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Benefits & Costs Transfer in Natural Resource Planning

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Benefits and Costs Transfer in Natural Resource Planning

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The W-133 Regional Research Project entitled "Benefits and Costs Transfer in Natural Resource Planning" was recently rechartered by the U.S. Department of Agriculture, Cooperative State Research Service. The focus of the W-133 Project is the economic valuation of environmental resources and the application of economic values to facilitate environmental and natural resource decision-making. Researchers in over 25 states across the United States are involved in the W-133 Project through Land-Grant University Experiment Station appointments. One of the strengths of the W-133 Project is that it has also attracted much interest and participation from researchers from a variety of public and private colleges and universities, federal resource management agencies, and state resource management agencies. The active and widespread participation in the W-133 Project is reflected in the topics and authorship of the research papers presented in this report.

The specific objectives of the W-133 Project are to: 1) provide site specific use and nonuse values of natural resources for public policy analysis, and 2) develop protocols for transferring value estimates to unstudied sites. Research under these two specific objectives is targeted by W-133 participants at four resource areas: water based recreation, groundwater quality, wetlands, and recreational fisheries. The papers presented in this report represent major progress towards addressing these four problem areas and meeting the specific W-133 regional research objectives.

The first set of papers present environmental resource valuation case studies. These case studies demonstrate and assess techniques for valuing non-marketed resources in different policy and management situations. The second set of papers address several general topics related to resource valuation theory and modeling. These topics include use values, nonuse values, and demand model specification and estimation.

The third set of papers focus specifically on the contingent valuation method. The primary concern discussed in these papers is the proper application of discrete choice contingent valuation questions. The fourth set of papers present and discuss issues related to the transfer of benefit estimates in the policy and management arena. The papers in this section provide insight into conceptual and empirical problems which must be faced when attempting to use benefits transfer to assess environmental and natural resource policy and management.

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A Market Test of the Contingent Valuation Method: The Case of Bison Hunting Permits in Alaska

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Introduction

The most important advances in economics have resulted from testable hypotheses regarding how people behave. These hypotheses have been meticulously tested against observations of actual behavior. In contrast, non-market valuation techniques such as the contingent valuation method (CVM) are based on survey responses of how people say they *would* hypothetically behave if the hypothetical market existed. These techniques rely on stated intentions of behavior, not on actual observations of behavior. Furthermore, the information is typically gathered from individuals who have incentives for affecting the results. Thus, in spite of two decades of research in this area, many economists remain highly skeptical of non-market valuation techniques for estimating economic value.

The two main types of issues that arise in relation to the use of CVM are its validity and its reliability. Validity is broadly defined as "the degree to which it measures the theoretical construct under investigation" (Mitchell & Carson 1989, p. 190). Reliability is concerned with how accurate the measure is. Our primary concern in this paper is with validity. In particular, we are concerned with what Mitchell and Carson term "criterion validity." Essentially, the test is whether or not CVM estimates of value are comparable to estimates of value that are obtained in situations where responses are based on actual behavior. Mitchell and Carson term such tests as "hypothetical-simulated-markets."

Several such tests have appeared in the literature. Bohm's (1972) study of the value of closed circuit television programming was the first construction of such a test. Bohm concluded that responses to hypothetical questions resulted in "irresponsible" responses. Bishop and Heberlein (1979) tested the validity of CVM surveys by sending negotiable checks to hunters having drawn a goose permit in the Horicon zone of central Wisconsin. The willingness to accept (WTA) estimates obtained from this simulated market were compared with CVM responses to both WTA and willingness to pay (WTP) surveys. They concluded that "contingent values could easily be in error by 50% or more" (Bishop, Heberlein, and Kealy, 1983, p. 620). Bishop and Heberlein followed this with two, more exhaustive studies in 1983 and 1984 involving special early season deer hunting permits in the Sandhill Wildlife Demonstration area (Bishop, Heberlein, Welsh, and

Baumgartner 1984; Bishop and Heberlein 1985, 1986; Bishop, Heberlein, McCollum, and Welsh 1988). These studies used a simulated market for both WTA and WTP. In addition, they examined the effect of various market forms, including an incentive compatible fifth price auction (to sell four permits) in the 1983 study. They concluded that "contingent valuation is accurate enough, at least for items like hunting permits, to be useful for policy analysis" (Bishop, Heberlein, McCollum, and Welsh 1988, p. 8-3). Another study, by Dickie, Fisher, and Gerking (1987), conducted an experiment in which CVM was compared with actual behavior for valuing strawberries which were being sold door to door. Several studies have also extended the test to areas where existence value is being measured. Siep and Strand (1991) compared CVM results with actual expenditure data collected on people's contributions to environmental groups. Duffield and Patterson (1991) examined contribution rates for in-stream improvements in Montana. Siep and Strand and Duffield and Patterson found that non-zero bids were much lower for actual transactions than for CVM. There have also been several laboratory experiments such as the 'SOA' experiment by Coursey, Hovis, and Schulze (1987) which looked at the effects of iterated bidding on convergence of WTP and WTA using an incentive-compatible demand-revealing pricing mechanism.

This paper reports the results of an experiment in which contingent valuation methods are compared with data obtained from hunters who were allowed to buy and sell hunting permits for a bison hunt in the Delta Junction area of Alaska.[1] The experiment was conducted in conjunction with the Alaska Department of Fish and Game (ADF&G), which manages the Delta bison herd. The participants in the experiment were all persons who had applied for a lottery-drawn bison hunting permit in the Delta Junction area.

This study is unique in several respects. Unlike each of the previous studies, this experiment literally creates a market. That is, the net number of permits held by hunters does not change as the sellers of permits are selling the permit to another hunter, rather than to a university researcher. Like Bishop and Heberlein's 1983 Sandhill study, we use an incentive compatible demand revealing mechanism as our control case, and, like the 1984 Sandhill study, we let the market determine the number of permits sold. However, our

[1]The bison herd was introduced to Alaska in the 1920s using stock from Montana. In addition to the herd at Delta, there is a small herd near McCarthy. However, the Delta herd is the more popular of the hunts because it is accessible by road.

experiment, in contrast to either of the Sandhill experiments, allowed *both* demand and supply conditions to determine the number of trades that would occur. Essentially, we use a one shot version of the SOA experiment used by Coursey, Hovis, and Schulze. In addition, the value of the goods being considered in this study is considerably higher than that of earlier studies. In this sense, the present study is less of a negative test than previous tests. Our pretest studies suggested that the selling price in our simulated market would be $500-600. The actual selling price turned out to be over $1000. In contrast, the bids used in the Sandhill studies were in the range $0-512 for deer permits and $0-200 for goose permits. We received cashier's checks up to $1750.00 in value in our WTP market experiment.

The Delta bison hunt is especially favored among hunters. It offers an experience in which the game is a trophy animal, possessing large quantities of high quality meat. In addition, the hunting area is generally road-accessible, a rarity in Alaska. Each year, approximately 8,000-10,000 people pay a non-refundable $10 fee to participate in the lottery, while only approximately 100 names drawn each year. All participants in the study were drawn from the population of 1992 applicants.

The bison hunting permit was chosen because it was a drawing in which many people applied year after year. Thus it was believed that the hunting community would have well formulated thoughts about how much the permit might be worth. In addition, the bison hunting permit was somewhat unique in Alaska hunting in that the hunting is exclusively sport hunting; no subsistence has occurred in recorded history on the bison herd. This was an important criterion since conflict between sport and subsistence hunters is likely to create problems for valuing species hunted by each group due to the enormous political problems associated with subsistence/sport hunting conflicts.[2]

[2]Two special sessions of the state legislature have been called in the last three years to deal with the conflict over subsistence hunting in Alaska. The legal problem stems from the conflict with the 1980 Alaska National Interest Land Claims Act and the Alaska State Constitution. See Boyce and Logan (1993) for a discussion.

Structure of the Experiment

The objective of the experiment was to test the validity of contingent valuation measurements of the value of a good such as a hunting permit. The methodology chosen for such a test was to compare the results with participants in an actual market. The "market" was constructed to be an incentive-compatible, demand-revealing process, using a variation of the Vickrey second-price auction. Participants were asked to make a bid (for buyers) or offer (for sellers). The market was set up so that those participants whose bid (offer) was accepted would receive a refund (bonus) equal to the difference between their bid (offer) and the market price. Those buyers whose bid was not accepted were promised a prompt refunding of their bids. Those sellers whose offer was not accepted kept their permit. The market price was determined endogenously as the intersection of the observed demand (WTP) and supply (WTA) schedules.

A number of restrictions of this "market" make it much different from markets the subjects are likely to have participated in the past (Carson 1991). Subjects were given a short window of opportunity (five weeks) to participate in the market. They were also restricted from transferring the permit outside of the market. (They had to pick up the permit at the site, and each permit had a name attached to it.) Each participant was allowed only one sealed bid (or offer). Furthermore, if the bid was to buy, they had to pay the amount of the bid *prior* to knowing whether they had been successful in obtaining a permit. Thus participants who might ultimately be unsuccessful were being asked to tie up money in the amount of their bid for a minimum of at least a week. The legal staff of ADF&G also required that the buy bids be tendered by means of a cashiers check or money order.[3] Also, the experiment was conducted using letter-head of ADF&G, rather than that of a university or research consultant. Our analysis of the protest bids suggest that a number of participants believed ADF&G was conducting the research in order to set fees for future hunts.

Participants for the market were drawn at random from the pool of permit applicants. No one was told in advance of the deadline for applying for the permit that the experiment was going to be conducted. A brief announcement of the upcoming experiment appeared in the "outdoors" sections of the major local

[3]Three bids, in fact, were rejected in the market transactions because the subject sent a personal check. The bids would not have affected the outcome of the market. We included these bid in our statistical analysis.

newspapers following the May 31, 1992 deadline for applying for a permit. It was felt that if people knew of the experiment before they received our letter in the mail, they would be more likely to find credible our request for their participation.[4]

All participants in the study were first contacted by mail on July 15, 1992. Those participating in the simulated market were given until August 24, 1992 to respond. A second mailing to non-respondents occurred on August 5th. No telephone contacts were made with any of the subjects except to answer questions about the study.

There were 80 either-sex hunting permits drawn in 1992.[5] The number of potential buyers was 284. In addition to the 364 people participating in the market experiment, we selected four other groups of participants. These groups were surveyed using contingent valuation survey instruments. Table 1 shows the structure of the entire validation experiment. The experiment collected data from both WTP and WTA groups.

In each case, in addition to the simulated market experiments (groups 1 and 6), two CVM studies were conducted: participants in groups 2 and 7 were asked to bid as if they had participated in the open ended Vickrey simulated market; and participants in groups 4 and 9 were given simple open-ended surveys. The sample sizes for groups 2, 4, 7, and 9 were 300 persons each.

The survey instrument was a 16 page booklet, color coded by groups. Return envelopes were bar-coded to keep track of non-respondents. The survey instrument varied from group to group only in the first three sections of the survey. These sections collected the WTP/WTA information and data on protest behavior. All groups received, in addition to the survey and cover letter, a sheet containing "Questions and Answers," intended to provide more information about the study. Those persons in groups 1 and 6 (the simulated markets) also received a separate "Offer to Sell" or "Offer to Buy" form, which included the legal description of the transaction and served as the actual contract. Persons in groups 1, 2, 6 and 7 (the simulated

[4]To give an idea of how effective the publicity was in getting people to thinking about the project, we received over a half-dozen surveys from people who had been non-respondents to the pretest, which occurred in March, 1992.

[5]There were also 20 "cow-only" permits drawn. Our study focused only on the "either-sex" permits.

and hypothetical Vickrey auctions) each received a "Description of the Market" page, which explained by use of an example how the market price would be determined. These documents are all available from the authors.

We began each survey with the valuation question. In our cover letter, we explained that we were interested in determining the value that hunters placed on the Delta bison permits, and that randomly drawn persons were being given the chance to actually buy permits from those who had drawn them in the lottery drawing. Section I thus posed the CVM question appropriate for the group. For the hypothetical WTP groups, the contingent valuation questions are given in Table 2.

Section II of each survey contained questions about why the respondent answered the way they did in the valuation question of Section I. This data was used to analyze protest bids. Section III repeated the valuation question (hypothetical to all participants) for a "cow-only" bison hunting permit. This part of the study is not reported here. Sections IV and V asked about attitudes regarding the Delta bison hunt and about the participant's other hunting activities, respectively. Section VI gathered information about several proposed management options for the Delta bison herd. Section VII collected demographic data.

Results

Our primary interest was to make a comparison of the estimates obtained using CVM relative to the simulated market. In addition, we only had time for two mailings for the simulated markets (groups 1 and 6) since the lottery drawing was not be held until July 10th, and the hunt began on September 1. The pretest results suggested that our total response rate on two mailings would be greater than 50%. Thus, we felt that for purposes of comparison, two mailings would be sufficient.

Table 3 shows the response rates obtained in the study. They are exceptionally high, ranging from 51% to 88% total returns. (The second round return percentages are the returns from non-respondents to the first mailing.) All but group 1 (51%) had overall response rates greater than 67%. The lowest first round response rates occurred with group 1. This was to be expected since this group was being asked to send a check in the amount of their offer.

However, the high response rates were offset by the equally high zero-bid response rates. A zero bid was recorded for each person who actually stated their bid offer was zero and for each person who did not indicate a bid or offer (but did return the survey). The highest zero bid rates occurred for group 1, the participants who actually were able to tender a bid to purchase a permit. While 51% of those in group 1 responded to the survey, only 1/3 of that number actually tendered a positive bid. Thus the total number of actual positive bids was about 1/6th of the sample size, or 47 bids. The open ended WTP contingent valuation surveys had the lowest zero bid rates, with 30% for group 2 and 28% for group 4. There were also a number of blank bid forms for the WTA groups. In this case, a zero bid was recorded, but is not likely to be a true reflection of the value of the permit. For the WTA groups, the zero bid rate averaged between 45-49%.

Table 3 also contains information about the means and medians of the *entire* sample of responses. There has been no correction for protest bids. We turn to that issue below. This data is provided to show the differences between the respondents to the first and second mailings. The results from this level of analysis are as expected: the means for the both WTA and the WTP are in every case greater than the medians. The means are influenced by the few very large bids and offers, while the medians are unaffected. The standard result that the WTA estimates are much larger than the WTP results also are found here, although this appears to be much more prevalent with the means than with the medians. There also appears to be evidence of avidity bias (Thompson 1991) in the response rates since the means and medians of the first mailings are generally larger than those of the second mailings.

The data in Table 3 on the means and medians of the WTP/WTA is not taken to be an estimate of the value of the bison hunting permits. A number of the bids may in fact be protest bids or attempts to influence the outcome. Zero bids may in fact represent an unwillingness to participate in the research rather than a true estimate of value. Some of the bids may be estimates of what the respondents believe to be a "fair" price or what the would expect a market price to be. All of these factors could influence the bid made by a particular individual. The dichotomous choice responses are subject to similar problems.

To control for such factors, we included a set of questions asking people why they bid the way that they did. Of course, for a sophisticated player, the information content of these questions is suspect. To control for the influence of factors that might lead people not to respond with their true WTP or WTA, we examine the reasons given for the bid or offer made. Table 4 shows the number of participants who stated that they agreed with statements at left. The data in Table 4 includes people who stated their sole motivation for their bid was for the reason at left. The columns do not add up because some people left this section blank and others gave more than one reason.

We eliminated all of the observations where the respondent indicated that she considered anything other than bidding what they truly believed the permit to be worth to them. For the WTA, this eliminated all of the zero-bid responses. However, a number of zero bid responses remained in the WTP. We also eliminated the small number of WTA bids over $100,000.[6] Table 5 shows the results.

The percentage of non-zero bids that also passed the test of being claimed as being "true" WTP or WTA is lowest for group 1, the simulated WTP market bids. Of the 67 persons responding that their bid reflected their true WTP, only 35 (52%) bid a price greater than zero. In contrast, in both group 2 and group 4, the number of zero bids that are "true willingness to pay" bids is extremely low, between 3 and 7%. On the WTA side, there were no zero bids that were reported as the true WTA.

In addition to the high percentage of zero bids for the simulated WTP market, the other feature that stands out in Table 5 is the difference between the means and medians between the simulated markets and the hypothetical markets. When the zero bids are included, the WTP mean estimate for the simulated market (group 1) is much smaller than the estimate obtained using each of the CVM methods. This is not simply due to the influence of a couple of high bids; the median values show the same tendency. For the WTA groups, the result is that the simulated market produces results much higher (ten-fold higher) than the result obtained from any of the CVM methods.

[6]There were no bids between $100,000 and $1,000,000. Thus all bids over $100,000 were actually $1,000,000 or over.

The result is less pronounced when the zero bids are excluded for the WTP cases. (The WTA data has no zero bids.) Indeed, the medians are virtually indistinguishable for the three open-ended methods for the WTP data. It is interesting to note that the hypothetical Vickrey CVM (group 2) produced higher results in the WTP estimates than did the pure open-ended format. This result could be due to the novelty and complexity of the Vickrey method in the minds of respondents.

Comparison of Distributions

The foregoing analysis is based entirely on a comparison of the means, with no account for variance being made. Clearly, this is not sufficient. In this section, we examine the entire distributions. A test of similarity of the distributions was made using Pearson's chi-square goodness of fit test. This test requires that the intervals be chosen with care.

Figure 1 shows the empirical cumulative density functions (cdfs) for the WTP experiments. All observations where the respondent claimed to be giving the "true" WTP are included in the construction of these cdfs. The solid line corresponds to group 1, the simulated Vickrey market; the dashed line corresponds to group 2, the hypothetical Vickrey market; and the dotted line corresponds to the open ended CVM, group 4; The cdf for group 1 is much higher at every bid value than either of the hypothetical distributions.

For comparison purposes, turn now to Figure 2. This figure contains the cdfs for the WTP groups with the zero bids removed. It is clear from comparing the two figures that the distributions in Figure 2 are much more alike than are the distributions in Figure 1. Thus, for both the means and the distributions, the WTP hypothetical markets appear to overstate the true value by the much lower preponderance of zero bids.

Figure 3 shows the empirical cdfs for the WTA experiments. Group 6 (simulated Vickrey market) is the solid line; group 7 is the dashed line, and group 9 is the dotted line. The simulated market shows a much lower propensity to give up a permit at lower prices than does the hypothetical markets. There is no "fix" for the WTA distributions by excluding the zero bids as there are no zero bids. Therefore, we must conclude that the WTA distributions for the hypothetical markets are different from the WTA distribution for the simulated market.

The Pearson's chi-squared test statistics and the data used to calculate the statistics are contained in Table 6.[7] The intervals were chosen to give roughly equal sized predicted proportions. For the WTP data, the tests in Table 6 use the zero observations for those respondents who claimed that zero was their true WTP. It is clear that the inclusion of the zeros is what causes the chi-squared test statistics to be so large in the WTP experiments. In each case, the test statistic is in the rejection region for the hypothesis that the two distributions are drawn from the same population. The test statistics for comparison of the simulated WTA data and the hypothetical WTA data are also in the rejection region. However, these latter tests may not be valid. Law and Kelton (1989, p. 385) suggest that two rules be observed in calculating the test statistic: that there are at least three groups, and that the predicted number of observations for each group be at least of size 5. However, Group 6 only had 15 total valid WTA observations, so it is impossible to satisfy both criteria.

Parametric Tests of the Differences Between Means

So far, we have established that the distributions of the observed bids (offers) for the WTP (WTA) obtained in the simulated markets are not the same as those obtained from the hypothetical CVM surveys. In this section, we explore some simple parametric tests of the differences between the means. In constructing these tests, we control for other factors, such as demographic characteristics and reported attitudes on issues related to hunting.

The demographic information we collected is the standard income, education, where raised, number in household, and hunting history type of data collected in such studies. As much of the data was collected using categorical variables, we follow Cameron's (1987) suggestion and use dummy variables to differentiate between the different categories. The consequence of this is that many of the variables are statistically insignificant. In each case, the left out variable is the last category in each list.

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[7]Two other tests were also conducted. They were a test of whether group 2 differed from group 4, and whether group 7 differed from group 9. Using 8 categories for the WTP experiment, the chi-squared test statistic was 8.71 (with 7 df). This has a p-value of 0.27. Thus, the hypothetical WTP open ended and Vickrey CVM distributions could not be distinguished from one another. The test statistic for a comparison of group 7 and 9 was equal to 1.05, with 4 df. Thus the hypothetical WTA open ended and Vickrey CVM empirical distributions were also indistinguishible from one another.

Table 7 shows the ordinary least squares estimation of the WTP or WTA value over each of the open-ended responses cases (groups 1, 2 and 4 for WTP and groups 6, 7, and 9 for WTA). The variables GROUP2, etc., are dummy variables indicating the observation was from group 2, etc. The test of whether the means are different for the simulated versus hypothetical groups is thus a *t* test on the significance of the dummy variable for the hypothetical groups. Since there are so many variables when the model is run with the complete list of exogenous variables, we ran a regression with the two dummy variables for the hypothetical groups along with the set of variables from each other set of dummy variables (e.g., with the 17 income dummy variables). Then we excluded all variables whose *t*-statistic is less than 1.5 for either equation. The variables that are reported are the only variables satisfying that criteria.

The variables which offered explanatory power in the WTP equations were a dummy variable indicating the person had answered "strongly agree" to the statement that they want to hunt bison for the trophy, and a dummy variable indicating the person had been raised in a town of less than 5,000 population. Each of these variables had a positive effect on price. For the WTA equation, four variables passed the first-round criteria: These were a dummy variable for persons responding "probably disagree" to the statement that bison are important to Alaska, and variables measuring the number of years the person has applied for a permit, the number of years they have lived in Alaska, and the average age of persons living in the household. All but the average age of household had a positive effect on the amount the person stated was the lowest they would accept to give up her permit.

In the WTP regression, the sign of both of the coefficients for the hypothetical group dummy variables is positive, indicating that the hypothetical responses are on average higher than the responses for the simulated market. However, the coefficient for the group 4 dummy variable is not significantly different from zero. The WTP regression had a very low adjusted R^2, only about 0.086. This may account for the acceptance of the hypothesis that the mean WTP response from group 4 is indistinguishable from the mean WTP response from group 1. For the WTA regression, the hypothetical group dummy variable coefficients are negative and highly significant. The coefficients for the years applied for hunting permits and the years lived in Alaska are not statistically different from zero. The fact that each variable came in significant in regressions in which the other was excluded suggests that these two variables are probably collinear.

The analysis now presents a two pieces of conflicting information. The WTP distribution for the hypothetical markets are statistically different from the WTP distribution from the simulated market. However, only for the hypothetical group 2 do the means of the WTP appear to be different. Thus, we conducted one more level of analysis to the data. Our analysis of the distributions suggested that the main difference was in the number of zeros. This is a testable hypothesis using probit analysis. We also use Heckman's (1976) two-stage method of Tobit analysis to correct the mean of the OLS regression in Table 7 for possible biases relating to the censoring of the WTP bids at zero.[8] The Heckman analysis results are contained in Table 8.

The probit analysis suggests that the probability of a zero WTP bid for the simulated group (which is the excluded dummy variable) is significantly higher than for the other two groups. However, when the Heckman two-stage correction for the censoring bias is applied to the ordinary least squares equation, neither of the hypothetical group dummy variables are significant. We conclude that the difference between the WTP bids for the simulated and hypothetical markets is in the zero bids.

Conclusions

The direction of the differences between the mean WTP and WTA found in this study are identical to those found in the Sandhill studies (Bishop, Heberlein, Welsh, and Baumgartner 1984; Bishop and Heberlein 1985, 1986; Bishop, Heberlein, McCollum, and Welsh 1988). However, in the Sandhill studies, the differences were statistically insignificant. We find that the distributions of WTP and WTA bids are statistically different when the data involves real cash transfers. On the willingness to accept side, it appears that the participants in the simulated market demanded a much higher price than those in the hypothetical groups. On the WTP side, the participants in the simulated market were willing to pay a lower price than those in the hypothetical groups. However, the WTP groups differ mainly in the number of zero responses. The simulated market had a much higher number of zero responses than did the hypothetical markets.

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[8]See Maddala, pp. 158-9.

The finding of a larger number of zero bids for the simulated WTP market is consistent with findings by Duffield and Patterson (1992) and Seip and Strand (1991). We believe that this is an important consideration in evaluating contingent valuation estimates. People appear to have responded to our CVM questions with values that actually exceeded what they truly were willing to pay when faced with the decision of whether or not to put their money on the line.

While there are a number of reasons that our study may have promoted such behavior (e.g., by requiring a cashier's check to be delivered with the bid), we believe that the consistency of this result with earlier studies should serve as a warning that something is driving this phenomenon. One reasonable answer appears to be that people are not always really willing to pay what they say they would be willing to pay on a contingent valuation survey.

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Table 1. The Experiments			
Group	Willingness to Pay	Group	Willingness to Accept
1	Simulated Market: Vickrey Auction	6	Simulated Market: Vickrey Auction
2	Hypothetical Market: Open Ended Vickrey	7	Hypothetical Market: Open Ended Vickery
4	Hypothetical Market: Open Ended	9	Hypothetical Market: Open Ended

[a]Two experiments (3 and 8) using dichotomous choice methods were conducted. That data will be reported in a later paper. Two additional experiments (5 and 10) were planned, which were to have repeated experiments 2 and 7, but with iterated bids. These were abandoned because we did not have the resources to adequately run the experiments.

Table 2. The Contingent Valuation Questions	
Group	Willingness to Pay Questions
2	"If I were really able to buy one 1992 either-sex Delta Bison Hunting Permit in a market like the one described, the *most* I would be willing to pay is $________.
4	"If I were really able to buy one 1992 either-sex Delta Bison Hunting Permit, the *most* I would be willing to pay is $________.
Group	Willing to Accept Questions
7	"If I had drawn a 1992 either sex Delta Bison Hunting Permit and I were really able to sell it in a market like the one described, the least I would be willing to accept in exchange for my permit $____________.
9	"If I had drawn a 1992 either sex Delta Bison Hunting Permit and I were really able to sell it, the least I would be willing to accept in exchange for my permit is $_______."

Table 3. Response Rates and Summary Statistics by Group

Group	N	Mailing	Number Returns	Percent Returns	Mean $	Median $	Zero Bids	Percent Zero
1	284	first	75	26	119	0	48	64
		second	69	33	128	0	49	71
		all returns	144	51	123	0	97	67
2	300	first	151	50	535	200	39	26
		second	49	33	256	100	20	41
		all returns	200	67	467	125	59	30
4	300	first	177	59	235	100	46	26
		second	53	43	252	50	18	24
		all returns	230	77	239	100	64	28
6	80	first	54	68	194,897	500	27	50
		second	16	62	84,974	6,050	5	31
		all returns	70	88	169,772	2,250	32	46
7	300	first	145	48	22,956	500	63	43
		second	60	39	18,554	0	37	62
		all returns	205	68	21,668	100	100	49
9	300	first	150	50	101,694	250	67	45
		second	54	36	19,905	150	25	46
		all returns	204	68	80,044	225	92	45

Table 4. Why People Stated They Responded in the Way They Did						
Zero bids/'NO' responses	1	2	4	6	7	9
Bid true WTP/WTA	32	10	4	--	--	--
Couldn't estimate WTP/WTA	4	2	3	1	0	4
Didn't want to participate in research	1	2	1	0	2	0
Thought might be charged to hunt	9	3	2	0	4	5
Selling permits is a bad idea	25	21	33	1	13	10
Cannot value wildlife or hunting	1	1	1	0	0	0
Not-for-sale (WTA only)	--	--	--	19	46	36
N	97	59	64	32	100	92
Positive bids/'YES' responses	1	2	4	6	7	9
Bid true WTP/WTA	35	108	123	17	53	50
Bid was a "fair" price	0	5	2	0	1	2
Value should be as high as possible	2	5	3	2	7	3
Thought might be charged to hunt	3	3	10	1	1	2
Selling permits is a bad idea	3	3	2	1	0	7
Cannot value wildlife or hunting	0	0	0	0	0	0
Not-for-sale (WTA only)	--	--	--	7	23	24
N	47	141	166	38	105	112

Table 5. Mean and Median Responses for those Who Bid their 'True' WTP/WTA						
	Group					
	1	2	4	6	7	9
All Bids						
Mean	$215	$618	$364	$12,233	$1,823	$1,757
Median	$25	$275	$200	$10,000	$1,000	$1,000
N	67	118	127	15	53	49
Non-Zero Bids						
Mean	$413	$676	$376	$12,233	$1,822	$1,757
Median	$300	$300	$250	$10,000	$1,000	$1,000
N	35	108	123	15	53	49
Percent Non-Zero	52%	92%	97%	100%	100%	100%

Table 6. Chi-Squared Tests of Similarity of the Empirical Distributions						
WTP Experiments						
	Group 1 against Group 2		Group 1 against Group 4		Group 1 against Groups 2 and 4 combined	
Range	N	Np	N	Np	N	Np
$0-$99	35	11.36	35	11.08	35	11.21
$100-$199	8	12.49	8	12.13	8	12.31
$200-$399	10	13.63	10	21.63	10	17.78
$400-$599	8	15.90	8	12.66	8	14.22
$600-$20,000	6	13.06	6	9.50	6	11.21
	67		67		67	
chi-squared statistic	59.55		62.32		60.52	
Pr(^{2}>observed)	0.00		0.00		0.00	
Degrees of Freedom	4		4		4	
WTA Experiments						
	Group 6 against Group 7		Group 6 against Group 9		Group 6 against Groups 7 and 9 combined	
	N	Np	N	Np	N	Np
$0-$599	5	5.09	5	6.12	5	5.59
$600-$1,499	5	4.25	5	3.06	5	3.68
$1,500-$30,000	5	5.66	5	5.82	5	5.74
N	15		15		15	
chi-squared statistic	19.86		19.02		19.45	
Pr(^{2}>observed)	0.00		0.00		0.00	
Degrees of Freedom	2		2		2	

Table 7. OLS Dummy Variable Tests of Differences Between the Means

WTP			WTA		
Variable	Estimate	*t*-ratio	Variable	Estimate	*t*-ratio
CONSTANT	-86.46	-0.45	CONSTANT	14042.88	10.21
GROUP2	523.59	2.31	GROUP7	-11909.04	-10.18
GROUP4	154.09	0.70	GROUP9	-11525.95	-9.98
TROPHY SA	616.04	3.50	IMPORTANT PD	8166.00	2.98
RAISED T5K	925.92	3.20	YRS PERMIT	77.76	1.08
			YRS ALASKA	44.98	1.12
			AVE AGE	-71.93	-2.61
F(4,242)	6,790		F(6.96)	21.188	
N	247		N	103	
Adjusted R^2	0.086		Adjusted R^2	6.543	

Table 8. Tobit Dummy Variable Tests of Differences Between the Means					
Stage 1: Probit Analysis of the probability of a positive WTP bid			Stage 2: OLS analysis of the WTP bids with a correction for censoring		
variable	Estimate	*t*-ratio	variable	Estimate	*t*-ratio
CONSTANT	-0.19391	-0.96	CONSTANT	4999.74	1.34
GROUP2	1.48896	5.64	GROUP2	-2819.68	-1.15
GROUP4	2.03786	6.56	GROUP4	-4176.02	-1.33
TROPHY SA	0.36412	1.41	TROPHY SA	168.06	0.38
RAISED T5K	-0.08713	-0.20	RAISED T5K	1286.73	3.77
			ESTIMATED PDF	-12421.21	-1.33
LLR Chi-square	64.66		F(5,241)	6.504	
% correct	85.42		Adjusted R^2	0.101	

