approach to investigate the importance of the scale parameter in benefit function transfer. In this way, choices are generated consistent with equation (4.3) where the vectors B, X, and ε are known *a priori*. With this framework, we investigate whether parameters and welfare measures systematically vary with the variance of the unobservable portion of the site specific error terms (ε). For each randomly drawn set of data, we estimate a study site model and a policy site model and recover welfare measures for non-marginal changes in trip quality and site closures using each set of parameters.

Table 4.1 outlines the five transfer scenarios considered in the chapter. In the first scenario, which termed RPRP transfer we assume that a random utility model of recreation demand is conducted in a study region and that we are performing an unscaled function transfer to a policy region. This is analogous to function transfers of trip demand models. In the second scenario, termed RPSP, we transfer an uncalibrated function estimated from a stated preference experimental design to some policy site of recreation demand sites and recreators. We also consider the case where an RP study exists at the policy site, but some important parameter must be transferred from an SP study site (termed Partial RPSP). This illustrates the case where the state of the world as it exists in the real world does not provide enough variation to estimate the parameter. We also perform a simple value transfer assumed to have been calculated from an RP study site and transferred to the RP policy site. In the fifth transfer, we include "real" data from beach usage in Maryland. I term this transfer the MD Beach transfer. We assume that the actual choices made by the beach users were not observed, but it was feasible to collect data on travel costs and site characteristics for a random sample of people in Maryland. We then transfer a function from an RP study site, where the X matrix is generated at random.

Denote the vector B to be the "true" parameter vector describing the observable portion of an individual's indirect utility function given in (1) for the study and policy sites. We restrict this vector of parameters to be equal across the study and policy sites to illustrate the case where the underlying preference function in (1) is identical across the study and policy site. To implement the Monte Carlo experimental design, we do the following:

1. For each region for an assumed sample of 1000 individuals, generate the vector X of independent variables[3].
2. Generate the vector d_{Sx1} of observed choice. Elements of d defined as follows:
    a. For a given scale parameter for the policy site (λ^P) or the study (λ^S) site models, generate the vector ε as identically independently distributed $\text{GEV I}\left(0, \frac{\pi^2}{6(\lambda)^2}\right)$.
    b. Using equation (1) set d_i=1 if $V(\mathbf{B}, \mathbf{X}_i, \varepsilon_i) > V(\mathbf{B}, \mathbf{X}_j, \varepsilon_j)\ \forall i, j \in S$; otherwise, set d_i=0.
3. Using d and X estimate the discrete choice model given by equation (3).
4. Recover parameter estimates $\lambda\mathbf{B}$, and estimate welfare changes for three scenarios: (a) a 75% decrease in site quality at one site, (b) a 25% decrease in sight quality at one site, and (c) closing one site[4].

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[3] For the MD Beach data transfer, we did not generate the data for the policy site but rather used a real dataset collected in 1983.

We conduct steps (1)-(4) for both the policy and study site models. We then perform a benefits transfer by calculating the welfare measure defined over the vector of X from the policy site model and the estimate of $\lambda\boldsymbol{\beta}$ from the study site model. This step can be denoted as

5. Compare $\text{CV}(\boldsymbol{\lambda}^{\text{S}}\boldsymbol{\beta}^{\text{S}},\mathbf{X}^{\text{P}})$ and $\text{CV}(\boldsymbol{\lambda}^{\text{P}}\boldsymbol{\beta}^{\text{P}},\mathbf{X}^{\text{P}})$ to examine the precision of benefits transfer model.
6. For each pair of scale parameters considered (λ^{S} and λ^{P}) we repeat the steps above 1000 times.

For a given scale factor at the study site (λ^{S}) we examine the relative performance of the function transfer method for three alternative scale parameters at the policy site (λ^{P}). Consequently, in our study we perform 9 (unique pairs of scale parameters) x 1000 (draws of the error vectors at each of the policy and study sites for a given pair of scale parameters) x 2 (two models, one at the study and one at the policy site) separate regressions. We perform this analysis for each of our five transfer scenarios.

A metric for comparing the performance of benefits transfer

In the monte carlo analysis we employ, the underlying behavioral parameters (B) are assumed to be equal. We do however vary the scale factor. Consequently, in evaluating the performance of the benefits transfer, we focus on the estimated welfare one would have gotten by estimating a model at the policy site (assumed to be the "truth" and denoted by $\text{CV}(\boldsymbol{\lambda}^{\text{P}}\mathbf{B}^{\text{P}},\mathbf{X}^{\text{P}})$) with an estimated welfare obtained by applying the study function to the policy site data (denoted by $\text{CV}(\boldsymbol{\lambda}^{\text{S}}\mathbf{B}^{\text{S}},\mathbf{X}^{\text{P}})$). For each set of scale parameters, we calculate the ratio $\frac{\text{CV}(\boldsymbol{\lambda}^{\text{S}}\mathbf{B}^{\text{S}},\mathbf{X}^{\text{P}})}{\text{CV}(\boldsymbol{\lambda}^{\text{P}}\mathbf{B}^{\text{P}},\mathbf{X}^{\text{P}})}$. We do this numerous times to estimate some distributional properties of the likely bias of the benefits transfer. If this ratio is equal to 1 then the transfer method performs perfectly relative to a welfare measure obtained by conducting a full valuation study at the policy site. If the ratio is greater (less) than 1, then the transfer method is overestimating (underestimating) the welfare change at the policy site.

Note that the ratio depends on the scale parameters from the two datasets, and the underlying behavioral parameters estimated from the choice model. Since these are linked from estimation, the difference in scale parameters in a probabilistic sense can have two effects. First, an increase in the scale parameter, *ceterus paribus*, will decrease the magnitude of the welfare change. As the scale parameter increases, the variance of the error term decreases. Therefore more information is embodied in the observable attributes of the conditional utility functions and in the estimated parameters. Consequently, the estimated compensation is lower than would be required with a lower scale parameter (and higher variance). Second, lower (higher) scale parameters are

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[4] The equation for compensating variation measure employed here and denoted by the function CV(.,.) can be found in Hanemann (1999).

associated with higher (lower) variance in the estimated parameters. So it is possible for a given draw of the error vector, that the scale effect could be overshadowed by an estimated parameter vector that is imprecise (which may lead to a lower or higher estimate of compensating variation). Consequently, the scale factor in RUM models can affect both the reliability and validity of benefit function transfer even when behavioral parameters are assumed to be the same across respondents. By repeatedly drawing error vectors under such circumstances, We are able to probabilistically compare the performance of the policy model to the transferred study model.

Results

In Table 4.1, we outline all of the experiments performed in the paper. For each experiment, we indicate the source and structure of the study and policy data and the assumed preference function for each population. This Table will likely serve as a useful reference as the results discussion unfolds. In Table 4.2, we present the estimated mean parameter estimates (and upper and lower 95% out of 1000 multinomial logit regression models) for the first experiment, the RP to RP transfer. This table illustrates the problems associated with transferring RUMs. While all behavioral parameters generating choices are assumed to be equal across the policy and study regions, the scale parameter influences mean estimates (and the size of confidence intervals). To appreciate the impact of the scale parameter on the estimated parameter vector, we will discuss three models (however, all of the estimates in the Table follow the same patterns with regard to the scale parameter). First, consider the case where the policy and the study scale parameters are both 1. In this case, the estimated parameters coincide with the truth (with minor differences). Since the estimated parameters are equal to the vector of underlying parameters times the scale parameter, these results are consistent with the multinomial logit model.

Next consider the case where the policy scale is 1 and the study scale is 5- denoted by {1,5}. Notice that the estimated parameters for the study region are five times that of the policy region. Further, notice that the confidence regions for the parameters at the policy sites (roughly +- 22%) are larger than those at the study site (roughly +-15%). Consequently, the higher scale parameter (lower variance) at the study region leads to (1) larger parameter estimates and (2) more precise estimates of those parameters than at the study site, even though the preferences of individuals are assumed to be identical up to an unobservable scalar value. The opposite case, where the policy scale is five times that of the study region, shows that the parameters at the policy region are five times that of the policy region, and the confidence intervals at the policy site are tighter than at the study site. These results, however, do not show how differences in the scale parameter in the two regions may affect welfare estimates.

To compare the performance of the transfer method we compare welfare estimates across five scenarios. The first transfer scenario, termed RPRP, represents the case where an unscaled function transfer based on a RP study is conducted from some study site to the policy site. Table 4.3, contains the results of this transfer by analyzing the ratio $\frac{CV(\boldsymbol{\lambda}^S\boldsymbol{\beta}^S, \mathbf{X}^P)}{CV(\boldsymbol{\lambda}^P\boldsymbol{\beta}^P, \mathbf{X}^P)}$. Table 4.3 summarizes the results of 1000 benefit transfers for each pair of scale parameters. All data and choices were generated with the methodology described above. In the table, we examine four different welfare measures: a 75% quality decrease at one of the sites (labeled site 1), a 75% quality decrease at all sites, a 25% quality decrease at site 1, and a closure of site 1. Confidence intervals were constructed by finding the values within the 95% range of the 1000 estimates by discarding the upper and lower 2.5% of the sample. Results show that two of the welfare change scenarios lead to significantly different welfare measures when performing the function transfer. Large quality changes at site 1 (a 75% decrease at site 1) indicate a significant bias of as much 75% overestimate of welfare change when the study scale parameter exceeds the policy scale parameter. The opposite occurs when the study scale parameter is smaller than the policy scale. Closures lead to the largest bias in the function transfer method with an overestimate of as high as 106% when the study scale exceeds the policy scale. Of course when the scale parameters are equal, the function transfer performs with near perfection.

It is important to note that these trends do not occur when the quality change is relatively small or when the quality change occurs at all sites. The first situation is easy to explain- the scale factor matters more when non-marginal changes occur and for changes that do not proportionately impact sites. Consequently, we should see smaller differences due to the scale factor when analyzing smaller quality changes.

We also conduct a simulated transfer of a stated preference study at the study site to a revealed preference policy site. We design a stated preference survey to effectively mimic the revealed preference choices of recreation sites at the policy site (for examples, see Adamowicz et al. (1994)). Again, we analyze a function transfer of preferences as quantified by the stated preference study at the study site to the revealed preference data at the policy site. We do this without calibrating for differences in scale. The experimental design employed is a fractional factorial design allowing for the estimation of main effects. The design is blocked with 10 sets of 10 questions. Like the revealed preference choice at the policy site, we design an experiment in which respondents choose sites based upon travel cost (ranging from $5 to $200) and site quality (from 1 to 10). Each simulated respondent was administered 10 questions and there were 1000 responses in our simple. Since respondents' choices are assumed to be independent, we employ the logit model in the analysis.

The results, reported in Table 4.4, are similar to those reported for the RP/RP transfer. The scale factor did play a large role in biasing the validity of the transfer when the scale parameter was appreciably different across the study and policy sites. We note that for the handful of studies that have estimated a relative scale parameter (and have found the RP scale parameters tend to be smaller than SP), the scenarios with the scenario pairs {2,1}, {5,1} and {5,2} coincide most closely with these findings. In these cases, we find that the benefit function transfer overstates the welfare loss by as much as 70% for the quality decrease at site 1 and by as much as 100% for the closure of site 1. It is also interesting to note that for smaller quality decreases at site 1 (or for decreases at all sites) the function transfer provides a reasonable approximation to the "true" welfare measure.

In Table 4.5, we present the results for the partial transfer method, where a stated preference study at the study site has a third attribute of interest (call it z) about site quality that is not quantifiable at the policy site. Perhaps the preference parameter for the attribute can not be recovered at the policy site because of a lack of variability across sites or because of collinearity with one of the other attributes. Consequently, the parameter can only be recovered using the stated preference survey.

We amended the stated preference design discussed above to include a third attribute. The partial transfer method we employ uses results at the policy site (which are assumed to be recoverable) to scale the parameter recovered at the study site. As a baseline for comparison, we assumed that the analyst at the policy region knows that the parameter on z is -10 times the parameter estimated on travel cost. That is, if a study was conducted at the policy region, then welfare changes could be measured using the above relationship. In a transfer setting, it is not likely that analysts have priors regarding the z parameter. However, if the assumption concerning equal behavioral parameters is maintained, then information from the policy and study sites can be used to scale the unknown parameter appropriately. Specifically, we approximate the parameter of interest at the policy site ($\hat{\beta}_z^P$) by using the relationship

$$\hat{\beta}_z^P = \frac{\beta_z^S}{\beta_{TC}^S} \beta_{TC}^P$$

which simply assures that the ratio of the quality parameter and travel cost parameter are equal across the two datasets. Note, this method requires that we have estimated the travel cost parameter (or some other parameter common across the two studies) at the study site.

This method effectively calibrates the quality parameter to the RP data. So long as the underlying preference parameters are the same (assumed) then the only other source of error in this transfer method is associated with the relative scale parameter (inversely related to the scale parameter) in the two datasets. Therefore, we would expect that as the scale parameter at the study site is high relative to the policy site (which corresponds to most studies which estimate the relative scale parameter), this transfer methods performs very well. This is indeed the case as evidenced by Table 4.5. The confidence intervals do tighten as the scale at the study site (the stated preference study) increases.

Results using a "real" policy region dataset

We had some concern that the above results due to policy and study data that of respondents randomly distributed across space choosing between sites having random quality characteristics. Because of this, we sought to replicate my results using a real policy site to which a function would be transferred. We apply the above model to data a policy region consisting of real sites and amenities- Chesapeake Bay beach usage by Marylanders in 1984 (data described in Bockstael et. al. (1988)). Information was collected on participants at publicly accessible beaches along the western shore of Chesapeake Bay in Maryland. Recreators are assumed to choose beaches based upon the travel cost, a water quality variable capturing the level of fecal coliform present at the beach, and an index of beach amenities. We estimate this model using the Maryland beach data to obtain the “truth” given in Table 1. We then perform the Monte Carlo simulation across the study and policy sites as previously described.

Results across the five transfer methods we investigate with the Maryland data largely tell the same story- that differences in the scale parameter does influence the validity of benefits transfer methods when the study and policy sites and preferences are, by design, unequal. However, we find that differences in the scale parameter don’t always lead to biased transfers. Consider a 75% decrease in site quality that affects all sites in the Bay. Table 6 shows that, even for such a large quality change, there is no statistically significant bias associated with the transfer. However, policies that impact one geographical subset of the Bay (the sites north of and including Annapolis, MD are affected), show that for smaller changes (a 25% reduction in site quality) the uncalibrated function transfer technique is biased by as much as 161%. For larger changes, such as a 75% decrease in site quality or a closure of these sites, this bias increases to as high as 177% and 328% respectively.

Conclusions

With the need for timely policy guidance in resource management and damage assessments, the use of benefit function transfer is likely to continue. However, with recent advances in stated preference techniques for environmental valuation, and movement in the literature away from continuous trip demand toward discrete choice models of recreation demand, the functions likely to be transferred will come from RUM models. In this chapter, through the use of controlled experiments, we investigate the performance of function transfer in RUM models.

The usual notions of validity and reliability as applied to continuous trip demand or bid functions are not complete with respect to RUM models. Because in RUM models, the estimated behavioral parameters are not identifiable independently of the scale parameter, a function transfer by definition carries with it information about both the parameter estimates and the error structure of the population at the study site. Because of this, stronger conditions must be met for valid function transfer in RUMS. In this paper the properties of the RUM function transfer and the scale parameter are investigated.

We show that the scale effect, when one transfers an uncalibrated function from a study to policy region, can create significant bias as compared to what would have been recovered had a study been conducted at the policy region. We do this by restricting preferences at the policy and study region to be equal up to unobservable error terms assumed to be consistent with RUM models. We then use monte-carlo analysis to alter the scale parameters for the study and policy regions. By doing this we investigate how differences in the error structure across the two populations can impact an unscaled function transfer when the underlying preference parameters are known to be equal.

The results indicate that the scale effect influences the viability of function transfer in some cases but not others. For large non-marginal changes, and in particular, large changes that do not affect all sites in the choice set in the same way, we find the unscaled function transfer approach can introduce significant bias. We analyze this type of change using Chesapeake Bay beach user data and find that large impacts affecting the northern part of the Bay (whether they be quality changes or closures), can lead to bias as high as 300% when scale parameters across the study and policy sites differ. However, for quality changes, even large ones, affecting all sites symmetrically, we find the patterns of bias very much reduced and not statistically different from the true measure. Consequently, the results show that the uncalibrated function transfer approach can be useful in some cases (e.g. a program improving water quality bay-wide) but not in others (e.g. an oil spill affecting only the northern beaches). In all cases we find that if the unscaled parameters from the study site are equal to the policy site (that is, preference and scale parameters are equal), the function transfer method works very well.

Analysts attempting to transfer an existing random utility model to a policy region may have priors concerning the likely magnitude of the scale parameters. For example if a stated preference study is being transferred to a policy region, then it is likely that the study scale parameter is larger (lower variance) than the scale one would have obtained at the policy region. With this in mind, these results can give analysts an idea of the likely direction and magnitude of bias[5].

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[5] Bayesian approaches offered by Aigner and Leamer (1984) and discussed in a benefits transfer context by Atkinson, Crocker, and Shogren (1992) and by Desvouges, Johnson, and Banzhaf (1999), may offer a way to scale the final estimate to account for prior knowledge. Additional research is needed in this area.

Table 4.1. Structure of Preferences and Models for Simulations

Transfer	Study Region	Policy Region
RP to RP	$TC_i^S \sim N(40,64), Q_i^S \sim N(6,1)$ $V(\beta^S, TC_i^S, Q_i^S) = -.05 * TC_i^S + .5 * Q_i^S + \varepsilon_i^S$ $\varepsilon_i^S \sim \text{GEV I}\left(0, \frac{\pi^2}{6(\lambda^S)^2}\right)$	$TC_i^P \sim N(40,64), Q_i^P \sim N(6,1)$ $V(\beta^P, TC_i^P, Q_i^P) = -.05 * TC_i^P + .5 * Q_i^P + \varepsilon_i^P$ $\varepsilon_i^P \sim \text{GEV I}\left(0, \frac{\pi^2}{6(\lambda^P)^2}\right)$
	Examine the ratio:	$CV(\hat{\beta}^S, TC^P, Q^P) \;/ CV(\hat{\beta}^P, TC^P, Q^P)$
SP to RP	TC_i^S, Q_i^S generated from experimental design having levels $TC_i^S : \{\$5, \$100, \$200\}$ and $Q_i^S : \{1,5,10\}$ $V(\beta^P, TC_i^P, Q_i^P) = -.05 * TC_i^P + .5 * Q_i^P + \varepsilon_i^P$ $\varepsilon_i^S \sim \text{GEV I}\left(0, \frac{\pi^2}{6(\lambda^S)^2}\right)$	$TC_i^P \sim N(40,64), Q_i^P \sim N(6,1)$ $V(\beta^P, TC_i^P, Q_i^P) = -.05 * TC_i^P + .5 * Q_i^P + \varepsilon_i^P$ $\varepsilon_i^P \sim \text{GEV I}\left(0, \frac{\pi^2}{6(\lambda^P)^2}\right)$
	Examine the ratio:	$CV(\hat{\beta}^S, TC^P, Q^P) \;/\; CV(\hat{\beta}^P, TC^P, Q^P)$
Partial SP to RP	TC_i^S, Q_i^S, V_i^S generated from experimental design having levels $TC_i^S : \{\$5, \$100, \$200\}$ and $Q_i^S : \{1,5,10\}$ and $V_i^S : \{1,5,10\}$ $V(\beta^S, TC_i^S, Q_i^S, V_i^S) = -.05 * TC_i^S + .5 * Q_i^S$ $+ .5 * V_i^S + \varepsilon_i^S$ $\varepsilon_i^S \sim \text{GEV I}\left(0, \frac{\pi^2}{6(\lambda^S)^2}\right)$	$TC_i^P \sim N(40,64), Q_i^P \sim N(6,1),$ $V_i^P = 2$ (assumed constant - can't identify) $V(\beta^P, TC_i^P, Q_i^P) = -.05 * TC_i^P + .5 * Q_i^P$ $+ .5 * V_i^P + \varepsilon_i^P$ $\varepsilon_i^P \sim \text{GEV I}\left(0, \frac{\pi^2}{6(\lambda^P)^2}\right)$
	Examine the ratios:	$CV(\hat{\beta}^S, TC_i^P, Q^P, V^P) / CV(\hat{\beta}_{TC}^P, \hat{\beta}_Q^P, -10 * \hat{\beta}_{TC}^P, TC_i^P, Q^P, V^P)$ $CV(\hat{\beta}_{TC}^P, \hat{\beta}_Q^P, \hat{\beta}_V^S * \frac{\hat{\beta}_{TC}^P}{\hat{\beta}_{TC}^S}, TC_i^P, Q^P, V^P) \;/\; CV(\hat{\beta}_{TC}^P, \hat{\beta}_Q^P, -10 * \hat{\beta}_{TC}^P, TC_i^P, Q^P, V^P)$

Table 4.1, continued.

Transfer	Study Region	Policy Region
Value	$TC_i^S \sim N(40,64), Q_i^S \sim N(6,1)$ $V(\beta^S, TC_i^S, Q_i^S) = -.05 * TC_i^S + .5 * Q_i^S + \varepsilon_i^S$ $\varepsilon_i^S \sim \text{GEV I}\left(0, \frac{\pi^2}{6(\lambda^S)^2}\right)$ $CV(\hat{\beta}^S, TC^S, Q^S)$	$TC_i^P \sim N(40,64), Q_i^P \sim N(6,1)$ $V(\beta^P, TC_i^P, Q_i^P) = -.05 * TC_i^P + .5 * Q_i^P + \varepsilon_i^P$ $\varepsilon_i^P \sim \text{GEV I}\left(0, \frac{\pi^2}{6(\lambda^P)^2}\right)$ $CV(\hat{\beta}^P, TC^P, Q^P)$
	Examine the ratio:	$CV(\hat{\beta}^S, TC^S, Q^S) / CV(\hat{\beta}^P, TC^P, Q^P)$
MD Beach	$TC_i^S, Q_i^S, Qindex_i^S$ collected in 1984 survey of Maryland Beach Participants $V(\beta^S, TC_i^S, Q_i^S) = -.044 * TC_i^S - .089 * Q_i^S + .379 * Qindex_i^S + \varepsilon_i^S$ $\varepsilon_i^S \sim \text{GEV I}\left(0, \frac{\pi^2}{6(\lambda^S)^2}\right)$	$TC_i^P \sim N(35.5,100), Q_i^P \sim N(11.83,5)$ $Qindex_i^P \sim N(1.25,.5)$ $V(\beta^P, TC_i^P, Q_i^P) = -.044 * TC_i^P - .089 * Q_i^P + .379 * Qindex_i^P + \varepsilon_i^P$ $\varepsilon_i^P \sim \text{GEV I}\left(0, \frac{\pi^2}{6(\lambda^P)^2}\right)$
	Examine the ratio:	$CV(\hat{\beta}^S, TC^P, Q^P) / CV(\hat{\beta}^P, TC^P, Q^P)$

Table 4.2. Parameter estimates and 95% Confidence Intervals for RP to RP transfer models*.

		Policy Region		Study Region	
Policy Scale	Study Scale	Travel Cost	Quality	Travel Cost	Quality
1	1	-0.050 (-.037,-.064)	0.504 (.615,.408)	-0.050 (-.037,-.063)	0.502 (.611,.400)
1	2	-0.050 (-.038,-.063)	0.504 (.610,.397)	-0.100 (-.085,-.120)	1.000 (1.156,.867)
1	5	-0.050 (-.039,-.064)	0.502 (.610,.402)	-0.252 (-.218,-.292)	2.52 (2.913,2.186)
2	1	-0.100 (-.084,-.120)	1.004 (1.164,.866)	-0.050 (-.038,-.063)	0.503 (.609,.400)
2	2	-0.101 (-.085,-.119)	1.007 (1.158,.866)	-0.100 (-.083,-.117)	1.003 1.159,.859
2	5	-0.101 (-.086,-.119)	1.005 (1.150,.859)	-0.252 (-.218,-.292)	2.515 2.911,2.199
5	1	-0.252 (-.216,-.296)	2.515 (2.877,2.209)	-0.050 (-.037,-.063)	0.504 .615,.403
5	2	-0.252 (-.208,-.293)	2.526 (2.912,2.195)	-0.100 (-.083,-.118)	1.001 1.154,.860
5	5	-0.252 (-.216,-.291)	2.515 (2.902,2.197)	-0.251 (-.216,-.292)	2.511 2.916,2.180

*The confidence intervals reported are for estimated parameters and do not ensure that the estimated parameters, for each of the thousand regressions, are significantly different from zero. Parameter significance tests for each regression show that all parameters are significantly different from zero 5% level.

Table 4.3. RPRP Transfer

Policy Scale	Study Scale	Quality Decrease 75% at site 1	Quality Decrease 75% all sites	Quality Decrease 25% at site 1	Close site 1
1	1	1.01	1.03	1.02	1.01
		1.42,.70	1.55,.63	1.49,.66	1.36,.72
1	2	.75	1.04	.93	.64
		.97,.54	1.47,.71	1.27,.66	.82,.47
1	5	.59	1.03	.87	.49
		.76,.45	1.42,.70	1.16,.64	.62,.39
2	1	1.36	1.00	1.10	1.58
		1.85,1.02	1.41,.69	1.54,.78	2.08,1.23
2	2	1.00	1.01	1.01	1.00
		1.20,.83	1.30,.78	1.26,.79	1.19,.84
2	5	.79	1.02	.94	.77
		.91,.68	1.24,.82	1.12,.78	.88,.66
5	1	1.73	.99	1.18	2.06
		2.25,1.31	1.35,.70	1.60,.86	2.61,1.60
5	2	1.27	.99	1.08	1.31
		1.48,1.11	1.23,.80	1.29,.91	1.52,1.14
5	5	1.00	1.00	1.00	1.00
		1.09,.91	1.15,.86	1.13,.88	1.09,.91

Table 4.4. RPSP Transfer

Policy Scale	Study Scale	Quality Decrease 75% at site 1	Quality Decrease 25% site 1	Close site 1
1	1	.99	1.00	.99
		1.26,.74	1.31,.72	1.25,.74
1	2	.74	.92	.63
		.94,.55	1.22,.67	.80,.48
1	5	.59	.87	.49
		.75,.44	1.14,.62	.61,.38
2	1	1.35	1.09	1.55
		1.57,1.13	1.30,.89	1.85,1.29
2	2	1.00	1.00	1.00
		1.15,.84	1.19,.83	1.15,.84
2	5	.79	.93	.76
		.90,.67	1.10,.78	.87,.66
5	1	1.70	1.16	2.02
		1.91,1.50	1.33,1.01	2.32,1.74
5	2	1.26	1.07	1.30
		1.41,1.14	1.20,.96	1.46,1.16
5	5	1.00	1.00	1.00
		1.08,.92	1.10,.91	1.08,.92

Table 4.5. Partial RPSP Transfer

Policy Scale	Study Scale	Quality Decrease 75% at site 1	Quality Decrease 25% site 1	Close Site 1
1	1	1.01	1.00	1.01
		1.22,.86	1.23,.85	1.19,.89
1	2	1.01	1.00	1.00
		1.23,.86	1.12,.88	1.04,.96
1	5	1.01	1.00	1.00
		1.20,.86	1.13,.87	1.07,.93
2	1	1.00	.99	1.00
		1.09,.92	1.09,.92	1.04,.96
2	2	1.00	1.00	1.00
		1.10,.92	1.08,.91	1.07,.95
2	5	1.00	1.00	1.00
		1.05,.95	1.09,.91	1.05,.95
5	1	1.00	.99	1.00
		1.05,.96	1.06,.94	1.02,.98
5	2	1.00	.99	1.00
		1.04,.95	1.05,.94	1.03,.98
5	5	1.00	.99	1.00
		1.05,.95	1.06,.94	1.03,.97

Table 4.6. MD Beach Data Transfer

Policy Scale	Study Scale	Quality Decrease 75% North	Quality Decrease 25% North	Quality Decrease 75% all sites	Close North
1	1	1.02	1.02	1.02	1.02
		1.42,.71	1.46,.68	1.49,.66	1.36,.72
1	2	.66	.68	.85	.49
		.87,.49	.94,.50	1.19,.60	.62,.37
1	5	.37	.40	.77	.24
		.50,.28	.54,.30	1.06,.57	.62,.37
2	1	1.58	1.53	1.23	2.10
		2.14,1.16	2.11,1.10	1.75,.84	2.66,1.63
2	2	1.00	1.00	1.01	1.00
		1.26,.79	1.28,.78	1.33,.75	1.21,.83
2	5	.57	.59	.92	.49
		.70,.46	.74,.47	1.18,.71	.57,.41
5	1	2.77	2.61	1.36	4.28
		3.77,2.04	3.66,1.87	1.95,.93	5.35,3.45
5	2	1.77	1.73	1.11	2.05
		2.23,1.44	2.23,1.37	1.49,.84	2.37,1.77
5	5	1.01	1.01	1.01	1.00
		1.26,.83	1.30,.81	1.35,.76	1.11,.89

Chapter 5. Applying Benefit Transfer Across Geographic and Temporally Disparate Data Collection Frames

In this chapter the results of original estimations in Hick et al. (1999) and Haab, Whitehead, and McConnell (2001) are compared with the results of benefit transfer estimations to evaluate the reliability and/or validity of benefit transfer technique in a marine recreational fishing environment with MRFSS data. Limitations and problems with current estimations and comparisons are addressed.