FIGURE 1

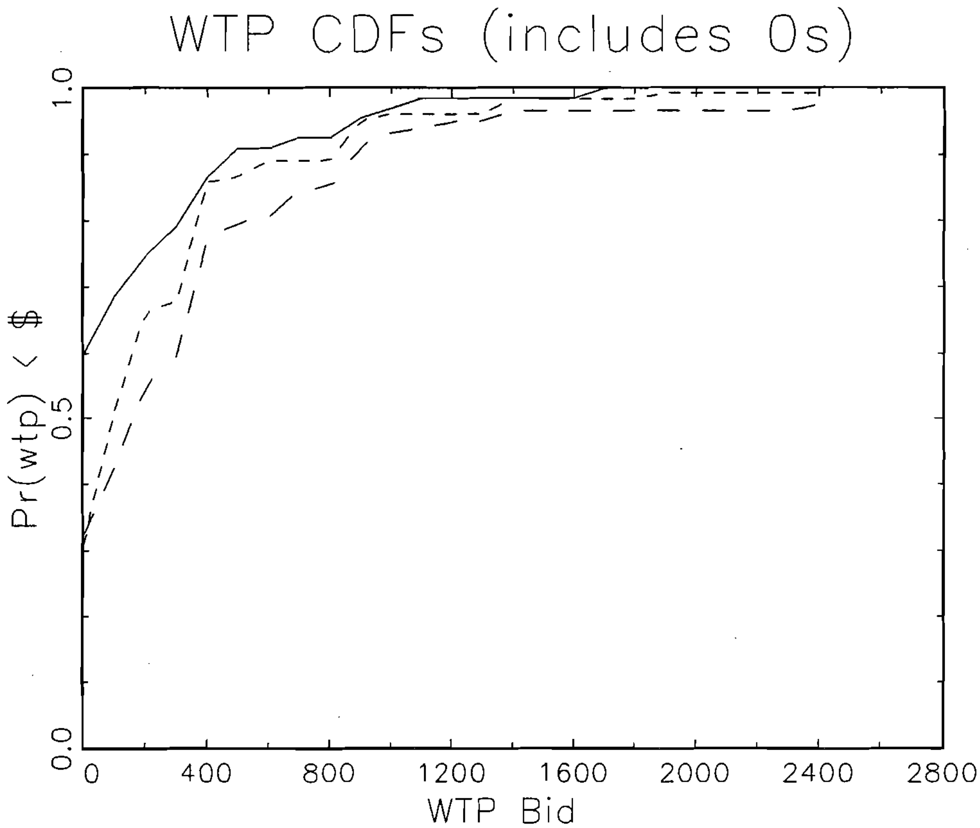
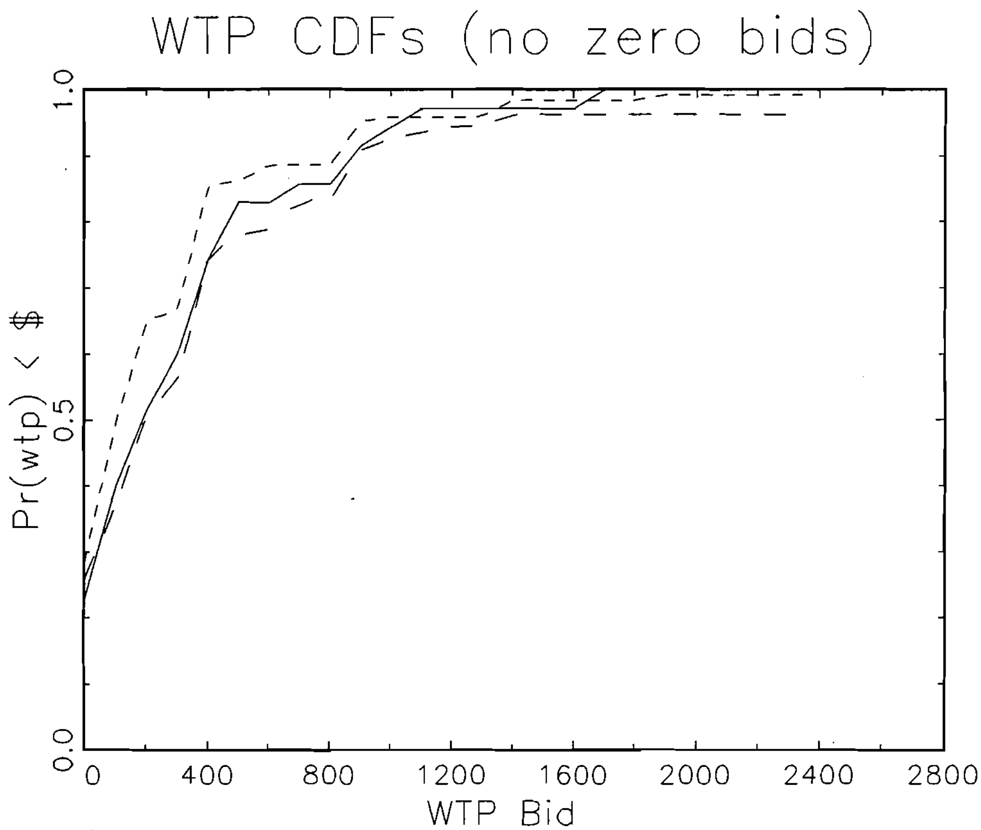
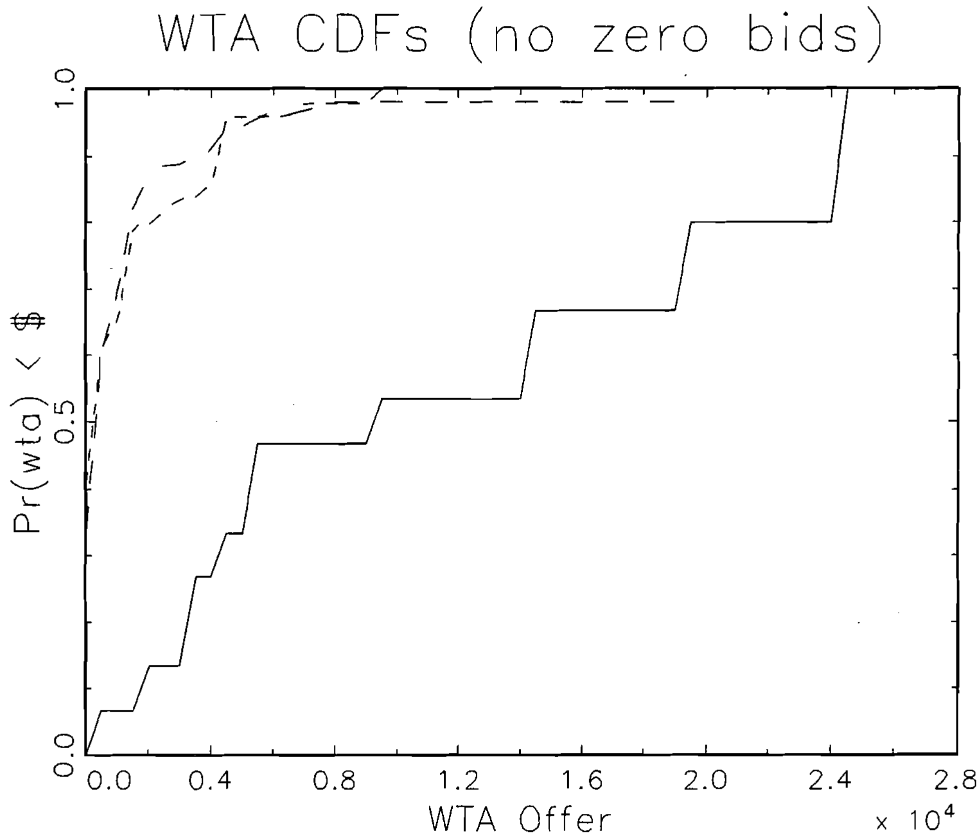

WTP CDFs (includes 0s)
1.0
0.5
0.0
Pr(wtp) < $
0
400
800
1200
1600
2000
2400
2800
WTP Bid

FIGURE 2



WTP CDFs (no zero bids)
1.0
0.5
0.0
Pr(wtp) < $
0
400
800
1200
1600
2000
2400
2800
WTP Bid

FIGURE 3



WTA CDFs (no zero bids)
Pr(wta) < $
0.0
0.5
1.0
0.0
0.4
0.8
1.2
1.6
2.0
2.4
2.8
WTA Offer
x 10⁴

Information, Risk Perceptions, and Contingent Values for Groundwater Protection

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Information, Risk Perceptions, and Contingent Values for Groundwater Protection

Information is an important input in value formation and the distribution of contingent values [Hoehn and Randall; Bergstrom and Stoll; Samples, Dixon and Gowen; Boyle; Bergstrom, Stoll and Randall]. Although critical assessments of the contingent valuation process stress that information provision should be 'adequate' in order to obtain satisfactory transactions and reliable values [Fischhoff and Furby; NOAA], very little empirical research has been devoted to establishing a minimum standard of information adequacy for contingent valuation studies. The need for such research is particularly cogent for valuing the benefits of reducing environmental risks such as groundwater contamination, as this is an unfamiliar commodity for most households and previous research indicates that risk perceptions are affected by new information [Viscusi and O'Connor; Smith *et al.*; Smith and Johnson].

Using nitrates in groundwater as a case study, this paper evaluates health risk perceptions and the distribution of contingent values for groundwater protection associated with different levels of information provision. The evidence presented in this analysis suggests that *general information* about nitrates, *specific information* about exposure levels, and *prior information* affect contingent values, and that individuals update their perceptions of groundwater safety with new information. Evaluations of individuals' abilities to assess their reference and target risks associated with a nitrate protection program suggest a *full-information* standard that includes both general and specific information for future contingent valuation research of groundwater protection.

Conceptual Framework

The conceptual framework underlying this analysis is based on the *ex ante* statistical life model in which exogenous risks are modified by self-protection activities [see Berger *et al.*, Shogren and Crocker]. In this framework it is hypothesized that individuals optimally select exposure and averting activities in order to maximize state dependent expected utility.

The extension of this framework to willingness to pay for groundwater protection from nitrates requires that the consumption and averting activities be specified. Here we assume that households consume drinking and cooking water (X) from three sources (i), each with an associated price (p) and nitrate level (N). These include:

water consumed from the household well ($X_1(N_1)$); water consumed from sources outside the home ($X_2(N_2)$) such as at school, restaurants, neighbors' homes etc.; and sources of water consumption intended to mitigate exposure ($X_3(N_3)$), including the installation of purification systems, importation of water from 'pure' wells, and the purchase of bottled water. Let $\bar{X}$ denote the vector of water consumption, $\bar{N}$ denote the associated nitrate levels, $\bar{p}$ denote the vector of prices, and define $\overline{pX} = p_1X_1(\bar{N}) + p_2X_2(\bar{N}) + p_3X_3(\bar{N})$. For simplicity assume that $N_3=0 < N_1,N_2$.

Uncertainty is introduced into the model at two levels. Because of the stochastic nature of biological and physical transport, it is reasonable to define N_1 and N_2 as random variables with a joint probability of $\eta = \eta(N_1, N_2)$. Second, under the state dependency framework of the statistical life theory, health outcomes play an important role. Yet, given exposure to a hazard, future health outcomes (h) remain a random variable. Let this uncertainty be characterized by the conditional probability density function $f(h;\bar{X},\eta)$ and let $F(h;\bar{X},\eta)$ represent the associated cumulative distribution function defined over the set of possible health states A.

The distribution of anticipated exposure levels and health risks are subjective and information dependent, implying that information levels need to be explicitly identified in the model. In specifying information levels, our analysis distinguishes between information about nitrates that is general in nature and information that is specific to a household's exposure level from its own water source. *General information* (GI) about nitrates would thus include possible health effects and sources of nitrates, government standards, and opportunities for mitigation. With this bundle of information, the decision maker could conceivably define health effects and optimal averting and consumption strategies for each hypothetical level of exposure. *Specific information* (SI) about nitrate levels found in an individual's well would affect the subjective distribution of nitrate exposure levels.

On this basis, general and specific information can be incorporated into the subjective distributions as follows. Joint distributions of nitrate exposure levels are treated as a function of both general and specific information

$$\eta = \eta(\bar{N}_1, \bar{N}_2 | GI, SI) \qquad (1)$$

as both types of information will likely have effects on the perceived distribution of N_1 and N_2. That is, individuals may extrapolate their own exposure to general groundwater effects and vice versa. It is postulated that the distribution of conditional health outcomes, however, is a direct function of general information alone

$$f(h|\bar{X},\eta,GI) \tag{2}$$

The corresponding joint conditional probability distribution of exposure and conditional health outcomes links the two distributions

$$g(h,\eta|\bar{X},GI,SI) = f(h|\bar{X},\eta,GI)\eta(N_1,N_2|GI,SI) \tag{3}$$

The ability to assess the exposure risk and the conditional health effects underlying this conditional joint probability distribution for both target and reference risks is of fundamental import in the valuation of groundwater protection programs. For this reason, the analysis of survey responses presented later in this paper focus on the ability of individuals to assess the safety level of their current exposure. Throughout the remainder of this paper, safety perceptions serve as a proxy for exact risk distributions implied in the equations.

The impact of new information on the joint conditional probability distribution will depend upon the degree of bias from the 'true' health risk and the weight placed on prior perceptions. In turn, the strength of the weights placed on prior perceptions and new information will likely be a function of the amount of prior information gathering.

It is essential to note that general and specific information will not only affect perceived risks, but may enter directly affect arguments in the constraint and utility functions. For instance, information about the price of substitute goods could affect the optimal consumption set through the budget constraint. Information about nitrate contamination may affect preferences based on non-use motivations such as altruism, bequest and existence values.[1] Incorporating these ideas, the utility maximization problem can be stated as

$$\max_{\bar{X}} \int_0^{\infty}\int_0^{\infty}\int_A U_h(W-\bar{p}\bar{X},\eta;GI,SI)\,dG(h,\eta|\bar{X},GI,SI)\,dN_1\,dN_2 \tag{4}$$

where W is wealth, the subscript h denotes state (health) dependent utility, and A depicts the range of possible health outcomes[2]. In this model information components are interpreted as a signal or an observation of a random variable that affects the joint probability distribution of nitrate exposure and health risks as well as elements of the utility and constraint functions. Complete isolation of effects is not possible because information affects both the subjective utility and risk aspects of the maximization problem.

Defining X^o to be the optimal vector of water consumption associated with the nitrate distribution (η^o) without the program and X' to be the optimal vector of water consumption associated with the post-program nitrate distributions (η'), state independent willingness to pay (WTP) is defined implicitly by

$$\begin{aligned}&\int_0^{\infty}\int_0^{\infty}\int_A U_h(W-\bar{p}\bar{X}^o,\eta^o;GI,SI)\,dG(h,\eta^o|\bar{X}^o,GI,SI)\,dN_1\,dN_2 = \\ &\int_0^{\infty}\int_0^{\infty}\int_A U_h(W-WTP-\bar{p}\bar{X}',\eta';GI,SI)\,dG(h,\eta'|\bar{X}',GI,SI)\,dN_1\,dN_2\end{aligned} \tag{5}$$

In this model, WTP is considered an *ex ante* total value that accounts for both use and non-use motivations.

Survey Design and Procedures

As a case study, this research focused on the very specific issue of groundwater protection from nitrate contamination in rural areas of Portage County, Wisconsin. Here, "rural" is defined as the 1980 census tracts which did not have municipally or centrally provided water. The population in this area was estimated to be 22,432 in 1990.

In order to assess how general information about contaminants and specific information about exposure levels affects WTP for a groundwater protection program that keeps nitrate levels in all county wells within government standards of 10 mg/l, the survey design consisted of two sequential stages. WTP for the groundwater protection program was elicited before (Stage 1) and after (Stage 2) individual nitrate test results were provided to survey participants. In addition, general information about nitrates was varied across groups in the Stage 1 survey.

Stage 1 survey participants were asked to complete a questionnaire and submit a water sample that would be analyzed for nitrates by the Wisconsin State Laboratory of Hygiene. All households selected for the Stage 1 survey received a package in the mail that included a questionnaire and water sampling kit. In addition, households selected for the Stage 1 survey were divided into two groups. One-half (With-GI) of the participants were provided written general information in the questionnaire about the possible health effects of nitrates, sources of nitrate contamination, government standards for nitrates, distribution of nitrate levels in Portage County wells, and opportunities for averting and mitigating actions. This information packet represented a composite of information taken from pamphlets available from local extension, university and other government sources -- i.e. sources that are readily accessible to Portage County residents through local extension offices. The other half (No-Info) of the Stage 1 sample did not receive this information packet. In the Stage 2 survey, all participants who returned samples and completed a Stage 1 survey were provided their nitrate test results for their household water supply along with general information about nitrates and a second questionnaire.

In all, this survey design resulted in three different treatments for the analysis of information effects: the 'No-Info' group received no information in the Stage 1 questionnaire; the 'With-GI' group received general information about nitrates in the Stage 1 questionnaire; and the 'Stage 2' participants received both general and specific information about nitrates. This design allowed the evaluation of the impacts of general information on questionnaire responses by comparing the No-Info and With-GI group responses. The effect of specific information was evaluated by comparing the Stage 1 and Stage 2 responses.

The implementation of the survey followed established procedures detailed by Dillman. A total of 480 Stage 1 surveys were mailed out in three separate waves that allowed for updating of dichotomous choice bid values[3]. After correcting for bad addresses, approximately 77.9 percent of the households returned a completed Stage 1 questionnaire and water sample. Differences in responses between Stage 1 information groups were relatively minor and not statistically significant. The conditional response rate to the Stage 2 survey was approximately 83.0 percent. Combined, the overall response rate to both stages was about 64 percent. Item nonresponse reduced the effective response rate for the contingent valuation analysis to 69-71 percent for the various Stage 1 models, and to about 55 percent for both the Stage 1 and Stage 2 surveys combined.

General and Prior Information, Learning, and Risk Perceptions: Stage 1

This section evaluates the effects of information on responses to select questions in the Stage 1 questionnaire. Difference in means tests of demographic characteristics across Stage 1 information treatments indicated that there was no significant difference in sex, age and education level of respondents, household size and age distribution, membership in environmental organizations, association with farming, and household income between the No-Info and the With-GI treatments. In addition, the well characteristics and mitigating activities were statistically similar across information groups.[4] On this basis, we concluded that information treatments were drawn from the same socioeconomic population. As such, observed differences in risk perceptions and contingent values across information groups can be attributed to informational rather than sampling effects.

In contrast, prior information --as measured by the existence of a previous water test for nitrates-- is associated with different socioeconomic characteristics. Using difference of means tests, it was determined that people who had previously tested their water for nitrates (With-Test) had significantly higher levels of education and income, were younger and had more family members (especially children) in the household than the people who had not previously tested their water (No-Test). The wells of the With-Test group tend to be newer than those of the No-Test group, and a significantly higher proportion had undertaken averting actions (e.g. using water from another well, purchasing bottled water, installing nitrate purification systems). Based on these comparisons it was concluded that the With-Test and the No-Test groups constituted self-selected subpopulations in Portage County, and were separated in the analyses that follow. In conjunction with the differences in information provision, four different subgroups are identified in the Stage 1 analysis. These subgroups, and the acronyms used to identify them, are depicted in Table 1.

Learning

A fundamental question in survey research is whether or not individuals learn from information provided with questionnaires. The degree of learning attributed to general information was measured in the Stage 1 survey responses to a 9 point quiz about nitrate contamination. In spite of the demographic similarities noted above, the mean score on this quiz was significantly different across information groups, providing an indicator that individuals were able to assimilate the information provided. Prior water testing also appears to be correlated with knowledge about nitrates, as demonstrated by higher scores for the With-Test groups. A summary of these quiz scores is provided in Table 2.

Hypothetical Conditional Safety Perceptions

The ability to link perceptions of safety to different nitrate levels was addressed by the following question:

> *Q17. Suppose that your well water was tested for nitrates, and that your well test indicated a nitrate level of _______ mg./l. In* ***your opinion*** *would you believe that this well is safe or unsafe for* ***your household*** *to use as the primary source of drinking water? (CIRCLE ONE NUMBER)*

for which nitrate levels 2, 4, 6, 8, 10, 12, 15, 20, 30, and 40 mg/l were randomly assigned to respondents within each information group. Categorical response options included "*Definitely Safe*", "*Probably Safe*", "*Not very safe*", "*Definitely Not Safe*" and "*Don't know*". For those who were able to respond, aggregated response patterns reflected government health standards of 10 mg/l: "*Definitely Safe*" responses are monotonically decreasing across increasing nitrate levels and "*Definitely Not Safe*" responses are monotonic in an increasing fashion. Both the "*Probably Safe*" and "*Not Very Safe*" responses peak at intermediate levels.

Of greater interest in this analysis is the magnitude of "*Don't know*" responses to safety questions, which provide an indicator of uncertainty in conditional health risk perceptions as defined in Equation (2). As depicted in the first column of Table 3, the proportion of "*Don't know*" responses to Q17 fell from 0.456 to 0.192 when general information was provided. Thus it appears that assimilation of general information does extend to the ability to assess the safety of different exposure levels. A similar reduction in uncertainty about conditional health risks was noted for the impact of prior nitrate tests. On average the proportion of "*Don't know*" responses fell from 0.450 to 0.103 between the No-Test and the With-Test groups. This observation suggests that previous experience with nitrate testing is associated with the gathering and retention of general as well as specific information.

Current Exposure Levels

The respondents' ability to assess their current levels of nitrates in their household well was evaluated with the following question:

Q23. Federal and state authorities have established safety standards for concentration of nitrates in the groundwater. Based on what you have heard and read, or any previous water tests that you may have taken, do you think that your well water has...(CIRCLE ONE NUMBER)

Categorical response options ranged from "*Much less nitrates than the safety standard (less than 1/2)*" to "*Much more nitrates than the safety standard (more than double)*". Again, a "*Don't know*" option was included. As demonstrated in the second column of Table 3, general information did not have a significant effect on the number of "*Don't know*" responses. A significant reduction was however associated with prior nitrate testing. Most notably, the high proportion of "*Don't know*" responses in the No-Test group (~53%) reflects the high degree of uncertainty about exposures for that group. In the context of Equation (1), this suggests a poorly defined (wide) distribution of exposure levels.

Personal Safety Levels

Further evidence of general information and prior testing effects on uncertainty in the joint conditional probability distribution expressed in Equation (3) is found in the responses to the following questions, each of which employed the response format presented in Q17 above,

Q24. ***In your opinion*** *are the nitrate levels found in your well safe for* ***adults*** *and* ***children older than 6 months*** *to use as their primary source of drinking and cooking water*

Q25. ***In your opinion*** *are the nitrate levels found in your well safe for* ***infants less than 6 months*** *to use as their primary source of drinking and cooking water?*

As demonstrated in the fourth and fifth columns of Table 3, uncertainty concerning assessments of the safety of their personal well water, as measured by the proportion of "*Don't know*" responses, was not significantly reduced by general information. In fact, when evaluating general information effects within the With-Test group, a significantly larger proportion of "*Don't know*" responses to safety perceptions for adults and infants was observed for the treatment that received general information about nitrates. This provides an indication that general information may induce some uncertainty and anxiety about personal exposure levels. In contrast, uncertainty

Future Exposure Levels

In addition to current exposure levels, individuals were asked to assess the likelihood of future exposure with the following question:

*Q26a. **Without**... a groundwater protection program, do you expect the nitrate levels in your own well to exceed the government standards for nitrates during **the next five years**?*

Responses to this question were categorical variables with probabilistic interpretations ranging from "*No, definitely not*" to "*Yes, definitely (100 percent chance)*". In order to force a response, a "*Don't know*" option was not included for this question.

In all cases, a bell shaped curve centered on "*Maybe (50 percent chance)*" was observed in the Stage 1 analysis (see Table 4), a response distribution characteristic of uncertainty about future exposures. Chi-squared tests of independence from contingency table analyses indicated that the With-GI and No-Info treatments were not independent ($\chi^2 = 1.24 < \chi^2_{4,.10} = 7.78$), and that the With-Test and No-Test response functions were also not independent ($\chi^2 = 3.25$). In this manner, neither general information nor prior testing strongly affect assessments of the likelihood of future exposure.

Specific Information and Risk Updating: Stage 2

As indicated, individuals received nitrate test results and general information along with the Stage 2 questionnaire. A graphical depiction of their nitrate level relative to natural levels and government standards was included on the inside front cover of the questionnaire, and thus participants were not asked to identify current levels of exposure. All participants in the Stage 2 survey received the same full information set, and separate Stage 1 information treatments were not isolated in the analysis of risk perceptions. Because of differences in socioeconomic characteristics, distinction between the With-Test and the No-Test group was maintained in the Stage 2 analysis.

Personal Safety Levels

The two safety questions for adults and children were repeated in order to assess the reductions in uncertainty associated with the conditional joint probability distributions of health outcomes presented in Equation (3). Again, the proportions of "*Don't know*" responses served as an indicator of uncertainty.

The Stage 1 and Stage 2 proportion of "*Don't know*" responses are presented in Table 5 for the subsample of respondents who completed both stages of the survey. As demonstrated by the comparison of columns, the proportion of "*Don't know*" responses was reduced for all groups and safety questions, indicating that some updating has occurred. Of these differences, only the proportion of "*Don't know*" responses to the adult safety question for the With-Test group was not significantly lower in the Stage 2 survey. The lack of significance for this group may indicate that adult safety was conveyed in prior testing.

Future Exposure Levels

As part of the contingent valuation question, individuals were again asked to assess their likelihood of exceeding government standards for nitrates during the next 5 years. A χ^2 contingency table analysis indicated that the Stage 1 and Stage 2 responses are statistically independent ($\chi^2 = 40.09 > \chi^2_{4,.10} = 7.78$), suggesting that updating has occurred. Notably, a comparison of the Stage 1 and Stage 2 distributions of future exposure expectations indicates that expectations shifted from a bell shaped distribution in Stage 1 to a bimodal distribution in Stage 2 with peaks at "*Yes, definitely (100 percent chance)*" and "*Probably not (25 percent chance)*". These patterns reflect nitrate test results for the sample: 16 percent of the wells tested currently exceed government standards of 10 mg/l for nitrates and about 60 percent had nitrate levels less than 5 mg/l.

Further analysis of updating within the No-Test and With-Test groups was conducted using a risk updating framework discussed in Smith and Johnson. Adapting the Smith and Johnson model and assuming a probabilistic interpretation of the likelihood of future exposures, Stage 2 probabilities (R_{S2}) of exposure were modeled as a two-limit linear probit function of the Stage 1 probabilities (R_{S1}) of exposure and the nitrate test levels (N):[5]

$$R_{S2} = \beta_o + \beta_1 R_{S1} + \beta_2 N \qquad (6)$$

where β_i are coefficients to be estimated. Positive and significant coefficients on prior risk and nitrate test values (see Table 6) suggest that respondents place weight on their prior perception as well as new information gained from nitrate testing. Treating the new information contained in the nitrate test as an information message equivalent to observing a sample risk [Viscusi and O'Connor; Smith and Johnson] it is possible to recover the relative weights (W_N/W_{S1}) placed on new information and prior probability assessments as follows:

$$\frac{W_N}{W_{S1}} = \frac{1}{\beta_1} - 1 \qquad (7)$$

where β_1 refers to the coefficient on the Stage 1 probability assessment in Equation (6). The estimates of relative weights provide strong evidence of risk updating in both groups but suggest that the relative weight placed on new information is higher for the No-Test group (2.560) than for the With-Test group (2.091). Such a result is intuitively appealing.

Information and Contingent Values

The previous sections demonstrated that information was assimilated and that new information did affect individual perceptions of safety levels. This section evaluates the impact of information on contingent values by estimating and comparing WTP distributions.

The dichotomous choice contingent valuation question consisted of two parts. As discussed previously, individuals were first asked in Q26a to provide their expectation of the likelihood that their own wells would exceed government standards for nitrates during the next five years. In the second part, individuals were asked the following question:

> *Would you vote for the groundwater program described above if the total* ***annual*** *cost to your household (in increased taxes, lower profits, higher costs, and higher prices) were $________________ each* ***year*** *beginning now and for as long as you live in Portage County?*

A dollar value (BID) was inscribed in each questionnaire.

A linear in the coefficients specification of the logit model was used to evaluate yes/no (1/0) responses to this question [Hanemann]. Because of small sample size[6] for individual cells depicted in Table 1, the data was grouped into With-Test and No-Test groups on the basis of the previous conclusion that these groups represent distinct subpopulations. Differences in information provision are accounted for by binary variables that shift the constant (DINFO) and the coefficient on dichotomous choice bid values (DINFO*BID).

Knowledge about nitrates was accounted for in the analysis using the score on the 9 point quiz about nitrates (QUIZSCORE) in the Stage 1 survey. In accordance with the theoretical model averting activities were also included, with binary variables that took a value of 1 if the averting activity was undertaken and 0 otherwise. The variable DAVTPERM captured permanent averting activities including the installation of a nitrate purification system and getting bottled water from another well. This binary variable was expected to have a negative coefficient because these activities represent somewhat irreversible substitute consumption choices that have high adjustment costs. Anecdotal evidence supports this line of reasoning. In response to a $216 dichotomous choice bid value, one respondent wrote "No, but I would have (voted yes) if I hadn't recently put in a H20 softener and reverse osmosis system for that reason". Similarly, in an in-person pre-study of the questionnaire, a participant indicated that his WTP for protecting his well water was bounded because he was able to get all the good quality water he needed from his daughter's well in town. With investment in water transporting containers, this represented a permanent solution. In contrast, purchasing bottled water (DBOTWAT) is less likely to be perceived as a permanent solution because of low investment costs. As a result, no sign expectation was formed on this coefficient.

The linear logistic model of the WTP function in a dichotomous choice framework is specified as

$$P(Yes) = (1 + e^{-\theta_T})^{-1}$$

where, $\theta_T = \alpha_T + \beta_{1T}(QUIZSCORE) + \beta_{2T}(DAVTPERM) + \beta_{3T}(DBOTWAT) + \beta_{4T}(FUTURE) + \beta_{5T}(DINFO) + \beta_{6T}(DINFO*BID) + \beta_{7T}(BID)$

In the above equation, β_{iT} are the coefficients to be estimated, and the subscript T refers to the prior nitrate testing category. The estimated logit response functions for the Stage 1 survey by prior test group for this model are presented in the 'Full Model' heading in Table 7. As demonstrated by the high χ^2 values, each model is highly significant.

Log likelihood values for the difference between the two prior-test Stage 1 models exceed the critical values at the 10 percent level (LR= 17.57 > $\chi^2_{8,.10}$ = 13.36). Three differences are particularly noteworthy. First, the coefficients on the binary averting variables are not significant for the No-Test group. Similarly, the coefficient on the FUTURE value is not significant for the No-Test group, but is highly significant for the With-

Test Group. This result corresponds with the earlier observation that the No-Test group had poorly defined reference conditions of exposure, and thus, future expectations should play a small role in responses across bid values. Finally, prior testing apparently dampens the effect of new information on the distribution of WTP, as the coefficients on information variables are significant for the No-Test group but not for the With-Test group. This may indicate that prior values are more established for the With-Test group or that much of the information provided with the survey had already been assimilated through prior testing.

Table 7 also presents the results from the Stage 2 estimates. In contrast to the above results, the Stage 1 level of information provision was not a significant explanatory variable in either of the Stage 2 response functions, averting actions did not play a significant role in the Stage 2 analysis, and the coefficient on FUTURE is found to be significant for both groups. This last observation contrasts with the Stage 1 result that expectations of future contamination were not a significant explanatory variable for the No-Test group. In conjunction with prior evaluation of risk updating, this result suggests that individuals receiving specific information are better able to incorporate their assessment of future exposure levels into their WTP for a groundwater protection program.

Further support that updating of WTP values does occur for the No-Test respondents is found in log-likelihood ratio (LR) tests across Stage 1 and Stage 2 models. Using the Short set of variables (DINFO, DINFO*BID, FUTURE, and BID) as the basis, the LR (17.32) exceeds the $\chi^2_{5,.10}$ value of 9.24. It thus appears that new information has affected the distribution of WTP for the No-Test group. Similar results are not found in the analysis across stages for the With-Test group: the LR test using the Short 1 variables provides an estimated value of 1.17. Thus, in spite of the evidence of risk updating, the estimated WTP distributions for the Stage 2 With-Test group is not significantly different from that of the Stage 1 distribution. Although individual updating did occur, it appears that, as a group, the WTP distribution of the With-Test group is relatively stable[7].

Existence of prior testing does appear to have a significant residual effect on WTP in the Stage 2 analysis, as the No-Test and With-Test estimates of the Short 2 model are significantly different (LR= 8.76 > 6.25 = $\chi^2_{3,.10}$).