Model

A marine recreational angler is assumed to jointly choose species to target and fishing mode to use first, and then choose among mutually exclusive fishing sites based on their attributes (mode/species-site choice model). In this two-stage nested random utility model, the angler chooses utility maximizing mode-species choice among 15 available combinations from three modes (private/rental boat, party/charter boat, and shore fishing) and five species groups (big game, small game, bottom fish, flat fish, and others) at the fist stage. Conditional on the mode-species choice from the first stage, the angler then chooses utility maximizing fishing site (county-level zone).

If we denote alternative sites and mode-species combinations with j (1,…,63 (NE 1994) or 70 (SE 1997)) and sm (1,…,15) respectively, the indirect utility function of an arbitrary angler can be written as:

$$(5.1) \quad v_{jsm} = \beta_1 c_j + \beta_2 tt_j + \gamma_1 log\, M_j + \sum_{s=1}^{5} \gamma_{2s} d_s \sqrt{q_{jsm}} + \varepsilon_{jsm}$$

where ν_{jsm} is the deterministic utility for site j and mode/species sm, c_j is the travel cost to site j, tt_j is the travel time for those who cannot value the travel-time at the wage rate, M_j is the number of intercept sites in the aggregated county level zone, q_{jsm} is a five-year historic harvest rate for species s through mode m at site j, d_s is a species dummy variable, and ε_{jsm} is a generalized extreme value random error term.

The probability of choosing site j conditional on mode/species choice sm is:

$$(5.2) \quad \mathrm{Prob}(j|sm) = \exp[(\beta_1 c_j + \beta_2 tt_j + \gamma_1 log\, M_j + \sum_{s=1}^{5} \gamma_{2s} d_s \sqrt{q_{jsm}})/\theta s]$$

$$/ \textstyle\sum_h \exp[(\beta_1 c_h + \beta_2 tt_h + \gamma_1 log\, M_h + \displaystyle\sum_{s=1}^{5} \gamma_{2s} d_s \sqrt{q_{hsm}})/\theta s]$$

$$(5.3) \quad I_{sm} = \ln(\textstyle\sum_h \exp[(\beta_1 c_h + \beta_2 tt_h + \gamma_1 log\, M_h + \displaystyle\sum_{s=1}^{5} \gamma_{2s} d_s \sqrt{q_{hsm}})/\theta s])$$

$$(5.4) \quad \mathrm{Prob}(sm) = \exp(\theta s\, I_{sm}) / \textstyle\sum_n \exp(\theta s\, I_n)$$

where θs is a species-specific inclusive value parameter and I_{sm} is a mode/species-specific inclusive value. The estimation of the second stage site choice decision (equation (5.2)) will give us estimates of $(\boldsymbol{\beta}, \boldsymbol{\gamma})/\theta$s, and then inclusive values (equation (5.3)) can be calculated using these parameter estimates for the estimation of the first stage mode-species choice decision (equation (5.4)). In both NE 1994 and SE 1997 data, the inclusive value parameters for the four targeted species groups (Big Game, Small Game, Bottom, and Flat) are assumed to be the same (θ_T), and the inclusive value parameter for the non-targeted species is assumed to be different (θ_{NT}) since the pattern of substitution between sites differs for those who do not target a particular species.

The standard welfare measure from a nested logit random utility recreational fishing model that is linear in travel cost compares the expected maximum utility after policy change (V^1) with a baseline level of the expected maximum utility (V^0), and then converts the difference into a money metric by normalizing with the marginal utility of income (β_1). Given the indirect utility function in equation (5.1), the expected maximum utility under policy situation z (V^z) is:

$$(5.5)\quad V^z = \ln\left[\sum\nolimits_{tm}\left(\sum\nolimits_j \frac{v^z_{jtm}}{\theta_T}\right)^{\theta_T} + \sum\nolimits_{nm}\left(\sum\nolimits_j \frac{v^z_{jnm}}{\theta_{NT}}\right)^{\theta_{NT}}\right]$$

where the first summation is over the 12 mode/species combinations that contain targeted species, the third summation is over the 3 mode/species combinations with no target, and v^Z_{jsm} is the estimated indirect utility function evaluated at independent variable values under situation z.

It is possible to introduce a policy regime that changes the value of independent variables included in the indirect utility function. Two policy situations considered in the analysis are a closure of all sites in a state and an increase in historic harvest rate at all sites to measure the access value of fishing in the state for all anglers and the marginal willingness to pay for a one fish increase in harvest rate at all sites respectively. In these cases, the expected maximum utility will be changed by either eliminating the affected sites (j) or increasing harvest rate (q_{jsm}) from the corresponding summations in equation (5.5). Using this notation, the willingness to pay (WTP) for a change or the welfare change from policy situation z = 0 to z = 1 is:

$$(5.6)\quad \text{WTP} = (V^0 - V^1)/\beta_1$$

where V^0 is a baseline level of the expected maximum utility under situation 0, V^1 is the expected maximum utility after a policy change to situation 1, and β_1 is the estimate of travel cost coefficient obtained from the estimation of the second stage site choice decision (equation (5.2)).

Original Model Estimation: NE 1994 and SE 1997 MRFSS-AMES Data

The estimation results of two-stage nested random utility model of marine recreational fishing using NE 1994 and SE 1997 data are presented in Table 5.1. In both

NE and SE models, both travel cost (including explicit out of pocket cost and implicit opportunity cost of leisure time) and travel time have a negative and significant effect on site choice implying that trip-related expenses and opportunity cost of lost income are inversely related to site choice. The number of available fishing sites included in an aggregated county zone also positively influences the probability of choosing that zone in both models. All historic harvest rate variables that represent the quality of fishing zone have positive effects on indirect utility with big game and flat fish species having the largest marginal utilities in NE 1994 and SE 1997 respectively. In general, targeted species give us higher utility than non-targeted species in both models suggesting that targeting a particular species could lead to more valuable fishing trip in a marine recreational fishing environment.

Inclusive value parameter estimates in both models support the appropriateness of two-stage nested RUM model instead of simple site choice model. If inclusive value parameter is close to one, nested RUM structure may not be appropriate. This may be the case for the anglers who do not target particular species in SE 1997 model (1.12).

Original Welfare Estimation

Table 5.2 and Table 5.3 represent welfare estimates of the *mean value of access per trip* by state and two-month wave and *willingness to pay for a one fish increase in historic harvest rate per trip* by state and species group.

In NE 1994 model, Virginia (22% of total fishing trips) has the largest access value followed by New York, New Jersey, Maryland, Massachusetts, and Maine while New Hampshire has the lowest access value among the Northeastern coastal states. There is no particular wave that generally has larger access value among all Northeastern states although the largest proportion (34.2%) of fishing trips occurred in wave 4 (July-August).

The big game species group provides the largest gain per trip from a one fish increase in 5-year historic harvest rate followed by flat fish and small game species groups while the bottom fish species group provides the lowest gain per trip in all Northwestern states. For all targeted species groups, Maine and Maryland show relatively larger gains per trip from a one fish increase in harvest rate although differences are not very considerable.

In SE 1997 model, Florida (60.26% of total fishing trips) has the largest access value followed by North Carolina and Louisiana while Alabama has the lowest access value among the Southeastern coastal states. Again, there is no particular wave that has larger access value among all Southeastern states, and most fishing trips (23.83%) occur during the wave 3 (May-June) unlike the Northeastern coastal states with most fishing trips occurring during the wave 4 (July-August).

In the Southeastern coastal states, the flat fish species group provides the largest gain per trip from a one fish increase in historic harvest rate followed by big game and small game species groups while the bottom fish species group provides the lowest gain per trip as in the Northeastern coastal states. There is not any noticeable variation across states in gains per trip from a one fish increase in historic harvest rate of all targeted species groups.

In evaluating the mean values of access per trip by state, we should not add these values together across states to calculate access value of multiple states since these values are calculated under the assumption that all of other alternative sites in other states are available to the angler. Adding these values will give us incorrect measure of access value of all states in the region. To accurately calculate the access value of whole region, survey data from different regions should be combined to create multi-regional data.

Benefit Transfer Estimation: Function Transfer

Since we have original estimation results of marine recreational fishing value from the Northeast 1994 (Table 5.2) and Southeast 1997 data (Table 5.3), both regions could be a candidate for the study site (the place for which original research was conducted) and the policy site (the place to which estimates of economic values from original research are transferred) for benefit transfer procedure. Function transfer procedure begins with inserting the policy site values for the independent variables of the study site benefit function ($WPT_{Study|Policy}$). Using the study site benefit function and parameter estimates along with the policy site independent variable values, benefit transfer estimates of the economic value of marine recreational fishing for the policy site can be described as:

$$(5.7)\quad WTP_{BT} = (V^0_{Study|Policy} - V^1_{Study|Policy}) / \beta_1^{Study}$$

where WTP_{BT} is benefit transfer welfare estimates for the policy site, $V^0_{Study|Policy}$ ($V^1_{Study|Policy}$) is the study site value function adjusted for the policy site by inserting the policy site values for the independent variables of the study site function, β_1^{Study} is the study site parameter estimate of travel cost variable.

Table 5.4 (Table 5.5) shows benefit transfer welfare estimates for NE 1994 (SE 1997) data using the benefit function and parameter estimates from SE 1997 (NE 1994) model, $WTP_{SE97|NE94}$ ($WTP_{NE94|SE97}$). The benefit transfer estimates of mean value of access per trip by state and wave show similar patterns with the policy site's original value estimates in terms of the states with the largest and the lowest access values for both Northeastern and Southeastern coastal states. As with the policy site's original estimates of access values per trip, benefit transfer value estimates do not show any clear pattern across waves in all Northeastern and Southeastern states. However, the benefit transfer estimates of marginal willingness to pay for historic harvest rate by species and state show the pattern appeared in the study site's original estimation results in terms of the species groups with the largest and the lowest marginal willingness to pay for a one fish increase in historic harvest rate for both Northeastern and Southeastern regions.

One way of testing the validity of benefit transfer procedure is to compare benefit transfer welfare estimates for the policy site with the original value estimates available at the policy site (convergent validity). The measure of convergent validity used in the analysis is:

(5.8) $\delta_{BT} = (WTP_{BT} - WTP_{Policy}) / WTP_{Policy}$

where WTP_{BT} is the benefit transfer estimates for the policy site and WTP_{Policy} is the original value estimates available at the policy site.

Table 5.6 and Table 5.7 presents the results of *convergent validity test* of benefit transfer welfare estimates for both NE and SE regions in terms of percentage difference with the policy site's original value estimates. At this initial trial of benefit transfer procedure, the magnitude of percentage difference falls within 100% of the policy site's original estimates in general except for the benefit transfer estimates of marginal willingness to pay for one bottom fish increase in historic harvest rate for the Southeast 1997 data. Another noticeable pattern is that benefit transfer estimates in both regions are generally underestimated compared to the policy site's original value estimates except for the marginal willingness to pay estimates for a one fish increase in big game, small game, and bottom game species groups for the Southeast 1997 data.

Discussion

The validity of the benefit transfer procedure in a marine recreational fishing environment was evaluated using two data sources from different regions and years (Northeast 1994 and Southeast 1997). One critical limitation of testing the benefit transfer procedure with these data is that the source of differences in benefit transfer estimates and policy site's original value estimates cannot be clearly identified between regional and temporal sources. Even when the benefit transfer procedure is assumed to adapt reasonably well to differences in population characteristics, we still have two undistinguishable sources of errors: regional and temporal differences not properly accounted for in the benefit transfer procedure. To minimize these sources of errors associated with benefit transfer, intra-regional and/or intra-temporal data could be used for testing the validity of benefit transfer procedure: data from different years in the same region or different regions in the same year.

At the first stage estimation (conditional site choice decision given mode-species combination) of two-stage nested RUM, all parameter estimates are normalized by the inclusive value parameter. Since we assumed different inclusive values for targeted species and non-targeted species, a weighted inclusive value parameter estimate was used to recover β_1 in equation (5.6). The proportions of anglers with targeted species and non-targeted species in each sample were used as weights.

Simple *value transfer technique* could also be tried with intra-regional or intra-temporal data to compare the results of convergent validity tests with those of function transfer techniques used in this analysis. Some empirical studies find that the magnitude of percentage differences in convergent validity tests is smaller with function transfer than with the value transfer technique.