General Information, Specific Information, Prior Information and Mean WTP

The information effects for the Stage 1 response functions suggest that general information flattens out the response function across bid values. These shifts in WTP are reflected in the corresponding distributions of mean WTP created using a simulation method detailed in Duffield and Patterson and the Short models presented in Table 7 (which retain only the significant coefficients for the information, future and bid variables). As presented in Table 8, general information appears to increase the mean WTP and reduces the precision of that estimate for the No-Test group. Because of the joint and individual lack of significance for the coefficients on the information variables, separate distributions for the No-Info and With-Info treatments were not estimated for the With-Test group.

Two causes for increased dispersion associated with general information provision alone are offered for the Stage 1 No-Test group . The first is that in assimilating general information households may selectively focus on, or react to, different facets of information that are pertinent to their life situation or preferences. For example, a household with small children will likely react quite differently to information about blue baby syndrome than a household of retirees. In contrast to homogeneous commodities for which information is expected to increase the uniformity of the service and reduce the variance of WTP [Boyle; Bergstrom, Stoll and Randall], such heterogeneity in the population and exposure levels would be expected to widen the distribution of WTP and decrease the precision of the mean value.

The relatively large spread of WTP and mean WTP for the With-GI group that had not previously tested their water may also be attributed to an informational imbalance. Previous research has suggested that too much information may create confusion about the value placed on a resource or commodity [Bergstrom and Stoll; Grether and Wilde]. In this study, possible confusion associated with general information could instead be attributed to the fact that there was not enough information presented in the general information packet. Individuals were presented with an abundance of information about nitrate related health risks and possible methods of avoiding exposure, but remained uncertain about their actual exposure levels. With such uncertainty about reference exposure and safety levels, individuals may become confused about the values that they place on groundwater protection and may need more information to make a satisfactory transaction. In this manner, information overload, which is an absolute concept, does not seem to be a problem. Rather, the wide dispersion of values may be attributed to an informational imbalance.

Using a significance level of α=0.10 and the convolutions technique presented in Poe, Lossin and Welsh, the difference between distributions of mean WTP for With-GI and No-Info groups is significant ($\hat{\alpha}$=3.0) for the

No-Test group. Comparing the Stage 1 and Stage 2 estimates, the Stage 2 mean WTP value lies below the previous estimates for the No-Test group. Calculated at the parameter means, the Stage 2 No-Test mean WTP value was $169, which compares to the Stage 1 values of $225 and $685 for the NINT and WINT groups respectively. The difference between the mean WTP distribution for the WINT (Stage 1) and the No-Test (Stage 2) groups was significant at the 10% level ($\hat{\alpha}$= 0.6). In contrast, although the Stage 2 value was lower, the difference in the distributions of mean WTP values between the NINT (Stage 1) and the No-Test (Stage 2) is not significant at the 10% level ($\hat{\alpha}$= 38.1). Combined these results further reinforce the Stage 1 conclusions that general information alone will inflate contingent values for groundwater protection programs when people have not previously tested their water.

Comparisons of the Stage 1 and Stage 2 mean WTP values for the With-Test group exhibited a different pattern. Calculated at the parameter means, the Stage 2 With-Test value was $348, which is almost identical to the Stage 1 value of $344. Comparison of the mean WTP distributions across stages were not significant at the 10 percent level ($\hat{\alpha}$= 90.5), supporting the previous inference that new information has less of an impact on groundwater protection values for those people who have already tested their water.

A comparison of the Stage 2 mean WTP values across prior nitrate test groups indicated that the With-Test group had a significantly higher mean WTP than the No-Test group ($\hat{\alpha}$=0.6). Because nitrate levels found in wells for each test group were not significantly different, this higher WTP value is attributed here to greater concerns about exposure for each nitrate level.

Concluding Remarks

Using data from a two stage survey design for a nitrate protection programs as a case study, this paper has demonstrated that prior information gathering and information provided with a survey can have a significant effect on estimated WTP distributions. Differences in prior information gathering, as measured by prior testing of wells for nitrates, had two effects on WTP distributions. First, people who had previously tested their water for nitrates had a greater concern and a higher WTP for groundwater protection than people who had not previously tested their water, suggesting that distinction between prior testing groups should be made in future studies of groundwater protection. Second, although some updating of risk preferences was observed for the With-Test group, the estimated distributions of WTP and mean WTP were relatively stable. In contrast, strong information effects were found for the No-Test group. Most notably, the provision of general information alone, without providing specific information about exposure significantly shifted the WTP distribution and grossly inflated the mean WTP estimates.

The fact that information effects were observed for the No-Test group--which we argue is most representative of the population outside of Portage County--raises the question of the appropriate level of information provision in the valuation of groundwater protection and programs that affect environmental risks. While this issue certainly has a philosophical component [e.g. Bishop and Welsh], we focus here on a transactions based criterion which asks how much information is needed in order for respondents to make satisfactory transactions that reflect their own best interests [Fischhoff and Furby]. From this perspective, the conclusion is obvious--full information provision that includes both specific information about personal exposure levels and general information about the contaminants are essential for valuing programs that change present or future exposure levels. Lacking information about their own personal exposure level, households remain uncertain about their reference exposure. Without general information, individuals do not appear to be able to assess the relative safety of reference and target levels. In this manner, general and specific information are viewed as complimentary and necessary in an adequate information bundle for valuing environmental risks. This brings into question the reliability and validity of past groundwater valuation studies which did not provide full-information set [e.g. Edwards; Schultz and Lindsay; Sun] and sets a full-information standard for future studies.

Unfortunately, this conclusion does not bode well for contingent valuation of groundwater protection programs. Water testing is relatively expensive and timely collection of water samples is difficult to organize. A full-information requirement will certainly escalate the cost and organizational requirements of future valuation studies. Perhaps some of these difficulties can be deflected by linking valuation studies with random sampling provided by public programs or hydrological studies based on private well readings. In regions or for chemicals in which testing is not prevalent, it may also be possible to substitute specific information with a hypothetical reference level.

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Table 1. Knowledge and Information Groupings

	Water Not Previously Tested for Nitrates No-Test (n=149)	Water Previously Tested for Nitrates With-Test (n=190)
Not Provided General Information in Survey (No-Info, n=169)	NINT (n=76)	NIWT (n=93)
Provided General Information in Survey (With-GI, n=170)	WINT (n=73)	WIWT (n=97)

Table 2: Summary of Responses and Differences in Stage 1 Quiz Scores by Group

	Avg. Questions Correct (Standard Error)	n	Max. Corr.	Differences Between Groups		
				NIWT	WINT	WIWT
NINT	2.57 (2.09)	76	7	-3.352***	-6.810***	-10.944***
NIWT	3.70 (2.30)	92	8		-4.127***	-7.587***
WINT	5.43 (2.97)	73	9			-1.926*
WIWT	6.24 (2.33)	97	9			

T-test values significantly different at 10% (*), 5% (**) and 1% (***)

Table 3: Comparisons of "Don't know" Responses to Selected Stage 1 Questions Defined in Text [a]

Group		Safety of Hypothetical Nitrate Levels Q17	Level of Nitrates in Well Q23	Adult Safety of Nitrate Levels in Well Q24	Infant Safety of Nitrate Levels in Well Q25
No-Info		0.456	0.311	0.230	0.291
With-GI		0.192	0.299	0.256	0.311
No-Test		0.450	0.531	0.424	0.503
With-Test		0.103	0.130	0.103	0.141
NINT		0.627	0.548	0.471	0.514
NIWT		0.330	0.121	0.066	0.103
WINT		0.288	0.514	0.426	0.493
WIWT		0.113	0.138	0.138	0.172
Groups Compared		Difference of Proportions Test			
No-Info	With-GI	8.246***	0.324	-0.734	-0.537
No-Test	With-Test	10.84***	10.82**	9.128***	9.684***
NINT	WINT	6.428***	0.91	1.487	0.600
NIWT	WIWT	4.098***	-0.45	-2.438***	-2.013**
NINT	NIWT	5.632***	11.36***	13.63***	11.983***
WINT	WIWT	10.84***	10.01***	9.710***	9.370***

a. Response Option to Question 23 was actually "I have no idea" rather than "Don't know". T-test values significantly different at 10% (*), 5%(**) and 1%(***)

Table 4. Stage 1 and Stage 2 Distribution of Expectations that Nitrate Levels in Household Well Will Exceed Government Standards for Nitrates in the next five years?

Responses	Stage 1					Stage 2
	No-Info	With-GI	No-Test	With-Test	All	
Yes (100% Chance)	13.7	10.8	8.6	15.2	12.3	18.0
Probably (75% Chance)	13.0	16.6	15.7	14.0	14.8	10.5
Maybe (50% Chance)	37.9	36.9	36.8	36.5	37.4	17.3
Probably Not (25% Chance)	27.3	28.0	29.3	26.4	27.7	37.2
No	8.1	7.6	7.9	7.9	7.9	16.9
n	161	157	140	178	318	266

Table 5. Comparison of "Don't know" Responses For Safety Questions[a]

	Stage 1	Stage 2	T-Value
Infant Safety (No- Test)	0.422	0.098	10.179***
Infant Safety (With-Test)	0.119	0.052	4.070***
Adult Safety (No-Test)	0.333	0.088	7.697***
Adult Safety (With-Test)	0.044	0.022	1.628

a. Only those who responded to Stage 2 Questionnaire are included. T-test values significantly different at 10% (*), 5%(**) and 1%(***)

Table 6. Updating of Expectations of Future Contamination by Prior Test Group Using Double Bounded Probit Model

	No-Test	With-Test
Constant	-0.0553 (0.0906)	-0.0505 (0.0683)
R_{S1} (Stage 1)	0.281^{*} (0.152)	0.324^{***} (0.111)
Nitrate Level	0.0592^{***} (0.00896)	0.0658^{***} (0.00776)
σ	0.368^{***} (0.0364)	0.330^{***} (0.0271)
n	102	134
Log(L)	58.29	62.84
ω_N/ω_{S1} (Weight Ratio)	2.560	2.091
Descriptive Statistics of Updating by Prior Test Group		
R_{S1} (Stage 1 Risk)	0.493 [0.269]	0.500 [0.295]
R_{S2} (Stage 2 Risk)	0.402 [0.341]	0.480 [0.344]
Mean Nitrate Level (mg/l)	5.71 [6.79]	6.65 [6.91]

T-test values significantly different at 10% (*), 5%(**) and 1%(***). Asymptotic Standard Errors in (), Standard Deviations in [ ]

Table 7. Stage 1: Estimated Logit Equations to Dichotomous Choice Contingent Valuation Question

	Stage 1				Stage 2			
	No-Test		With-Test		No-Test		With-Test	
	Full	Short	Full	Short	Full	Short	Full	Short
Constant	0.342 (0.606)	1.064*** (0.394)	-1.081 (0.698)	0.105 (0.361)	-0.576 (0.559)	-0.258*** (0.431)	-0.581 (0.448)	-0.248 (0.338)
Quiz Score	0.133 (0.096)		0.180* (0.092)					
Davtperm	0.586 (1.381)		-1.667** (0.784)		-8.352 (20.777)		-0.800 (0.754)	
Dbotwat	-6.862 (35.413)		2.245** (1.026)		-7.139 (38.16)		0.632 (1.087)	
Future	0.807 (0.841)		2.306*** (0.824)	1.247* (0.679)	2.205*** (0.832)	2.225*** (0.791)	2.278*** (0.637)	2.113*** (0.605)
DInfo	-2.340*** (0.705)	-1.701*** (0.542)	-0.337 (0.563)		0.860 (0.676)		-0. 694 (0.538)	
Dinfo*bid	0.00653*** (0.00207)	0.00546*** (0.00174)	0.00096 (0.00128)		-0.00203 (0.00334)		0.000112 (0.00163)	
bid	-0.00700*** (0.00200)	-0.00606*** (0.00167)	-0.00455*** (0.00110)	-0.00326*** (0.00063)	-0.00503** (0.00203)	-0.00615*** (0.00168)	-0.00339*** (0.00123)	-0.00321*** (0.000809)
n	135	143	168	168	102	103	140	140
χ^2	46.19***	41.26***	56.29***	42.17***	43.22***	36.97***	34.46***	29.77***

Notes: Asymptotic standard errors in (). Significance levels are denoted * (10 percent), ** (5 percent) and *** (1 percent).

Table 8. Mean WTP Distributions for Different Information Flows Using Duffield and Patterson Simulation Method (truncation point = $6,000)

Group			Calculated at Parameter Means	Based on 1000 Draws		
				Lower Bound 10%	Mean	Upper Bound 10%
No Prior Nitrate Test	Stage 1	No-Info,No-Prior-Test	224.72	143.30	222.98	312.35
		With-Info, No-Prior-Test	684.95	306.56	708.38	1409.32
	Stage 2		168.72	117.16	167.80	226.16
With Prior Nitrate Test	Stage 1		344.15	264.31	342.18	441.39
	Stage 2		348.15	255.90	355.38	477.22

Table 9. Significance Levels of Difference Between Mean WTP Distributions in Table 8

Groups Compared			Signif. Level of Diff.
No Prior Nitrate Test	NINT (Stage 1)	WINT (Stage 1)	3.0
	NINT (Stage 1)	No-Test (Stage 2)	38.1
	WINT (Stage 1)	No-Test (Stage 2)	0.6
With Prior Nitrate Test	With-Test (Stage 1)	With-Test (Stage 2)	90.5
	No-Test (Stage 2)	With-Prior-Test (Stage 2)	0.6

Notes

1. McClelland *et al.* provide an interesting two period model that accounts for these motivations. In the current analysis it is postulated that non-use motivation may enter into the valuation function, but the exact linkages are not specified.

2. A more complete model might include severity effects as measured by the costs of illness. This aspect may be important, but is ignored here. See Berger *et al.*, Shogren and Crocker, Crocker, Forster and Shogren, and Quiggen for a discussion of this issue.

3. Bid values for the first wave of the Stage 1 survey (225 surveys) were based on estimated logit functions from Sun's analysis, with bid values ranging from $1 to $2,500. Bid values for subsequent waves (255 surveys) and the Stage 2 survey were revised downward based on preliminary responses to the first wave of the survey. The range in Stage 2 was bound between $1 and $1,000.

4. One anomaly did occur in comparing information groups. A higher proportion of people within the With-GI group reported having attended public meetings. This attendance did not appear to have been translated into other public actions or concerns. A complete comparison of demographic characteristics is provided in Poe.

5. Because the probabilities of exceeding the standard have a lower bound of 0 and an upper bound of 1, it is necessary to define R^*_{S2} as an index variable of predicted outcomes as follows.

$$R_{S2} = 0 \text{ if } R^*_{S2} \leq 0$$
$$R_{S2} = R^*_{S2} \text{ if } 0 \leq R^*_{S2} \leq 0$$
$$R_{S2} = 1 \text{ if } R^*_{S2} \geq 1$$

The corresponding likelihood function for this two limit probit model is

$$L(B, \sigma | R_{s2}, X_i)$$
$$= \prod_{R_{s2}=0} \Phi\left(\frac{-BX_i}{\sigma}\right) \prod_{R_{s2}=R^*_{s2}} \frac{1}{\sigma} \phi\left(\frac{R_{s2} - BX_i}{\sigma}\right) \prod_{R_{s2}=1} \left[1 - \Phi\left(\frac{1 - BX_i}{\sigma}\right)\right]$$

where ϕ and Φ are the normal probability density function and cumulative distribution function respectively, and X_i is a vector that includes the variables R_{si} and N defined in equation (8)

6. Aldrich and Nelson note that large sample size properties of unbiasedness, efficiency and normality seem to hold reasonably well for logit models once sample size exceeds the order of N-K=100 (p. 53).

7. Because of the two stage process, there exists a possibility of selection bias in the second stage. A difference of means comparison of demographic characteristics, well characteristics and averting actions shows that there are no significant differences in these variables across stages within the test and information groups, as would be expected from the high stage 2 response rate. Selection effects on the WTP and mean WTP distributions were evaluated by re-estimating the Stage 1 dichotomous choice models for only those who responded to the Stage 2 questionnaire, while some slight shifts in distributions did occur, these shifts did not affect the conclusions in this analysis. General information still had a significant effect on the Stage 1 No-Test group and less of an effect on the With-Test group. Updating of the WTP and mean WTP distribution across stages was significant for the No-Test groups identified in the text, but not for the With-Test group.

An Estimate of the Economic Value of Selected Columbia and Snake River Anadromous Fisheries, 1938-1990

Edgar L. Michalson

An Estimate of the Economic Value of Selected Columbia and Snake River Anadromous Fisheries, 1938-1990

Introduction

The major goal of this paper is to estimate the economic value of the Columbia and Snake River anadromous fisheries of Idaho and eastern Washington. Emphasis in this paper will be placed on the salmonoid fisheries in the Snake/Clearwater/Salmon rivers because of the availability of data. Since the first dam on the lower Columbia River was built in the 1930's, anadromous fish have had to navigate fish ladders. The U.S. Army Crops of Engineers has maintained fish counting facilities, and has provided an annual count of the numbers of fish by major species since 1938, and a portion of the data used in this study was obtained from this source[1]. The species of fish counted and included in this study include Chinook, Coho, Sockeye, and Steelhead. Over the years the number of fish counted has varied greatly. This variation is related to a number of factors such as the cycle of spawning, river and ocean conditions, and a number of other factors which are not well understood. Among these factors is the nature of the particular species to respond to changing environmental conditions which is reflected in that some fish return to spawn after one year (jacks), some after two years, some after 3 years, some after four years, and some after 5 years.

Fish Passage

Chinook, Coho and Steelhead fish runs over the Columbia river dams all have showed an upward trend since 1938. The fish runs for Sockeye salmon show a downward trend over this same time period. On the Snake river the variability is much greater than it is on the Columbia river. This variability is in some cases 6 or 7 times that of the lowest fish passage numbers reported.

The run pattern for Chinook Salmon on the Columbia as counted over Bonneville, dam tends to show an increasing population over the 53 year period. The largest run over Bonneville occurred in 1986 with 570,881 fish, and the smallest run in 1944 with 240,764 fish. However that changes with the fish counts on the Snake River. Beginning in 1962 and ending in 1990 there has been a 50 percent decline in Chinook

[1]U.S. Army Corps of Engineer District. "1990 Annual Fish Passage Report: Columbia and Snake Rivers for Salmon, Steelhead, and Shad." North Pacific Division Corps of Engineers, 1990.

runs over Ice Harbor Dam as measured by the trend line. The largest run occurred in 1969 with 100,514 fish, and the smallest in 191984 with 17,757 fish Further, the decline in the counts over Lower Monumental Dam have declined by 2/3's over this same time period. What is interesting is that the decline in Chinook returning to Idaho, those which pass over Lower Granite Dam has been the slightest, declining less than 10 percent. This latter phenomenon is most likely explained by the earlier building of dams on the middle Snake river in the 1950's. In the case of Lower Granite dam the largest fish run occurred in 1978 with 54,246 fish and the smallest in 1980 with 10,986 fish.

The run pattern for Steelhead on the Columbia and Snake rivers has shown a positive trend over the 53 year period. One of the reasons has been the successful operation of Steelhead hatcheries in Idaho and eastern Washington which have supplemented the wild Steelhead stocks. In addition the barge transport system used to move juveniles downstream below Bonneville Dam has also contributed to the Steelhead survival rate. The largest run over Bonniville Dam occurred in 1986 with 389,891 fish. The largest run over Ice Harbor dam occurred in 1986 with 144,292 fish, and the smallest occurred in 1974 with 12,528 fish. The largest run over Lower Granite Dam occurred in 1986 with 134,519 fish, and the smallest in 1975 with 17,311 fish .

The run pattern for Coho Salmon on the Columbia river as measured over Bonneville dam also shows an increasing trend between 1938 and 1990. In fact the rate of increase is approximately 30 times that of the counts made in the early years of the dam's operation. However, the numbers of fish passing over the upriver dams all show a declining trend, and the rate of decline with the fish counts over Ice Harbor Dam. If the data can be believed, Coho Salmon are on their way to extinction in the Snake River. The last fish counted over Lower Monumental Dam was in 1984, and the last fish counted over Lower Granite Dam occurred in 1983. The largest run of Coho over Bonneville Dam occurred in 1986 with 130,853 fish, and the smallest in 1945 with 790 fish. The largest run over Ice Harbor Dam occurred in 1968 with 6,227 fish and the smallest in 1990 with one fish. The largest run of Coho over Lower Granite Dam occurred in 1975 with 921 fish the smallest in 1986 with one fish.

The run pattern for Sockeye Salmon on the Columbia generally show a decline pattern since 1938. The rate of decline as measured at Bonneville is approximately 20 percent over the 53 year period. The rate of decline tends to increase for the upriver dams with McNary showing a 50 percent decline in numbers. As one moves up the Snake River the situation worsens, an the last fish counted over Ice Harbor Dam was in 1989. The largest run of Sockeye occurred in 1955 with 237,748 fish, and the smallest in 1945 with 9,501 fish.

The pattern of fish migration on the Columbia and Snake rivers needs to be interpreted in light of the many changes which have occurred over this period. These changes include the development of irrigation, the addition of dams to the river system, population growth in the region, increased timber harvest levels, and the expansion of the river as a recreational resource. It also reflects an increasing reliance of fish hatcheries to supply fish to the river system to make up for the short fall of natural fish production. This is especially clear in the case of the Chinook Salmon and the Steelhead Trout. The development of fish hatcheries for these species has contributed more than half of the fish counted.

Estimated Value of Upriver Fisheries

The question arises as to what is the worth of these fish which are part of the natural environment, and which existed prior to the building of the dams on these rivers. This question has become even more pressing with the threat of naming some of these fish endangered species. Arguments are being made that the economic losses from the strategies being proposed far outweigh the value of these fish, but little evidence is presented on the side of the fish. This paper proposes to begin a dialog concerning the value of these fish. The approach used in this study was to estimate fishery value was based on the American Fisheries Society document entitled "Monetary Values of Freshwater Fish and Fish-Kill Counting Guidelines published in 1982. The "Monetary Values of Freshwater Fish Committee" and the "Pollution Committee" set forth procedures and estimated the value of each specie of freshwater fish.[2] The values developed in the above study have

[2]The Monetary Values of Freshwater Fish Committee and the Pollution Committee. "Monetary Values of Freshwater Fish and Fish-Kill Counting Guidelines." American Fisheries Society, Special Publication No. 13. 1982.

been updated from 1982 to 1990 using the producer price index.[3] The Value per fish are shown in Table 1 along with the average weight, the estimated value per fish, and the capitalized value per fish. This latter value may be interpreted as that amount of money which would have to be set aside to generate the corresponding cash flow generated by the fish value. The next step was to calculate the value of the fishery based on the numbers of fish shown in the Corps of Engineers fish count data. These values are shown in table 2 for Bonneville, Ice Harbor, and Lower Granite dams. These dams were chosen because Bonneville and Ice Harbor dams are gateway dams for the Columbia and Snake rivers, and Lower Granite is the gateway for the Idaho fisheries. The values shown are aggregate values for each specie of fish based on the average run over the time period covered by the fish counting procedure. The Total value of the fish going over Bonneville Dam was estimated to be $1.8315 billions. These values are based on the average runs over Bonneville and may be considered as conservative under most circumstances. This estimate also should be recognized as only part of the value of the upriver Columbia River fishery, because the Chum and Pink Salmon, and Shad are not included in these calculations.

The next point at which values were measured was at Ice Harbor Dam near the mouth of the Snake River. The values determined here are interpreted as the value of the whole Snake River Salmonid fishery. The estimated value of the Snake river fishery was $514.2 millions. The major contributors to this value were Chinook Salmon and Steelhead trout. Given the current condition of the runs of Coho and Sockeye salmon in the river, it is clearly evident that the estimated value understates the original value of these fisheries.

The last dam to be considered is Lower Granite on the Snake River. All of the upper Snake River Idaho, Oregon, and Washington, Chinook and Sockeye salmon, and Steelhead trout pass this dam. The value of the Idaho fishery is shown in table 4. The estimated stock value of the Idaho fisheries was $236.6 millions, and it generates an annual flow of $9.5 millions. Again, the valuation of the Coho and Sockeye runs is flawed in that the average estimated runs do not reflect these runs potential under alternative management scenarios.

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[3]USDA. "Agricultural Outlook." March 1992/AO-183.

Conclusions

The purpose of this paper was to attempt to develop a methodology for estimating the value of the Columbia River fisheries, and a preliminary estimate of the value was made. The total value based on the average runs over the 53 year period arrived at using this method was $1.83 billions for all of the upriver fisheries resources. It should also be pointed out that the average method of valuing these fish runs does not reflect their ultimate or highest value in terms of past values. This method of valuation assumes that at least the average fish runs will be maintained in future years. This may be an heroic assumption given past experience.

The method used to value these fisheries relied upon the values estimated for the upriver fisheries published by the American Fisheries Society. The weaknesses of this method is that it is arbitrary, and it relies primarily upon judgments made by various professionals including this author. It also begs the question as to whether there are values beyond the monetary value of the fish, such as aesthetic, spiritual and cultural values related to these fish. The main reason for doing this type of a study is provide the alternative approach to valuing the Columbia River resources.

Table 1. Monetary Values of Columbia and Snake River Anadromous Fish, 1990.				
Species	Weight (lbs.)	Value* per lb. ($)	Value per fish ($)	Capitalized value ($)
Steelhead Trout	17	$4.56	$77.88	$1,947
Chinook Salmon	33	$4.56	$151.17	$3,779
Coho Salmon	9	$4.56	$41.23	$1,031
Sockeye Salmon	10	$4.56	$45.81	$1,145
Source: American Fisheries Society. "Monetary Values of Freshwater Fish and Fish-Kill Counting Guidelines." American Fisheries Society. Special Publiction No. 13, 1982.				
*Adjusted by the Producer Price Indes to update the values from 1982 to 1990.				

Table 2. Estimated Economic Value of Columbia River Salmonid Fisheries, 1990					
Dam/species	Year	Average number of fish	Value per fish ($)	The annual value of fishery (mil. $)	Capitalized value of fishery* (mil. $)
Bonneville Chinook	1938	365,686	$151.17	$55.3	$1,382.0
Steelhead	1938	162,061	$77.88	$12.6	$315.5
Coho	1938	31,362	$41.23	$1.3	$32.3
Sockeye	1938	88,767	$45.81	$4.1	$101.7
Totals	n/a	567,876	n/a	$73.3	$1,831.5
*A four percent discount rate was used in this study.					

Table 3. Estimated Value of Snake River Salmonid Fisheries, 1990					
Dam/species	Year	Average number of fish	Value per fish ($)	The annual value of fishery (mil. $)	Capitalized value of fishery* (mil. $)
Ice Harbor Chinook	1962	52,159	$151.17	$7.88	$197.1
Steelhead	1962	67,211	$77.88	$5.23	$315.5
Coho	1962	1,509	$41.23	$0.06	$1.6
Sockeye	1962	354	$45.81	$0.02	$0.4
Totals	n/a	121,233	n/a	$13.19	$514.24

*A four percent discount rate was used in this study.

Table 4. Estimated Value of the Idaho Fisheries, 1990					
Dam/species	Year	Average number of fish	Value per fish ($)	The annual value of fishery (mil. $)	Capitalized value of fishery* (mil. $)
Lower Granite Chinook	1975	28,631	$151.17	$4.313	$107.8
Steelhead	1975	65,923	$77.88	$5.134	$128.4
Coho	1975	161	$41.23	$0.001	$0.2
Sockeye	1975	134	$45.81	$0.001	$0.2
Totals	n/a	94,849	n/a	$236.6	$514.24

*A four percent discount rate was used in this study.

Does Contingent Valuation Work in Non-market Economies?

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ABSTRACT

This paper examines the use of contingent valuation to value forest resources for a rural population in Africa. Welfare losses resulting from land-use restrictions associated with a newly established national park in Madagascar are estimated. Because of a limited local cash economy, the contingent valuation questions are denominated in baskets of rice. Our analysis indicates that contingent valuation can be successfully applied to rural households within the developing country context. The econometric analysis undertaken indicates a systematic association between various socio-economic variables of interest and the expressed willingness-to-accept compensation.

Does Contingent Valuation Work in Non-Market Economies?

Introduction

Contingent valuation is a commonly used methodology for es timating the value of environmental and public goods in developed economies. The contingent valuation method (CVM) obtains value estimates of different non-market goods by eliciting information on peoples' preferences within a known context, i.e. the context of market exchange. Contingent valuation (CV) studies implicitly assume that people are capable of responding to questions on value, because they fully understand their preferences, and because they are familiar with the concept of value as established by prices, trade and the consumption of marketed goods.

Estimates of the value of environmental goods are becoming increasingly important for policy making in the developed and developing world. Contingent valuation is a powerful tool, and, in many cases the only means of obtaining such value estimates. In fact, the fastest growing literature in non-market valuation is on the CVM (Smith 1993). However, there are few examples of the use of the CVM in developing countries. Two studies by Whittington et al. (1990; 1991) on the demand for water in Southern Haiti and in the Anambra state of Nigeria, are two of the few contingent valuation experiments undertaken within a mainly rural economy. Their results were promising with regard to how useful CVM can be for valuing a market good. Our paper seeks to explore further the potential for undertaking valuation exercises in the developing country context for non-market goods, and to add to the limited information on how best CV can be applied in such situations. More specifically, this paper presents the results from a contingent valuation study on the *value to rural households in Madagascar, of the loss of access to tropical rain forests.*

Madagascar and the Mantadia National Park

Madagascar lies off of the south-east coast of Africa, between 10° and 30° latitudes south of the equator. It covers an area of 587,000 km^2 and is the fourth largest island in the world. Madagascar is acknowledged to be one of the biologically richest countries in the world. It is one of the twelve currently identified "megadiversity" countries (McNeely et al, 1990). Madagascar is primarily known for its high rates of endemism. It is estimated that 150,000 of the island's 200,000 species of biota are endemic to Madagascar (World Bank 1990). Though biologically rich, Madagascar with a GNP per capita of $190 per annum, is the twelfth poorest nation in the world

(EIU 1991). An estimated 50% of the Malagasy population lives below the official poverty line. Madagascar is fast losing its phenomenal biological heritage to more immediately important economic forces. For example, only half of the 7.8 million hectares of Madagascar's eastern rain forests, which existed in 1950 remain today (Green and Sussman, 1990).

Recognizing that steps need to be taken to conserve Madagascar's unique natural heritage, the Malagasy Government has developed plans for establishing a system of parks and protected areas (World Bank, 1988). The Mantadia National Park in the Andasibe region of eastern Madagascar is one of the new parks being established. The Mantadia National Park extends over an area of 9850 hectares representative of the eastern rain forests. These forests are characterized by thick undergrowth, trees of all sizes and ages, and a mixture of different species. They contain the habitat of the Indri indri, the largest known extant lemur in Madagascar (Harcourt and Thornback 1990).

Although there are no human settlements within the boundaries of the Mantadia National Park, several villages lie in close proximity. There are approximately 10,366 people who live in the vicinity of the Mantadia National Park. The establishment of the park has very important implications for these local residents. The area within the park will no longer be available for swidden agriculture or for foraging forest products, the two primary traditional activities undertaken by the local residents in this region. In essence, villagers will lose access to a large area of land they previously had access to. The Government of Madagascar has plans for establishing a *buffer area* around the park to compensate local residents for the use benefits accruing from the land (World Bank 1988). Initial work to meet this goal has already begun.

In July 1991, a contingent valuation study was undertaken in 17 villages around the Mantadia National Park. This survey covered 15% of the resident population. The aim of the contingent valuation study was to assess rural household preferences for the forests. More specifically the contingent valuation exercise aimed at estimating the loss to rural households from no longer having access to a large area of forest lands, given that compensatory buffer zones were planned for the future.

Survey Development

The first phase of the survey comprised of a pilot study of village households living on the periphery of the Mantadia National Park. This reconnaissance visit to five villages was conducted in the Fall of 1990. This pre-survey provided the necessary background information on socio-economic characteristics and economic activities for the design of the survey. Next the initial survey module for obtaining village level information was developed. This module was based on theoretical considerations and a prior understanding of rural households in the Mantadia region. The survey contained questions on socio-economic characteristics, household economic activities, and contingent valuation. Survey development was aided by an expert group comprised of the survey research team which was experienced in socio-economic surveys.

The next task was to hold a focus group in Andasibe, a town near the national park. Focus groups are increasingly recognized as a critical component in the development of successful survey instruments (Desvouges and Smith 1984). The primary purpose of this group discussion was to obtain feedback from local villagers on the contingent valuation questions in the survey. Based on the focus group discussion, the survey instrument was revised once again. A pre-test of the revised survey instrument was conducted with 25 households in a nearby village. The pre-test motivated several additional changes in the questionnaire. It was especially critical in the final formulation of the CV question. Based on the pre-test the CV format was changed from a willingness-to-pay to a willingness-to-accept format.