Table 5.1. Nested RUM Parameter Estimates

		Northeast 1994			
Variable	Definition	Coeff.	Std. Err.	Mean: Visited Sites	Mean: All Sites
TRAVELC	Travel Cost	-0.028	0.002	31.79	193.14
TTIME	Travel Time	-0.9355	0.0432	2.36	10.55
LNM	Log(Number of NMFS interview sites in aggregated zone)	1.1507	0.032	3.54	3.14
MBIG	Square Root of Historic Harvest Rate: Big Game	1.1247	0.2803	0.01	0
MSMALL	Square Root of Historic Harvest Rate: Small Game	0.5229	0.0602	0.52	0.43
MBOTTOM	Square Root of Historic Harvest Rate: Bottom	0.5625	0.0494	0.33	0.2
MFLAT	Square Root of Historic Harvest Rate: Flat	0.7777	0.0789	0.29	0.17
MOTHER	Square Root of Historic Harvest Rate: Other	0.3349	0.0732	0.24	0.2
INC_T	Inclusive Value: Targeted Species	0.2473	0.0281		
INC_NT	Inclusive Value: Non-targeted Species	0.2387	0.0311		

		Southeast 1997			
Variable	Definition	Coeff.	Std. Err.	Mean: Visited Sites	Mean: All Sites
TRAVELC	Travel Cost	-0.0163	0.0008	38.45	330.31
TTIME	Travel Time	-0.5522	0.0136	2.47	22.53
LNM	Log(Number of NMFS interview sites in aggregated zone)	0.7941	0.0232	3.09	2.67
MBIG	Square Root of Historic Harvest Rate: Big Game	0.3551	0.157	0.03	0.02
MSMALL	Square Root of Historic Harvest Rate: Small Game	0.1804	0.0642	0.34	0.35
MBOTTOM	Square Root of Historic Harvest Rate: Bottom	0.0619	0.0523	0.1	0.09
MFLAT	Square Root of Historic Harvest Rate: Flat	0.4952	0.1773	0.01	0.01
MOTHER	Square Root of Historic Harvest Rate: Other	0.0098	0.0542	0.11	0.1
INC_T	Inclusive Value: Targeted Species	0.7326	0.1057		
INC_NT	Inclusive Value: Non-targeted Species	1.1162	0.1113		

Table 5.2. Welfare Estimates from Northeast 1994 MRFSS-AMES Data

The Mean Value of Access Per Trip

State	All Waves	Wave 3	Wave 4	Wave 5	Wave 6
Connecticut	$5.31	$5.56	$5.70	$4.97	$4.58
Delaware	$2.42	$3.42	$2.78	$0.93	$2.50
Maine	$18.76	$20.29	$23.51	$21.83	$0.00
Maryland	$29.66	$32.86	$27.99	$35.94	$17.24
Massachusetts	$21.08	$22.31	$20.38	$25.50	$12.94
New Hampshire	$1.31	$1.91	$1.52	$1.21	$0.00
New Jersey	$34.90	$40.91	$33.19	$34.83	$28.89
New York	$58.93	$58.39	$56.12	$57.85	$68.19
Rhode Island	$9.91	$9.10	$10.35	$11.12	$8.18
Virginia	$117.46	$79.89	$95.29	$113.04	$238.64
Obs.	4897	1220	1675	1271	731

Willingness to Pay for a One Fish Increase in Historic Harvest Rate Per Trip

State	Obs.	Big Game	Small Game	Bottom Fish	Flat Fish
Connecticut	281	$21.85	$8.10	$5.92	$16.12
Delaware	190	$20.07	$7.38	$5.28	$15.19
Maine	273	$25.12	$9.55	$6.91	$21.59
Maryland	501	$25.67	$9.35	$6.52	$20.50
Massachusetts	529	$22.29	$7.74	$5.55	$16.03
New Hampshire	225	$22.83	$8.07	$5.77	$17.30
New Jersey	793	$18.15	$6.54	$4.71	$12.96
New York	678	$17.67	$5.81	$4.50	$12.00
Rhode Island	349	$20.70	$7.50	$5.41	$15.73
Virginia	1078	$16.27	$5.72	$4.76	$12.05
All States	4897	$19.96	$7.10	$5.28	$14.88

A wave is a two-month period: Jan/Feb (wave1) ~ Nov/Dec (wave6).

Table 5.3. Welfare Estimates from Southeast 1997 MRFSS-AMES Data

The Mean Value of Access Per Trip

State	All Waves	Wave 2	Wave 3	Wave 4	Wave 5	Wave 6
Florida (All)	$300.12	$351.54	$287.54	$299.89	$270.55	$306.23
Florida West (Gulf)	$60.66	$74.53	$58.09	$56.23	$58.15	$59.10
Florida East (SA)	$16.33	$17.01	$13.81	$16.84	$15.51	$19.23
Georgia	$3.41	$1.17	$5.10	$4.45	$3.35	$2.40
N. Carolina	$37.19	$21.74	$39.61	$38.02	$49.58	$32.44
S. Carolina	$9.93	$10.02	$8.07	$9.37	$12.12	$10.12
Louisiana	$16.58	$12.23	$16.81	$19.34	$16.41	$17.61
Mississippi	$4.87	$4.61	$4.64	$4.64	$5.41	$4.96
Alabama	$2.09	$2.37	$2.53	$1.85	$1.61	$2.07
Gulf Coast	$113.42	$118.61	$109.15	$113.93	$114.82	$112.28
S. Atlantic	$162.37	$112.10	$168.07	$161.58	$201.10	$154.29
Obs.	6379	1039	1520	1115	1417	1288

Willingness to Pay for a One Fish Increase in Historic Harvest Rate Per Trip

State	Obs.	Big Game	Small Game	Bottom Fish	Flat Fish
Alabama	206	$20.17	$9.79	$3.32	$27.78
Florida East (SA)	1398	$20.36	$9.83	$3.38	$28.09
Florida West (Gulf)	2446	$20.78	$10.10	$3.47	$28.87
Georgia	207	$20.23	$9.66	$3.40	$27.91
Louisiana	776	$20.67	$9.90	$3.38	$28.92
Mississippi	220	$20.85	$10.11	$3.48	$29.03
N. Carolina	603	$20.47	$10.00	$3.46	$28.62
S. Carolina	523	$20.89	$10.35	$3.60	$29.18
All States	6379	$20.62	$10.00	$3.44	$28.64

Table 5.4. Benefit Transfer Welfare Estimates for Northeast 1994 Using Southeast 1997 NRUM Parameter Estimates

The Mean Value of Access Per Trip

State	All Waves	Wave 3	Wave 4	Wave 5	Wave 6
Connecticut	$5.89	$6.57	$6.15	$5.55	$4.76
Delaware	$3.40	$3.88	$3.33	$3.08	$3.35
Maine	$8.01	$9.52	$9.52	$8.98	$0.34
Maryland	$11.33	$12.32	$10.90	$12.10	$9.34
Massachusetts	$12.18	$12.32	$13.86	$13.06	$6.55
New Hampshire	$1.49	$1.71	$1.91	$1.45	$0.24
New Jersey	$15.24	$17.58	$15.22	$14.30	$13.03
New York	$24.04	$23.93	$23.55	$23.54	$26.22
Rhode Island	$8.29	$7.99	$9.22	$8.84	$5.71
Virginia	$40.16	$26.57	$31.97	$40.57	$80.90
Obs.	4897	1220	1675	1271	731

Willingness to Pay for a One Fish Increase in Historic Harvest Rate Per Trip

State	Obs.	Big Game	Small Game	Bottom Fish	Flat Fish
Connecticut	281	$1.76	$0.81	$0.18	$3.24
Delaware	190	$2.09	$0.93	$0.18	$3.86
Maine	273	$2.20	$1.05	$0.25	$4.80
Maryland	501	$2.29	$0.98	$0.22	$4.40
Massachusetts	529	$1.75	$0.77	$0.15	$3.26
New Hampshire	225	$1.98	$0.87	$0.18	$3.87
New Jersey	793	$1.79	$0.80	$0.14	$3.08
New York	678	$1.50	$0.66	$0.12	$2.57
Rhode Island	349	$1.67	$0.77	$0.16	$3.24
Virginia	1078	$2.13	$0.98	$0.17	$3.82
All States	4897	$1.90	$0.86	$0.17	$3.51

Table 5.5: Benefit Transfer Welfare Estimates for Southeast 1997 Using Northeast 1994 NRUM Parameter Estimates

The Mean Value of Access Per Trip

State	All Waves	Wave 2	Wave 3	Wave 4	Wave 5	Wave 6
Florida (All)	$205.70	$240.70	$196.95	$206.00	$185.34	$209.90
Florida West (Gulf)	$38.39	$47.23	$36.68	$35.68	$36.88	$37.28
Florida East (SA)	$10.49	$10.91	$8.82	$10.80	$10.06	$12.31
Georgia	$2.19	$0.75	$3.28	$2.86	$2.15	$1.52
N. Carolina	$25.44	$15.00	$26.80	$26.04	$34.09	$22.20
S. Carolina	$6.37	$6.41	$5.17	$5.99	$7.78	$6.52
Louisiana	$10.88	$7.89	$11.13	$12.75	$10.65	$11.63
Mississippi	$3.04	$2.87	$2.89	$2.90	$3.39	$3.09
Alabama	$1.34	$1.52	$1.63	$1.19	$1.02	$1.32
Gulf Coast	$74.07	$76.76	$71.33	$74.80	$75.08	$73.38
S. Atlantic	$113.29	$77.69	$117.13	$112.56	$140.84	$107.81
Obs.	6379	1039	1520	1115	1417	1288

Willingness to Pay for a One Fish Increase in Historic Harvest Rate Per Trip

State	Obs.	Big Game	Small Game	Bottom Fish	Flat Fish
Alabama	206	$38.39	$16.86	$17.04	$26.02
Florida East (SA)	1398	$38.67	$16.78	$17.66	$26.49
Florida West (Gulf)	2446	$38.67	$16.84	$17.68	$26.68
Georgia	207	$39.83	$16.87	$18.72	$26.98
Louisiana	776	$40.40	$17.31	$17.92	$28.00
Mississippi	220	$40.32	$17.62	$18.63	$27.82
N. Carolina	603	$40.55	$17.90	$19.31	$28.03
S. Carolina	523	$40.53	$18.27	$20.07	$28.00
All States	6379	$39.30	$17.13	$18.10	$27.06

Table 5.6. Convergent Validity (Percentage Difference) Test of Benefit Transfer Estimates for Northeast 1994

	The Mean Value of Access Per Trip				
State	All Waves	Wave 3	Wave 4	Wave 5	Wave 6
Connecticut	11.03%	18.11%	7.96%	11.74%	4.12%
Delaware	40.64%	13.20%	19.69%	230.29%	33.61%
Maine	-57.31%	-53.08%	-59.50%	-58.86%	NA
Maryland	-61.80%	-62.51%	-61.07%	-66.34%	-45.80%
Massachusetts	-42.21%	-44.77%	-31.96%	-48.77%	-49.37%
New Hampshire	13.72%	-10.54%	25.66%	19.69%	NA
New Jersey	-56.33%	-57.02%	-54.15%	-58.96%	-54.92%
New York	-59.21%	-59.01%	-58.04%	-59.31%	-61.54%
Rhode Island	-16.38%	-12.26%	-10.88%	-20.52%	-30.17%
Virginia	-65.81%	-66.74%	-66.45%	-64.11%	-66.10%

	Willingness to Pay for a One Fish Increase in Historic Harvest Rate Per Trip			
State	Big Game	Small Game	Bottom Fish	Flat Fish
Connecticut	-91.96%	-89.97%	-97.03%	-79.90%
Delaware	-89.58%	-87.42%	-96.65%	-74.60%
Maine	-91.23%	-88.98%	-96.43%	-77.76%
Maryland	-91.06%	-89.53%	-96.66%	-78.52%
Massachusetts	-92.16%	-90.09%	-97.29%	-79.65%
New Hampshire	-91.35%	-89.18%	-96.94%	-77.63%
New Jersey	-90.15%	-87.76%	-97.13%	-76.27%
New York	-91.52%	-88.60%	-97.25%	-78.56%
Rhode Island	-91.93%	-89.79%	-97.03%	-79.37%
Virginia	-86.90%	-82.84%	-96.41%	-68.31%
All States	-90.46%	-87.94%	-96.87%	-76.40%

Table 5.7. Convergent Validity (Percentage Difference) Test of Benefit Transfer Estimates for Southeast 1997

	The Mean Value of Access Per Trip					
State	All Waves	Wave 2	Wave 3	Wave 4	Wave 5	Wave 6
Florida (All)	-31.46%	-31.53%	-31.50%	-31.31%	-31.49%	-31.46%
Florida West (Gulf)	-36.71%	-36.62%	-36.86%	-36.55%	-36.58%	-36.92%
Florida East (SA)	-35.80%	-35.86%	-36.12%	-35.89%	-35.15%	-36.00%
Georgia	-35.86%	-36.20%	-35.72%	-35.75%	-35.63%	-36.62%
N. Carolina	-31.60%	-31.02%	-32.33%	-31.50%	-31.24%	-31.57%
S. Carolina	-35.86%	-36.01%	-36.02%	-36.06%	-35.82%	-35.49%
Louisiana	-34.38%	-35.55%	-33.80%	-34.07%	-35.09%	-33.94%
Mississippi	-37.64%	-37.88%	-37.84%	-37.47%	-37.37%	-37.71%
Alabama	-35.98%	-35.91%	-35.70%	-35.75%	-36.34%	-36.31%
Gulf Coast	-34.69%	-35.28%	-34.65%	-34.35%	-34.61%	-34.64%
S. Atlantic	-30.23%	-30.70%	-30.31%	-30.34%	-29.96%	-30.12%

	Willingness to Pay for a One Fish Increase in Historic Harvest Rate Per Trip			
State	Big Game	Small Game	Bottom Fish	Flat Fish
Alabama	90.30%	72.17%	413.15%	-6.36%
Florida East (SA)	89.95%	70.63%	421.61%	-5.67%
Florida West (Gulf)	86.06%	66.68%	409.95%	-7.58%
Georgia	96.89%	74.63%	450.53%	-3.36%
Louisiana	95.44%	74.88%	429.90%	-3.15%
Mississippi	93.40%	74.30%	435.66%	-4.18%
N. Carolina	98.03%	78.99%	457.40%	-2.05%
S. Carolina	94.04%	76.45%	457.18%	-4.05%
All States	90.57%	71.20%	425.71%	-5.52%

Chapter 6. Nested RUM Results

Following the structure laid out in previous chapters, we now turn to the task of estimating nested random utility models on all available MRFSS data sets. These include The 1998 Pacific coast data collection effort, the 1994, 1997, 1998, 1999 and 2000 Northeast data collection efforts and the 1997 and 2000 Southeast data collection efforts. The remainder of this chapter is organized as follows: First we briefly describe the model structures for each data year. For the most part, there is a common structure for all estimated models. Any differences in model structure are highlighted in this section. Second, we report in detail the results of the 1998 Pacific nested RUM model. We spend significantly more time dealing with the Pacific model as this is the first attempt to estimate the pacific model in the accepted RUM framework. Finally we report the results of the Northeast and Southeast model estimations. We focus our results here on the estimated structural parameters of the random utility models with an eye towards comparison across years and regions in the next chapter. For each model we report the estimated model parameters and estimates of the value of access and catch.

Model Structures

The estimated random utility models for the MRFSS data sets follow the basic structure set forth by McConnell and Strand (1994) and followed subsequently by Hicks et al. (1999) and Haab, Whitehead, and McConnell (2000)[6]. The model assumes that anglers first make a decision between the set of available mode/species combinations. The modes are shore fishing, fishing from party or charter boats, and fishing from private or rental boats. The species groups are big game, small game, bottom fish and flatfish (flounder, fluke and other flounder like species). An additional species category is included to capture those anglers that do not target a specific species. Conditional on the mode/species choice, the angler then chooses the specific destination for angling that maximizes the utility of a fishing trip conditional on the first stage mode/species choice.

Sites are defined at the county level such that MRFSS intercept sites are aggregated across counties. Any distance measures required for travel time and travel cost calculation are measured to the mid-point of the coast for that particular county. The conditional site choice is explained using a series of angler and site specific attributes. The conditional indirect utility function is assumed to be a linear in parameters (and variables) function of the travel costs to the county of intercept and the expected catch rate for each of the species groups.

Travel costs are split into two separate variables depending on the ability of the angler to trade-off labor and leisure. Ideally, travel costs would represent the full opportunity costs of taking an angling trip in the form of foregone expenses and foregone wages associated with taking an angling trip. Because not all anglers can trade-off labor and leisure at the margin, we use the procedure introduced by Bockstael, Hanemann and Strand to allow for flexibility in modeling these tradeoffs. For anglers that can directly trade-off labor and leisure at the wage

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[6] See Haab, Whitehead and McConnell (Chapter 2) for a detailed exposition of the model structure.

rate (those that indicate they lost income by taking the trip), travel costs are defined as the sum of the explicit travel cost ($.30 per mile* travel distance) and the travel time valued at the wage rate. Travel time is calculated by dividing the travel distance by an assumed 40 miles per hour for travel. For anglers that do not forego wages to take a trip, travel cost is simply defined as the explicit travel cost ($.30 per mile *roundtrip distance). For these anglers, those that did not lose wages, the travel time to the site is included as a separate variable to directly estimate the opportunity cost of time. All welfare measures are converted to 2000 dollars using the consumer price index.

As in previous MRFSS modeling efforts, the square root of historic catch rates of targeted species for the five species groups (big game, small game, bottom fish, flat fish and other) are used as the site specific characteristic. For anglers that do not target a specific specie the catch rate variable is the small game catch rate, but we allow parameter estimates to differ. To control for aggregation to the county level, the natural logarithm of the number of MRFSS sites in each county is used as an independent variable.

Any differences in model estimation across geographic regions come in the mode/species choice model. For the Northeast models, we chose to specify the model with a single inclusive value parameter for all lower level nests. That is, the inclusive value parameter is restricted to be the same across all mode/species combinations. To identify the mode/species choice, two variables are included: PRDUM, a dummy variable equal to one if the individual owns a boat and we are modeling the private/rental mode, and CPRDUM, the same variable multiplied by a dummy variable which equals one during cold weather months. The first variable captures the increased likelihood of a boat owner of choosing to fish from a private boat, and the second variable captures the decreased likelihood of such anglers fishing during cold weather in the Northeast.

Southeast and Pacific models take a slightly different approach to identification of the species/mode choice. Instead of incorporating a mode/species specific variable, the SE and PAC models allow the inclusive value parameter to differ between the targeted mode/species choices and the non-targeted choices. In Chapter 7, we reconcile these differences for a subset of the models to look at similarities and differences in estimation across geography and time.

Pacific 1998 Nested RUM Model

There are 7745 Pacific coast day-trip anglers with complete data available for analysis. Most (58%) anglers fish from private or rental boats (Table 6.1). Twenty-one percent fish from party or charter boats and shore. Most (62%) of the Pacific coast anglers target other species or do not target species (Table 6.2). Fifteen percent target bottom fish, 14% target small game fish, 7% target flat fish and only 2% target big game fish.

The 15 species/mode choices are presented in Table 6.3. Thirty-five percent of all anglers do not target species and fish from private or rental boats. Seventeen percent of all anglers do not target species and fish from shore. Ten percent of all anglers do not target species and fish from party or charter boats. All other mode/species combinations are below 10%. Private or rental boat anglers are most likely to target small game fish (8% of all anglers), bottom fish (8%), and flat fish (5%). The most likely species target for party and charter boat anglers is bottom fish (6%) and small game fish (4%). Between 1% and 2% of all anglers target big game fish from private or rental boats and small game and bottom fish from shore. Less than 1% of all anglers target big game and flat fish from shore and big game and flat fish from party and charter boats.

Forty-seven percent of all trips were taken to southern California (defined as San Luis Obispo County and south) (Table 6.4). Seventeen percent of all trips were to northern California (Santa Barbara County and north) and Washington. Eighteen percent of all trips were to Oregon.

There are 38 county choices available to respondents (Table 6.5). The county choices are, for the most part, descending north to south. Most of the fishing sites are on the Pacific Ocean. However, in Washington and California some of the sites are inland. In Washington, Whatcom, Skagit, Snohomish, King, Pierce, and Kitsap Counties border Puget Sound. San Juan County is an island in Puget Sound. Clallam and Jefferson Counties are on the Pacific Ocean and Puget Sound. Mason and Thurston Counties have sites that are on rivers that flow into Puget Sound. Grays Harbor and Pacific Counties have sites on the Pacific Ocean and bays. In California, Marin, Sonoma, and San Mateo Counties have sites on the Pacific Ocean and San Francisco Bay. All of the sites in Solano, Contra Costa and Alameda Counties are on the San Francisco Bay.

The most likely destinations in Washington are King County (3%), Island County (3%) and Pierce County (3%). The most likely destinations in Oregon are Curry County (6%) and Lincoln County (3%). Within northern California, the most likely destinations are Santa Cruz County (3%) Monterey (3%), and Alameda County (3%). Within southern California, the most likely destinations are Los Angeles County (16% of all trips) and San Diego County (11%).

The nested RUM model is presented in Table 6. The average trip cost to the 38 counties across 7745 anglers is $439. The average travel time is 28 minutes. Note that this mean includes 92% zero values for those who would be working for pay if they did not take the trip. The square root of the historic small game catch rates across all sites for those targeting other species or not targeting species is largest of all catch rates. The historic catch rates of big game, small game, and flat fish are very low. The historic catch rate for bottom fish is high relative to big game, small game, and flat fish catch rates.

The model chi-squared statistic for the site choice model indicates that all parameters are jointly significantly different from zero. The likelihood that an angler would choose a county fishing site is negatively related to the travel cost and travel time. The likelihood that an angler would choose a county fishing site is positively related to the square root of the big game, bottom fish, flat fish, and other fish historic catch rates. The log of the number of interview sites in the county is positively related to the choice of county.

The mode/species choice is specified to depend on the inclusive value for all targeted species and a separate inclusive value for other and nontargeted species. This is due to the large number of anglers who do not target species or target other species and the low utilities associated with this choice. The model chi-squared statistic for the mode/species choice model indicates that all parameters are jointly significantly different from zero. Both of the parameter estimates on the inclusive values are significantly different from 0 and 1 which indicates that the nested model is appropriate.

The welfare estimates from the nested RUM model are presented in Tables 6.7 and 6.8. Table 6.7 presents the willingness to pay values for access to a one-day fishing trip to each state with California divided into northern and southern regions. The values are presented for each two-month sampling wave and all waves combined. These welfare measures are equivalent to the losses that would be suffered if all of the fishing sites in the state were unavailable for the two-month period (e.g., due to an oil spill). Table 6.8 presents the one-day trip willingness to pay values for a one-unit increase in the historic catch rate across all sites. The values are presented for each state and all states combined.

The willingness to pay values for access to the states are largest for southern California. The willingness to pay values range from $93 to $112 across survey wave. These are greater than $100 in each wave except for July through October. The value of northern California sites ranges from $40 to $49 across survey wave. The largest values are from July through October. This pattern of results makes sense if during the warmer months the more northerly sites are better substitutes for southern California sites. The willingness to pay values range from $19 to $40 across survey wave. The willingness to pay values are lowest for Oregon. The willingness to pay values range from $7 to $21 across survey wave. The highest willingness to pay value for Washington and the lowest willingness to pay value for Oregon is in July through August.

The willingness to pay values for a one-unit catch increase is largest for flat fish. These range from $4 to $7 across state with the value largest for anglers choosing fishing sites in Oregon and lowest for anglers choosing fishing sites in southern California. The value for a one-unit increase in flat fish is almost $5 for all anglers. The willingness to pay values for a one-unit catch increase in big game fish range from $3 to $6 across state with the value largest in Washington and lowest in southern California. The value for a one-unit increase in flat fish is $4.55 for all states. The willingness to pay for a one-unit increase in bottom fish averages $2.44 for all anglers with the lowest value ($2) in southern California and the highest value in Washington ($3). The willingness to pay values for a one-unit increase in small game fish are all less than one dollar reflecting the small coefficient on small game fish in the nested RUM.

Northeast and Southeast Nested RUM Results

The remainder of this chapter focuses on the estimation results for the yet unestimated MRFSS random utility models. In particular, we present estimation results for the Northeast MRFSS for 1994, 1997, 1998, 1999, and 2000 and for the Southeast MRFSS in 2000. This chapter focuses on the estimation results for each individual year, with an eye toward comparison across years and region in chapter 7. The structure of the random utility model is the same as that used throughout this report, with anglers first choosing the mode/species combination for the current angling trip, and then conditionally choosing the destination.

Table 6.9 reports the variable means for the six northeast and southeast data sets. Travel costs and travel time variables are stable across years and region with the exception of the 1997 Northeast data set. This is because the question used to define those that can trade-off work hours for leisure hours was asked differently in 1997 than in the other years. In 1997, the question, “Can you choose to work more or fewer than (number of hours specified in Q.29) per week?” did not force respondents to answer in the context of the trip on which they were intercepted. The correct version used in most other surveys asks, “Did you take time off from work without pay in order to make this fishing trip?”. Consequently, the 1997 question categorized too many people as giving up wages to take the trip. For this reason, travel costs were overestimated and travel time underestimated for this dataset. The number of intercept sites in each county is similar for all Northeast years (about 3.2 sites per county) while the Southeast averages about .5 sites less per county. As expected, catch rates of various species groups vary across years and region.

Table 6.10 shows the percentage of interviewees choosing each mode/species combination by year. In all years, private boat fishing is the most popular mode, with a significantly larger portion of anglers in the southeast choosing this mode relative to the northeast. Small game fish are the most frequently targeted species in the Northeast, while the majority in the Southeast for 2000 chose not to target. For reference, Tables 6.11 and 6.12 report the visitation rates for each region and year by county.

Nested random utility models for the six year/region data sets are reported in Table 6.13. The top half of the table reports the conditional site choice model estimates, while the bottom half reports the mode/species choice estimates. As expected, the travel cost and travel time variables are negatively related to site choice for all years and regions. Ignoring magnitudes for the moment, the catch rate variables are positive and similarly significant across year and region.

As mentioned in the introduction to this chapter, the Northeast models and Southeast models have slightly different structures for the mode/species choice portion of the RUM. Because we will be using the 1997 data for comparison across regions, Table 6.13 contains the Northeast model estimated with both the NE and SE structures.

Table 6.14 reports the compensating variation of a one fish increase in catch for each species for the five northeast years. The value of big game catch varies widely across years ($.93 per fish to $21.21 per fish: a 2,180% difference), while the values for other species are much more stable across time: small game values range from $1.28 to $2.77 (116% difference), bottom fish values range from $1.13 to $3.49 per fish (209% difference, and flat fish values range from $4.20 to $6.90 per fish (64% difference). There is no discernible pattern in the relative values across species or time.

Table 16.15 gives the compensating variation for closure of states across years. These values are to be interpreted as per trip values. More discernible patterns emerge for trip closure values, with New York having the highest value followed by New Jersey in every year. For the years available, New Hampshire and Delaware have the lowest values per trip. With the exception of Virginia, the value of lost access is highest for 1999 or 2000 for all states. There also appears to be a general upward trend in lost use value over time.

Tables 16.16 and 16.17 report the value per fish and the lost value of access respectively for the SE 2000 MRFSS. In contrast to the Northeast, Southeastern anglers appear to place a high value on flat fish. Not surprisingly, Florida generates the highest use value per trip due to a lack of substitute sites when sites are eliminated.

In general, the Northeast estimates appear to be arguably stable over time. In particular, model structure and the value of access by state demonstrate some stability. But, measures of welfare for specific species vary widely across time. Chapter 7 will focus attention on the ability to use data and model estimates from various years and regions to estimate values across time and geography.

Table 6.1. Fishing Mode Choice

Mode	Frequency	Percent
Party/Charter	1658	21.41
Private/Rental	4491	57.99
Shore	1596	20.61

Table 6.2. Species Group Choice

Species	Frequency	Percent
Big Game	140	1.81
Small Game	1097	14.16
Bottom	1160	14.98
Flat	544	7.02
Other	4804	62.03

Table 6.3. Species/Mode Choice

Species/Mode	Frequency	Percent
Big Game Party/Charter	8	0.1
Big Game Private/Rental	131	1.69
Big Game Shore	1	0.01
Small Game Party/Charter	345	4.45
Small Game Private/Rental	652	8.42
Small Game Shore	100	1.29
Bottom Party/Charter	462	5.97
Bottom Private/Rental	593	7.66
Bottom Shore	105	1.36
Flat Party/Charter	55	0.71
Flat Private/Rental	423	5.46
Flat Shore	66	0.85
Other Party/Charter	788	10.17
Other Private/Rental	2692	34.76
Other Shore	1324	17.09

Table 6.4. State Choice

State	Frequency	Percent
Washington	1320	17.04
Oregon	1407	18.17
California (Northern)	1347	17.39
California (Southern)	3671	47.40

Table 6.5 County Choice

State	County	Frequency	Percent	Number of Sites
Washington	Whatcom	19	0.25	1
Washington	Skagit	94	1.21	5
Washington	Snohomish	294	3.8	7
Washington	King	241	3.11	5
Washington	Pierce	196	2.53	12
Washington	Kitsap	5	0.06	7
Washington	Island	227	2.93	12
Washington	Clallam	69	0.89	8
Washington	Jefferson	32	0.41	4
Washington	Grays Harbor	50	0.65	8
Washington	Pacific	41	0.53	9
Washington	Mason	41	0.53	5
Washington	Thurston	11	0.14	3
Oregon	Clatsop	70	0.9	5
Oregon	Tillamook	213	2.75	14
Oregon	Lincoln	260	3.36	11
Oregon	Lane	20	0.26	4
Oregon	Douglas	194	2.5	7
Oregon	Coos	221	2.85	11
Oregon	Curry	429	5.54	10
California	Del Norte	42	0.54	5
California	Humboldt	103	1.33	9
California	Mendocina	59	0.76	16
California	Sonoma	176	2.27	9
California	Marin	182	2.35	11
California	Solano	80	1.03	2
California	Contra Costa	112	1.45	5
California	Alameda	195	2.52	10
California	San Francisco	24	0.31	10
California	San Meteo	106	1.37	14
California	Santa Cruz	268	3.46	9
California	Monterey	208	2.69	10
California	San Luis Obispo	78	1.01	12
California	Santa Barbara	167	2.16	29
California	Ventura	469	6.06	39
California	Los Angeles	1230	15.88	55
California	Orange	638	8.24	31
California	San Diego	881	11.38	46