The interviewers who conducted the survey were the staff of a NGO called SA.FA.FI, a rural agriculture extension organization, which is well versed in socioeconomic surveys. To provide an incentive to villagers to participate in the survey, a health team accompanied the interviewers. The health team provided medical exams and dispensed basic medicines. The final version of the survey was administered to 351 households over a 2 week period in July 1991.

WTA versus WTP and the Contingent Valuation Exercise

The households were asked to provide a "yes" or "no" response to a *willingness-to-accept* (WTA) bid. There were several important reasons why a willingness-to-accept format was used instead of the more common willingness-to-pay (WTP) format. One criterion for choosing between the WTP and the WTA formats was the issue of property rights. The pre-test results, and conversations with several experts prior to the actual survey, suggested that willingness-to-pay questions might elicit meaningless responses. The rural households in this region have lived amidst the Mantadia forests for a very long period of time. Therefore, while property rights over forested land are legally held by the state, local people *perceive* that they have traditional rights to the land. Mitchell and Carson (1989) suggest that perceived property rights may be more important than actual rights.

In the past, the literature on contingent valuation has contended that under reasonable assumptions, theoretical grounds exist for WTP and WTA bids to be close in value to each other (Willig 1976; Randall and Stoll 1980). However this argument is no longer widely accepted (Carson 1991). WTA and WTP bids are expected to differ if the public good in question is unique, i.e., if the (Hicksian) elasticity of substitution between the public good and market goods is zero or small (Hanemann 1991). In the Mantadia case there is no reason to believe that the elasticity of substitution between the forested lands to which the villagers have lost access and other lands to which they do have access is zero. This situation makes the question of property rights even more significant in the decision to use a WTA or WTP format.

Cummings et al. (1986) argue for the use of WTP questions over WTA questions based on the findings of some experimental studies undertaken to test the CVM. They base their case for WTP mainly on a study undertaken by Coursey et al. (1983) which showed that the *true* WTA and WTP are likely to be very close to each other, while this is not true of *hypothetical* bids. They therefore conclude that the WTA measure obtained in CV studies may be biased upwards. Gregory and Furby (1985) have since shown using the same data but different statistical techniques that the WTA and WTP bids do not converge. Mitchell and Carson (1989) continuing on the same track conclude that although the WTA bid may not *actually* converge to the WTP bid, it does show a *tendency to converge* toward the WTP bid.

Carson (1991) has argued that WTP is more appropriate than WTA if a quantity or price change of a public good affects the same group of agents from both sides of the transaction. For example, with the Mantadia National Park, WTP would be appropriate if losing access to the park area resulted in an increase in non-use benefits to the villagers, and they *were willing to pay* for these benefits emerging from conservation. The pre-survey information did not suggest this.

The households in the Mantadia region clearly considered loss of access to forests within the park boundaries as a decrease in welfare. This perception persisted even when asked to consider the possibility of being compensated with the establishment of buffer zones. In the pre-test, a WTP format was tested. We found respondents willing to pay, but later realized that the positive responses were more a result of a perceived sense of coercion[1], than because respondents were actually willing to pay for conservation. This in conjunction with the property rights issues and the severe income constraints of the respondents suggested that the WTA format was more appropriate. Consequently, in the final survey the format was changed from WTP to WTA.

The numeraire used in the survey to obtain WTA bids was rice. Money was not used as a numeraire because of the subsistence nature of the economy. Rice is the main crop in this region and its value well established. Some amount of the rice produced by most households is sold, and transactions of rice are understood. The unit of measure used was a "vata" of rice, which is a locally used unit for rice transactions. A vata equals 30 kgs of rice. The elicitation method used in the survey was the dichotomous choice method. The contingent valuation question used was:

> *Suppose you are asked to use only the buffer zone, set aside for collecting forest products and for growing crops and are asked not to use the rest of the forests any more. Suppose in order to make up for asking you not to use the forests in the park, you are given ______ vata of rice every year from now on. Would this make you as content as before when you could use the forest in the national park?*
>
> *If YES, would ____ vatas of rice make you as content?*
>
> *If NO, would ____ vatas of rice make you as content?*

Respondents were randomly assigned to seven groups, corresponding to the seven different amounts of vata (1 to 7 vata) used as the offered bid levels. The range of 1 to 7 was based on our a priori understanding of the total amount of rice annually consumed per household.

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[1]There had been cases in the past where villagers had been arrested for undertaking slash and burn agriculture. They therefore seemed to feel compelled to respond positively to questions on how much they would be willing-to-pay for conserving the forests in the park.

Theoretical and Econometric Specification of the Dichotomous Choice Model

The following section presents a theoretical model of rural household preferences, choices, and valuation of forests. This section follows theoretical developments by McConnel (1990) and Cameron (1988).

Let Y_i be the true WTA bid of household i. Y_i is the bid which will make the household as well off with the buffer zones and no access to forests, as it would have been with access to forests and no buffer zones. Y_i is defined by the following relationship:

$$Y_i(.) = e(BZ,0,p,U_o;s_i) - e(0,T,p,U_o;s_i) + \epsilon_i^1 - \epsilon_i^o$$
$$Y_i(.) = \hat{e}(BZ,0,p,,U_o;s_i) - Y^*{}_i + u_i$$

where T refers to forested lands, BZ to buffer zones, p to vector of prices faced by the household, U_o to the initial utility level, and s_i is a vector of socioeconomic characteristics of household i. The e(.) function is the household expenditure function, which is known to households, but known with a margin of error, epsilon_i, to investigators. Y_i^* is current income. Y_i is therefore the difference in the minimum expenditure required to make the village household as well off without the forests as it was with the forests.

Y_i is a continuous function of a number of different variables which appear in the expenditure function. It can be expressed as:

$$Y_i = X_i\beta + U_i \qquad where \quad U_i \sim (0,b^2)$$
$$\kappa = b\frac{\sqrt{3}}{\pi}$$

where X_i is a vector of explanatory variables including household size, hectares of land used for cultivation, the number of years the household has lived in the vicinity of forests, a dummy variable which reflects preference for buffer zones over forests, the education level of the head of the household, household rice yield, which acts as a proxy for household income, a series of dummy variables for each of the interviewers in the surveys, and a series of variables which reflect forest use for non-agricultural purposes. The error term U_i is distributed logistically with mean 0 and standard deviation b.

The dichotomous choice analysis is based on information asked in the household surveys. WTA is the yes/no response to the CV questions. Along with the responses to the WTA bid question, data on a number of other socio-economic variables have been used. Table 1 presents the data used, defines the variables, and specifies the names used for each variable in the logit model presented in next section.

Results and Discussion

Table 2 provides the maximum likelihood estimates of the parameters of the fitted logit function relating the dichotomous response variable to a number of socio-economic variables. As indicated by signs, significant coefficients, and goodness of fit measures, the logit model performs well in explaining variations in responses to the CV questions. Although the estimated regression coefficients cannot be interpreted as marginal influences on the probability of a positive response to the CV questions, the sign of the estimated coefficients indicates the direction of influence.

The offered bid level is significant and has the expected sign, i.e. with an increase in the offered bid amount, the probability that the respondent would say "yes" to the bid increased. With regard to the other independent variables, the parameter estimates which are significant at the five percent level are the number of hectares households use for cultivating crops, the number of years households have lived amidst forests, the dummy on household preference of forests over buffer zones, household rice yield, frequency of construction wood and raphia harvests and, some of the interviewer dummies.

The coefficient on hectares suggests that a one unit increase in the number of hectares households' control, would result in an increase in the probability of the bid being accepted. This apparently surprising result has a good explanation. The larger the acreage of land a household controls or has the labor to utilize for agriculture, the smaller the need for the more fertile lands in the park, and the higher the probability of agreeing to not use the land within the park.

The coefficient on the dummy PREFDUM, is positive as would be expected. Respondents who found buffer zones acceptable were more likely to agree to the offered bid.

The coefficient on rice production has a negative sign. This suggests that production decisions are not independent of consumption decisions. As long as rice yields are directly correlated to rice demand, the negative coefficient on rice yields can be taken to reflect the consumption needs of the household, i.e., the probability of saying yes to the offered bid decreases, the larger the rice requirements of the household.

Several of the dummy variables for interviewers have significant coefficients. While this may reflect interviewer bias, it may also reflect village level differences not captured in other variables.

Interestingly, the coefficients on the frequency of harvest of construction wood and Raphia have opposite signs. The more frequently construction wood is harvested, the less likely the respondent is willing to accept the bid, while the opposite is true for the harvest of Raphia. This suggests that the harvesting of construction wood is threatened by the park, while that of Raphia is not.

One way of deriving a goodness of fit measure for a dichotomous choice model is to estimate McFadden's R^2, also known as the Likelihood Ratio Index (Green 1990), which reflects how well the independent variables can explain the variation in the dependent variable. This is similar to a regular R^2 calculated in ordinary least square estimations except that the maximum likelihood estimator is not chosen such that the R^2 is maximized. The McFadden R^2 derived in this case is .47, which is relatively high for cross sectional data. Another goodness of fit measure often used for logit and probit models is the proportion of correctly predicted responses. This model predicts 165 of the 191, or 86% of the observations correctly.

The Mean Willingness to Accept

Since we have followed the Cameron's (1988) difference in expenditure approach, and not Hanemann's (1984) difference in utility approach, the mean WTA is also estimated as suggested by Cameron (1988). The conditional expectation of the WTA bid, is therefore given by the predicted value of willingness to accept function estimated at mean values of the covariates.

More specifically, the expected value of the WTA bid is given by :

$$
\begin{aligned}
E(Y_o) &= E(X_i\beta + U_i) \\
&= \bar{X}b + E(U_i) \\
&= X_o b
\end{aligned}
$$

Re-parameterizing the logit parameter estimates allows us to obtain the parameters of the valuation function. The predicted mean willingness to accept is then obtained by using these estimates in conjunction with the mean values of the covariates. The mean willingness to accept is estimated to be 8.03 vata of rice. A vata of rice is equal to

30 kgs of husked rice and each kg of rice is worth FMG 500. Translating this to dollar terms, the mean WTA is estimated to be $108.34 per household per year. Aggregating over the relevant population in the area and assuming a twenty year project life at 10% discount rate, the aggregate net present value of the welfare loss is $673,078.[2]

While a point estimate of the mean WTA bid is a necessary and policy-relevant statistic, it is important to also establish the "robustness" of the fitted WTA function. Following Cameron (1991), confidence intervals are constructed by using the variance-covariance matrix from the maximum likelihood logit estimation, to obtain Σ_β, the variance-covariance matrix of the re-parameterized coefficients of the valuation function. This information is then used to estimate the variance of the mean predicted WTA bid. The estimated standard error for the mean bid is 2.44 vata. The 95% confidence intervals for the mean bid are given by (Cameron 1991),

$$CI_{.95}[E(Y_o)] = X_o'b \pm \frac{t_{.025}}{\sqrt{X_o \Sigma_\beta X_o'}}$$

Based on the estimated variance, 95% of the time, the mean WTA bid is bounded above by 8.83 vata and below by 7.22 vata. This implies that 95% of the time, the predicted mean bid per household per year lies between $119 and $97.

Conclusions

In this study, contingent valuation was used to estimate the welfare change perceived by local residents as a result of loss of access to lands currently within the Mantadia National Park. Our analysis indicates that contingent valuation can be successfully applied within the developing country context. The econometric analysis undertaken indicates a systematic association between various socio-economic variables of interest and the expressed willingness-to-accept compensation. The results also show that responses to a rather difficult valuation question were non-random.

Further evidence of the usefulness of this approach can be obtained by comparing the household WTA estimates with welfare loss estimates calculated with opportunity cost analysis based on cash flow models constructed from a socio-economic survey of the villages. Although not reported in this paper, the opportunity cost results were

[2]The real exchange rate used is FMG 1111.8 / 1 $.

quite similar to the CV estimates (Shyamsundar, Kramer, and Sharma 1993). Furthermore, based on the cash flow analysis, household annual average income is estimated to be $243, which is approximately 2.25 times the estimated mean compensation. This again is indicative of the validity of the CV responses.

Our experience suggests that an in depth understanding of household attitudes toward the good being valued, and of household socio-economic characteristics is required for successful implementation of the CV method in this context. Variables considered important in market economies, like wage income, may not be as relevant within the rural economies of some developing countries. Pre-tests and focus group interviews are perhaps even more important in these settings than in Western countries. The use of the CVM in non-market economies also requires a very well trained set of interviewers. As our analysis indicates, interviewer bias may be an important consideration. More than one pre-test may be helpful, not only to "fine tune" the questions but also to observe and train interviewers.

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Table 1. Definition of Variables Used in Logistic Regression	
Household Level Variables Used in Dichotomous Choice Analysis	Names used for the variables in the logit model
Yes/No responses to the bid (the dependent variable)	WTA
Offered bid (ranging from 1 to 7)	BID
Number household members	HHMEM
Number of hectares of land available for cultivation	LAND
Number of years the household has lived near forests	FORYR
A dummy representing preference for buffer zones over forests (Buffer Zones = 1, Forests = 0)	PREFDUM
Years of education of head of household	EDUCN
Rice yield per household	RTOT
A dummy for the first interviewer in group 1	INT1DUM
A dummy for the second interviewer in group 1	INT2DUM
A dummy for the first interviewer in group 2	INT3DUM
A dummy for the second interviewer in group 2	INT4DUM
A dummy for the second interviewer in group 3	INT6DUM
A dummy for the first interviewer in group 4	INT7DUM
A dummy for the second interviewer in group 4	INT8DUM
A dummy for the first interviewer in group 5	INT9DUM
A dummy for the second interviewer in group 5	INT10DUM
Frequency of construction wood collection per household	CNFREQ
Frequency of fuel wood collection per household	FWDFREQ
Frequency of Herana[@] collection per household	OGRFREQ
Frequency of Raphia[*] collection per household	ROFFREQ
Frequency of Harefo[@] collection per household	HARFREQ

[@] Herana and Harefo are two different types of grass collected by households, and used for making household articles. [*]Raphia is a palm used for making fibre.

Table 2. Maximum Likelihood Estimates of the Logit Regression of Contingent Valuation Responses			
Variable	Coefficient	Standard Error	t-ratio
Constant	-8.3432	2.287	-3.648
BID	0.26043	0.1282	2.031
HHMEM	-0.11920	0.1102	-1.082
LAND	0.67927	0.2702	2.514
FORYR	0.038293	0.01969	1.945
EDUCN	-0.064296	0.07398	-0.869
RTOT	-0.0018125	0.0006585	-2.752
INT1DUM	6.2003	1.776	3.492
INT2DUM	4.5902	1.445	3.176
INT3DUM	2.7460	1.628	1.686
INT4DUM	-4.3075	2.794	-1.542
INT6DUM	-0.52062	1.419	-0.367
INT7DUM	5.8346	1.662	3.511
INT8DUM	6.6151	1.728	3.829
INT9DUM	0.086763	1.575	0.055
INT10DUM	6.7610	1.730	3.908
CNFREQ	-0.25724	0.1236	-2.082
FWDFREQ	0.004275	0.002516	1.699
OGRFREQ	-0.76782	0.5010	-1.533
ROFFREQ	1.9439	0.9399	2.068
HARFREQ	0.070036	0.04609	1.520
Maximum Log Likelihood		-68.27945	
n		191	
McFadden's R^2		.47	
Percent Correctly Predicted		.86	
McFadden's R^2 = 1 - {Log Likelihood/Log Likelihood (slopes=0)}			

Test-Retest Reliability of Contingent Valuation Estimates For an Unfamiliar Policy Choice: Valuation of Tropical Rain Forest Preservation

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Test-Retest Reliability of Contingent Valuation Estimates For an Unfamiliar Policy Choice: Valuation of Tropical Rain Forest Preservation

Contingent valuation (CV) is becoming more widely recognized as an important method of resource economists, but it is still a new method. As such, it is subject to methodological concerns, such as the validity and reliability of the estimates produced. Loomis shows that more studies have examined the validity of CV than have tested the reliability. This fact was also noted by Musser and his colleagues. If information from contingent valuation surveys is to be used when making policy decisions, it is essential that the results of the surveys are reliable. That is, the results must be reproducible, or consistent over time.

This study reports a test-retest reliability study of the CV method. We examine the reliability of willingness-to-pay (WTP) responses to a question about preserving the wilderness characteristics of a substantial area of tropical rain forest. This study is interesting for two reasons. First, it expands the relatively small number of reliability studies. Second, it examines the reliability of responses concerning an environmental condition that, while it has considerable policy relevance and occasional news value, is not familiar to most individuals.

Previous Studies

Several studies of CV reliability examined products that were familiar to the respondents. This enhances the ability of respondents to give meaningful answers and may be presumed to give more reliable answers. Examples of reliability studies using familiar commodities or services include Kealy, Montgomery and Dovidio (candy bars), Musser and colleagues (cross-country skiing) and Loomis (sample of visitors to Mono Lake). Others examined situations with which the respondents were familiar, but may not have given much prior thought to the specific characteristic presented in the CV survey. Examples include Jones-Lee, Hammerton and Phillips (highway safety) and Kealy, Montgomery and Dovidio (acid precipitation effects on the Adirondack Park). Only Loomis has tested the reliability of CV responses to questions where a large number of respondents might not know about the topic (protection of Mono Lake for the sample of the entire population of California).

Another important aspect of test-retest reliability studies is the time interval between the initial test and the follow-up. It is desirable that the respondents use the same process for determining their response to each presentation of the willingness-to-pay (WTP) question. Thus, the interval between tests should be long enough that respondents do not merely repeat their previous response from memory when deciding upon their response to the retest question. This suggests longer rather than shorter intervals, although the subject being evaluated may influence the memorability of a particular response. Trivial items where a response can easily be determined may not be remembered for more than a few days. A decision that requires thought and consideration or which is quite important to an individual may be remembered for a longer time. Thus, the interval used should be long enough that it is reasonable to assume that most respondents do not remember their previous responses. The interval should be short enough that the important determinants of a person's bid have not changed between the two tests. Alternatively, if changes in variables that influence the bid are expected during the interval, measures of the determining variables need to be taken with each test.

Kealy, Montgomery and Dovidio used a two week interval in their studies of students' WTP for candy bars and for prevention of additional damage to the Adirondack region from acid precipitation. Loehman and De, also using a student population, used a three week interval in their studies of WTP for improved air quality. Loomis used retest intervals of nine months for the general population sample and a five month interval with the sample of visitors to Mono Lake. Musser and his colleagues used a retest interval of about one year when studying WTP for cross-country skiing opportunities. While there is no hard-and-fast rule about the appropriate time interval, it seems logical that policy makers would want to base policies on values for non-marketed goods or services that remain valid over periods of several years or longer.

The Study

The study consisted of two rounds of mailed CV survey. Each round followed the Total Design Method (Dillman), with the exception that the final mailing of each round was sent by regular first-class mail. The first round was initiated in June 1990 and the re-test round began in April 1991, about 10 months after the first round. This time interval is believed to permit sufficient time for respondents to forget their previous WTP amount and require that they rethink their WTP. As described below, the survey instrument for each round obtained data about variables that are believed to influence WTP so that the influence of changes in those variables could be considered.

The first round survey instrument included a brief description of a way to protect a high proportion of the remaining area of tropical rain forest including suggestions that non-governmental organizations might organize a specific protection scheme. Respondents were then asked the following question.

> Thinking about your current monthly expenditures for food, clothing, charities, bills, etc., what is the maximum, one-time amount your household would be willing to pay to promote tropical rain forest protection?

This question was followed by a question examining several possible reasons for a $0.00 answer, including several protest and non-protest answers and a final opportunity to list "other" reasons. The first round instrument also included questions to measure the respondent's level of agreement with the New Environmental Paradigm (Dunlap and Van Liere), three different ways to measure respondent's knowledge of tropical rain forests (Griffith), level of schooling attained by the respondent, household income, knowledge of household use of tropical rain forest products, previous or intended visits to tropical rain forests, and sources of information or news about tropical rain forests.

The questionnaire was sent to 1000 individuals drawn randomly from all Pennsylvania residents listed in current telephone directories. The sample was drawn by a professional survey research company. Questionnaires were returned by 416 households. After adjustment for incomplete or duplicate returns, 386 useable returns were analyzed, a response rate of about 39 percent.

The second round questionnaire was mailed to all respondents to the first round and to 200 additional households not surveyed in the first round. The added households permit testing for any influence on responses due to prior exposure to the first round questionnaire. Statistical analysis showed that WTP responses of those who participated in the first round were not significantly different from those who received only the second round. For this study of reliability, only the respondents who participated in both rounds are analyzed.

Two hundred eighteen usable responses were received from the 386 respondents to the first round, giving a second round response rate of about 57 percent. The questionnaire for the second round was modified slightly in light of results from the first round questionnaire. First, a section of true-false questions about tropical rain forests was eliminated. Analysis of the first round data indicated that other ways of determining respondent knowledge were more significant in explaining WTP. Second, three demographic questions were added including the number of people in the household, the number of people who work outside the home, and the number of people in high school and in college. The format of the income question was changed to increase the number of responses to that item. The first round used an open-ended income question; the second round presented 20 income categories and asked the respondent to indicate the category that included their household income for the previous year. The second round questionnaire also included a few sentences explaining why the income data was important to the interpretation of the survey data.

A fourth change incorporated Dillman's suggestion to have the first question be neutral, easy, and applicable-to-everyone. The questions added asked respondents to describe where they live (large city, town, farm, etc.) and to describe family participation in outdoor recreational activities by checking activities that apply from a list. The fifth change made the final question about attitude toward the environment more neutral. The last statement in the first survey asked for level of agreement or disagreement with the statement, "Humans are severely abusing the environment." Rather than end on such a strong statement that led several respondents to comment on a perceived bias in the questionnaire, we added another statement so that the final question is, "As we learn more about the environment, people are managing it more wisely."

The final changes modified the WTP scenario by suggesting a more specific form of organization to provide tropical rain forest protection and added a question about willingness to donate time (in addition to money) to a sponsorship organization for tropical rain forest protection. All of the changes were believed to correct minor deficiencies in the initial questionnaire, yet leave the task presented to the respondents unchanged.

Results

A measure is reliable if the same results occur on repeated trials of an experiment, test, or any measuring procedure. Five methods were used in this study to assess the reliability of WTP bids: the correlation between two parallel measures, stability in the type of bid, common significant variables appearing in the "best fit" regression equation for each period, similarity of coefficients in a "best fit" regression equation applied to each period, and a pooled-data test of independence of the two sets of responses.

Correlation Between Two Parallel Measures

The standard measure of reliability is the correlation between parallel scores on the same test at two different points in time. This method can be used with a test-retest survey. The correlation coefficient between the two WTP bids of the 146 respondents who provided non-protest bids on each round was 0.63, indicating a fairly high level of reliability.

Simple Regression of WTPs

Another way to examine reliability is to regress the second bid against the first bid. If each person gave the same bid each time, the estimated coefficient would equal one and the estimated constant would equal zero. Table 1 presents the results of the regression. The adjusted R^2 shows that almost 40% of the variation in the second bids is accounted for by variation in the first bids. However, each of the estimated parameters is significantly different from the hypothesized value. These results reject the hypothesized reliability of the WTP responses.

Change in Type of Bid

Another way to assess the reliability of WTP bids is to check whether the respondents who answered both surveys changed the type of bid. Three types of bids could be given: a positive bid, a zero non-protest bid, and a zero protest bid. Table 2 is divided into three sections--those who gave the same type of bid, those who gave a similar type of bid (a positive bid and then a zero non-protest bid or vice versa), and those who had a complete change of bid type (a zero non-protest bid to a zero protest bid or vice versa). Nearly two-thirds of the respondents kept the same bid and 78% gave the same or similar types of bids.

Common Significant Variables

If a measure of WTP is reliable, the independent variables that explain variation in the responses from each round should be the same. The responses for each round were used to estimate a regression equation explaining the variation in expressed WTP. Similar independent variables were considered in developing the models for each round, although the exact form of some variables differed between the two questionnaires. The second round added a question about the respondent's willingness to donate time in addition to the monetary bid given.

The best equation for each round is presented in Table 3. Independent variables used are INCOME, representing income categories ($5,000 interval), KNOWLEDGE, a dichotomous variable based on evaluation of responses to an open-ended question, INCOME*KNOWLEDGE, an interaction of two dichotomous variables with income = 1 for income greater than $65,000 and knowledge = 1 for knowledge scores 5 or 6, SCHOOLING, representing categories of schooling completed ranging from "no formal education" to "a graduate degree," ENVIRONMENTAL ATTITUDE, the score on the test of agreement with the new environmental paradigm (range = 12-60, higher scores indicate stronger agreement), DONATE TIME, a dichotomous variable where 1 = willing to donate time (only asked on the second round), AREA, a dichotomous variable where 1 = an urban residential location, LEARNING, a dichotomous variable where 1 = active forms of learning about tropical rain forests, such as, attending meetings or talking with family or friends (passive learning included watching TV news programs or specials), and USE VALUE, a dichotomous variable where 1 = an affirmative response to any of three questions about visits or intended visits to tropical rain forests or the household use of products from tropical rain forests.

The independent variables included in the best fit equations all have coefficient estimates significantly different from zero at the usual levels of significance except income in the 1st survey equation. The significance level ($\alpha = 0.18$) is greater than usually accepted in hypothesis testing, but the variable is retained in the equation because of its theoretic significance and the fact that other variables considered but rejected required much higher significance levels to be considered significantly different from zero. With that *caveat*, we observe that each equation includes several of the same variables with statistically significant coefficients. Income and knowledge of tropical rain forests, either directly or as an interaction, schooling, and environmental attitude are included in each equation. These variables account for four of the five variables included in the 1st round equation and three of the six variables in the 2nd round equation. While this test shows a weaker indication of reliability than the previous ones examined, it indicates a stability of underlying relationships over time.

Similarity of Coefficients

In this test of reliability we applied a relatively simple model using independent variables that are conceptually the most attractive and that previous analysis showed to be statistically significant in explaining variations in WTP. The dependent variable in this model was the indicated willingness-to-pay for protection of tropical rainforests. The independent variables were income (as categories), knowledge of TRF (dichotomous variable), amount of schooling (as categories), environmental attitude score, and whether or not the respondent had a use value for TRFs (dichotomous variable). The model was estimated separately with data from the two surveys using tobit regression. The results (Table 4) show that for the first survey, all of the coefficient estimates are significantly different from zero, although the income coefficient is marginal by conventional choices for the alpha level. For the second survey, the estimated coefficients for schooling and for use value were not significantly different from zero. These two variables had a rather substantial impact on WTP in the first survey. Income had a similar effect in the two equations while knowledge of TRFs and environmental attitude had the same effect in each equation, although environmental attitude had a sign opposite of that hypothesized for each equation. It is our judgement that this test provides only weak support for the reliablilty of the CV method.

Pooled-Data Test

The sixth test of reliability used in this study was to pool the data from both rounds and test for independence of the two sets of data. The restricted and unrestricted models were estimated using Tobit regression. The test of independence of the data sets required calculating the value of two times the difference in the log-likelihood values of the two Tobit regression estimates. The Chi-square test rejected the hypothesis of independence at an alpha level equal to 0.005. This result provides strong support for the hypothesis of CV reliability.

Conclusions

This study examines the reliability of CV estimates of a general population's willingness to pay for the protection of tropical rain forests. This is a subject that was unfamiliar to most respondents. Of the six measures of reliability examined (Table 5), three support reliability and two others give weak support. Only one test rejects the hypothesis of reliability.

First, there was a rather high correlation between the bids given in the two rounds of the study. The second test, a simple regression of WTP from the first survey on WTP from the second survey, rejected the hypothesis of reliability. Third, bids given nearly a year apart were of the same type for most respondents. That is, there was little switching among positive value bids, zero bids, and protest bids. The fourth measure of reliability, common significant variables in best fit regression equations for each round, showed that the underlying explanatory relationships are rather stable over the period of the study, although some differences appear. The fifth measure, similarity of coefficients in equations estimated with the same variables using data from each round was not strong in supporting similarity and thus, gives only weak support to a hypothesis of reliability. The sixth measure, a pooled-data test, rejected the hypothesis of independence of the two rounds of data at a level of significance that gives strong support to a conclusion of reliability of the CV method. Overall, we conclude that respondents seem to have given similar answers to each round of the study and there is evidence that the CV method is reliable even when applied to problems not familiar to most respondents.

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Table 1. Simple Regression of WTP Bids				
Variable	Parameter Estimate	Hypothesis: Coefficient =	T Value	Probability > |T|
Intercept	15.301	0	2.24	0.026
WTP 1	0.638	1	-5.52	0.001
N	146			
Adjusted R^2	.393			

Table 2. Comparing Types of WTP Bids		
1st --> 2nd Bid	Number in Both Surveys	Percent of Total
Kept The Same Type	141	64.68%
BID --> BID	35	16.06%
NP --> NP	82	37.61%
P --> P	24	11.01%
Similar Type of Bid	29	13.30%
BID --> NP	17	7.80%
NP --> BID	12	5.50%
Complete Change	48	22.02%
BID --> P	5	2.29%
NP --> P	17	7.80%
P --> BID	8	3.67%
P --> NP	18	8.26%
Total	218	100%

BID: The respondent gave a positive, non-zero WTP bid.

NP: The respondent gave a zero non-protest bid.

P: The respondent gave a zero protest bid.

Table 3. Comparision of Tobit Regression Variables				
Variables	1st Survey Coefficients	1st Survey Significance	2nd Survey Coefficients	2nd Survey Significance
Intercept	159.80	0.0000	144.50	0.0000
Income	5.43	0.1825		
Knowledge (1=High)	115.95	0.0084		
Income* Knowledge (1=Each Hi)			308.13	0.0000
Schooling	18.98	0.0829	18.07	0.0134
Environmental Attitude	-5.65	0.0001	-4.34	0.0002
Donate Time (1=YES)	N/A	N/A	85.70	0.0122
Area (1=Urban)			-55.25	0.0932
Learning (1=Active)			149.44	0.0322
Use Value (1=Yes)	87.05	0.0239		
N	123		130	
Adjusted R^2	.2016		.4268	
Log-Likelihood	-358.18		-337.35	

Table 4. Comparison of Tobit Regression Coefficients For a Common Model						
Variables	First Survey			Second Survey		
	Mean	Coeff.	Signif.	Mean	Coeff.	Signif.
Sigma		157.73	0.000		174.98	0.000
Income	7.32	6.35	0.113	7.89	11.76	0.003
Knowledge (1=Hi)	0.15	107.82	0.013	0.12	104.40	0.043
School	5.92	20.12	0.055	5.85	11.62	0.283
Environ. Attitude	46.3	-5.89	0.000	46.4	-5.69	0.000
Use Value (1=Yes)	0.30	82.92	0.029	0.45	21.27	0.590
N	123			151		
Log-Likelihood	-356.53			-373.18		

Table 5. Summary		
Test	Result	Ho: CV Reliable
Correlation	0.63	Support
Simple OLS Regression	H0: a=0, reject H0: b=1, reject	Reject
Type of Bid	75% Same or Similar	Support
Common Significant Variables	4/5 Round 1 3/6 Round 2	Weak Support
Similarity of Tobit Coefficients	3/5 Similar	Weak Support
Pooled-Data	Reject Independence at $\alpha=0.005$	Support

Benefit-Cost Analysis of a Trout Fishery in Southeastern Oklahoma[1]

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Benefit-Cost Analysis of a Trout Fishery in Southeastern Oklahoma

Introduction

The development and operation of a natural resource project frequently impacts adjacent resources; e.g., ecological change downstream of a multi-purpose water project. This benefit-cost study represents an analysis of the ecological change downstream from Broken Bow Dam in southeastern Oklahoma. Construction of the dam altered the periodic rate of water flow and water temperature, which in turn changed ecology downstream of the dam (Harper). Broken Bow Lake was authorized by the Flood Control Act and approved 3 July 1958 (Public Law 85-500 85th Congress, 2d Session) in accordance with the recommendations of the Chief of Engineers in House Document No. 170, 85th Congress, 1st Session (U. S. Army Corps of Engineers). Preconstruction planning was initiated in 1959, and initial construction funds were appropriated for FY 1961. Project purposes included flood control, water supply, and hydroelectric power. Conservation of fish and wildlife was a project purpose authorized by the Fish and Wildlife Coordination Act (Public Law 85-624).