Table 6.6. Nested RUM Parameter Estimates

Site Choice Model				
Variable	Description	Mean	Coefficient	t-stat
TC	Trip cost to county	439.41	-0.014	-19.90
TT	Travel time to county (minutes)	28.44	-0.496	-39.09
BIG	Square root of historic big game catch	0.01	0.599	3.73
SMALL	Square root of historic small game catch	0.06	0.084	1.15
BOTTOM	Square root of historic bottom fish catch	0.20	0.510	13.43
FLAT	Square root of historic flat fish catch	0.03	0.696	5.77
OTHER	Square root of historic catch for other speices	0.25	0.065	3.20
LOGNSITE	Log of number of sites in zone	2.19	0.896	26.01
Model Chi-square	All parameters = 0		36,632	
Cases			7745	
Choices			38	
Mode/Species Choice Model				
Variable	Description	Mean	Coefficient	t-stat
IV	Inclusive value: Targeted Species	1.90	0.286	8.42
IVOTHER	Inclusive value: Non-Targeted Species	0.41	0.880	22.27
Model Chi-square	All parameters = 0		3647	
Cases			7745	
Choices			15	

Table 6.7. Willingness to Pay for a One-Day Fishing Trip by Wave (2000 dollars)

State	March-April	May-June	July-August	Sept-Oct	Nov-Dec	All Waves
Washington	31.93	19.01	40.33	29.15	20.99	29.40
Oregon	15.94	17.30	7.08	21.26	11.40	14.61
California (Northern)	47.18	40.41	48.93	49.03	42.21	45.99
California (Southern)	103.81	111.79	93.99	92.93	109.15	100.95

Table 6.8. Willingness to Pay for a One-Unit Increase in Catch (2000 dollars)

State	Big Game Fish	Small Game Fish	Bottom Fish	Flat Fish
Washington	6.16	0.69	3.21	6.47
Oregon	5.76	0.69	3.05	6.81
Northern California	4.43	0.39	2.28	4.48
Southern California	3.49	0.18	1.95	3.71
All States	4.55	0.40	2.44	4.91

Table 6.9: Variable means by dataset (mean taken over choices in choice set)

Variable	NE 94	NE 97*	NE 98	NE 99	NE 00	SE 00
Travel Cost	78.758	146.80	73.968	74.377	73.769	76.274
Travel Time	5.242	1.99	5.326	5.398	5.264	5.673
Log(# sites)	3.160	3.187	3.236	3.244	3.255	2.758
$\sqrt{BG}$	.0019	.0038	.0037	.0034	.0049	.0272
$\sqrt{SG}$	.366	.541	.594	.639	.561	.568
$\sqrt{BT}$	.192	.133	.124	.104	.173	.117
$\sqrt{FF}$	.214	.200	.169	.155	.220	.0124
$\sqrt{OT}$	.161	.207	.218	.231	.285	.702
IV	4.187	4.365	5.299	5.183	4.835	2.240
PR Dummy	.188	.192	.200	.357	.184	.357
PR Dummy*Cold Dummy	.0274	.036	.044	.050	.021	.0690

*Respondents who did take off work without pay were not properly identified in this dataset.

6.10: Summary Data: Percentage of mode and species choices by year of data collection used in analysis

	NE 1994	NE 1997	NE 1998	NE 1999	NE 2000	SE 2000
			Mode Choice			
Party/Charter	13.57	15.17	18.44	15.64	15.26	3.29
Private Boat	57.50	56.71	52.60	56.58	57.12	68.78
Shore	28.93	28.12	28.97	27.29	27.62	27.94
			Species Choice			
Big Game	1.50	1.55	1.28	1.83	1.91	5.57
Small Game	42.57	50.03	50.16	53.85	45.38	34.49
Bottom Fish	12.73	8.93	10.30	9.32	10.53	6.58
Flat Fish	22.81	18.67	16.73	13.82	18.21	3.14
Other	20.39	20.82	21.53	21.18	23.97	50.22

6.11: Summary Data: Percentage of zones chosen by year of data collection used in analysis

County Name	State	NE 94	NE 97	NE 98	NE 99	NE 00
Fairfield	CN	.45	.33	.37	.94	.42
Middlesex	CN	1.71	1.33	2.10	3.18	2.70
New Haven	CN	.68	.95	.74	.63	1.08
New London	CN	2.79	3.05	4.75	4.93	5.04
Kent	DE	.80	1.78	.99	.75	.59
New Castle	DE	.54	1.31	1.20	.16	.41
Sussex, South of Lewes	DE	1.31	3.78	2.78	1.96	3.99
Sussex, North of Lewes	DE	1.13	2.61	2.11	2.67	3.79
Cumberland	ME	.45	.13	.50	.84	.00
Hancock	ME	.02	.02	.01	.01	.00
Knox	ME	1.08	.10	.24	.34	.00
Lincoln	ME	.30	.01	.17	.13	.00
Washington	ME	.12	.00	.00	.00	.00
York	ME	1.90	.66	2.08	2.58	.00
Kennebec and Sagahadoc	ME	1.08	.88	1.25	1.31	.00
Penobscott and Waldo	ME	.68	.02	.19	.06	.00
Anne Arundel	MD	2.81	2.68	1.58	2.44	3.48
Calvert	MD	1.08	1.50	1.75	1.20	1.97
Worcester	MD	1.59	1.96	1.15	1.21	1.28
Baltimore, Cecil, Harford	MD	3.49	2.75	2.37	1.98	1.49
Caroline, Kent, Queen Annes, Talbot	MD	.19	.42	.45	.75	.80
Charles, St. Marys	MD	.47	.47	.13	.37	.69
Dorchester, Somerset	MD	.61	.42	.34	.76	.94
Barnstable	MA	2.02	6.57	6.17	5.28	6.38
Bristol	MA	.91	1.37	1.29	1.46	.63
Dukes	MA	.52	1.29	.16	.78	.44
Essex	MA	4.08	6.51	10.83	10.16	11.01
Nantucket	MA	.28	.98	.17	.09	.06
Norfolk	MA	.45	.47	.57	.69	1.22
Plymouth	MA	2.32	2.61	3.30	3.79	4.32
Suffolk	MA	.19	.36	.35	.66	.5
Rockingham, Hudson	NH	4.74	5.43	7.10	6.86	.00
Atlantic	NJ	2.58	1.42	1.17	.77	1.95
Cumberland	NJ	.45	.29	.23	.03	.09
Middelsex	NJ	.19	.16	.21	.10	.01
Ocean	NJ	3.40	4.50	2.95	2.58	2.19
Cape May Bay Side	NJ	.56	.27	.58	.45	.78
Cape May Oceanside	NJ	3.05	3.58	1.28	1.69	2.31
Monmouth County Oceanside	NJ	1.88	1.96	1.69	1.47	2.10
Monmouth County Bayside	NJ	4.13	3.24	1.96	3.65	2.91

Bronx	NY	.09	.08	.04	.04	.07
Kings	NY	1.05	.46	.58	.15	.00
Queens	NY	.30	.03	.18	.01	.00
Richmond	NY	.45	.06	.13	.10	.00
Westchester	NY	.33	.25	.56	.14	.02
Nassau Sound Side	NY	.28	.10	.13	.15	.13
Nassua Ocean Side	NY	3.09	2.15	1.97	2.25	2.19
Suffolk Soundside	NY	2.48	2.55	2.81	1.96	2.73
Suffolk Sound Internal	NY	.38	.16	.14	.43	.37
Suffolk Oceanside	NY	4.85	4.59	3.94	5.10	7.08
Bristol	RI	.05	.29	.48	.52	.54
Kent	RI	.59	.47	.59	.54	.79
Newport	RI	.73	.70	.90	1.30	.71
Providence	RI	.49	.09	.40	.16	.11
Washington	RI	5.16	6.03	9.95	8.81	9.29
Virginia Beach City	VA	7.08	6.36	4.90	3.71	4.47
Accomack, North Hampton	VA	2.46	2.31	.79	.27	.52
Essex, Gloucester, King William, Mathews, Middlesex, Caroline, Fredericksburg City	VA	2.02	.38	.32	.46	.68
Hampton City, Newport News, Poquoson	VA	6.02	1.97	1.96	1.49	1.95
Isle of Wight, Suffolk, Surry	VA	.28	.28	.19	.20	.05
James City, York	VA	1.20	.40	.22	.34	.21
King George, Lancaster, Northumberland, Richmond, Westmoreland,	VA	.19	.10	.11	.13	.30
Norfolk, Portsmouth	VA	3.45	2.02	1.48	2.00	2.25

6.12. SE 2000 Chosen Counties

State	County	Percentage
Alabama	Baldwin	1.16
Alabama	Mobile	2.51
Florida	Bau	.47
Florida	Brevard	6.82
Florida	Broward	1.29
Florida	Charlotte	1.15
Florida	Citrus	2.15
Florida	Collier	.90
Florida	Dade	2.31
Florida	Dixie	.73
Florida	Duval	2.40
Florida	Escambia	1.75
Florida	Franklin	.27
Florida	Gulf	.07
Florida	Hernado	1.07
Florida	Hillsborough	5.28
Florida	Indian River	1.80
Florida	Lee	2.49
Florida	Levy	1.73
Florida	Manatee	2.27
Florida	Martin	1.97
Florida	Monroe	.04
Florida	Nassau	.49
Florida	Okaloosa	.77
Florida	Palm Beach	5.16
Florida	Pasco	3.92
Florida	Pinellas	8.62
Florida	St. Johns	1.22
Florida	St. Lucie	1.68
Florida	Santa Rosa	.93
Florida	Sarasota	1.19
Florida	Taylor	.80
Florida	Volusia	4.08
Florida	Wakulla	.55
Florida	Walton	.00
Georgia	Bryan	.21
Georgia	Camden	.17

Georgia	Chatham	1.13
Georgia	Glynn	1.95
Georgia	Liberty	.11
Georgia	Mcintosh	.20
Louisiana	Calcasieu	.19
Louisiana	Cameron	.46
Louisiana	Jefferson	1.99
Louisiana	LaFourche	.65
Louisiana	Orleans	.69
Louisiana	Plaquemines	1.72
Louisiana	St. Bernard	1.18
Louisiana	St. Mary	.42
Louisiana	Tammany	.97
Louisiana	Terrebonne	.74
Louisiana	Vermillion	.33
Mississippi	Hancock	.09
Mississippi	Harrison	3.06
Mississippi	Jackson	1.86
North Carolina	Beaufort	.06
North Carolina	Brunswick	.09
North Carolina	Carteret	2.11
North Carolina	Dare	1.54
North Carolina	Hyde	.16
North Carolina	New Hanover	.31
North Carolina	Onslow	.27
North Carolina	Pamlico	.09
North Carolina	Pender	.12
South Carolina	Beaufort	.31
South Carolina	Berkeley	.24
South Carolina	Charleston	3.87
South Carolina	Colleton	.02
South Carolina	Georgetown	1.33
South Carolina	Horry	1.28

Table 6.13. NE/SE MRFSS Individual Year Results

	SE 2000	NE 1994	NE 1997 with NE Mode/Species Structure***	NE 1997 with SE Mode/Species Structure***	NE 1998	NE 1999	NE 2000
			Site Choice				
Travel Cost	-.0204*	-.0232*	-.0185*		-.0179*	-.0156*	-.0157*
Travel Time	-.772*	-.746*	-.540*		-.824*	-.861*	-.712*
ln(M)	.733*	1.176*	1.144*		1.424*	1.321*	1.259*
Big Game	.456*	2.243*	.976*		.301*	.608*	.467*
Small Game	.445*	.555*	.413*		.479*	.508*	.694*
Bottom Fish	.329*	.543*	.403*		.651*	.645*	.527*
Flat Fish	1.38*	.864*	.878*		.851*	.834*	1.046*
Other	.440*	.261*	.325*		.268*	.349*	.287*
Choices	15750	4266	18224		18729	13392	14182
-2*LogLike	37493.33	13685.65	71737.85		58624.65	41985.97	45377.20
Model Chi Squared (all parms =0)	47899.13	12242.46	37259.06		56766.56	40491.95	37144.02
			Mode/Species Choice				
IV	.677*	.157*	.066*	N/A	.141*	.237*	.349*
IV_Target	N/A	N/A	N/A	.054*	N/A	N/A	N/A
IV_NoTarget	N/A	N/A	N/A	.045*	N/A	N/A	N/A
PRDUM	2.632*	2.169*	2.035*	2.036*	1.785*	2.326*	2.089*
CLDDUM	N/A	-.565*	-.560*	-.560*	-.814*	-1.016*	-.702*
Choices	15750	4266	18224	18224	18729	13392	14182
-2*LogLike	51298.00	20409.23	88382.98	88333.82	92642.06	29309.35	66381.55
Model Chi Squared (all parms=0)	13196.38	2064.98	7631.20	7680.36	6143.13	6861.57	7052.86

*Denotes significance at 1% level.

**Uses NE choice set definition, but anglers can choose from both NE and SE sites.

***Site choice models identical, only inclusive value structure differs.

**** Site choice models identical, only inclusive value structure differs.

6.14. NE Results, CV per trip for a +1 fish change by year of analysis

Species	1994	1997	1998	1999	2000
Big Game	$ 21.21	$ 5.11	$ 0.93	$ 2.78	$ 1.48
Small Game	1.8	1.28	1.54	1.91	2.77
Bottom Fish	1.76	1.13	3.06	3.49	1.84
Flat Fish	4.2	5.22	4.59	5.25	6.9

6.15. NE Results, CV per trip for closure of state by year of analysis

State	Closure 1994	1997	1998	1999	2000
Connecticut	-$ 3.17	-$2.13	-$4.67	-$6.35	-$5.33
Delaware	-1.13	-0.78	-1.26	-1.73	-1.85
Maine	-8.54	-2.14	-7.42	-9.09	N/A
Maryland	-14.83	-15.31	-16.33	-20.03	-20.36
Massachussetts	-7.82	-11.95	-21.71	-19.45	-29.78
New Hampshire	-0.87	-0.38	-1.35	-2.6	N/A
New Jersey	-16.28	-16.83	-17.96	-18.73	-22.74
New York	-22.41	-18.35	-24.29	-31.18	-34.15
Rhode Island	-3.84	-6.72	-9.76	-10.95	-9.86
Virginia	-47.3	-28.94	-36.27	-33.61	-34.65

6.16. SE 2000 CV for a +1 fish change in catch rate

Species	2000
Big Game	$ 1.98
Small Game	1.63
Bottom Fish	1.1
Flat Fish	13.89

6.17. SE 2000 Results, CV per trip

State	2000
Alabama	-$2.01
Florida, Atlantic	-66.18
Florida, Gulf	-21.02
Georgia	-3.62
Louisiana	-14.84
Missippi	-6.05
North Carolina	-9.84
South Carolina	-11.19

Chapter 7. Controlling for Temporal Variation in Benefits Transfers

In this chapter, we focus on the ability to use the results of MRFSS RUM models for the purposes of benefits transfer. We demonstrated in earlier chapters the difficulties with applying benefits transfer techniques in general. In this chapter, we focus transferring recreational angling benefits across time in the Northeast. The temporal transfer exercise focuses on the Northeast because a significant time series of virtually identical data collection efforts exists: 1994, 1997, 1998, 1999 and 2000.

In discussing the results, we focus on their statistical and economic significance. As will be seen, even when data collection efforts are almost identical and identical modeling techniques and decisions are used, benefit transfer results are tenuous at best. Due to the overwhelming amount of data available from the MRFSS statistical tests overwhelmingly reject the use of benefit transfers across time or geography. However, in certain circumstances, the benefit transfer error is relatively small in terms of economic significance. The large number of observations leads to small standard errors (and consequently confidence intervals) on model parameter estimates. The statistical rejection of the similarity of model parameters across time might lead to an unjustified rejection of benefit transfer as a valid technique.

To begin, we look at the stability of RUM parameter estimates across the five estimated RUM models for the NE MRFSS. Recall Table 6.13 which presents the results of RUM models estimated on the Northeast data for each individual year. To test the stability of model parameters across time, we pool the five years of Northeast data and estimate a single pooled Northeast model using the same decision structure as chapter 6.

Table 7.1 presents the results of this pooled model. Focusing on the site choice decision only, the pooled model provides a log-likelihood function value of -190,245.57. This represents the likelihood function value for the site choice model estimated under the null hypothesis that the models structural parameters are the same in all years. Under the alternative hypothesis, parameters are allowed to vary by year, as in Table 6.13. Summing the log-likelihood function value across all years yields an unrestricted log-likelihood function value of -264,268.27. A likelihood ratio test of the null hypothesis of identical parameters across all years for the Northeast data can be performed by taking twice the difference between the restricted and unrestricted log-likelihood function values. This likelihood ratio statistic is distributed chi-squared with 44 degrees of freedom. Forty-four is the number of parameter restrictions: 11 model parameters across 5 years. One year is allowed to vary freely and all other years are restricted to be equal to the free year. Thus, 11 parameters restricted across 4 years. The likelihood ratio statistic for this case is 148041.36. The critical value for the test is 73.17 leading to overwhelming rejection of the null hypothesis of equal parameter estimates across all years. We also conducted numerous other parameter restriction tests using combinations of year pairs (e.g. restricting variables for only 1998 and 1999), variables (e.g. restricting only travel cost and travel time across years, restricting only catch parameters across years), and by not restricting the big game parameter. Each set of restrictions was rejected. These results are available from the author.

The overwhelming rejection of equality of parameter estimates across years is not surprising given the number of observations we are dealing with. The pooled model consists of 68,793 observations. The large number of observations yields extremely small standard errors on parameter estimates and thus even very small changes in parameters across time will be rejected due to the minute standard errors. Perhaps of more interest is an investigation of whether the models from any randomly chosen year are accurate in predicting the value of recreational fishing in other years.

To look at the ability of any given model year's ability to predict welfare values in other years we employ a benefit function transfer for the Northeast data. For each data collection year, we take that year's data and plug it into the models estimated for the other four years. We then calculate transferred value estimates for each of the reference years. For example, suppose 1994 is chosen as the baseline year. Our benefits transfer exercise asks the question, suppose we use the estimates from 1997, 1998, 1999, and 2000 to estimate the benefits in 1994 based on the 1994 data. That is, suppose we transferred the benefit function from any of the other 4 years to 1994, do we accurately estimate the value of recreational fishing in 1994?

Tables 7.2-7.6 present the results of the function transfer exercise. Consistent with Chapter 6 two sets of value estimates are reported: the value of an increase in catch by 1 fish for each targeted species and the value of lost access by state. Each table reports the benefit transfer error relative to the baseline year value estimates. Table 7.2 uses 1994 as the baseline, Table 7.3 uses 1997 as the baseline, and so on.

As an example, consider Table 7.2. Using 1994 as the baseline, table entries represent the percentage error in the transferred value relative to the counterfactual 'true' value from the baseline year's estimation. For example, Table 6.14 reports that the 1994 value of an additional big game fish in the northeast is \$21.21. According to Table 7.2, if we use the estimated parameters from 1997 as the candidate transfer function, that is, we plug the 1994 data into the 1997 estimate model, we would obtain a transferred value estimate 69% lower than the 'true' 1994 value (\$6.58 versus \$21.21). Using this presentation, the maximum underestimate is 100%, while any overestimate is unbounded.

Tables 7.2-7.6 tell a story about temporal benefits transfer that is slightly more optimistic than the overwhelming rejection on statistical grounds. The performance of the functional transfer estimates of the value of additional catch is highly variant. For example, Table 7.2 reports transfer errors ranging from -2.8% for small game in 1997 to 128.3% for small game in 2000. Although not definitive, a stylized observation emerges in Table 7.2: Benefit transfer estimates from the most recent year provide the smallest transfer error. That is, it appears that the further we diverge temporally from the baseline year, the less reliable are the estimated structural parameters for use in transfers. In Table 7.2, thirteen of the fourteen smallest transfer errors occur when the 1997 parameter estimates are used to calculate 1994 welfare. For the 1997 baseline year, 9 of the 14 smallest errors fall in contiguous years. The results are not as good for 1998, with only five of the fourteen smallest errors occurring within a data year of the baseline. For the

1999 baseline year, 9 of the 14 smallest errors are within one data year, while for the 2000 baseline year only four of the fourteen smallest are within one year.

In general, the value of lost access appears to be more accurately estimated in transfer than does the value of additional catch. Suppose we choose an arbitrary threshold of 50% error for a 'good' transfer estimate. This threshold is not supposed to represent our judgement of a good estimate but instead act as a threshold for determining the performance of two different measures of value (additional catch and value of lost access). From Table 7.2, 9 of the 16 transferred catch errors (56%) are greater than 50%, while only 5 of the 40 (12.5%) access estimate errors are greater than 50%. Similar for Table 7.3, 50% of the catch transfer errors are greater than 50% while 0% of the value of lost access errors are greater than the threshold. For Table 7.4, 50% of the catch errors and 2.5% of the lost access errors exceed the threshold. Baseline 1999 (Table 7.5) provides the closest performance between the two value measures with 44% of the catch value errors and 40% of the access value errors exceeding the 50% threshold. Baseline 2000 (Table 7.6) has 50% of the catch value errors and 5% of the access values exceeding the threshold. In general it appears that the value of lost access is more stable across time than is the value of additional catch.

In Tables 7.2-7.6, we also conduct a similar analysis for the value transfer method by comparing values estimated from a study site that are directly transferred to the policy site. There is less of a temporal pattern as compared to the function transfer results. For example, examining Table 7.4 it is evident that the closure values do not necessarily move toward the "true" value for state closures. For the function transfer values, they nearly all get smaller as the time of the study site approached 1998. For the function transfer estimates, this does not happen in all cases. In fact for New Jersey, New York, and Delaware the errors are increasing as the study time gets closer to the time of the policy dataset.

Table 7.7 summarizes the relative performance of the value versus function approaches undertaken here. Because of the poor performance of big game transfers in both sets of results, we ignore it in the comparisons presented in the table. First, we took the absolute value of the percentage error results presented in Tables 7.2-7.6. This was done because we needed a measure of performance that was invariant to over or underestimates due to the transfer. For example, using our measure of performance, a +25% and -25% error are equally bad and therefore, we assign both a score of 25%. We then average the scores across all combinations of years for Catch and Closure Changes, Catch only, and Closure Only. The results show that across all welfare measures, the Function Method has the smallest error, however, the value transfer method only trails by approximately 5%- meaning that on average the noise associated with the value transfer method is 5% larger than the function transfer method. For catch changes, the value transfer outperforms function transfer by approximately 8%. For Closure changes, the function transfer outperforms the value method by nearly 9%. Finally, we examine the number of individual welfare measures where the function approach outperforms the value transfer approach. The average absolute error approach may be misleading if one of the methods has a few cases that are either very large or very small. Function transfer is the preferred approach for nearly 64% of the welfare results analyzed in this report.

These results are interesting since the literature gives little guidance as to why the value function approach may outperform the function transfer for some types of transfers and not others. It is likely, that the result is partially because of differences in the scale of the error structure of the two models, and that for some type of welfare changes scale matters more (consistent with our findings in Chapter 4). For these cases, the function transfer method is not scaled properly, whereas the value transfer may be more scale independent across datasets.

The transfer results are hardly a ringing endorsement of the function transfer method. We show that both methods give rise to errors that are very close to one another, on average. Further, for some types of policy changes, the value function is clearly preferred. Consequently, value transfer seems like a viable approach using the MRFSS data.

For both transfer approaches, it is evident that there is a high degree of noise associated with big game parameter and welfare estimates from one year to another. This is consistent with the design of the MRFSS in that it might capture some big game fishing participants, but additional data methods are used by the agency to fully capture this group (the Highly Migratory Species Survey).

Table 7.8 presents the results of the joint estimation of the 1997 NE and SE random utility models. Given the poor performance of benefits transfers across time in the Northeast, it is not surprising that we overwhelmingly reject the consistency of the benefit functions across geographic regions for 1997. Likelihood ratio tests readily reject the equivalence of the estimated preference functions for the 1997 Southeast and Northeast MRFSS. Based on these results, we conclude that similar to temporal benefits transfers, geographic benefits transfers are unlikely to provide reliable transfer results. This may be partially attributable to the parsimonious specification of the RUM models. Given the size of the data sets and estimation tasks, it is not possible to fully control for differences in demographic characteristics and preferences across regions. To that end we conclude that separate geographic data collection efforts will need to continue into the future.

Table 7.1. Pooled Northeast Model (All Years).

Variable	Parameter
Site Choice	
Travel Cost	-.0160*
Travel Time	-.723*
ln(M)	1.246*
Big Game	.553*
Small Game	.489*
Bottom Fish	.533*
Flat Fish	.884*
Other	.296*
Choices	68793
-2*LogLike	240832.58
Model Chi Squared (all parms =0)	175999.20
Mode/Species Choice	
IV	.304*
IV_Target	N/A
IV_NoTarget	N/A
PRDUM	2.190*
CLDDUM	-.723*
Choices	68793
-2*LogLike	139662.55
Model Chi Squared (all parms=0)	30181.90

Table 7.2. Benefits Transfer Results for Northeast Datasets Across Time (baseline data from 1994, parameter vectors for function transfers denoted by year*)

	Function Transfer			
	+1 fish for each group			
	1997	1998	1999	2000
Big Game	-69.0%	-94.4%	-84.3%	-88.7%
Small Game	-2.8%	7.8%	32.8%	128.3%
Bottom Fish	-13.1%	98.3%	119.3%	47.7%
Flat Fish	55.5%	31.7%	43.1%	122.6%
	Closure			
	1997	1998	1999	2000
Connecticut	-21.8%	28.4%	61.5%	36.3%
Delaware	-19.5%	9.7%	40.7%	40.7%
Maine	-14.5%	15.5%	34.5%	17.2%
Maryland	-0.4%	37.0%	54.8%	31.8%
Massachussetts	2.4%	43.5%	61.9%	44.1%
New Hampshire	-20.7%	26.4%	62.1%	46.0%
New Jersey	2.6%	39.6%	53.5%	33.4%
New York	-2.9%	20.9%	37.6%	27.8%
Rhode Island	-5.5%	8.1%	25.5%	26.6%
Virginia	-4.7%	27.7%	46.9%	29.5%

	Value Transfer			
	+1 fish for each group			
	1997	1998	1999	2000
Big Game	-75.9%	-95.6%	-86.9%	-93.0%
Small Game	-28.9%	-14.4%	6.1%	53.9%
Bottom Fish	-35.8%	73.9%	98.3%	4.5%
Flat Fish	24.3%	9.3%	25.0%	64.3%
	Closure			
	1997	1998	1999	2000
Connecticut	-32.8%	47.3%	100.3%	68.1%
Delaware	-31.0%	11.5%	53.1%	63.7%
Maine	-74.9%	-13.1%	6.4%	N/A
Maryland	3.2%	10.1%	35.1%	37.3%
Massachussetts	52.8%	177.6%	148.7%	280.8%
New Hampshire	-56.3%	55.2%	198.9%	N/A
New Jersey	3.4%	10.3%	15.0%	39.7%
New York	-18.1%	8.4%	39.1%	52.4%
Rhode Island	75.0%	154.2%	185.2%	156.8%
Virginia	-38.8%	-23.3%	-28.9%	-26.7%

*Percentage Error is calculated by $\left(\frac{\mathrm{CV}^{\mathrm{transfer}}}{\mathrm{CV}^{\mathrm{Truth}}}-1\right)\times 100$.

Table 7.3. Benefits Transfer Results for Northeast Datasets Across Time (baseline data from 1997, parameters vectors for function transfers denoted by year*)

	Function Transfer +1 fish for each group			
	1994	1998	1999	2000
Big Game	227.4%	-84.3%	-56.4%	-68.1%
Small Game	-13.3%	3.1%	26.6%	134.4%
Bottom Fish	-1.8%	146.9%	168.1%	67.3%
Flat Fish	-47.9%	-22.8%	-17.4%	38.1%
	Closure			
	1994	1998	1999	2000
Connecticut	26.8%	0.9%	21.1%	30.5%
Delaware	37.2%	-21.8%	2.6%	24.4%
Maine	18.2%	19.6%	36.4%	36.0%
Maryland	-20.2%	24.8%	31.4%	12.7%
Massachussetts	-30.8%	37.7%	43.1%	47.6%
New Hampshire	-2.6%	5.3%	42.1%	50.0%
New Jersey	19.1%	21.8%	29.6%	24.4%
New York	-7.2%	17.4%	26.9%	18.6%
Rhode Island	221.3%	-19.5%	-7.4%	-20.2%
Virginia	4.3%	21.3%	34.7%	25.2%

	Value Transfer +1 fish for each group			
	1994	1998	1999	2000
Big Game	315.1%	-81.8%	-45.6%	-71.0%
Small Game	40.6%	20.3%	49.2%	116.4%
Bottom Fish	55.8%	170.8%	208.8%	62.8%
Flat Fish	-19.5%	-12.1%	0.6%	32.2%
	Closure			
	1994	1998	1999	2000
Connecticut	48.8%	119.2%	198.1%	150.2%
Delaware	44.9%	61.5%	121.8%	137.2%
Maine	299.1%	246.7%	324.8%	N/A
Maryland	-3.1%	6.7%	30.8%	33.0%
Massachussetts	-34.6%	81.7%	62.8%	149.2%
New Hampshire	128.9%	255.3%	584.2%	N/A
New Jersey	-3.3%	6.7%	11.3%	35.1%
New York	22.1%	32.4%	69.9%	86.1%
Rhode Island	-42.9%	45.2%	62.9%	46.7%
Virginia	63.4%	25.3%	16.1%	19.7%

*Percentage Error is calculated by $\left(\frac{\mathrm{CV}^{\mathrm{transfer}}}{\mathrm{CV}^{\mathrm{Truth}}}-1\right)\times 100$.