Before construction of the Broken Bow Dam, the Mountain Fork River was inhabited by warm water fish species. After construction, operation of the hydropower complex released large volumes of cold water (water released from lower depths of the reservoir) into the river making water temperature regimes uncertain and irregular (mixed warm water and cold water) and changing the periodic rate of water flow. For several miles below the dam, the changed environment of the river was less habitable to the native fish species. Presumably, these downstream costs were included in the original evaluation of the project. But what about current conditions? By converting the water temperature regime from uncertain and irregular to certain and regular cold water, a different type of fishery could be established.

In 1986, a preliminary biological feasibility study for a trout fishery was initiated by the Oklahoma Department of Wildlife Conservation (Department) and the U.S. Fish and Wildlife Service (Service) with assistance from the U.S. Army Corps of Engineers (Corps). Positive results of that study provided the basis for developing a trout fishery on the river below Broken Bow Dam. On 1 January 1989, the Department designated approximately 12 miles of the river and tributaries from Broken Bow Dam downstream to the U.S. Highway 70 bridge as a year-round trout fishery (Harper).

The Department established the trout fishery by stocking 3,850 catchable (8.5-inch minimum) rainbow trout on a biweekly basis from 1 January 1989 to the current period (Harper). The trout were stocked in areas below the Broken Bow Dam to U.S. Highway 70, including the Beavers Bend State Park (Figure 1). The Corps was contracted to release water from the Broken Bow Lake at appropriate times and in sufficient volume to maintain the environment for the operation of the year-round trout fishery.

Problem Statement

An economic evaluation of the trout fishery was needed to (1) assess the feasibility of the project, (2) justify current and future public expenditures, and (3) manage the public resource efficiently. Costs of the project included operation and maintenance (O&M) costs of the trout fishery and opportunity costs foregone from implementation of the project. Trout stocking costs were included as O&M costs. Opportunity costs included the value of water released exclusively for maintaining the trout fishery and the benefits foregone from prior fishing activities. Benefits of the project were determined from angler use of the trout fishery. Market transaction information on the demand for the trout fishery by anglers was not available for estimating benefits. Thus, nonmarket valuation methods were required to achieve the needed evaluation. No other benefits or costs were considered.

Objectives

The principal objective of this study was to assess the economic value of the year-round trout fishery in the river below Broken Bow Dam. In addition, summary characteristics data about the anglers and the fishery are presented. Finally, management and policy decisions are evaluated based on the study results.

Procedures

Several surveys were used to obtain three years of characteristics data about anglers and the trout fishery[6]. We used a pressure count survey and a creel survey administrated by the Department at the site of the trout fishery. The pressure count survey was used to estimate the total number of angler hours, and the creel survey was used to estimate the return rate of stocked trout and to obtain limited information about anglers. The creel survey was a random, non-uniform probability survey conducted on each of three sectors along the 12-mile length of river. There were 20 survey days (12 weekends and 8 weekdays) for each 3-month period during the year.

During the first year (1989), anglers were given a third survey instrument at the conclusion of the creel survey, which was a postage-paid postcard with a minimum number of questions. Because the creel survey was random, the postcard survey would have been a random sample of total anglers if all postcards were returned. Of 620 postcard surveys handed out in 1989, only 180 were returned (29 percent return rate). Results indicated a geographic bias when compared with the creel survey sample. Anglers residing in McCurtain County, where the trout fishery is located, were underrepresented and anglers from other regions were overrepresented. Because of the low response rate and geographic bias, the postcard survey was not used in 1990 or 1991. Instead, angler telephone numbers were obtained during the creel survey and were used in a telephone survey.

We used a follow-up telephone survey of a randomly selected sub-sample of replies to the postcard survey (1989) and the creel surveys (1990 and 1991). The telephone surveys included socioeconomic data about anglers and information on trout fishing trips, alternative recreational activities, and so forth. In 1989, 112 telephone surveys were completed. Because seasonal differences existed in the estimated number of angler trips,

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[6]The fishing year starts on December 1 of each year and goes to November 31 of the following year. However, because the trout fishery was not started until 1 January 1989 the first year of the fishery was 11 months. The second (1990) and third (1991) years of operation were for 12 months each.

the telephone surveys were administered by quarter for 1990 and 1991[7]; 366 surveys were completed in 1990 and 322 in 1991.

Questionnaires for the telephone survey were changed each year based on experience in administering the survey in the previous year. Most changes were in wording of questions to assist anglers in interpreting and responding to the interviewer. However, significant changes were made at the end of the first year because some of the questions were judged to add little useful information. There was also a need to reduce the amount of time needed to complete an interview. The most significant change in 1990 and 1991 was in reference to seasonal information on the number of trips. Only annual information regarding trips was collected for 1989. There were about twice as many questions asked in the 1989 survey compared with the 1990 and 1991 surveys.

In addition to characteristics data, surveys provided information for benefit estimation of the trout fishery. Based on the Principles and Guidelines (U.S. Water Resources Council), benefits of recreation projects are measured by willingness to pay (WTP). Total WTP was the sum of entrance fees, actual (travel) costs paid to visit the site, and any unpaid value (surplus) enjoyed by users. Total benefit was the maximum amount that individuals were willing to pay rather than go without the recreation activity and thus was equal to total WTP. Net benefit was total benefit less the amount actually paid as direct costs and thus identical to the surplus or unpaid value. Consumer surplus was defined as the willingness of consumers to pay in excess of their actual payment and was represented as the area under the demand curve above the price line (Vincent et al.)[8]. Thus an estimated demand curve for the trout fishery was needed.

Most public recreational goods are not priced in the market place, and therefore demand can not be estimated directly. However, such nonmarket goods can generally be valued indirectly by either the expenditure function approach or the income compensation approach (Randall). The travel cost method (TCM) is categorized

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[7]The quarters are December - February, March - May, June - August, and September - November.

[8]Though not a rigorous measure of welfare change, consumer surplus is commonly used in empirical analysis because Marshallian demand curves are more easily estimated than Hicksian demand curves. In addition, Willig has demonstrated and justified consumer surplus as an approximation of the Hicksian measure of welfare if the income effect of a price change is small.

as an expenditure function approach but the contingent valuation approach (CVM) is classified as an income compensation approach.

The TCM approach relies on observed information from anglers on actual trip (travel) costs and the frequency of trips. Presumably, an inverse relationship exists between trip cost (price) and number of trips an angler takes to the fishing site. The CVM approach involves construction of a hypothetical market of which the features of nonhypothetical markets and institutions are employed as mechanisms to reveal demand for the nonmarket good (Durden and Shogren). By using survey methods, respondents' values for nonmarket goods are revealed.

General criteria for selecting alternative approaches to value nonmarket goods was taken from the literature (Clawson and Knetsch, 1963, 1966; U.S. Water Resources Council; Walsh). The TCM is generally applicable for parks and facilities that provide hiking, camping, fishing, boating, and hunting while on day outings and weekend trips within a 2-hour drive from home (Walsh). This usually ensures sufficient variation in travel cost (price) to allow statistical estimation of the relationship between price and frequency of trips. Sample data of anglers in our study showed an average one-way travel distance from their residence of about 100 miles. Furthermore, expenditure data for anglers were available from the telephone survey. Therefore, the TCM was selected as the empirical demand and benefit estimation approach for this study.

Survey Data Results

Total angler hours and trout harvest were estimated by the Department for the three years of the trout fishery based on the pressure count and creel surveys (Table 1). Estimated total angler hours was slightly higher for the second year compared with the first year (11 months) and substantially higher for the third year compared with the first and second years. Estimated trout harvest increased from about 40,000 in the first year to about 54,200 in the second year, but decreased in the third year to about 38,600. Trout harvest rate, which was computed by dividing estimated trout harvested by angler hours, increased in the second year because of the increment in the trout harvest for that year, but decreased substantially in the third year because of a decrease in the total number of trout harvested and an increase in total angler hours. Seasonal variation showed substantially greater numbers of angler hours during spring and summer seasons compared with fall and winter.

Information from the creel and telephone surveys was combined to map the estimated number of angler trips by county of residence (Figure 2). The geographic data seemed consistent for 1990 and 1991 and thus were combined into one map. The highest number of trips were taken by local anglers from McCurtain County. The metropolitan areas of Dallas/Fort Worth, Oklahoma City, and Tulsa also showed high frequency of angler trips. The state boundary of Oklahoma appeared to limit the number of anglers from Arkansas and Louisiana but was less of a constraint to anglers from Texas. The cost of out-of-state fishing permits and alternative in-state trout fisheries probably limited Arkansas anglers from participating in the trout fishery. There are fewer in-state trout fisheries in Texas. Over 90 percent of the anglers were Oklahoma and Texas residents. Except in fall, Oklahoma residents made up > 50 percent of the total anglers. A higher proportion of anglers was from McCurtain County during the winter season compared with the other seasons. The percentage of anglers from out-of-state was higher for spring, summer, and fall compared with winter.

Frequency of angler trips per 1,000 population is mapped in Figure 3. The darker shading shows a higher proportion of angler trips per 1,000 county population. Except for some counties across state boundaries in Arkansas and Louisiana, the more proximate counties to the trout fishery showed higher frequency of participation, which implies travel distance is a principal constraint.

Average seasonal one-way travel distance to the fishery ranged from 74 to 167 miles, depending on season and year. The annual average one-way travel distance increased from 60 miles in 1989 to 137 in 1991, indicating the trout fishery had become more widely known. Survey results of travel distance indicated that spring, summer, and fall showed a higher percentage of anglers coming from a one-way distance of > 150 miles compared with the winter season. A substantially higher proportion of winter anglers compared with other seasons (ranging from 46 percent to 53 percent, depending on year) came from the local area (within a 25 mile radius of the fishery).

Number of fishing trips per angler by season are shown in Table 2. Average number of fishing trips per angler was highest during winter, with more than eight trips. Other seasons ranged from about 2.7 to 5.9 trips per season. Over 48 percent of the anglers who took a trip to the fishery during spring, summer, and fall did not take another trip during the same season.

Anglers were asked how many fishing trips they took to other locations during the same season. Average number of fishing trips to other locations was lowest in winter (3.1 trips for 1990, 2.7 for 1991) and highest in summer (7.2 trips for 1990, 9.5 for 1991). More than a third of the anglers indicated that they did not fish at any location other than the trout fishery during the same season in 1990 and 1991. However, for those who did fish one or more times at another location, the average number of trips per angler per season ranged from 7 (winter 1991) to 18 (spring 1990). In 1989, about 14 percent of the anglers fished exclusively at the trout fishery. There seems to be an inverse relationship between the seasonal average number of trout fishing trips and the average number of fishing trips to other locations, indicating that the trout fishery provided a unique fishing experience, particularly in winter.

Frequency of fishing trips per year to the river prior to establishment of the trout fishery on 1 January 1989 is shown in Table 3. Almost half of the anglers were first-time visitors to the river after the trout fishery began operation. About 34 percent of the anglers visited one to five times a year and about 20 percent of the anglers visited more than five times a year. Average annual number of fishing trips to the river prior to 1 January 1989 for this sample of anglers was about six, but the average after 1 January 1989 was > 15. Establishment of the trout fishery greatly enhanced the frequency of fishing trips to the river.

Angler expenditures per trip were estimated and classified by category (food, lodging, transportation, etc.) and by location of purchase (≤ 25-mile radius of the fishery, outside 25-mile radius but within State of Oklahoma, or outside State of Oklahoma). The expenditure per angler per trip averaged over all seasons ranged from about \$60 to \$90 over the three years. Seasonal differences in angler expenditures were evident for 1990 and 1991. Spring, summer, and fall expenditures per angler per trip were two to three times greater than for winter in 1990, but differences were smaller for 1991. Most anglers spent < \$20 per trip during winter, but most spent > \$50 per trip in the other seasons. Generally, > 70 percent of angler expenditures occurred in the local area or within a 25-mile radius of the fishery. There were no significant seasonal differences in the distribution of angler expenditures by location.

Anglers were asked the specific purpose of their trip to the trout fishery. Over 70 percent of the angler trips were just for trout fishing during all seasons for 1990 and 1991. The winter season had the highest percentage of trips for the single purpose of trout fishing. Purposes of the trip other than trout fishing included recreational activities such as camping, bass fishing, canoeing, sightseeing, and taking a break away from home.

Anglers were asked to express their satisfaction of the trout fishing trip by a quality index from 1 to 10, with 10 being the highest value. Results indicated a high level of satisfaction with the trip. Over 70 percent gave it a quality index of 7 or higher. Less than 13 percent of anglers gave a quality index of < 5. There appeared to be little seasonal variation and little difference among the three fishing years in anglers' evaluation of trips to the trout fishery.

Angler perceptions of the trout fishery were solicited in 1989. No angler replied that the fishery was inadequate and should be discontinued. Most of the anglers (65 percent) perceived that the fishery was adequate and should be maintained as is. About 32 percent indicated that the fishery was adequate but needed to be improved. Potential problem areas included the size of trout stocked, availability of sanitary facilities, catch limit, number of anglers, and water swiftness during electricity generation. Parking facilities, size of stream, and road accessibility to river were perceived to be least problematic.

About 10 percent of the anglers had an annual average household income $< \$15,000$ but > 30 percent had an annual household income $\geq \$45,000$ or more after spring 1990. Average household income was slightly higher for anglers making trips in fall compared with the other seasons.

Generally, ≥ 70 percent of the anglers were employed at the time of their interview. In 1989 anglers were asked separately if they were retired or unemployed; 18 percent replied retired, and two percent replied unemployed. Of 112 anglers interviewed in 1989, 88 were male. This was roughly 80 percent of total anglers interviewed. Over 39 percent of the anglers were > 50 years of age; average age of anglers was 47.

Data on estimated angler hours were combined with data on average hours fishing per trip per angler and average expenditure per trip per angler to estimate aggregate number of angler trips and aggregate angler expenditures associated with establishment of the trout fishery (Table 4). The estimated number of angler trips was positively related to total angler hours and negatively related to average number of hours fishing. Because of higher aggregate angler hours and lower number of hours fishing per trip for spring and summer 1991 compared with 1990, the number of trips was substantially higher for 1991. The fall season for 1990 and 1991 had fewer total trips than any other season.

The estimated aggregate expenditures ranged from about $517,000 in 1989 (11 months) to about $792,000 in 1990 (12 months). Geographic distribution of angler trips and expenditures in 1991 is shown in Figure 4. The graphic data is useful in showing the approximate zonal source of anglers and their associated expenditures. The two closer zones (categories 1 and 2) accounted for about 49 percent of the angler trips but only about 18 percent of the angler expenditures but the two most distant zones (categories 3 and 4) accounted for about 51 percent of the angler trips but 82 percent of the angler expenditures. Some counties are unshaded indicating no angler trips, which is due only because the data are based on sample information. Counties not shaded may well be considered part of the zone indicated by surrounding shaded counties.

Several important findings result from the sample data of the trout fishery and participating anglers:

1. Seasonal variation in angler hours was significant. The number of angler hours in spring and summer was about 66 percent of the total angler hours in 1989, about 58 percent in 1990, and about 67 percent in 1991. The trout harvest per angler per hour was much higher in winter than in the other seasons, which may indicate a need to adjust stocking rates by either lowering the rate during winter or increasing the rate during spring and summer. This result will depend on seasonal differences in the benefit-cost ratios (see later conclusions).

2. Seasonal differences existed in where anglers were coming from. Most winter anglers were from local areas, but higher proportions of anglers came from areas of greater distance in other seasons. Over 29 percent of the anglers came from McCurtain County; roughly 55 percent of the anglers came from the State of Oklahoma (including McCurtain County).

3. The average length of trip was shorter in winter than spring, summer, or fall. Annual average length of trip increased slightly year by year, from 1.51 days in 1989 to 2.39 days in 1990 to 2.63 days in 1991.

4. Except for winter, the number of fishing trips per angler by season was fairly uniform and ranged from 2.7 to 5.9 trips per season. During winter, the average number of trips per angler was over 8.

5. The median annual household income was $30,000 - $40,000, indicating a relatively high income class of anglers. Anglers using the fishery in winter and summer had lower income level than anglers in spring and fall.

6. Average expenditure per angler per trip ranged from $42 to $136, depending on season and fishing year.

7. Establishment of the trout fishery increased the frequency of trips by anglers. The average number of trips per year increased from 6 before 1 January 1989, to 15 after 1 January 1989.

8. Aggregate trip expenditures were estimated as $517,000 to $792,000 per year. From 73 to 84 percent of these expenditures (i.e., $413,000 - $655,000) were estimated to occur within a 25-mile radius of the fishery.

Benefit Estimation Results

A demand function for the trout fishery was used for benefit estimation. Using the travel cost model, an individual trip demand function was specified[9]. The dependent variable was number of trips taken per individual per year (1989) or number of trips taken per season (1990 and 1991). Independent variables in the trip demand function were travel cost (own price) proportion for the trout fishing activity, travel cost proportion associated with other activities of the trip, travel time cost associated with the trip, substitute (complement) effect of other fishing sites to the trout fishery, annual household income, attractiveness or quality of the trip, and other socio-economic variables of the anglers[10]. Travel cost was defined as all monetary (out-of-pocket) costs incurred on-site (food, lodging, services, etc.) as well

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[9]See Choi for a discussion of the individual versus zonal TCM as applied to the trout fishery.

[10]See Choi for a further discussion of each variable and the related literature.

as the transportation cost of the trip. No entrance fee existed at the trout fishery. Although a trout license and fishing license are required, these costs were not included as trip costs unless anglers identified them only with this trip.

Monetary trip costs were allocated to the trout fishing activity and to all other trip activities in proportion to the angler's specification of trip purpose. The monetary cost of other activities was thought to complement the trout fishing experience. Note that the river runs through Beavers Bend State Park, and the park provides several recreational activities other than the trout fishery. Because of high correlation between monetary cost and time cost, these variables were combined into one total travel cost variable. Total trip cost was allocated to the trout fishing activity and other activities in proportion to trip purpose as recorded by anglers.

Complete data were not available to measure a substitute (complement) effect to the trout fishing trip. However, for 1989 and 1990 data were available on the number of other fishing trips and distance from residence to each alternative fishing site. An average transport cost for alternative sites was computed for each individual. The expected sign for a substitute effect was positive, and for a complement effect, it was negative.

Perceived quality of the trout fishing trip was used as a surrogate for attractiveness of the fishery. Presumably, those anglers giving a higher quality index will make more trips to the fishery. Annual household income was used as an independent variable for all three years; socioeconomic variables of age and sex were available only for 1989. A semilog functional form was used to estimate the individual trip demand function. Because heteroscedasticity was detected for some seasons, the estimated generalized least squares estimator was used. Results of the estimated demand functions are given in Appendix A.

All of the coefficients of the "own-price" variables (TCFt) had the expected signs and were statistically significant ($P < 0.05$). Coefficients of the "trip cost for other activities" variable (TCOt) were negative, which indicated that the lower the cost of other activities provided to the anglers, the more trips these anglers made to the trout fishery. The negative sign indicated that other activities were considered complementary to the trout fishery. About three-fourths of the coefficients were statistically different from zero.

Coefficients for the substitute (complement) variable (SUB) were not consistent in sign and in general, were not statistically different from zero, which would imply that the trout fishery was a unique recreational site to the anglers.

Coefficients for the household income variable (Y) and the quality or attractiveness variable (A) generally did not have consistent signs and most were not statistically significant. The positive sign and significance of the variable AGE in 1989 implied that the older the angler, the more trips taken to the trout fishery. The gender variable (SEX) was not statistically significant; however, the sign of the coefficient was positive, indicating that the number of trips was higher for male anglers.

In the context of this study, the own-price elasticity was the percent change in number of trips (demand) in response to a one percent change in TCFt (price). Generally, own-price elasticities fell within the range from -0.30 to -0.80, indicating that number of trips to the trout fishery was price inelastic.

Demand curves were derived from demand functions by multiplying mean values of the other explanatory variables by their corresponding coefficients and adding the product to the intercept terms. Estimated demand curves for the anglers of the trout fishery are shown in Appendix B.

Estimated number of trips to the trout fishery by year, season, and origin of trip is presented in Table 5. Total number of trips increased over the three year period from about 8,400 in 1989 to about 11,100 in 1991. Number of trips by origin decreased slightly for McCurtain County but increased for all other locations, especially from 1990 to 1991. Proportionately, more trips originated from residents of McCurtain County in winter; during other seasons, more trips originated from residents of other locations in Oklahoma and from other states.

Net angler benefits (consumer surplus) from trips to the fishery were calculated from estimated demand curves. For the current formulation of trip demand, estimated consumer surplus varied depending on the assumed highest price anglers were willing to pay. Consumer surplus was computed from the demand curve using the highest price for 90 percent of the trips ranked from lowest to highest price, which provided a conservative measure. Estimated net angler benefits from trips to the fishery by season and by year are presented in Table 6. All monetary values were inflated to represent 1991 dollar value using the Consumer Price Index (CPI)[11]. Aggregate net angler benefits ranged from $965,000 in 1990 to $1,126,000 in 1991.

Estimated net angler benefit per trout harvested was higher spring and summer compared with fall and winter (Table 7), partially because trout fishing in spring and summer was less successful (lower harvest rate) than in other seasons. Estimated net angler benefit per hour of fishing ranged from $14.81 in 1989 to $12.20 in 1991 (all values adjusted to the 1991 price level).

The estimated economic value of the trout fishery was compared to other empirical studies of cold water fishing (Table 8). However, comparisons among empirical studies should be made with caution. The estimated benefits were standardized for methods of measurement and methodology following Sorg and Loomis. The adjusted values reported in Table 8 are per fishing day basis. The estimated economic value of the trout fishery is comparable to other cold water fishery studies.

Benefit-Cost Analysis

Benefits and costs of the trout fishery are summarized in Table 9. Benefits were limited to the estimated consumer surplus attributed by anglers to the fishery by means of the TCM analysis. Costs of the fishery included operation and maintenance and opportunity costs. O&M costs explicitly associated with the fishery were the costs of stocking 3,850 rainbow trout at different locations on the river on a biweekly basis and were adjusted to the 1991 price level[12].

Opportunity costs included the cold water releases from Broken Bow Lake and value of fishing activities prior to implementation of the fishery. Water storage capacity in Broken Bow Lake is for flood

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[11]Source of the CPI is Economic Report of the President, 1992.

[12]O&M costs excluded Department costs for personnel, equipment, and travel for purposes of law enforcement and water quality monitoring. Because revenue from trout license was excluded as a benefit, it was assumed that Department costs above normal fishery management should also be excluded.

control, hydroelectric power generation, municipal and industrial water supply, and recreation and wildlife use, as reported in the U.S. Army Corps of Engineers Master Plan (Uwakonye). There is currently abundant unallocated water in Broken Bow Lake; hence, conflict in water usage is not an issue. For this study, the value of water used from Broken Bow Lake was assumed at a zero opportunity cost. When the situation changes and conflicts occur in the amount of water use, the value of water used for the trout fishery should be included in total project costs.

Prior to implementation of the trout fishery, there were an average 6.3 trips per angler and after implementation 15.2 trips for the sample of anglers in 1989. Estimated trips taken prior to 1 January 1989 were approximated at 3,483, which was 42 percent of the estimated trips in 1989. Unit day value of $19 recommended by the U.S. Forest Service[13] was multiplied by 3,483 to estimate total benefits prior to implementation of the fishery. All fishing trips were assumed to be 1-day trips. This assumption is plausible because most trips were probably taken by local anglers prior to 1 January 1989. Opportunity costs are shown in Table 9.

Overall benefit-cost ratios were computed based on the estimated net angler benefits and costs of the trout fishery. The benefit-cost ratios, excluding opportunity costs, were about 14:1 for 1989, 13:1 for 1990, and 16:1 for 1991. When opportunity costs of foregone fishing activities were included, benefit-cost ratios decreased to about 6:1 for 1989 and 1990, and 7:1 for 1991. However, all of the ratios were greater than 1:1, implying that the angler benefits from the trout fishery were far greater than the costs of the project from 1989 to 1991.

Seasonal analysis for 1990 and 1991 showed that the benefit-cost ratios, excluding opportunity costs, were substantially higher for spring and summer compared with fall and winter. The stocking rate does not vary by season even though angler hours, angler trips, and type of angler (local, state, or out-of-state) varied substantially by season.

[13]Unit day value (1982 price level) per visitor day (12 hours) for wildlife and fish activity with standard quality provided in Southeastern region (Walsh).

Public revenue from fishing licenses or trout licenses is generally excluded in benefit-cost analysis because such analysis is concerned with real resource benefits and costs, not transfer payments (Propst and Gavrilis). The number of trout licenses and revenue increased each year of the trout fishery. The revenue was equal to 55 percent of stocking costs in 1989, 68 percent in 1990, and 77 percent in 1991 (Harper). Furthermore, because a fishing license was required before purchase of a trout license[14], a portion of the license revenue may be allocated to the trout fishery activity. The number of trout fishing trips to total fishing trips per angler ranged from 36 percent to 59 percent, depending on year. Including these proportions of the fishing license revenue with the trout license revenue would substantially exceed the trout stocking costs for each year of the fishery. However, more intensive management of the trout fishery compared with other fisheries may increase cost.

Discussion

The overall objective of this study was to analyze the economic value of a trout fishery established in the Mountain Fork River below Broken Bow Dam. Economic evaluation of the fishery was conducted employing benefit-cost analysis for the three years of operation (1989-1991). Benefits were assumed equal to the surplus value (consumer surplus) anglers placed on the fishery. This value was estimated using the individual travel cost method. Costs of the fishery project included operating costs and opportunity costs. Operating costs were limited to the cost of trout stocking. Opportunity costs were identified as costs of cold water released from Broken Bow Lake and benefit loss from fishing activities prior to implementation of the fishery. Abundant unallocated water in the Broken Bow Lake allowed for the assumption of zero opportunity cost of cold water releases for the trout fishery. Information on the average number of trips taken before the trout fishing project in 1989 was used to estimate benefit loss. The benefit-cost ratios for the fishery excluding opportunity costs ranged from 13:1 to 16:1 and including opportunity costs ranged from 6:1 to 7:1. Seasonal variation in benefit-cost ratios were shown for 1990 and 1991.

[14]A fishing license is required of all persons with exemptions as specified in the 1992 Oklahoma Fishing Regulations (Oklahoma Department of Wildlife Conservation, 1992).

Conclusions

(1) Travel cost and time cost of trip affected the number of trips taken by anglers significantly and consistently throughout the three year period of analysis (1989-1991). This conclusion is based on the analytical results of the classical travel cost model and empirical significance of the estimated demand equations.

(2) The trout fishery has been widely accepted by residents in Oklahoma and frequent visitors from other states. Reasons to support this conclusion include the following:

(a) Annual number of trips has increased in each of the three years, with an estimated 11,075 trips in the last year.

(b) Over 70 percent of the sampled anglers in each of the years gave a quality index of 7 or more out of a scale of 1 to 10. Over 65 percent of the anglers sampled in 1989 stated that the trout fishery was adequate and should be maintained, and an additional 32 percent stated that the fishery was adequate but needed to be improved.

(c) The estimated one-way travel distance for the sampled anglers has increased each year of the project, implying that a wider population base is becoming aware of the fishery. Similarly, the average length of trip has increased each year, implying anglers are not only coming from greater distances but also staying longer each trip.

(3) Seasonal variation in composition of angler trips is significant. More trips are taken by local anglers (McCurtain County) in the winter season compared with the other seasons. In general, expenditure per angler trip is less in winter compared with the other seasons. The purpose of the trip is exclusively for trout fishing more in winter than other seasons. Finally, the estimated net benefits per trout harvested are lower in winter compared with other seasons.

(4) The trout fishery generated roughly $1 million dollars of angler net benefits for each of the three years, 1989-1991.

(5) Overall benefit-cost ratio implies that benefits of the trout fishery far exceed costs.

(6) Summary sample data show that about 33 percent of angler trips originated from residents in McCurtain County but only accounted for about 6 percent of angler expenditures. Conversely, 67 percent of the angler trips originated from residents outside of the county and accounted for 94 percent of total expenditures. A high proportion of total angler expenditures associated with the trout fishery originates with anglers from outside of the county, which provides for a potential significant impact on the local economy.

Management and Policy Decisions

Based on the above conclusions and on other results of the study, the following are suggested guidelines for management and policy decisions concerning the trout fishery.

(1) The benefit-cost analysis justifies strong consideration for continuing the fishery. Public acceptance, and associated attributed value, of the fishery is the basis for this proposed policy decision. If opportunity costs on water release should change or if anglers change their apparent value of angler trips, there should be a reevaluation of the trout fishery.

(2) Net angler benefits per trout harvested and variation in the seasonal benefit-cost ratios indicate change in stocking rates among seasons would increase net benefits of the trout fishery. In particular, it would enhance overall angler benefits if a higher proportion of trout were stocked during the spring and summer seasons compared with winter and fall.

(3) The trout fishery has been well received by the public as evidenced by results of a quality index ranking. However, size of trout is an important factor in the quality of the fishing trip, as assessed by anglers. Therefore, increasing the size of trout stocked or a portion of the stocking has potential for inducing more angler trips.

(4) The primary beneficiaries of the trout fishery are the anglers themselves. Therefore, the anglers should be assessed the major costs of operation of the fishery. Increasing the cost

of the trout license as costs of stocking increase is one way to ensure that anglers are paying in accordance with benefits received.

(5) Expenditure data indicate that anglers from outside the county account for a major part of total expenditures. Thus, county businesses and county population benefit from the trout fishery. A county sales tax would be one means of generating revenue to maintain the fishery and associated facilities such as access roads.

Limitations of the Study

(1) The current study accounts for only user benefits and excludes possible non-user benefits, which may lead to an underestimation of the total benefits of the trout fishery.

(2) Opportunity cost of water used from the Broken Bow Lake for the trout fishery was assumed to be zero, which may not be true in the future as demands for water increase or conflicts in timing of water use become important. Projection of demands and conflicts in timing of water use were not critically analyzed.

(3) Congestion at the river and capacity of the local economy to handle angler demands were not problems, however, such constraints were not measured. There may be critical areas where congestion or capacity constraints limit the ability of the trout fishery to handle increased demand. Such areas may include access roads, sanitary facilities at the fishery, or hotel lodging.

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Table 1. Estimated Seasonal Angler Hours and Trout Harvest for the First Three Years of the Mountain Fork River Trout Fishery[a], 1989-1991

Season	1989 Jan. 1, 1989 - Nov. 30, 1989			1990 Dec. 1, 1989 - Nov. 30, 1990			1991 Dec. 1, 1990 - Nov. 30, 1991		
	Angler Hours (no.)	Trout Harvest (no.)	Trout Harvest (no./hr.)	Angler Hours (no.)	Trout Harvest (no.)	Trout Harvest (no./hr.)	Angler Hours (no.)	Trout Harvest (no.)	Trout Harvest (no./hr.)
Winter (Dec. - Feb.)	11,493[b]	10,146	0.9	16,181	23,890	1.5	13,512	10,332	0.8
Spring (Mar. - May)	18,606	13,353	0.7	21,569	13,589	0.6	29,893	12,147	0.4
Summer (June - Aug.)	26,472	9,536	0.4	18,209	9,056	0.5	32,155	9,026	0.3
Fall (Sept. - Nov.)	11,520	6,905	0.6	12,686	7,701	0.6	16,688	7,113	0.4
TOTAL	68,091[c]	39,940[c]	0.6	68,645	54,236	0.8	92,248	38,618	0.4

[a]The source for these data are Oklahoma Department of Wildlife Conservation surveys.
[b]Two months (Jan. and Feb., 1989)
[c]Eleven months (Jan. - Nov., 1989)

Table 2. Seasonal Number of Fishing Trips per Angler to the Mountain Fork River Trout Fishery, 1989-1991

Number of Trips	1989[a] Percent	1990 (percent)				1991 (Percent)			
		Winter	Spring	Summer	Fall	Winter	Spring	Summer	Fall
1	16.6	21.1	58.1	48.4	53.2	25.0	50.6	48.1	61.3
2-5	30.6	30.5	24.4	33.0	26.6	26.3	25.9	27.2	30.0
6-10	18.5	16.8	5.8	8.8	5.3	22.5	8.6	4.9	5.0
11-15	10.8	11.6	3.5	4.4	4.3	20.0	8.6	8.6	2.5
16-20	1.3	5.3	2.3	1.1	3.2	3.8	3.7	2.5	0.0
>20	22.2	14.7	5.8	4.4	7.5	2.5	2.5	8.6	1.3
TOTAL	100.0	100.0	100.0	100.0	100.0	100.0	100.0	100.0	100.0
Average (No.)	15.15	11.80	4.61	4.87	5.09	8.21	5.89	5.94	2.74

[a]Annual average number of fishing trips to MFR for a sample of anglers.

Table 3. Frequency of Fishing Trips per Year to the Mountain Fork River Prior to Establishment of the Trout Fishery on January 1, 1989[a]

Number of Trips	Number of Respondents	Percent[b]
None	53	45.7
1-5	39	33.5
6-10	8	7.0
11-20	5	6.1
>20	7	7.7
TOTAL	112	100.0
Average (No.)	6.3	N.A.

[a]Limited to the 1989 survey.
[b]Weighted by zip code proportions in creel survey.

Table 4. Estimated Aggregate Angler Expenditures by Season for the Mountain Fork River Trout Fishery, 1989-1991

	1989	1990					1991				
		Winter	Spring	Summer	Fall	Annual	Winter	Spring	Summer	Fall	Annual
Angler Hours[a] (No.)	68,091	16,181	25,569	18,209	12,686	68,645	13,512	29,893	32,155	16,688	92,248
Average Number of Hours Fishing per Trip per Angler	8.13	5.01	9.70	10.15	10.35	8.04	5.82	8.25	9.30	10.01	8.32
Number of Angler Trips[b]	8,376	3,230	2,225	1,794	1,226	8,475	2,327	3,623	3,458	1,667	11,075
Expenditure per Angler Trip ($)	61.72	42.18	122.19	135.97	113.84	91.13	49.97	68.92	66.93	72.88	64.77
Aggregate Expenditure[c] ($1,000)	517	136	272	244	140	792	116	250	231	121	718

[a]Table 1.
[b]Angler hours divided by average number of hours fishing per trip per angler.
[c]Number of angler trips multiplied by expenditure per angler trip.

Table 5. Estimated Number of Trips to the Mountain Fork River Trout Fishery by Origin of Trip[a]

Year	Oklahoma			Texas	All Other States	Total
	McCurtain County	All Other Locations	Total Oklahoma			
1989	4,723	1,366	6,089	2,151	136	8,376
Winter, 1990	1,978	586	2,565	630	35	3,230
Spring, 1990	885	545	1,430	766	28	2,224
Summer, 1990	577	476	1,053	719	23	1,794
Fall, 1990	351	260	611	585	30	1,226
TOTAL 1990	3,791	1,867	5,659	2,700	116	8,475
Winter, 1991	1,261	364	1,625	673	29	2,327
Spring, 1991	1,368	871	2,239	1,278	106	3,623
Summer, 1991	850	1,032	1,882	1,551	25	3,458
Fall, 1991	287	518	805	856	6	1,667
TOTAL 1991	3,766	2,785	6,551	4,358	166	11,075

[a]An example of computations for estimated number of trips is given in Choi (Appendix B).

Table 6. Estimated Consumer Surplus (Net Angler Benefits) of the Mountain Fork River Trout Fishery by Origin of Trip ($1,000)[a]

Year	Oklahoma			Texas	All Other States	Total
	McCurtain County	All Other Locations	Total Oklahoma			
1989	513	234	747	231	31	1,009
Winter, 1990	181	37	218	19	3	240
Spring, 1990	96	88	184	122	19	325
Summer, 1990	116	54	170	84	3	257
Fall, 1990	58	18	76	58	9	143
TOTAL 1990	451	197	648	283	34	965
Winter, 1991	117	18	135	17	9	161
Spring, 1991	213	101	315	119	16	449
Summer, 1991	105	95	201	108	17	326
Fall, 1991	30	46	76	109	4	189
TOTAL 1991	466	261	726	353	46	1,126

[a]Value for 1989 and 1990 were inflated to 1991 dollars using the Consumer price Index. An example of computations for estimated net angler benefits is given in Choi (Appendix C).

Table 7. Consumer Surplus per Trout Harvested and per Hour of Fishing at the Mountain Fork River Trout Fishery[a]		
Year	Per Trout Harvested ($)	Per Hour of Fishing ($)
1989	25.25	14.81
Winter, 1990	10.06	14.86
Spring, 1990	23.89	15.05
Summer, 1990	28.37	14.11
Fall, 1990	18.55	11.26
1990 Overall	17.79	14.06
Winter, 1991	15.51	11.93
Spring, 1991	36.97	15.02
Summer, 1991	36.10	10.13
Fall, 1991	26.62	11.34
1991 Overall	29.10	12.20

[a]Values for 1989 and 1990 were inflated to 1991 dollars using the Consumer Price Index.

Table 8.	**Comparison of Estimated Consumer Surplus from Various Cold Water Fishing Studies**			
Source (Author)	Year Studied	Location	Reported Value Per Fishing Day ($)	Adjusted Value Per Fishing Day ($)
Gum and Martin	1970	Arizona	10.15	40.98
Vaughan and Russell	1979	U.S.	19.49	36.58
USFWS	1980	Idaho	12.93	21.38
Weithman and Hass	1982	Missouri	15.67	29.41
Present Study	1991	Oklahoma	38.64	38.64
Source: Sorg and Loomis				

Table 9. Benefits and Costs of the Mountain Fork River Trout Fishery, 1989-1991					
Year	Benefits[a] ($)	Costs		Benefit-Cost Ratio	
		Operation[b] ($)	Opportunity Costs[c] ($)	Excluding Opportunity Costs	Including Opportunity Costs
1989	1,009,000	72,951	85,036	13.8	6.4
Winter, 1990	240,000	17,994		13.3	
Spring, 1990	325,000	17,994		18.1	
Summer, 1990	257,000	17,994		14.3	
Fall, 1990	143,000	17,994		7.9	
TOTAL 1990	965,000	71,976	89,630	13.4	6.0
Winter, 1991	161,000	17,267		9.3	
Spring, 1991	449,000	17,267		26.0	
Summer, 1991	326,000	17,267		18.9	
Fall, 1991	189,000	17,267		10.9	
TOTAL 1991	1,126,000	69,068	93,402	16.3	6.9

[a]From Table 6.
[b]Cost of trout stocking adjusted to the 1991 price level. Trout license revenue was assumed ≥ management costs of fishery.
[c]Opportunity costs represent value of fishing days prior to 1989 (see text). costs are in 1991 price level. Seasonal information not available.
[d]11 months.

APPENDIX A
ESTIMATED DEMAND FUNCTIONS FOR THE
MOUNTAIN FORK RIVER TROUT FISHERY

Year	Independent Variables[a] Constant	TCFt	TCOt	SUB	Y	A	AGE	SEX	Adj.R^2	Ep[b]
1989[c]	0.6018	-0.0045	-0.0125	-0.0001	0.0040	0.0432	0.0184	0.4995	0.22	-0.37
	(0.95)	(-3.10)	(-2.26)	(-0.25)	(0.82)	(0.80)	(2.19)	(1.80)		
1990[d]										
Winter										
	1.3367	-0.0091	-0.0155	-0.0020	0.0152	0.0768			0.31	-0.48
	(2.89)	(-4.26)	(-3.60)	(-2.32)	(2.39)	(1.50)				
Spring										
	0.7990	-0.0016	-0.0022	0.0012	-0.0011	0.0072			0.32	-0.84
	(3.13)	(-5.56)	(-2.82)	(1.92)	(-1.76)	(0.28)				
Summer										
	1.4511	-0.0018	-0.0043	-2E-05	-0.0058	-0.0117			0.37	-0.52
	(4.62)	(-5.45)	(-3.66)	(-0.11)	(-2.97)	(-0.46)				
Fall										
	0.9691	-0.0031	-0.0028	0.0008	0.0011	0.0122			0.38	-0.47
	(4.20)	(-4.53)	(-4.39)	(1.62)	(0.57)	(0.48)				
1991[e]										
Winter										
	2.3048	-0.0095	-0.0050		-0.0083	0.0232			0.48	-0.57
	(4.55)	(-5.77)	(-1.09)		(-1.50)	(0.43)				
Spring										
	1.8751	-0.0054	-0.0051		0.0003	-0.0615			0.25	-0.74
	(3.44)	(-4.62)	(-2.12)		(0.07)	(-1.16)				
Summer										
	1.0026	-0.0084	-0.0057		0.0090	0.0330			0.48	-1.53
	(2.01)	(-6.77)	(-5.13)		(2.43)	(0.59)				
Fall										
	0.4122	-0.0020	-0.0006		-0.0042	0.0494			0.21	-0.34
	(2.01)	(-2.55)	(-0.70)		(-1.53)	(2.31)				

[a] The data in parentheses are t statistics. The dependent variable is number of trips. Independent variables are trip monetary and time cost of the trout fishing activity in current dollars (TCF_t), trip monetary and time cost of all other recreational activities in current dollars (TCOt), substitute or complement effect (SUB), annual household income in thousands of current dollars (Y), quality of trip (A), age of angler in years (AGE), and sex of angler (SEX=1 for male and 0 for female).

[b] Ep represents the own price elasticity measured at mean level for each variable.

[c] Only annual data were available for 1989.

[d] Data were not available for AGE and SEX.

[e] Data were not available for SUB, AGE, and SEX.

APPENDIX B

ESTIMATED DEMAND CURVES FOR ANGLERS OF THE MOUNTAIN FORK RIVER TROUT FISHERY[a]

1989[b]

$Q = f(TCF_t, TCO_t, SUB, Y, A, AGE, SEX)$

$\ln Q = 2.0974 - 0.0045015\ TCF_t$

1990[c]

$Q = f(TCF_t, TCO_t, SUB, Y, A)$

Winter	$\ln Q$	=	$2.1907 - 0.0090624\ TCF_t$
Spring	$\ln Q$	=	$0.8045 - 0.0015624\ TCF_t$
Summer	$\ln Q$	=	$1.0201 - 0.0017607\ TCF_t$
Fall	$\ln Q$	=	$1.0681 - 0.0031368\ TCF_t$

1991[d]

$Q = f(TCF_t, TCO_t, Y, A)$

Winter	$\ln Q$	=	$2.1468 - 0.0094606\ TCF_t$
Spring	$\ln Q$	=	$1.2642 - 0.0054448\ TCF_t$
Summer	$\ln Q$	=	$1.4123 - 0.0084378\ TCF_t$
Fall	$\ln Q$	=	$0.6423 - 0.0020242\ TCF_t$

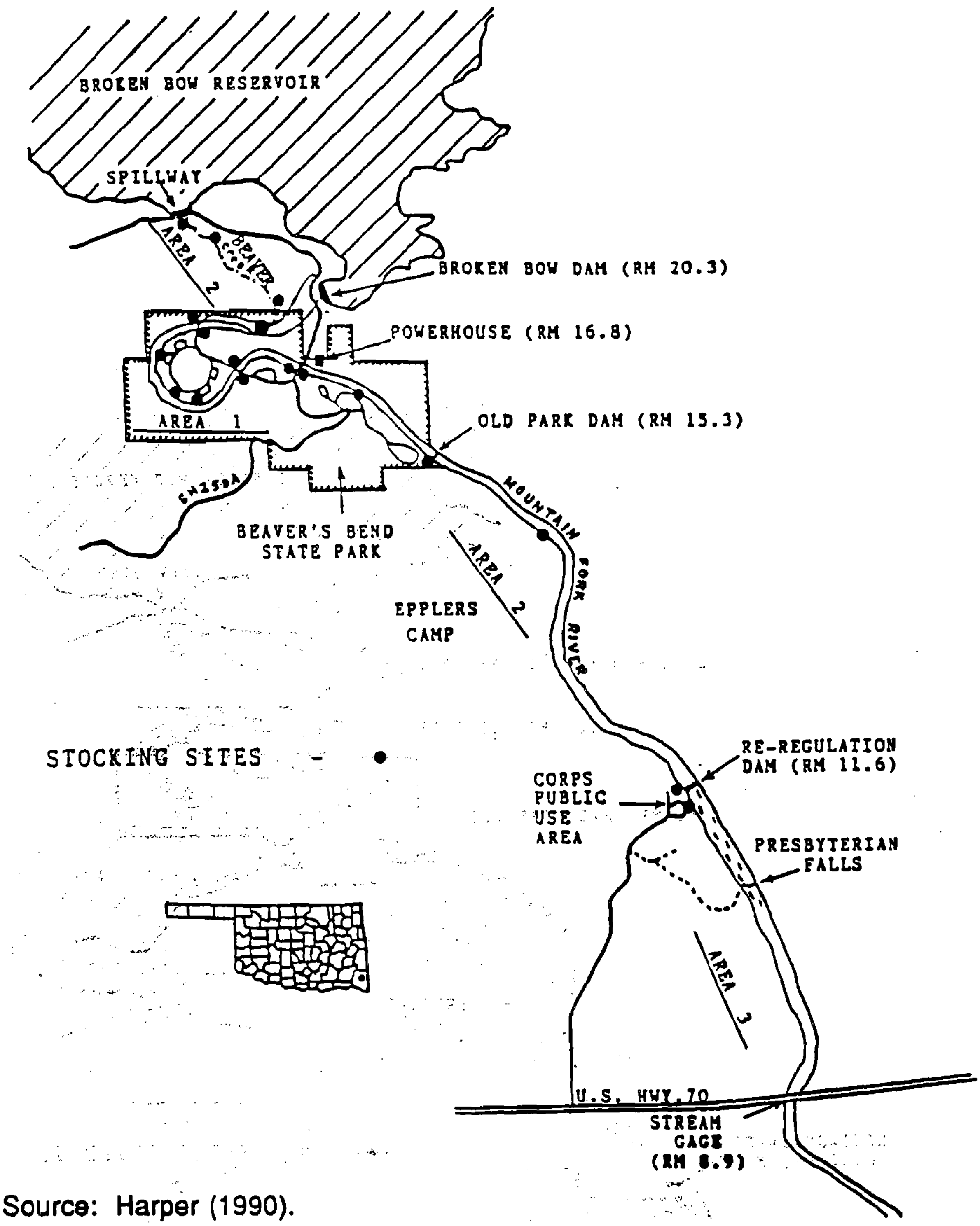
[a] Except for the price variable (TCF_t), mean values of the other explanatory variables were multiplied by corresponding coefficients and added to the intercept term of the demand function. Mean values of the variables are:

	Q	TCF_t	TCO_t	SUB	Y	A	AGE	SEX
1989	13.262	81.716	19.193	83.889	39.754	8.0654	46.542	0.7757
1990								
Winter	11.894	52.676	9.625	54.390	35.014	7.8118		
Spring	3.767	150.43	21.717	40.660	39.199	7.0000		
Summer	5.308	119.70	27.569	98.530	37.526	7.8333		
Fall	4.623	100.74	34.751	69.028	42.661	7.6494		
1991								
Winter	8.468	60.684	5.466		39.548	8.4935		
Spring	5.589	81.888	17.944		42.048	8.6575		
Summer	5.431	64.550	35.659		36.676	8.5278		
Fall	2.276	95.127	20.648		41.395	8.3947		

[b] Only annual data were available.

[c] Data for AGE and SEX were not available for 1990.

[d] Data for SUB, AGE and SEX were not available for 1991.



Source: Harper (1990).

Figure 1. Mountain Fork River Trout Stream Showing Trout Stocking Sites During 1989

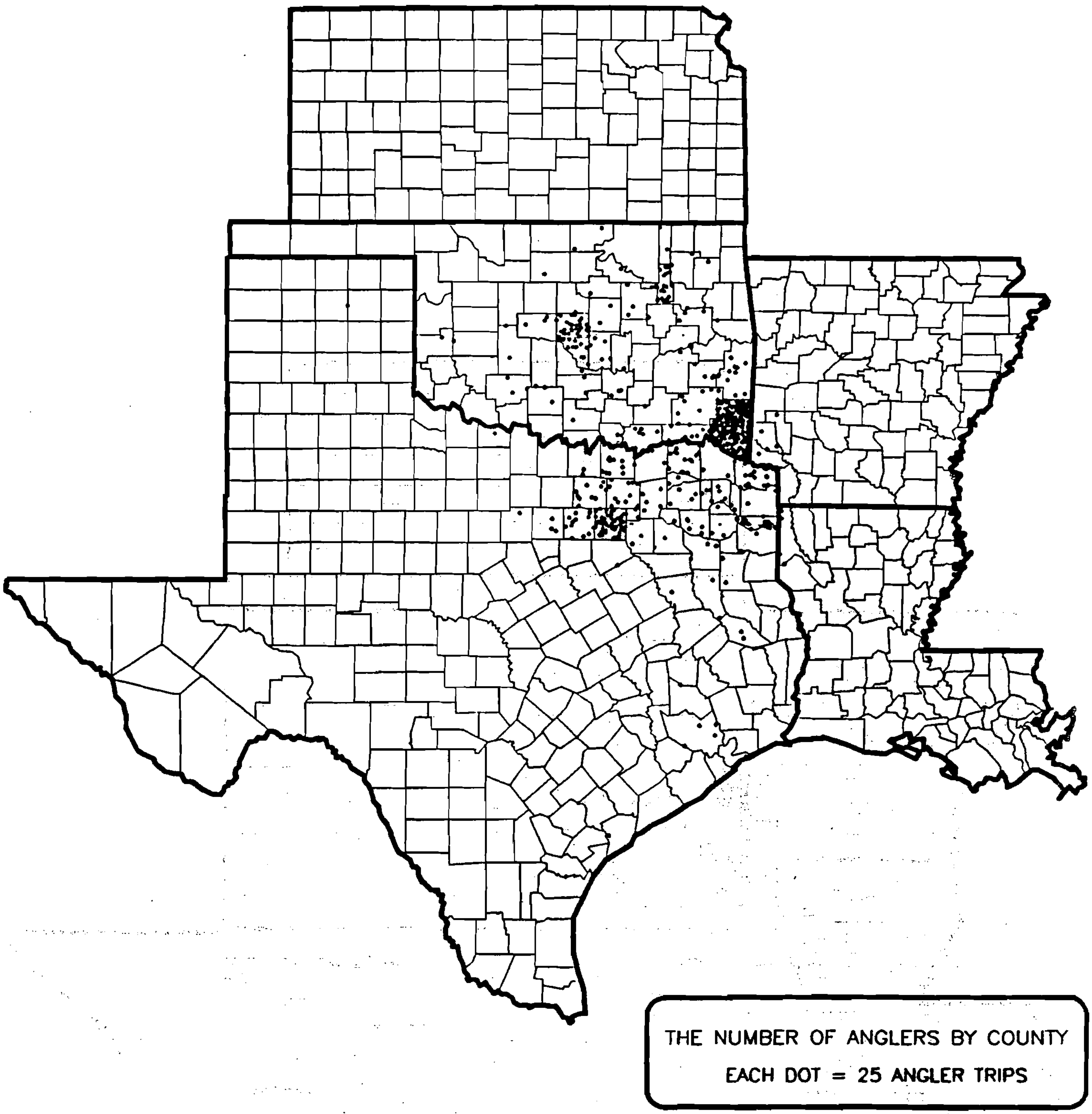


Figure 2. The Estimated Number of Angler Trips to the Mountain Fork River Trout Fishery Based on the Creel Survey, 1990-1991.

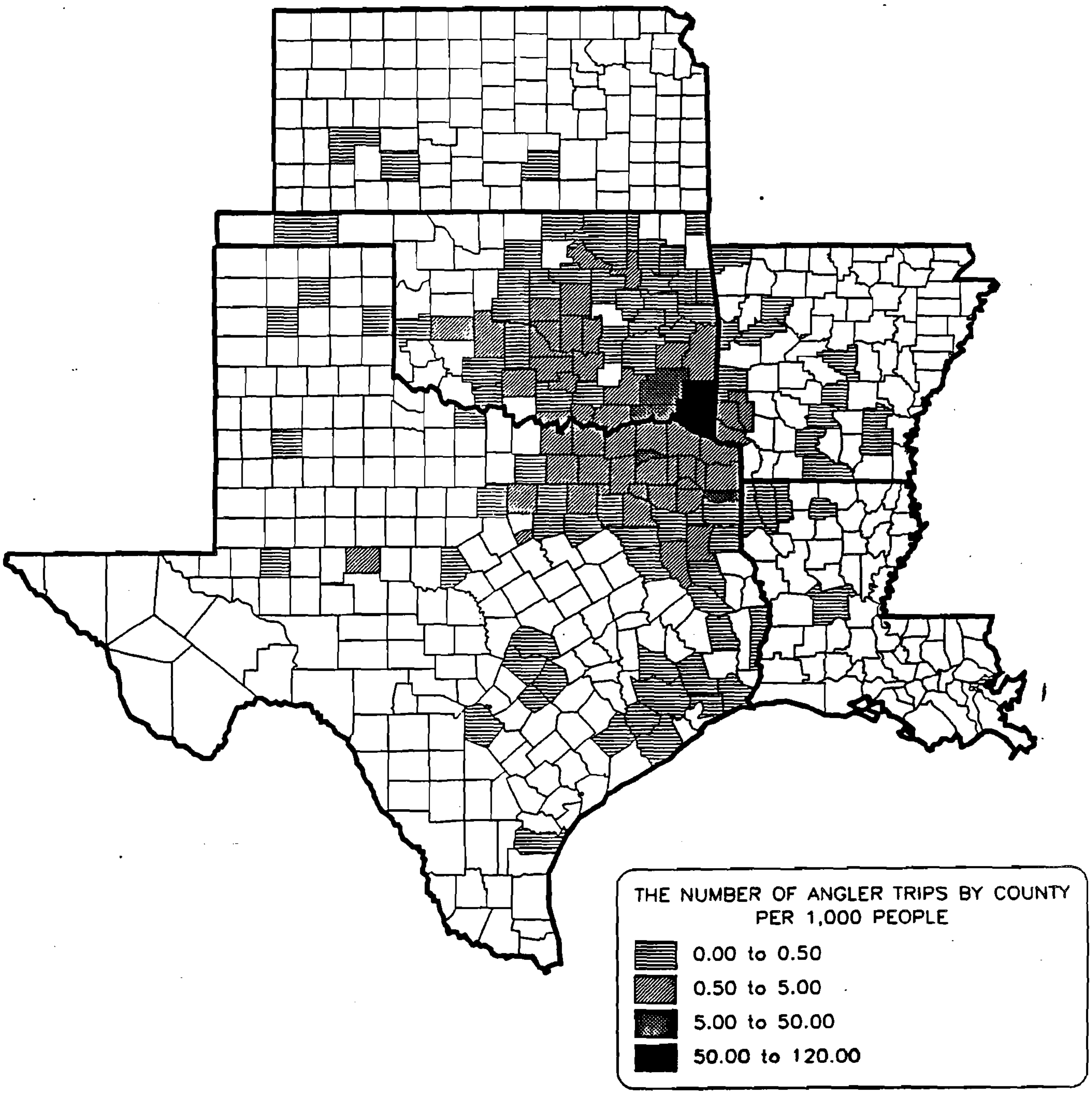


Figure 3. The Estimated Number of Angler Trips to the Mountain Fork River Trout Fishery Per 1,000 County Population Based on the Creel Survey, 1990-1991

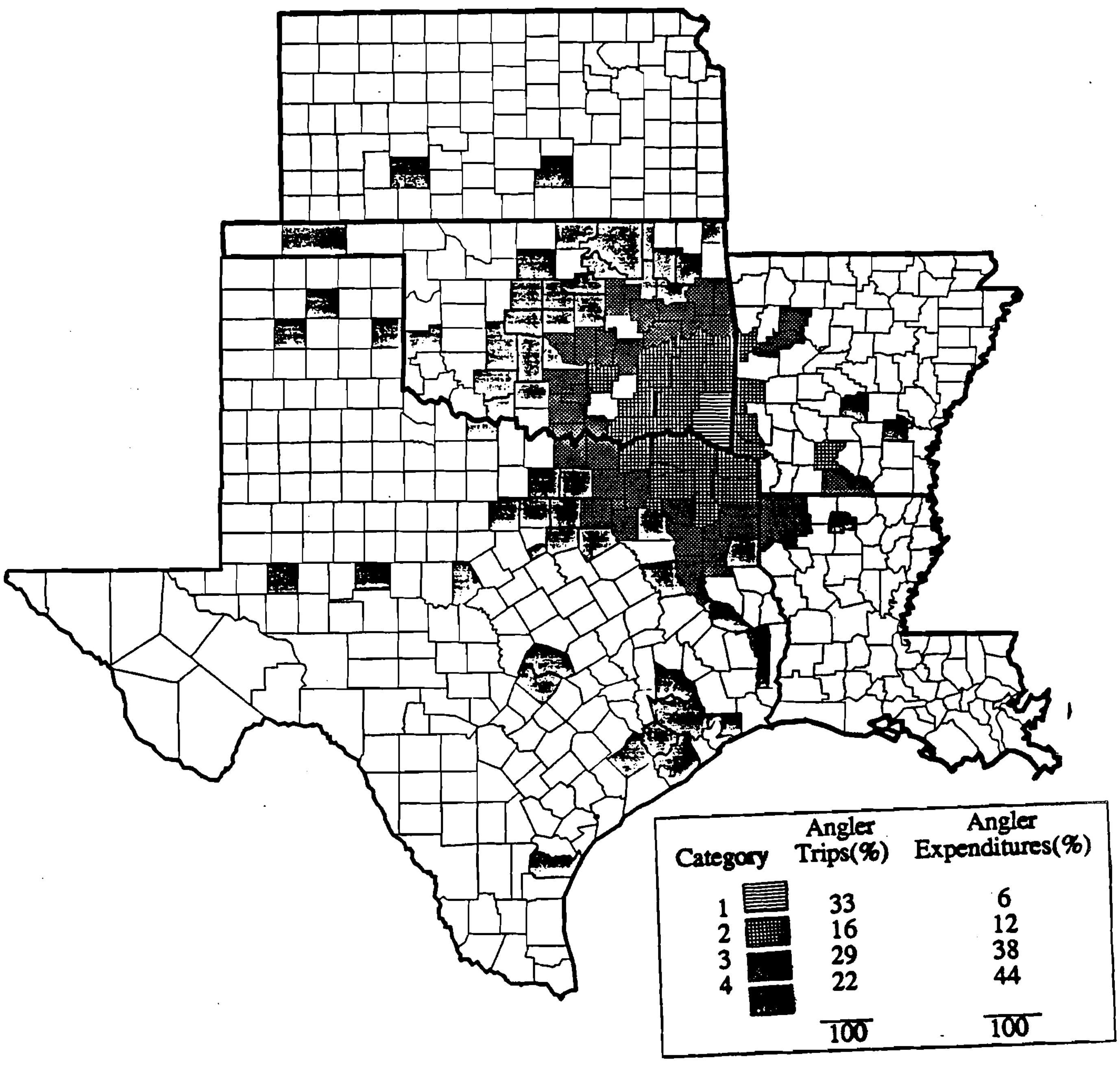


Figure 4. County Distribution of Total Angler Trips and Angler Expenditures for the Mountain Fork River Trout Fishery, 1991

Toward Measuring Use and Nonuse Values From Related Market Behavior: Some Preliminary Results

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Toward Measuring Use and Nonuse Values From Related Market Behavior: Some Preliminary Results

Interest in methods for determining the values of nonmarketed amenities is increasing, in response to the growing array of public policy questions which involve tradeoffs between marketed and non-marketed goods. Both in the course of benefit-cost analysis and in assessing environmental damages under CERCLA or the Oil Pollution Act of 1990, there is often potential for individuals to hold nonuse or "passive use" values associated with environmental quality changes, in addition to benefits they receive as users of the amenity in question. This has resulted in a discussion about the possibilities for measuring nonuse value, and the suitability of different empirical approaches for doing so.

This discussion is characterized by several dichotomies. First is the distinction among the types of affected individuals when there is a change in an environmental amenity. People who have some direct contact with the environmental amenity, through hunting, fishing, viewing, etc., are termed "users," while those with no such direct contact are termed "nonusers."

Second is the type of value which may be received, or lost, by changes in the amenity. The fundamental measure of the change in individual welfare is the difference in the minimum expenditure function required to achieve a given level of satisfaction, associated with the reference and subsequent levels of the amenity. For users, this change in the expenditure function can be expressed as the sum of two terms which are useful pedagogically: "use value," or the change in areas behind (Hicksian) demand functions for private market goods, which may change as a result of a change in the level of the amenity, and "nonuse value," which is the part of the total value not directly connected with use, or changes in private market good demands. In contrast, for nonusers, the entire value of the amenity change is nonuse value, since by definition they have no direct contact with the environmental amenity and, therefore, no use value. These individuals may derive a benefit from just knowing that an environmental amenity is protected and available for use by others, in the sense described by Krutilla.

The third dichotomy concerns which empirical approach is to be used in measuring the values of changes in environmental amenities. Economists have a long tradition, when measuring the net economic values of privately-marketed goods, of relying on and making inferences from observed individual behavior, in the form of demand functions for the goods in question. For public goods, which frequently are not marketed directly and which, therefore, have no easily-observed price, the issue is not so simple. One immediate limitation, in many situations where the value of an environmental amenity is needed, is that there simply are not adequate data to implement the behavioral approach, because of missing prices, no data on peoples' responses to variations in the level of the amenity, or other factors. This has stimulated development of the direct questioning, or contingent valuation, approach (see, e.g., Mitchell and Carson). Thus the dichotomy in methods of measurement concerns whether the researcher should (or can) watch what people do, or listen to what they say.

One approach which is well-accepted is to exploit, where possible, the relationships between the non-marketed public good and private goods, whose demands may change when the public good changes. As noted above, the changes in the areas to the left of these private goods demand curves is use value. However, only under special conditions (weak complementarity of the private goods with the environmental amenity) is the use value equivalent to the total value of the amenity. The other part, nonuse value, has not been thought to be directly measurable from the demands for private goods.

A central purpose of this paper is to argue, and demonstrate, that nonuse value can, in fact, be measured from demand systems, as a consequence of the specification of the demand system chosen by the researcher for the purpose of measuring use value. When all goods identified by the researcher as sources of use value are not being consumed, any change in value to the individual must necessarily be nonuse value. Thus, the researcher's specification implies a condition on the demand system which enables measurement of the total value of the amenity change (use plus nonuse value), without assuming that nonuse value is zero. This is referred to as "weak neutrality," a condition on the Hicksian composite commodity which must hold at a single price vector, the choke price vector which sets consumption of all the use-value generating goods at zero. This is a weak version of Hicks-neutrality, a term introduced into the nonmarket valuation literature by Neill and developed and analyzed by Larson (1992).

The idea is that empirical specifications of demand systems for private goods which are the source of use value have direct implications for the measurement of nonuse value. If the empirical demand system is viewed as a valid means of estimating use value, then when the price vector is such that none of the private goods are consumed, the adding up conditions required by economic theory can be used to determine the slope of the minimum expenditure function at that point. This serves as an initial condition to identify how preferences must change with quality and with price, which is sufficient to determine the total value of a quality change (i.e., the difference in the minimum expenditure function evaluated at the new and old quality levels). We present empirical results from some initial explorations of the implications of weak neutrality on a data set on people who go whale-watching, and therefore are users of the gray whale population resource.

The Empirical Setting

Gray whales are a prominent marine population off the west coast of the United States, and their annual migration from summer grounds in the Bering Sea to Baja California for winter calving and rearing is well-documented and well defined in time and space. Particularly on the southward leg of the migration, which is typically nearer shore, the gray whales attract a substantial interest in whale viewing. There are both opportunities for viewing at prominent California shore locations such as Point Reyes and Point Loma, and a significant industry involving boats which take viewers on gray whale viewing trips, from December to March of each season.

The survey instrument was developed to allow collection of data to support travel cost demand modelling. Thus questions on number of trips so far that season, expected future trips, travel time, travel costs, whether the trip was their primary destination, etc., were asked. Other information included actual contributions to marine mammal groups, time spent reading, watching, or thinking about wildlife and whales, as well as purchases of whale-related merchandise. Lastly, demographic information including work status, wage rates, income, paid vacation time, and when people choose to go whalewatching, was asked. The survey was presented in booklet form. A draft of the survey instrument was pretested using individuals who had gone whale watching in the previous year. A survey was given or mailed to them and they were asked to fill it out and make additional comments on the questions and format.