Table 7.4. Benefits Transfer Results for Northeast Datasets Across Time (baseline data from 1998, parameters vectors for function transfers denoted by year*)

	Function Transfer +1 fish for each group			
	1994	1997	1999	2000
Big Game	1741.9%	463.4%	177.4%	90.3%
Small Game	-23.4%	-15.6%	23.4%	118.2%
Bottom Fish	-61.4%	-60.5%	8.5%	-31.7%
Flat Fish	-39.4%	14.2%	7.8%	66.0%

	Closure			
	1994	1997	1999	2000
Connecticut	-7.5%	-37.3%	1.7%	17.3%
Delaware	13.5%	-23.8%	7.9%	27.8%
Maine	-12.8%	-27.5%	4.6%	5.1%
Maryland	-32.0%	-27.4%	8.1%	-2.9%
Massachussetts	-48.4%	-36.2%	-18.8%	2.3%
New Hampshire	-35.6%	-41.5%	43.0%	24.4%
New Jersey	-23.9%	-25.9%	-6.5%	2.3%
New York	-29.3%	-25.5%	14.5%	-3.0%
Rhode Island	282.6%	29.3%	1.7%	0.3%
Virginia	-21.5%	-26.7%	-19.5%	1.4%

	Value Transfer +1 fish for each group			
	1994	1997	1999	2000
Big Game	2180.6%	449.5%	198.9%	59.1%
Small Game	16.9%	-16.9%	24.0%	79.9%
Bottom Fish	-42.5%	-63.1%	14.1%	-39.9%
Flat Fish	-8.5%	13.7%	14.4%	50.3%

	Closure			
	1994	1997	1999	2000
Connecticut	-32.1%	-54.4%	36.0%	14.1%
Delaware	-10.3%	-38.1%	37.3%	46.8%
Maine	15.1%	-71.2%	22.5%	N/A
Maryland	-9.2%	-6.2%	22.7%	24.7%
Massachussetts	-64.0%	-45.0%	-10.4%	37.2%
New Hampshire	-35.6%	-71.9%	92.6%	N/A
New Jersey	-9.4%	-6.3%	4.3%	26.6%
New York	-7.7%	-24.5%	28.4%	40.6%
Rhode Island	-60.7%	-31.1%	12.2%	1.0%
Virginia	30.4%	-20.2%	-7.3%	-4.5%

*Percentage Error is calculated by $\left(\frac{\mathrm{CV}^{\mathrm{transfer}}}{\mathrm{CV}^{\mathrm{Truth}}}-1\right)\times 100$.

Table 7.5. Benefits Transfer Results for Northeast Datasets Across Time (baseline data from 1999, parameters vectors for function transfers denoted by year*)

	Function Transfer +1 fish for each group 1994	 1997	 1998	 2000
Big Game	541.0%	96.8%	-63.7%	-30.2%
Small Game	-38.7%	-30.9%	-19.4%	79.1%
Bottom Fish	-64.2%	-62.5%	-8.6%	-35.8%
Flat Fish	-44.0%	7.0%	-7.8%	54.1%
	Closure			
	1994	1997	1998	2000
Connecticut	102.8%	41.6%	67.5%	84.5%
Delaware	185.8%	76.1%	-16.8%	39.8%
Maine	-13.9%	-22.4%	-1.6%	-7.1%
Maryland	-32.6%	-24.9%	19.2%	13.1%
Massachussetts	6.6%	40.2%	156.8%	121.1%
New Hampshire	8.0%	-55.2%	51.7%	163.2%
New Jersey	-18.6%	-24.6%	0.4%	1.9%
New York	-12.7%	-13.9%	23.0%	19.7%
Rhode Island	947.9%	844.8%	140.9%	166.9%
Virginia	-51.6%	-53.4%	-38.4%	-37.8%

	Value Transfer +1 fish for each group 1994	 1997	 1998	 2000
Big Game	662.9%	83.8%	-66.5%	-46.8%
Small Game	-5.8%	-33.0%	-19.4%	45.0%
Bottom Fish	-49.6%	-67.6%	-12.3%	-47.3%
Flat Fish	-20.0%	-0.6%	-12.6%	31.4%
	Closure			
	1994	1997	1998	2000
Connecticut	-50.1%	-66.5%	-26.5%	-16.1%
Delaware	-34.7%	-54.9%	-27.2%	6.9%
Maine	-6.1%	-76.5%	-18.4%	N/A
Maryland	-26.0%	-23.6%	-18.5%	1.6%
Massachussetts	-59.8%	-38.6%	11.6%	53.1%
New Hampshire	-66.5%	-85.4%	-48.1%	N/A
New Jersey	-13.1%	-10.1%	-4.1%	21.4%
New York	-28.1%	-41.1%	-22.1%	9.5%
Rhode Island	-64.9%	-38.6%	-10.9%	-10.0%
Virginia	40.7%	-13.9%	7.9%	3.1%

*Percentage Error is calculated by $\left(\frac{CV^{\text{transfer}}}{CV^{\text{Truth}}}-1\right)\times 100$.

Table 7.6. Benefits Transfer Results for Northeast Datasets Across Time (baseline data from 2000, parameters vectors for function transfers denoted by year*)

	Function Transfer +1 fish for each group			
	1994	1997	1998	1999
Big Game	1004.1%	217.6%	-52.0%	41.9%
Small Game	-63.5%	-59.9%	-57.4%	-46.6%
Bottom Fish	-41.8%	-44.0%	47.3%	61.4%
Flat Fish	-63.8%	-31.9%	-46.2%	-41.6%
	Closure			
	1994	1997	1998	1999
Connecticut	12.4%	-41.1%	-5.4%	23.8%
Delaware	76.8%	-29.7%	-15.1%	11.9%
Maine	N/A	N/A	N/A	N/A
Maryland	-36.4%	-27.0%	3.6%	17.9%
Massachussetts	-42.5%	-36.4%	5.6%	15.5%
New Hampshire	N/A	N/A	N/A	N/A
New Jersey	-25.9%	-26.4%	1.1%	13.1%
New York	-25.6%	-24.4%	2.4%	15.4%
Rhode Island	213.4%	17.5%	-10.2%	4.5%
Virginia	-18.2%	-26.4%	-1.6%	13.2%

	Value Transfer +1 fish for each group			
	1994	1997	1998	1999
Big Game	1333.1%	245.3%	-37.2%	87.8%
Small Game	-35.0%	-53.8%	-44.4%	-31.0%
Bottom Fish	-4.3%	-38.6%	66.3%	89.7%
Flat Fish	-39.1%	-24.3%	-33.5%	-23.9%
	Closure			
	1994	1997	1998	1999
Connecticut	-40.5%	-60.0%	-12.4%	19.1%
Delaware	-38.9%	-57.8%	-31.9%	-6.5%
Maine	N/A	N/A	N/A	N/A
Maryland	-27.2%	-24.8%	-19.8%	-1.6%
Massachussetts	-73.7%	-59.9%	-27.1%	-34.7%
New Hampshire	N/A	N/A	N/A	N/A
New Jersey	-28.4%	-26.0%	-21.0%	-17.6%
New York	-34.4%	-46.3%	-28.9%	-8.7%
Rhode Island	-61.1%	-31.8%	-1.0%	11.1%
Virginia	36.5%	-16.5%	4.7%	-3.0%

*Percentage Error is calculated by $\left(\frac{CV^{\text{transfer}}}{CV^{\text{Truth}}}-1\right)\times 100$.

Table 7.7. Relative Performance of Value versus Function Transfer

	Function	Value
Average Absolute % Error (Catch + Closure (no big game))	43.0%	48.2%
Average Absolute % Error (Catch Only (no big game))	48.6%	40.7%
Average Absolute % Error (Closure Only)	41.2%	50.7%
% Cases where Function outperforms Value Transfer		63.6%

Table 7.8. Joint Estimation of Nested Random Utility Models

	NE 1997 with NE Structure***	NE 1997 with SE Structure***	Pooled NE/SE 1997 with NE Structure**,****	Pooled NE/SE 1997 with SE Structure**,****
Travel Cost	-.0185*		-.0177*	
Travel Time	-.540*		-.388*	
ln(M)	1.144*		1.093*	
Big Game	.976*		.792*	
Small Game	.413*		.367*	
Bottom Fish	.403*		.329*	
Flat Fish	.878*		.779*	
Other	.325*		.236*	
Choices	18224		25594	
-2*LogLike	71737.85		99900.16	
Model Chi Squared (all parms =0)	37259.06		43323.19	
IV	.066*	N/A	.022	N/A
IV_Target	N/A	.054*	N/A	.009
IV_NoTarget	N/A	.045*	N/A	.014
PRDUM	2.035*	2.036*	2.139*	2.139*
CLDDUM	-.560*	-.560*	-.442*	-.442*
Choices	18224	18224	25594	25594
-2*LogLike	88382.98	88333.82	129615.25	129609.06
Model Chi Squared (all parms=0)	7631.20	7680.36	123339.64	12345.84

*Denotes significance at 1% level.

**Uses NE choice set definition, but anglers can choose from both NE and SE sites.

***Site choice models identical, only inclusive value structure differs.

**** Site choice models identical, only inclusive value structure differs.

Chapter 8. Recommendations and Conclusion

The purposes of this report are two-fold. First, we present the complete set of estimation results for all of the economic valuation add-ons undertaken from 1994 to 2000. The National Marine Fisheries Service has invested significant resources to collect and analyze this data. It is important to note that this data collection was undertaken initially to quantify the total value of marine resources for various resources in the United States. For example, the data and models are well suited to address questions such as what is are anglers' willing to pay to avoid statewide closures of all marine fishing, or their willingness to pay for a program that would increase the biological conditions and hence the catchability of aggregated groups of species. The methodology is not designed to value access for a particular species, however, studies have shown that the data can be used to extend the models to the most commonly caught species (see Hicks and Steinback and the US EPA). By estimating all of the available data using a common methodology, we show none of models for a region and year are statistically equivalent to any other region and year pair.

This finding is both disappointing and not surprising- not surprising because of the large number of observations available for estimation (no models presented here use fewer than 4,500 observations, and most use over 10,000). Consequently estimated parameter estimates and the corresponding welfare estimates are statistically different, and in some cases different by orders of magnitude. At the same time, it is disappointing that an examination of value estimates across time and geography using identical data collection protocols, analysts, and models don't yield results that are more comparable.

Why care about comparability? If our results demonstrated some degree of comparability (to be determined by the agency or policy maker) then the agency might choose to collect this data less frequently secure in the knowledge that model parameter and welfare estimates are stable across time and perhaps even space. Scarce resources could then be diverted to other pressing data collection needs such as species-specific models that could analyze more policy relevant welfare changes, the value of aquatic habitat, or the value of protected resources.

What if the agency collected the base economic valuation data less frequently, say every two or every five years? Would the use of these models, compared to models based on data collected every year lead to large errors in estimating the economic value of large-scale policy changes? The National Marine Fisheries Service has a unique dataset for a systematic analysis of these questions. The dataset offers a natural experiment to answer these what-if questions by comparing errors associated with value transfer (e.g. taking the value from a given time period, region, and policy change and applying it to some other situation) and function transfer (e.g. taking the estimated function and applying it to some other situation). Using the set of results presented in this report, it is possible to examine the implications for welfare measures if, for example, instead of using 1997 values, 1994 values or parameter estimates were substituted.

Our results show that when comparing value versus function transfer, the function transfer method is the preferred method albeit just barely. Our results hardly find compelling reasons to use the more difficult function transfer method. Indeed for examining the economic benefits of catch changes, the value transfer method actually outperforms the function transfer method. The average absolute error of the function transfer method is only slight better than the value transfer method when examined over the broad range of policies examined here. Of course, there are particular cases where one method outperforms another in a striking way, but there is no clear winner when examining the entire range of transfers attempted in this report (the intra region transfers). This is contrary to findings in the literature showing that the value transfer approach is inferior to function transfer.

The question of whether benefits transfer is a feasible way to compute the economic implications of marine policy alternatives should be divorced from the issue of whether transferred benefits are statistically equivalent given the large number of observations available in the MRFSS data. The real question is how to best tradeoff the cost of imprecise economic valuation estimates (if benefits transfer is used) with the benefits of using scarce survey money to tackle some other important issue.

When looking over the broad range of policy changes analyzed here, our results suggest that within a geographic region, transferring benefits across time yields more reliable estimates when the study and policy issues are only a few years apart. As time between the policy event and the study increases the imprecision seems to increase. This lack of precision as time increases affects the function transfer more than the value transfer method.

Of course all of the function transfers investigated here assumed the existence of complete economic add-on data (question necessary to estimate the labor leisure trade-off, boat ownership, the trip was primarily made for fishing, etc.) at the policy site. If these add-ons are not available, then it is clearly not possible to conduct a function transfer as we have. In this case, the value transfer method would seem to be a reasonable way to proceed. Its advantages are obvious- preexisting programs could be easily adapted for an analysis of a particular policy.

Given our complete analysis of all of NOAA Fisheries' economic data, we do have some recommendations for which datasets to use in the future. We recommend the use of Southeast 2000, the Northeast 1999, and Pacific 1998 data for any benefit transfer exercises in the near future. The southeast data is the most recent, and while a Northeast 2000 dataset exists, it is not geographically complete since Maine and New Hampshire were omitted from the data collection effort.

We discourage the use of the Southeast 1997 and Northeast 1994 data. There is nothing inherently wrong with either of these datasets, however, changes in questionnaire design beginning in the Northeast in 1997 avoided asking the income question for those respondents not taking time off work without pay. By avoiding the income question, many more observations were collected. We would also recommend avoiding the

Northeast 1997 data since it does not allow analysts to properly quantify variables in the RUM model quantifying the labor leisure trade-off. We would also caution against placing too much faith in the estimated parameter on the Big Game species group, since the MRFSS survey was never intended to fully capture this species group nor its participants.

All of our estimation and comparisons presented in this report assume that the sampling effort (by mode, county, and wave) is consistent with observable proportions of the fishing population. If this is not the case, then our estimates can not disentangle angler behavioral parameters and sampling intensity (see Haab and McConnell). We strongly recommend that the agency re-estimate the "flagship" models for each region using sample weights that can be calculated using the random phone survey. If this weights are found to be equal to sampling proportions, then re-estimation is not necessary.

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