Visitor Sample Frame

The visitor sample frame is defined by both time and space. We sampled over the months of the gray whale migration along the California coast. Whales start arriving in northern California in late December and migrate south to San Diego and Baja California during January. Then in February whales begin to migrate back north, passing through northern California by late March or early April. Many people taking trips to the coast during these months are out to view whales rather than for traditional summertime ocean-related activities. To be cost-effective in sampling, we sampled weekends and holidays at the locations described below. The choice of whether to sample on Saturday or Sunday was random, which allowed for a balance of Saturdays and Sundays to be represented.

The visitor intercept survey took place at four locations along the California coast: San Diego (Point Loma National Seashore), Monterey, Half Moon Bay (south of San Francisco), and Point Reyes National Seashore (north of San Francisco). At Point Loma and Point Reyes people intercepted were viewing whales from the shore, whereas people intercepted at Monterey and Half Moon Bay had just disembarked from 2-6 hour cruises being run at that time of the year by commercial operators specifically for whale watching. This choice of sampling sites enabled us to sample people using both modes (shore watching and boat trips) of whale viewing.

Every n^{th} adult visitor (age 16 or older) was contacted by one of our trained interviewers. For boat trips n equaled 5 and for shore intercepts n equaled 10. Interviewers were dressed in University of California jackets with "Whale Watching Survey" in large letters on the back, and also wore University of California name tags identifying them. The interviewer introduced him- or herself to the visitor, explained the purpose of the survey, answered any questions about the project or mechanics of completing the survey, and asked the visitor to take home a self-explanatory packet which included a mail survey to be returned. Interviewers also recorded names and addresses of visitors contacted for follow-up mailings if necessary. The take-home packet included a cover letter, the questionnaire, and a postage-paid return envelope.

We chose this survey administration mode because it combined the best of personal interview and mail questionnaire techniques. The personal contact allowed the interviewer to stress the importance of the respondent to the representativeness and completeness of the study, and to obtain a "good faith" commitment to return the survey. The interviewer was able to answer any concerns the visitor had about the survey or how they were selected to participate. By giving them the survey after they finished the trip (rather than mailing it to them) they could record their trip details while on the drive back home or shortly after the trip.

However, we did not think it desirable to conduct personal interviews at the intercept site itself, for several reasons. First, weather at the coast in the winter is not always conducive to lengthy outdoor interviews, and weather-proof shelter for interviewing was not available at the interview sites. Also, some passengers disembarking from boat trips, particularly at Half Moon Bay, were not in good physical or mental condition to answer detailed questions about their trip. By giving the questionnaire to the visitor after a brief explanation of its purpose and their importance to the study, they could devote adequate thought to answering the variety of questions concerning their activities and their valuations of whale-related recreation at their own pace.

In total, 1,402 surveys were handed out, and 1,003 were returned, for an overall response rate of 71.3%. The response rate was reasonably similar across the four locations, varying from a low of 65.2% for intercepts at Point Loma (San Diego) to a high of 80.3% for intercepts at Point Reyes. On-site refusals were not a problem. For example, at Point Reyes, only 10 people of roughly 600 contacted (about 1.6%) refused to take a survey packet. The analysis in this paper focuses on the 236 respondents who indicated that their trip was for the primary purpose of watching whales (as opposed to being an incidental trip) and who provided complete information on time and money prices and budgets.

Measuring Nonuse Value from Demand Systems Under Weak Neutrality

It should be emphasized at the outset that this paper focuses on measuring the total value of environmental amenity changes to *users* of those amenities; that is, the focus is on people who clearly have a "behavioral trail" with respect to the amenity that can be identified and analyzed.[1] An example would be recreational use of an amenity whose character or quality changes, such as viewing scenic vistas at the Grand Canyon or at Yosemite National Park as air quality changes, or fishing, boating, or swimming as water quality changes. The term "users," in this context, refers to people who engage in recreational use which is related to an environmental amenity, and which changes as the level of the amenity changes.

The choice problem frequently used in discussions of use and nonuse value posits a consumer who values an exogenous environmental amenity, represented by the quality variable z. There are n market goods denoted

[1]This is not to suggest that nonusers, or those who do not visit a recreational site, have no value associated with the amenity change; while they may (or may not) value the amenity change, it is more difficult to identify relevant behaviors for which a demand relationship can be estimated. Larson (1993) argues that all amenities valued by an individual induce some behavior, whether in the form of purchases of marketed goods or in uses of time, which can in principle be measured by tracking the behavioral changes as an amenity changes. In practice, though, identifying such "behavioral trails" is likely to be challenging in many circumstances.

by $x=(x_1,...,x_n)$ with corresponding prices $p=(p_1,...,p_n)$, and the consumer is presumed to choose market goods in a way that minimizes the cost of utility, represented by the choice problem

(1) $$\min_x \; px \quad s.t. \; u_0 = u(x,z)$$

In practice, the researcher estimates an m good incomplete demand system (where m<n) and, assuming weak integrability (LaFrance and Hanemann), aggregates all other goods into a composite commodity $x_c \equiv y - \sum_{i=1}^{m} p_i x_i$ with unit price. The solution to (1) is the Hicksian demands $x^h(p,z,u)$, which when substituted into (1) yield the minimum expenditure function e(p,z,u). The issue of concern is how the consumer's valuation of a change in z from an initial level z_0 to a subsequent level z_1, which is defined as

(2) $$TV(z_0, z_1) \equiv e(p,z_0,u_0) - e(p,z_1,u_0).$$

It has been noted by LaFrance and Hanemann that for any non-price variable, such as the quality variable z, there is a limit to the information that can be recovered from observable behavior (i.e., from demand functions) about the curvature of the expenditure function in that variable. This point has recently been reiterated forcefully by LaFrance, who questions the validity of the relatively common practice of assuming weak complementarity (Mäler)[2] in measuring welfare for environmental quality changes.

A natural response to this concern is that weak complementarity is a suitable assumption, and should be invoked, under some circumstances. In particular, it is suitable when the researcher expects that all of the value of the amenity in question is directly related to consumption of a set of related private goods. The standard example in the recreation demand context is valuing local environmental amenities with plentiful good substitutes, such as water quality at specific lakes which support recreational uses of various types. In such situations, it may be very plausible that only those who travel to the lake to use its recreational services will care about changes

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[2]If an exogenous public good is weakly complementary to a set of private goods, when the private goods are not being consumed there is no change in the minimum expenditure function when the public good changes. this implies that the individual experiences no change in welfare when the public good changes, if all the weakly complementary private goods are not being consumed.

in the lake's water quality. Thus the researcher invokes a prior belief about the scope of values associated with lake water quality in the process of valuing a change in lake water quality. This is not unlike the many other judgments which necessarily inform any empirical analysis.

This paper reports on the use of another "prior" about the way that quality-type variables enter the individual's preferences function, referred to as weak neutrality. This, it is argued, is a plausible assumption in a wider set of circumstances than is the standard weak complementarity assumption, in that it follows directly from the specification of demands systems which involve quality-type variables, and because of the fact that it nests the principle involved with weak complementarity. It is possible in principle to test, within the framework of weakly neutral preferences, whether or not all the value associated with a quality change is in fact tied to use of a set of private market goods.

The main purpose of valuing quality changes is to come up with estimates of the "total" value of the quality change for an individual, given in (2). It turns out to be useful both pedagogically and in making calculations based on the weak neutrality restriction to use a standard way (e.g., McConnell) to express the total value as the sum of use value and nonuse value. For a single private good x (like travel to a recreation site) whose demand depends on the quality variable z, the total value can be written as

$$(3)\qquad TV(z_0,z_1) = \left[\int_{p_0}^{\hat{p}_1} \frac{\partial e(t,z_1,u)}{\partial t}dt - \int_{p_0}^{\hat{p}_0} \frac{\partial e(t,z_0,u)}{\partial t}dt\right] + \int_{z_1}^{z_0} \frac{\partial e(\hat{p}_1,t,u)}{\partial t}dt$$
$$= \{UV(z_0,z_1)\} + NUV(z_0,z_1)$$

where use value, $UV(z_0,z_1)$, is the difference in two price integrals of the Hicksian demand, one conditioned on subsequent quality level z_1 and the other conditioned on the initial level z_1, as price varies from the initial level p_0 to the level $\hat{p}_i$, i=0,1, that chokes off demand to zero before and after the quality change. Each of the integrals over price is sometimes referred to as an "access value;" if, for instance, the private good x is travel to a recreation site, the integral over price is a compensating variation measure of the maximum willingness to pay for travel to the site. Thus use value is the change in access value as the quality variable affects the (Hicksian) demand for travel. Intuitively, use value is generated because the Hicksian demands of the private goods shift as quality changes. This logic just described for a single good which generates use value easily extends to the measurement of use value when multiple private goods are related to quality (e.g., Bockstael and Kling).

The remaining term, $NUV(z_0,z_1)$, is termed nonuse value, since it can be seen as the integral taken over the quality variable as quality changes, when the private good is not being consumed (since price is at the choke level $\hat{p}$). This leads to a natural interpretation which motivates the weak neutrality condition:

When the set of goods designated by the researcher as generators of use value and included in the empirical demand system are not being consumed, all change in $TV(z_0, z_1)$ is nonuse value.

This implies a condition on the Hicksian composite commodity which augments the empirical demand system $x_c^h(p,z,u)$ which must hold at one specific

rice (vector). At the price vector $\hat{p}$ that chokes off demand for all of the use value-generating goods ("use goods," for short), it must be the case that

(4) $$\frac{\partial x_c^h(\hat{p},z,u)}{\partial z}=0$$

though this need not hold at any other price vector at which one or more of the use goods are being consumed. However, the fact that the condition in (4) holds at a single price vector is sufficient to provide the additional information needed to determine the total value of a quality change from an empirical demand system. This total value will, in general, consist of both use and nonuse value.

Why does (4) follow from the researcher's specification of the empirical demand system? At the choke price vector, since all of the private goods which generate use value are not being consumed, there is no use value as the environmental amenity changes. The remaining good being consumed, $x_c^h(\hat{p},z,u)$, must not shift with quality, for if it did, a use value would be generated, which is a contradiction.

The calculation of the total value of a quality change in practice can be usefully described in terms of the 3-step heuristic proposed by Mäler (pp. 185) and expanded upon by Bockstael and Kling. First, in turn each of the prices of the m private goods is raised to the choke level, thereby reducing consumption to zero. Each of these price increases traces out a compensating variation which represents the willingness to pay for access to the private good, given the reference level of environmental quality. The series of sequential price changes ends at the choke price vector where none of the m private goods is being consumed; at this point, the initial "access value" is the sum of compensating variations for availability of the m private goods at their original prices, given the initial level of z, z_0.

The second step of the Mäler heuristic is to then change z from z_0 to z_1, allowing all the choke prices to adjust in the process so that consumption of the m private goods remains at zero. At this point (i.e., at the choke price vector), Mäler invoked the so-called "weak complementarity" condition, which ensured that there was no change in value (i.e., in the expenditure function) as quality changed. This, in effect, is a statement that nonuse value is zero. The weak complementarity condition has been often used in empirical studies valuing quality changes from demand systems; it says the total value of the quality change is simply use value.

Weak complementarity is but one of many assumptions about preferences that could be used. It is plausible in settings where it is reasonable to expect there is no nonuse value, such as valuing water quality changes at recreation sites with local appeal and a plentiful supply of good substitutes. As has been noted by several authors, it is not a tenable assumption in all cases.

The alternative implemented in this paper, weak neutrality, says something more general: it says that at the choke price vector, the change in the expenditure function as quality changes is *solely nonuse value*, not zero.

As noted above, this follows from (3), given the researcher's specification of the demand system. It leads to an observable expression for marginal nonuse value from the adding up conditions and the identity relating Hicksian demand to the structure of Marshallian demands as income varies to keep utility constant. The empirical estimate of marginal nonuse value, μ, for an m-good empirical demand system is [see Larson 1992, equations (6) and (11)]

$$\mu = \frac{-\sum_{i=1}^{m} \hat{p}_i \partial x_i / \partial z}{1 - \sum_{i=1}^{m} \hat{p}_i \partial x_i / \partial y}, \tag{5}$$

where $\hat{p}_i$ is the choke price for good i and $\partial x_i / \partial z$ and $\partial x_i / \partial y$ are the quality and income slopes of the observable, Marshallian demand function for good i. Whether nonuse value is in fact zero is an empirical question, and can be tested. If the restriction that μ=0 is not rejected, weak complementarity results. If not, the total nonuse value can be calculated by numerically integrating (5) over the range of the quality change.

The third step of the Mäler procedure is to sequentially lower prices from their choke levels to their original levels, given that demands are now all conditioned on the new level of quality. The summed areas to the left of the Hicksian demands over these price ranges is the subsequent "access value," conditioned on the new level of z; the difference between this and the initial access value is use value. The desired estimate of the total value of the quality change is then obtained by summing the calculated use and nonuse values.

Issues in Empirical Implementation

Flexibility of the Demand System

One of the issues that arises in implementing the weak neutrality condition in (4) or (5) is that flexibility is needed to allow both use and nonuse values to vary with individual characteristics. While an estimate of total value of the amenity change is the primary goal, when using demand systems for this purpose it is necessary to derive an estimate of nonuse value and add that to the use value calculated from the demand system. One would wish to have the most flexible form possible for nonuse value so that it can take either positive or negative sign, and can vary with individual characteristics. This is particularly true when the empirical demand system is small, e.g., when a single equation is estimated as in most empirical studies which measure use value and in the present application. When m=1, the marginal nonuse value is

$$\mu = \frac{-\hat{p}_1 \partial x_1/\partial z}{1 - \hat{p}_1 \partial x_1/\partial y} \tag{6}$$

and with the common single equation functional forms that are either linear or linear in logs, the marginal nonuse value does not meet this criterion. Another implication of a single equation demand system for use value, from (6), is that for marginal nonuse value to be positive ($\mu>0$), the Hicksian quality slope must be negative. This can better be seen by writing (6) in elasticity form, as

$$\mu = \frac{-\hat{w}_1 \eta_z Y/z}{1 - \hat{w}_1 \eta_y} \tag{7}$$

where $\hat{w}_1$ is the budget share of good 1 at the choke level (when consumption is one unit and price is at the level that makes the individual between consuming and not consuming the first unit), and η_z and η_y are the elasticities of demand with respect to the environmental amenity and income, respectively. Since the denominator of (7) and the budget share, Y, and z are all positive, or μ to be positive requires $\eta_z<0$. This suggests the desirability of a functional form for demand that approximates slopes and elasticities well, and which can allow the estimated elasticities to be nonmonotonic with respect to changes in the environmental amenity and other characteristics.

A Fourier Flexible Recreation Demand Model

One approach to demand estimation which offers substantial flexibility in estimating demand slopes and elasticities is the Fourier flexible form (Gallant 1981, 1984; Chalfant; Wohlgenant). The Fourier flexible form exploits the fact that any function can be represented throughout its range by a Fourier series, so that global approximations to a function are possible by choice of suitably long series expansion. This contrasts with the local-only approximation properties of other series expansions such as the Taylor or MacLaurin. The periodic nature of the Fourier series, through use of sine and cosine terms, permits arbitrarily close approximations of the derivatives of the function (in addition to its level) throughout its range.

The demand model results from consumers maximizing utility subject to time and money constraints. The shadow value of time is not, in general, assumed to be the wage rate (though it could be imposed if deemed appropriate). The implication is that the resulting demand equation is of the form

$$x = f(t,p,z,s,T,Y) \tag{8}$$

where x is the natural log of trips taken for whalewatching, t is the time price (i.e., travel time), p is the money price (the money cost of travel), z is a quality measure (expected whale sightings weighted by an index of the importance an individual attaches to whales and the marine environment), T is the discretionary time budget (paid vacation days plus weekend days during the whalewatching season), and Y is money income per household wage earner. (The superscript denoting travel as good 1 is dropped for notational simplicity.) The implication of not assuming equality of the shadow value of time with the wage rate is that time and money prices and budgets enter as separate arguments in the demand equation (see, e.g., Bockstael, Strand, and Hanemann).

Typically a Fourier flexible form is specified by appending sine and cosine expansions to a quadratic function of the exogenous variables. In light of (6), in the present application the primary interest is in flexibility of the demand derivatives $\partial x/\partial p$, $\partial x/\partial z$, and $\partial x/\partial y$, the Marshallian quality and income slopes respectively. Thus the demand specification is based on (8) with an expansion in quality and income, with main effects only:

$$x = a_0 + a_1B + b'w + 0.5w'cw + 2[a_{sp}\sin(p) + a_{cp}\cos(p)] + 2[a_{sy}\sin(Y) + a_{cy}\cos(Y)]$$
$$+ 2[a_{sz}\sin(z) + a_{cz}\cos(z)],$$

where B is a dummy shifter explained below, and w = (t,p,z,Y,T). Parameters to be estimated are the scalars a_0, a_{sp}, a_{cp}, a_{sy}, a_{cy}, a_{sz}, and a_{cz}; and elements of the vector b and the matrix c. The data on p, z and y are expressed in natural logs, and are scaled to be in the interval $(0,2\pi)$, the period of the sine and cosine functions used in the Fourier expansion (see, e.g., Gallant 1981). The scaling is inconsequential in estimation but must be adjusted for when predicting welfare changes.

The specific variable definitions are:

p = the variable cost of travel, in dollars per trip, from the individual's home to their whale-watching site, including gasoline, food, film, and other;

t = the round-trip travel time on the trip from the individual's origin to the site;

z = the product of expected sightings on the trip and an index I of the individual's preference for whales and the marine environment (I=1 for least and I=4 for strong preference);

Y = household income before taxes, in dollars per year, divided by the number of wage earners in the household;

T = discretionary time for whale-watching trips, defined as the number of weekend days plus annual paid vacation days;

Boat = A dummy variable indicating whether the trip was a shore-based viewing trip or a boat excursion (0=shore, 1=boat).

Summary statistics of the data set, based on usable responses on all variables from 236 users, are presented in Table 1. The number of whalewatching trips taken per year ranged from 1 to 11, with a mean of 2.1. Respondents expected to have between 3 and 4 whale sightings, on average, and spent two hours viewing whales per trip. The money cost of travel to the site averaged just less than $9/trip, while the one-way travel time averaged approximately 35 minutes. Roughly half the sample viewed whales from the shore, and half from a boat on a whalewatching excursion. The age of respondents averaged approximately 40 years, and the average education level and income were quite high, averaging 16 years of schooling and $66,000 per household, respectively. When the average number of wage earners per household of 1.5 was taken into account,[3] the income per wage earner was just over $45,000 per year.

Results of estimating the Fourier demand function are in Table 2. Because of very low Student's-t statistics and concern over multicollinearity, the cross-product terms in c were restricted to be zero. Also, when sin(y) and cos(y) were included, none of the income variables were significant; without these terms, the linear and quadratic terms on income were significant, so they were dropped from the specification. The magnitudes of coefficients in the Fourier demand function are not directly interpretable as slopes or elasticities because of the presence of the trigonometric terms, but the curvature implied by the linear and quadratic terms of travel time, quality, and time budget are consistent with expectations. Both the time prices and budget and the money price and budget enter significantly, indicating the importance of both constraints to the whale-watching trip choice. The dummy variable for type of trip indicates that the demand for boat trips is significantly less than the demand for shore-based trips, *ceteris paribus*. The model explains 38% of the variation in log trips overall, and the F-test indicates a high level of statistical significance to the overall model.

Determining Choke Prices Within the Model

To implement the expression for marginal nonuse value in (6), it is necessary to determine the choke price of demand, $\hat{p}$. (The subscript identifying the good is dropped since the application here is to a single-good demand function.) This can be done consistently within the model by noting that trips are taken in discrete units, and the minimum positive consumption is one unit.

The choke price of demand is not simply the price on the demand curve corresponding to one unit of consumption, because at this point the individual's utility is greater than the utility when zero consumption occurs.

[3]Eight households did not report the number of wage earners but did report a positive household income. For these cases the number of wage earners per household was assumed to be one.

The individual would be willing to pay at least a slightly higher price, and perhaps a greatly higher price, to still consume the single unit, instead of having the alternative of no consumption. Thus, the choke price $\hat{p}$, defined as the minimum price that chokes off demand from one unit to zero, is also the maximum price at which the individual would be indifferent between consuming one unit at $\hat{p}$ and consuming zero units. Determining $\hat{p}$ requires the use of virtual prices (e.g., Neary and Roberts), because it is implicit in a comparison of two conditional, or constrained expenditure functions.

Referring to Figure 1 for a visual depiction, the virtual price *phat* is determined by

$$\tilde{m}(\hat{p},z_0,u_0|x^h=0)\equiv e(\hat{p},z_0,u|x^h=1) \qquad (9)$$

where $\tilde{m}(\hat{p}|x^h=0)$ is simply initial income plus the compensating variation associated with availability of the good at its reference price. Calling this level of expenditure m_0, it can be calculated as the total area under the Hicksian demand from reference price to the price at which quantity along the continuous demand (ignoring discreteness) goes to zero. The constrained expenditure when Hicksian demand is one unit is

$$e(\hat{p},z_0,u|x^h=1)=e(p_1,z_0,u)+1\cdot(\hat{p}-p_1), \qquad (10)$$

where p_1 is the price along the Hicksian demand corresponding to $x^h=1$. Thus, combining (9) and (10), the choke price $\hat{p}$ can be found as

$$\hat{p}=p_1+m_0-e(p_1,z_0,u_0).$$

The choke price $\hat{p}$ plus the estimates of the Hicksian quality and income slopes obtainable from the econometric model when income is adjusted to keep utility constant permit one to calculate the marginal nonuse value, from (6).

Numerically Approximating the Total Value of Quality Changes

The Fourier demand model reported in Table 1 fit the data reasonably well, but it has not been possible to recover an analytic quasi-expenditure function by integrating back from the estimated demand equation using the approach of Hausman. To determine the value of changes in quality, it is instead necessary to use numerical approximations (e.g., Vartia) rather than an analytic version of the quasi-expenditure function recovered from the estimated demand.[4] Corresponding to the three-step Mäler heuristic discussed earlier, there are three parts to the numerical approximation of use and nonuse value using the estimated Fourier demand model. Each is briefly described in turn.

1) Determine the initial access value. For each individual in the sample, set accumulated compensating variation equal to zero. Then

a) Raise price by a small increment; predict resulting Marshallian quantity.

b) Calculate adjustment to income needed in order to keep utility constant; add to accumulated compensating variation.

c) Predict Hicksian quantity with new price and new income level.

d) Check whether Hicksian quantity equals 1; if yes, save the corresponding price p_1 (interpolated if necessary).

e) Check whether Hicksian quantity is within ε of zero (where =.01); if no, repeat steps (a)-(e); if yes, report accumulated compensating variation as initial access value.

2) Determine the nonuse value of a 50% increase in quality. For each individual in the sample, divide the increase in quality, Δz, into n equal increments of $Z=\Delta z/n$ (n=10 in the application reported here). Then, for j=1,...,10,

(a) Increase quality by Z.

(b) Calculate μ from (6).

[4]The globally flexible, almost ideal demand system developed by Chalfant represents an attractive direction for future applications since it is based on a Fourier expansion of the AIDS model (Deaton and Muellbauer), so the expenditure function can be identified directly from the estimated demand function parameters and welfare measures can be developed analytically.

(c) Calculate the increment to nonuse value as $\mu \cdot Z$; accumulate.

(d) Reduce money income by the nonuse value increment to keep utility constant.

(e) Calculate the new Hicksian quantity given new quality and new income.

(f) Calculate new choke price by adjusting Hicksian quantity back to 1.0.

3) Determine subsequent access value. To complete the calculation of the total value of a quality change, it is necessary to reduce prices to the original level for each individual, thereby tracing out the compensating variation for provision, or access value, based on subsequent quality. For each individual, start with compensating variation equal to zero, then reduce price from $\hat{p}$ to p_1; this is the first increment to compensating variation.[5] Then

a) Reduce price by a small decrement.

b) Calculate adjustment to income needed in order to keep utility constant; add to accumulated compensating variation.

c) Predict Hicksian quantity with new price and new income level.

d) Check whether price equals the original price; if no, repeat steps (a)(d); if yes (interpolating if necessary), report subsequent Hicksian quantity; and report accumulated compensating variation as subsequent access value.

This procedure makes clear why, even though the goal of the valuation exercise is to come up with a total value of the quality change, it is necessary to keep track of use and nonuse value separately when doing the numerical approximations. Since the weak neutrality condition on preferences is invoked only when the individual is "out of the market," it is necessary to raise prices to the choke level so that Hicksian demand is in fact held at zero.

[5]Hicksian quantity is held at 1 identically for this price change, by definition of the virtual price $\hat{p}$; thus $\hat{p}-p_1$ is the compensating variation for the price change.

Then, maintaining the requirement that the individual be out of the market (through adjusting the choke price), the change in minimum expenditure when quality changes can be calculated. This by definition is the nonuse value, which is then added to the change in compensating variations for access (the use value) to get the total value.

Predicting The Proportion of Respondents with Zero Values

An advantage of the flexibility in the Fourier demand specification is that one can predict the fraction of the sample and corresponding population which has no value associated with the quality change, and the fraction that has a positive value. Assuming that quality is a "good," the change in total value as quality increases is non-negative, and it seems quite plausible that a fraction of the population will have zero value. This, indeed, is a common finding on contingent valuation surveys: after removing protest zeroes, some fraction of the population expresses a zero valuation of the amenity increase. Many simple functional forms for demand do not allow for this possibility, instead imposing the fact that all in the sample have positive value for the amenity change. The Fourier demand function will predict positive or negative value for each individual.

Let $TV_i^*(z_0,z_1)$ be the model prediction for individual i of the value of the increase in quality from z_0 to z_1. Then the proposition that an increase in quality is an economic good implies that the true total economic value, $TV_i(z_0,z_1)$, is

$$TV_i(z_0,z_1)=\begin{cases} TV_i^*(z_0,z_1) & \text{if } TV_i^*(z_0,z_1)>0 \\ 0 & \text{if } TV_i^*(z_0,z_1)\leq 0. \end{cases}$$

A similar approach is taken for nonuse, or existence, values associated with whale sightings increases. The nonuse value, if it is present at all, will be non-negative since increases in whale sightings have no obvious deleterious effects on an individual's utility if they are not whalewatching. Thus, letting $NUV_i^*(z_0,z_1)$ be the model prediction of nonuse value and $NUV_i(z_0,z_1)$ be the true value,

$$NUV_i(z_0,z_1)=\begin{cases} NUV_i^*(z_0,z_1) & \text{if } NUV_i^*(z_0,z_1)>0 \\ 0 & \text{if } NUV_i^*(z_0,z_1)\leq 0. \end{cases}$$

Results

Table 3 presents the model predictions about how the number of whalewatching trips will change with a 50% increase in whale sightings, and the estimates of the total net economic value of the quality change. Mean trips taken is 1.83 before the quality change, and 2.17 after, for an increase of approximately 19%. No effort has been made in this preliminary work to predict trips taken by new entrants, though that will be possible in subsequent work. The model underpredicts trips with initial quality by about 13%, even with an adjustment for the log transform in the dependent variable of the demand model.

Using the propositions that both total economic value and nonuse value are economic goods, the mean total value of the increase in whale sightings was predicted to be $20.57, of which $14.75 is use value, or the increase in area to the left of the Hicksian demand for whalewatching as quality shifts out. Mean nonuse value was estimated to be $5.82. The model predicts that 36% of the sample have zero total values of the quality change, and 64% have positive total values. When an individual has zero total value, this is taken to mean zero use value and zero nonuse value. Taking this into account, the predicted fraction of the sample with positive nonuse values is 61%.[6]

[6]The model predicted a positive NUV_i^* in 97% of cases, though in the 86 cases where total economic value is zero the positive NUV_i^* was outweighed by a negative use value and both use and nonuse value are set at zero.

Table 4 focuses separately on the subsets with positive total value (150 observations) and those with zero economic value (86 observations). For those with positive total value, the mean was $30.44, with use value of $21.83 and nonuse value of $8.61. The (Hicksian) demand for whalewatching was relatively responsive for this group, with predicted trips increasing from 2.0 to 2.5 with the 50% increase in quality. Access value, or the area to the left of the Hicksian demand curve, was $99 initially and $121 after the quality increase.

For those with zero total economic value, the model predictions for each observation were a positive but small nonuse value (averaging about $1.20, with a maximum of $8), outweighed by a negative but also small use value (averaging about $6.20, with a minimum of -$34). The predicted total value in each case was negative, which implies a zero total value under the presumption that quality increase is an economic good. These individuals took fewer trips on average, had a small decrease in trips with the quality increase (from 1.56 to 1.46) and had a smaller access value for travel to whalewatching sites ($79).

Figure 2 presents some further detail on the empirical estimates, in the form of frequency distributions for computed total, use, and nonuse values. The mode of total economic value was the range from $20-$30, with 3 values exceeding $100. For use value, 86 observations were zero because total economic value was zero, and for another 20 observations use value was zero but total value was positive. For the remaining 130 observations, use value was positive and concentrated in the range of $0-$30. Nonuse value was zero for the 86 cases where total value was zero, and for seven additional cases where total value was positive. For the remaining 143 cases, nonuse value was positive and heavily concentrated in the $0-$15 range.

Figure 3 provides a graphical interpretation of the demand shifts involved for the majority of cases. The effect of increasing quality is to flatten the demand curve, making it more elastic. The shifts depicted in Figure 3 are similar to what could be obtained by other flexible models, such as the varying parameters model (e.g., Vaughn and Russell). For those who are relatively infrequent users, who live farther away and pay a higher price (say p_1 in the graph) for access, the effect of the increase in quality is to reduce demand. These individuals are taking fewer, high quality trips instead of more lower quality trips. The likelihood is that these individuals will have little or no economic value from the quality (whale sightings) increase. Relatively more avid users, and those who live closer and pay a lower price for whalewatching (say p_0 in the graph), take more trips as quality increases. For these individuals, the demand curve shifts in with quality change at the choke price level, but the total access value (area to the left of the Hicksian demand curve) increases. Thus, they have both positive use value and positive nonuse value. As Table 4 shows, those with positive economic value tend to be the individuals who are more active in whalewatching, with a higher level of mean trips and a higher access value to begin with.

Implications

The results presented here are preliminary, but suggest the possibilities which are opened up when plausible prior information is brought into the process of valuing quality changes from empirical demand systems. In this case, the prior information takes a different form than what has been used previously. Instead of assuming there is no nonuse value associated with the quality increase (as implied by weak complementarity), the statement is that under certain conditions -- namely, when the private goods which are the source of use value are not consumed -- the change in value must be solely nonuse value. This implies a restriction on the individual's minimum expenditure function at the choke price vector, which is sufficient to identify the curvature of the expenditure function with respect to quality globally. This restriction follows directly from the researcher's specification of the demand system.

This research may help to revitalize another avenue of measurement of the total value of amenity or public good changes, namely the use of observable behavior in the form of demand systems. In particular, two directions seem promising for additional work. One is the extrapolation of sample results for total economic value (including nonuse value) from users to the larger population. While it is clear that not everyone in the population shares the same preferences for whales as those who have been self-selected as users, some do. Attention to predicting why people are or are not whale watchers should permit the general population to be categorized as nonusers (those who for reasons of preference would never enjoy whales), potential users (those who are not current users for reasons of price or quality), and users. Methods developed here would be applicable to potential users as well as to users.

Another, perhaps overlooked, reason why the general approach explored here is important has to do with comparison or cross-checking the results of different methodologies when measuring the value of amenities. When the amenity potentially involves nonuse value, the contingent valuation method and the travel cost method using weak complementarity are fundamentally noncomparable. The former by its nature picks up both use and nonuse value, and the latter picks up only use value. Only if the travel cost method allows for nonuse value, as a part of the total value calculation, can there be a possibility of comparing results from the two methods. The weak neutrality approach permits this, and has the additional virtue of being a preference restriction, or form of prior information, that follows directly from the researcher's specification decisions regarding use value.

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Table 1. Descriptive Statistics for the Sample of Whalewatchers						
Variable	Units	Mean	S.D.	Min	Max	Cases
Trips	No./Yr.	2.1	1.6	1.0	11.0	236
Expected Sightings	No./Trip	3.3	5.6	0.0	50.0	236
Whale Importance Index		3.19	.44	1.75	3.75	236
Hours Spent Watching	Hrs/Trip	2.0	1.3	0.3	15.0	235
Money Cost of Travel	$/Trip	8.9	34.2	0.0	500.0	236
Travel Time	Hrs/Trip	0.6	0.8	0.0	6.5	236
Boat Trip	1=Yes	0.5	0.5	0.0	1.0	236
Age	Years	40.3	11.1	20.0	78.0	235
Education	Years	16.1	2.4	8.0	21.0	232
Hours Worked Per Week	Hrs/Wk	37.7	17.1	0.0	72.0	203
Discretionary Time	Days/Yr	107.5	5.9	104.0	136.0	236
Household Income	$/Yr.	65953.	36253.	5000.	150000.	236
Wage Earners/ Household	No.	1.5	0.8	0.0	4.0	228
Income Per Wage Earner	$/Yr.	45701.	29080.	5000.	150000.	236

Table 2. Fourier Series Model Estimates of Whalewatching Demand

Dependent Variable	ln(Trips)
Number of Observations	236
Mean of Dep. Var.	.56
Std. Dev. of Dep. Var.	.60
R - squared	.38
Adjusted R - Squared	.33
F(15, 220)	8.85

Variable	Symbol	Coefficient	Std. Error	Student's- t
Constant		16.876	8.2169	2.05
Money Cost of Travel	p	-87.230	38.214	-2.28
	p^2	13.949	6.1145	2.28
Travel Time	t	-.10470	.08234	-1.27
	t^2	.02251	.01334	1.69
Expected Sightings[a]	z	4.1202	.80921	5.09
	z^2	-.83438	.16995	-4.91
Money Income/Earner	Y	-.16362	.08909	-1.84
	Y^2	.02476	.01502	1.65
Discretionary Time	T	-.27336	.12724	-2.15
	T^2	.00125	.00054	2.32
Fourier expansion terms	sin(p)	82.380	36.313	2.27
	cos(p)	-2.8062	2.3431	-1.20
	sin(z)	-1.9496	.50860	-3.83
	cos(z)	1.8730	.36094	5.19
Boat Trip Dummy	B	-.47705	.07415	-6.43

[a]Sightings variable weighted by the whale importance index of preference intensity for whales and the marine environment.

Table 3. Changes in Value with a 50% Increase in Gray Whale Sightings

Variable	Mean	S.D.	Min.	Max.	Cases
Total Value	20.57	51.16	.0000	436.3	236
Use Value	14.75	51.48	-20.17	436.3	236
Nonuse Value	5.815	6.856	.0000	31.62	236
Access Value (z_1)	107.4	73.98	7.923	480.8	236
Access Value (z_0)	92.69	54.65	7.923	368.8	236
Trips (z_1)	2.169	1.550	.3062	9.941	236
Trips (z_0)	1.831	1.059	.4443	7.503	236

Percent with Zero Total Value: 36%
Percent with Zero Nonuse Value: 39%

Table 4. Changes in Value for Individuals with Positive Total Value, and Individuals with Zero Total Value

Variable	Mean	S.D.	Min.	Max.	Cases
Those with Positive Total Value					
Total Value	30.44	59.82	.0287	436.3	150
Use Value	21.83	61.44	-20.17	436.3	150
Nonuse Value	8.607	6.748	.0000	31.62	150
Access Value (z_1)	121.0	83.34	15.78	480.8	150
Access Value (z_0)	99.23	60.84	19.84	368.8	150
Trips (z_1)	2.511	1.749	.3062	9.941	150
Trips (z_0)	1.968	1.182	.4443	7.503	150
Those with Zero Total Value					
Total Value	.0000	.0000	.0000	.0000	86
Trips (z_1)	1.456	.5409	.5362	2.211	86
Trips (z_0)	1.546	.6636	.5680	2.518	86
Access Value	79.08	35.35	7.923	128.5	86

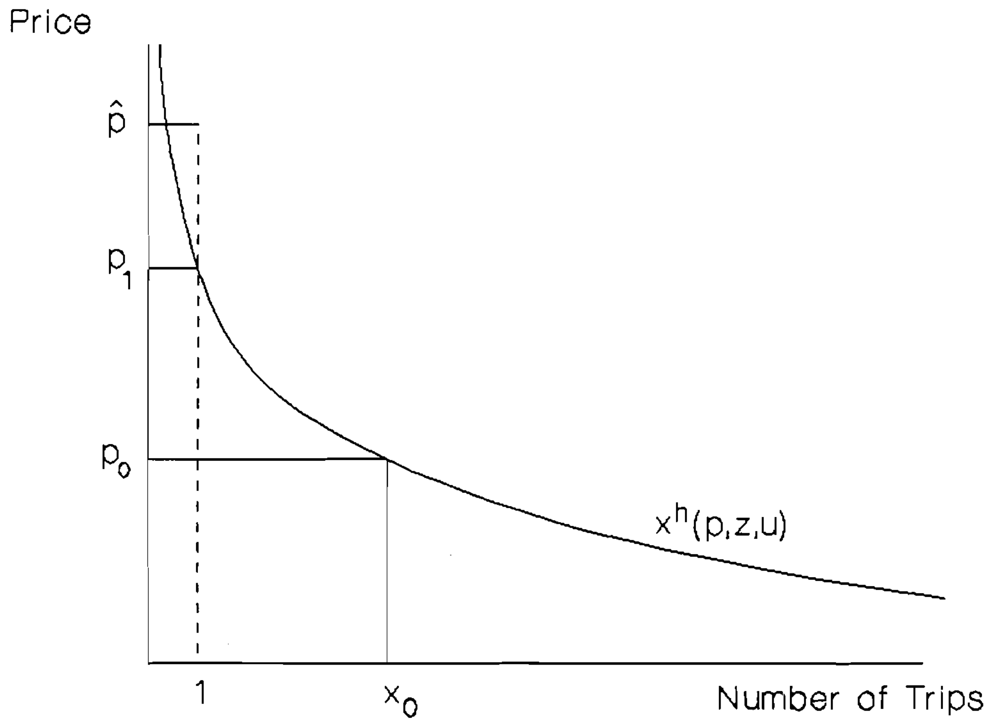
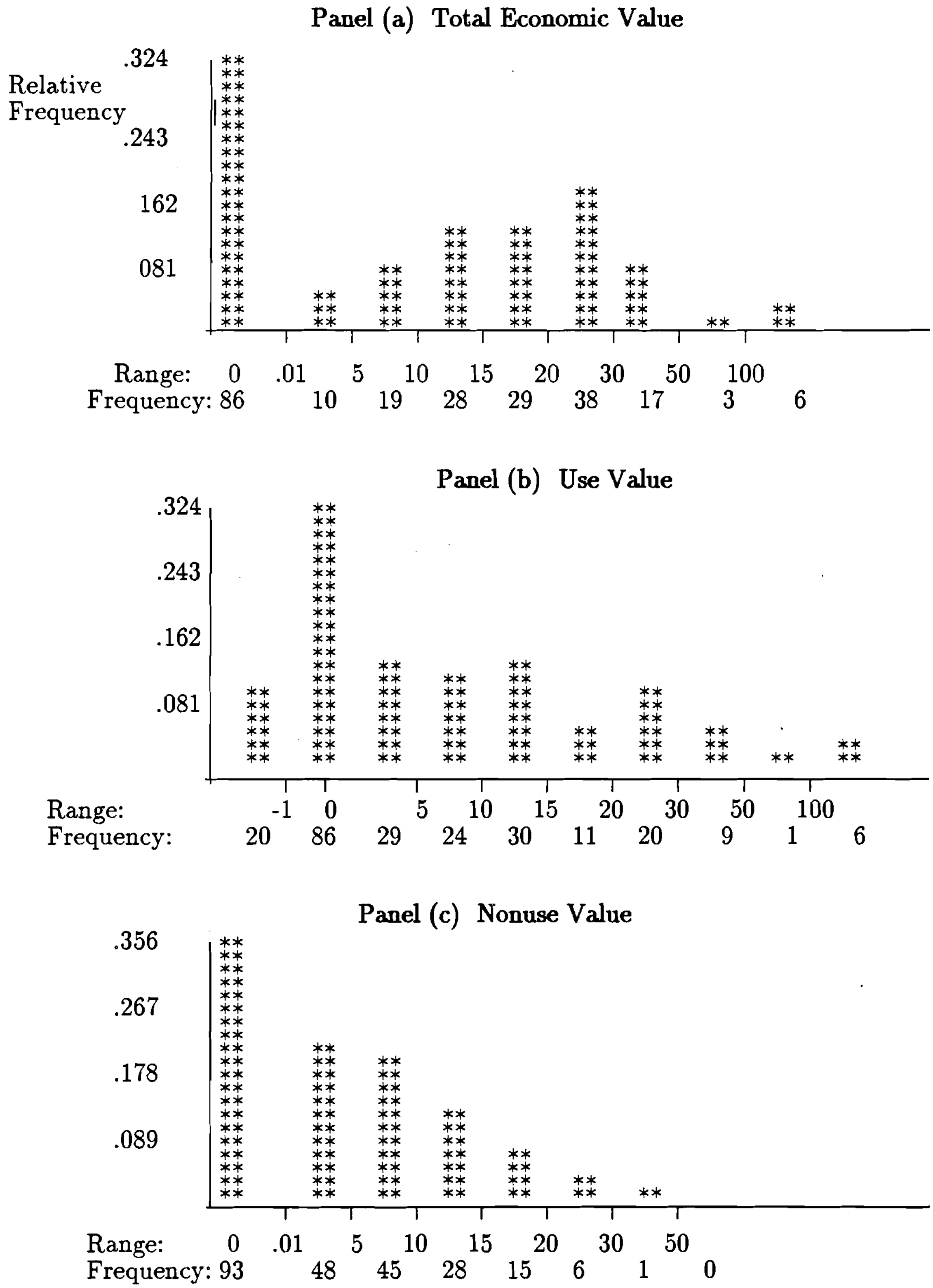


Figure 1. Defining the Choke Price

Figure 2. Frequency Distributions of Total Value, Use Value, and Nonuse Value for a 50% Increase in Whale Sightings.



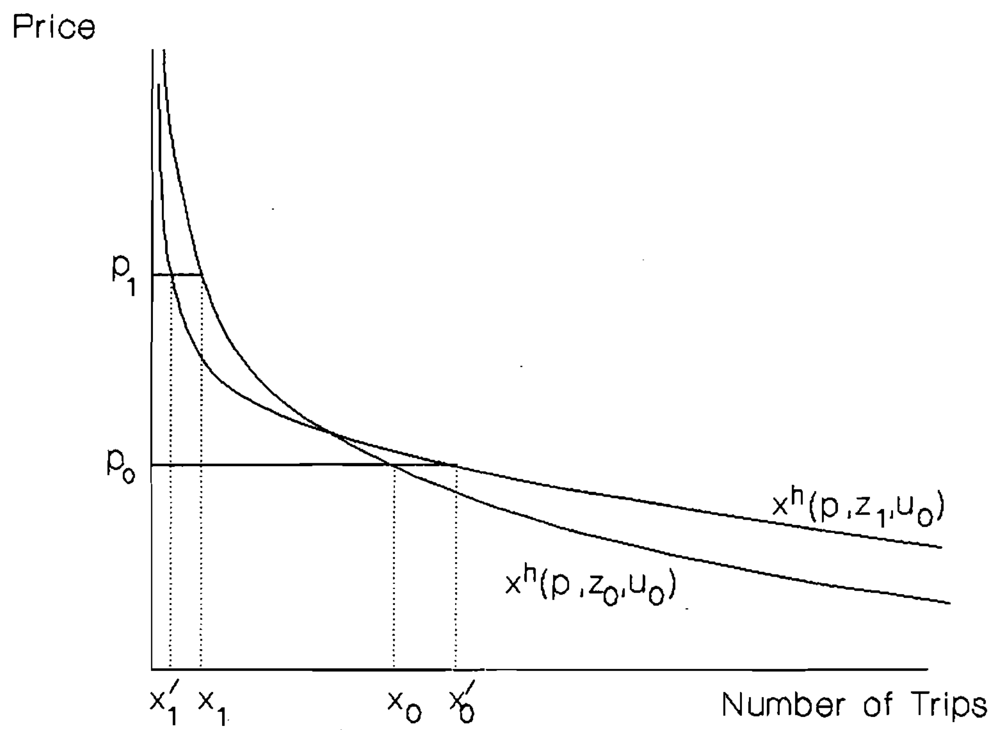


Figure 3. Demand Shift with A Quality Increase

Measuring Nonuse Value: A Comparison of Recent Contingent Valuation Studies

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Abstract

Thirty-one contingent valuation studies published since 1980 that have estimated nonuse value were summarized and compared. These studies estimated willingness to pay for many different types of goods, used a variety of methods, and produced a wide range of value estimates. Six different methods were used to isolate nonuse value. Lower estimates of nonuse willingness to pay resulted from mail surveys, in contrast to personal interviews; from using a contribution payment vehicle, in contrast to increases in prices or taxes; and from estimating nonuse value as the total willingness to pay of nonusers, in contrast to other methods of estimating nonuse value. Respondents of most studies indicated that nonuse value exceeds use value. Several studies found that nonuse value was higher for users than for nonusers of the good, suggesting that basing nonuse value solely on the responses of nonusers will underestimate nonuse value.

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Introduction

Using contingent valuation, economists have measured willingness to pay (WTP) for "nonuse" or "passive use" benefits (called "nonuse value" herein) for over 20 years. This paper briefly summarizes and compares 31 such studies published in the United States since 1980. In addition to the text, a lengthy table in the Appendix contains basic information about each study. This paper updates and expands on Fisher and Raucher's (1984) comparison of the first 6 studies (all published in the 1970s) to estimate nonuse value.

Recently, much controversy has surrounded the measurement of nonuse value, centered on the validity of contingent valuation as used to measure the value of public goods, with particular emphasis on the "embedding" effect (Kahneman and Knetsch 1992). It is not the purpose of this paper to address the validity question. This paper's more modest goal is to look across the variety of studies now available in order to (1) summarize the methods used, (2) report on the range of nonuse value estimates that have been obtained, and (3) compare estimates of use and nonuse value.

This paper was motivated largely by two considerations. First, among the post-1980 studies that have measured nonuse value, a wide range of methods has been used. This range of methods occurred partially because of the different types of goods that were studied, but also because of the lack of accepted methodological guidelines in the young field of contingent valuation. The ample recent activity in measuring nonuse value offered the opportunity to investigate the relationship of method to result, to perhaps indicate whether we should be more concerned about our methodological choices. Second, many of the studies measured both use and nonuse value, allowing a comparison of these two parts of total economic value. If studies consistently found similar ratios of use to nonuse value, we might have a basis for obtaining a rough estimate of nonuse value, and therefore total value, for the many studies that measured only use value.

Although an attempt was made to include all recent contingent valuation studies that have estimated nonuse value, some studies have undoubtedly been missed. Apologies are due to the authors of any studies that were inadvertently overlooked. Hopefully those that are included adequately sample the population of such studies.

The basic distinction between use value and nonuse value proposed by Randall and Stoll (1983) has been adopted here, which assigns option value to the use value category and assigns existence or intrinsic value plus bequest value to the nonuse value category. Because the studies summarized here differed in how they defined individual components of use value or of nonuse value, estimates for individual components were added in an attempt to achieve comparability across studies. Thus, this summary compares aggregate estimates of use value and nonuse value.

General Description of the Studies

About half of the 31 studies were published in the 1980s, with the remainder published in the 1990s. Eleven of the 31 studies focused on wildlife and fish protection. Another third of the studies focused on water quantity or quality, and the remaining third dealt with wilderness preservation, forest quality, air quality, and other types of goods (Table 1).

Mail surveys were used in 18 of the 31 studies, while seven studies used household interviews and four performed phone surveys (Table 2). Four studies distributed questionnaires to respondents onsite, with two of these asking respondents to mail them back and the other two collecting the questionnaires onsite. Among the 19 mail surveys used in the 18 studies, response rates ranged from 21% to 84%, with a median of 39%; this is nearly identical to the median response rate among the 16 contingent valuation studies listed by Mitchell and Carson (1989, Table 12-3). The two highest mail response rates were obtained in surveys that sampled only persons known to be interested in the good: Loomis (1987) obtained a response rate of 84% from users who were given the questionnaire onsite and mailed it back, and Bishop and Boyle (1987) obtained a response rate of 81% from taxpayers who had recently donated to the state's endangered species program. Only one other of the mail surveys (Duffield 1992) sampled only users. The wide range of response rates for the general public samples (21% to 61%) may be due the nature of the good and to methodological choices such as repeat mailing procedure and length of survey. Response rates of the other survey methods were inconsistently reported (see the Appendix).

The two most common elicitation methods were the open-ended response, used in 12 of the 31 studies, and the dichotomous choice response, used in 11 of the studies (Table 3). In addition, four of the dichotomous choice studies followed the yes/no response with an open-ended question. Five studies used payment cards, and only three, performed in the early 1980s, used a bidding game. See Mitchell and Carson (1989) for descriptions of each of the elicitation methods.

About half of the studies used a contribution payment vehicle, whether it was to a "trust fund" or a "special fund" (Table 4). The payment was sometimes called a "contribution" and elsewhere called a "membership." Six studies used as a vehicle an increase in taxes and/or prices, four used special (ear-marked) taxes, four used increases in utility bills, and one used a "payment" to a "program."

Most of the studies asked for annual bids, but five used monthly payments (this was common for utility bill payment vehicles) (Table 5). Two used one-time payments.

Measuring Nonuse Value

Description of Methods

Six methods were used for isolating nonuse value (Table 6). With *method 1*, used in 12 of the 31 studies, respondents provide estimates of total WTP and then apportion their bids among different valuation motives or value categories. For example, Walsh et al. (1984:17) asked respondents to allocate their bids among (1) actual recreation use, (2) a "payment of an insurance premium to retain the option of *possible* future use," (3) "the satisfaction from knowing that it exists as a natural habitat...," and (4) "the satisfaction from knowing that wilderness will be protected for future generations" (emphasis in the original). Similarly, Loomis (1987a:132) asked respondents to apportion their bids among (1) "value to actually visit Mono Lake **this year**," (2) "value to maintain the opportunity to visit Mono Lake **next year**," (3) "the value to you from just knowing that Mono Lake **exists** as a natural place for birds and other wildlife even if your household could not visit it," and (4) the value to you from knowing that Mono Lake will be preserved for future generations" (emphasis in the original). As demonstrated by these two examples, studies differ in how they describe the components of use value. In some studies, option value is separated from bids for actual future use, but in other studies option price is described. Similarly, in some studies use in the current year is separated from use in future years, while in other studies current and future use are combined. Studies also differed in how they described nonuse motives. As explained above, categories referring to actual or potential use were lumped herein to estimate use value, and categories referring to existence or bequest motives were lumped herein to estimate nonuse value.

With *method 2*, used in 11 of the studies, total WTP is estimated for a subsample of respondents that can in some sense be considered nonusers of the resource. Definitions of nonuse varied among the studies, but conformed to one or both of two basic possibilities: the respondent (1) did not use the resource during some past time period, or (2) does not anticipate using the resource during some future period. Five studies focused on past use. For example, Boyle and Bishop (1987) asked respondents if they had ever made a trip with the intention to view bald eagles, Whitehead and Blomquist (1991) asked respondents if they had ever visited Clear Creek wetland, and Duffield et al. (1993) asked respondents if they had visited the subject river(s) in the last three years. Four studies focused on future use. Three of these studies did not specify a specific future time period. For example, Stoll and Johnson (1984) asked respondents if they anticipated future visitation to the refuge. Walsh et al. (1985) focused on use "next year." The following two studies used combinations of past and future use: Olsen et al. (1991) distinguished respondents who neither had fished for the subject species in the past five years nor expected to do so in the next five years, and Silberman et al. (1992) distinguished respondents who had not and did not expect to use the beaches of interest.

With *method 3*, used by 5 of the 31 studies, respondents are asked to assume that they would not use the resource. For example, Boyle and Bishop (1987) told respondents that the bald eagle habitat at issue would be in remote parts of the state where viewing was not possible, Duffield (1992) told respondents to "suppose...that you personally would not have an opportunity to see or hear a wolf in Yellowstone...," and Greenley et al. (1981) prefaced the WTP question with "if it were certain you would not use the South Platte River Basin for water-based recreation..."

With *method 4*, all (or nearly all) respondents are assumed to be nonusers. Minimal use is reasonable if the resource is difficult to observe, such as the striped shiner (Boyle and Bishop 1987), or if travel cost is significant and the sample is drawn from a general household population, as with Atlanta residents valuing waterfowl in the Great Plains and Rocky Mountains (Desvousges et al. 1992). The surveys of seven of the studies were considered here to allow for the assumption of nonuse. Note, however, that not all of the seven papers argued that all respondents were nonusers or that the estimated values were totally nonuse values. Nevertheless, their inclusion here allows a comparison of the results of this method with the other methods.

With *method 5*, used only by Duffield et al. (1993), total WTP is partitioned to use and nonuse portions based on statistical associations between WTP and responses to a series of behavior and attitude questions. Duffield et al. queried respondents about their past and expected use of the rivers in question and asked them to indicate the extent to which they agreed with 23 different statements related to resource use and protection. Factor analysis of the responses allowed isolation of a small set of variables focusing on past and future use, altruism, personal contributions, and environmental protection. Regression of WTP on these variables allowed estimation of the relative share of WTP attributable to use and nonuse motives.

Finally, with *method 6*, used only by Silberman et al. (1992), respondents were asked a nonuse value question without specifically being told to assume zero use. After answering a WTP question about use, respondents were told: "The previous questions were based on your possible use of the new beaches shown in the picture. It may be worth something to you simply knowing that more people will be able to use the beach or because you believe more beaches are good for your community. For example, you might be willing to pay something to maintain a public park even though you won't use it" (p. 227). Each of the six approaches has its advantages and disadvantages. Here are some of them. (1) Asking respondents to apportion their bid among various reasons for valuing the good directly asks for the essential information and is quite easily administered, but it asks respondents to make difficult cognitive distinctions. The distinctions might be confusing or seem arbitrary to respondents, leading to poorly considered or misleading responses. Further, in separating the motive (percentage apportionment) response from the monetary response, this method might allow respondents to switch to a separate mental construct and list proportions that are quite unrelated to actual WTP. (2) Separating users from nonusers so that the bids of the nonusers can be attributed totally to nonuse value works only if nonusers can be reliably separated. For some goods at least, it may be difficult to define nonuse. For example, past nonuse does not preclude future use, so that the bids of past nonusers may include some use value. Even a negative response to the question "Do you expect to visit this area in the future?" does not necessarily preclude the respondent from holding out the possibility of future use and including option value in his or her bid. A further problem with this method, discussed in a subsequent section, is that users and nonusers may assign different values to nonuse motives, leading to inaccuracies when nonuse value is based totally on nonusers' WTP. (3) Asking respondents to assume that they will not use the good in the future is easy to administer, but it makes an already hypothetical scenario even more so. Further, in studies where the conditioned (assumed zero use) WTP question follows an unconditioned WTP question (e.g., as in Duffield

1992 and King et al. 1986), it might be argued that the method almost challenges respondents to demonstrate their environmental awareness by not lowering their previously stated WTP. (4) Assuming that the population of potential respondents contains no persons who consider themselves to be current or potential personal users of the good is perhaps the simplest way to estimate nonuse value, but the assumption may be heroic for all but the most obscure goods. (5) Separating the use from the nonuse portion of the bid based on respondents' answers to a series of related questions avoids asking the respondent to make unrealistic assumptions or difficult cognitive distinctions. However, the method requires a longer questionnaire and is subject to the common specification errors related to selecting the right questions to isolate the key motive variables. (6) Asking direct nonuse value questions, like the apportionment approach, requires the respondent to make potentially difficult cognitive distinctions between use and nonuse value.

Nonuse Value Estimates

Fifty-one estimates of nonuse value, obtained from the 31 studies, are listed in the Appendix (in nominal dollars, along with the year of estimation). Adjusting for inflation (to 1990 dollars), these estimates varied from $1 to $184 per year per household, with a median of $23 (Table 7). A third of the estimates was obtained using method 1, another third using method 2, and most of the remaining third was obtained using methods 3 or 4. The range of estimates for each of these four methods is broad, suggesting that method (of isolating nonuse value) alone does not account for all variation in the estimates.

Among the medians of the estimates obtained using the four more commonly used methods (Table 7), the most notable finding is that the median for method 2 ($12) is considerably smaller than the median for the other three methods. The higher medians with methods 1 and 3, as opposed to method 2, may be attributable to the fact that the former two methods base nonuse value estimates on responses of both users and nonusers (more on this in a later section). The higher median with method 4, as opposed to method 2, may result from the inclusion of some use value in the bids of these "assumed" nonusers (i.e., from the inclusion of users or potential users among the respondents). The assumption of zero use value may not apply to some of the seven method-4 studies, for two reasons. First, two of the studies focused on the northern spotted owl (Hagen et al. 1991 and Rubin et al. 1991). While the respondents most likely realized that their chances of observing the secretive owl was remote, they may have considered owl preservation a vehicle for preserving old growth ecosystems that they did hope to visit for recreation. Second, three of the studies valued specially designated areas, either wilderness areas (Diamond et al. 1992) or national park lands (Schulze et al. 1983 and Hoehn 1991). While the areas were distant enough from the general population samples that use was unlikely for the large majority of respondents, the areas were special enough that the hope of visitation, and therefore option value, may have been substantial. These results suggest that care should be used in applying method 4.

Comparisons across studies are difficult because of the many methodological differences between surveys. A larger sample of studies would be needed to allow an adequate statistical analysis of the effects of survey characteristics on measured values. However, some interesting patterns appear by examining characteristics of the studies that produced the highest and lowest nonuse value estimates. We will consider five characteristics for the bottom and top quartiles among the 31 studies listed in the Appendix. The 14 estimates and their methodological characteristics are listed in Table 8.

Nature of the good. Both lists include a variety of types of goods covering a range of uniqueness. While there are no obvious differences between the goods in the bottom quartile and those in the top quartile, the top-quartile probably contains more high visibility goods (e.g., Grand Canyon, national parks, spotted owls) than the bottom-quartile. Other evidence suggests that the nature of the good does matter. For example, Bishop and Boyle (1987) found that reported nonuse value was several times higher for bald eagles than for striped shiners.

Survey administration. Six of the seven lowest estimates were obtained in mail surveys, while only two of the seven highest estimates were so obtained. All five of the remaining high estimates were household interviews.

Payment period. Six of the seven lowest estimates used an annual payment period. Among the seven highest estimates, three used monthly payments and four used annual payments.

Payment vehicle. All of the seven lowest estimates used a contribution payment vehicle, while none of the seven highest estimates used a contribution. Among the high estimates, two used utility bill increases (these were both monthly payments), four used increases in taxes and/or prices, and one used a payment to air quality "program."

Method of isolating nonuse value. Five of the seven lowest estimates were obtained using method 2, while only one of the seven highest estimates was obtained using that method. Note that method 2 is the only method that bases nonuse value solely on the WTP of self-reported nonusers. Three of the high estimates were obtained using method 4 (assuming all respondents are nonusers) and another two were obtained using method 3 (asking respondents to assume zero use).

To summarize, among the 31 studies, higher estimates tended to be obtained (1) in personal interviews, (2) using a monthly payment period, (3) using a vehicle of increases in taxes or prices, and (4) using methods 3 or 4 to estimate nonuse value. And lower estimates were obtained (1) using a mail survey, (2) an annual payment period, (3) a contribution payment vehicle, and (4) method 2 to isolate nonuse value.

Among the studies for which more than one estimate is listed in the Appendix, once one estimate was selected for inclusion here, no other estimates from that study were considered for inclusion.

The reasons for these differences are not entirely clear. However, the differences suggest the following hypotheses: (1) respondents in personal interviews tend to elevate their WTP responses in comparison with mail responses (perhaps respondents seek to please the interviewer); (2) respondents to monthly payment questions indicate a larger annual WTP than respondents to annual payment questions (perhaps respondents to monthly questions fail to compute the annual total); (3) a contribution payment vehicle tends to yield a lower WTP than a tax or price vehicle (perhaps because of an aversion to free riders); and (4) users are willing to pay more for nonuse motives than are nonusers. Each of these hypotheses is testable in a carefully controlled study.

Comparing Use Value and Nonuse Value

Twenty-three of the 31 studies allow a total of 34 comparisons of use value to nonuse value (all are listed in the Appendix). Some studies allow more than one comparison because more than one population was sampled or more than one method was used. For these 34 comparisons (Table 9), nonuse value was isolated using methods 1, 2, 3, or 5, as described above. Corresponding estimates of use value were obtained using the following methods: (1) apportionment by the respondent, (2) total WTP of the sample of users minus total WTP of the sample of nonusers, (3) total WTP of the sample of actual or potential users minus total WTP of the sample asked to assume zero use, (5) statistical apportionment based on responses to behavior and attitude questions, and (6) asking a separate use value question (see Table 6 for a summary of these methods).

Use and nonuse values were compared by computing the ratio of nonuse value to use value (Table 9). The 34 ratios range from 0.1 to over 10. The median ratio of 1.92 indicates that most studies found nonuse value to exceed use value. However, the results for the specific methods tell an interesting story. Most ratios were estimated using methods 1 (direct apportionment) and 2 (separation of sample into user and nonuser groups based on past or expected behavior), allowing a fairly strong comparison of these two methods. Median ratios for methods 1 and 2 are 2.56 and 0.85. All but one of the 17 method-1 ratios are above 1. Conversely, only five of the 12 method-2 ratios are above 1. One possible explanation for this difference is that respondents using method 1 want to feel or appear magnanimous by indicating that they value existence and bequest more highly than their personal use. The same claim could also be made about nonuse value method 3 (i.e., asking respondents to assume that they would not use the resource). Only the method-5 ratios (where use and nonuse values were apportioned based on behavior and attitude responses) offer a somewhat independent evaluation of this potential explanation. These two ratios, both from Duffield et al. (1993), are similar to the median ratios obtained using methods 1 and 3. Thus, we have some evidence that the high (relative to method 2) ratios obtained with methods 1 and 3 are not simply the result of a feel-good motive.

Another explanation for the difference in ratios between methods 1 and 2 is that method 2 underestimates the true ratio. Recall that with method 2, nonuse value is total value of nonusers and use value is equal to total value of users minus total value of nonusers. Method 2 assumes that nonuse value is the same for users and nonusers. If nonuse value of users exceeds that of nonusers, this method would underestimate the ratio for any good that is subject to "use." The next section examines this critical assumption of method 2.

Comparing Users' and Nonusers' Nonuse Values

Six of the 31 studies allow comparisons of users' and nonusers' nonuse values (Table 10). Three of the studies used method 1 (direct apportionment) with user and nonuser subsamples. Two studies used method 3 (assumed zero use) with user and nonuser subsamples. The other study obtained users' nonuse WTP using method 6 (asking a specific nonuse value question) and nonusers' nonuse WTP using method 2 (total WTP of the nonuser subsample). In three of the studies, users were distinguished from nonusers based on past behavior, while in the other three studies the distinction was made based on expected future behavior. It is not clear to what extent past nonusers might be future users, or to what extent expected future nonusers were past users.

The 7 ratios of users' to nonusers' nonuse values range from 1.4 to 2.6 (Table 10), suggesting that past or expected future use tends to enhance nonuse WTP. Note that the findings of two other studies (Loomis 1987a,b and Stoll and Johnson 1985) tend to substantiate this finding. However, in each study the users' and nonusers' estimates of nonuse value were obtained using different survey administration procedures, so the users' and nonusers' estimates may not be directly comparable.

Whitehead and Blomquist (1991) provide additional evidence of the effect of "use" on nonuse value. Past nonusers of Clear Creek wetland were separated into two subsamples depending on whether they had "information" about wetlands. This "information" was either onsite use of other Kentucky wetlands or offsite sources such as television or conservation literature. The ratio of mean WTP of the informed subsample to WTP of the uninformed subsample is 3.14.

One explanation of the results of the six studies listed in Table 10 is that past or expected future use is associated with information about the good, and that information, as Whitehead and Blomquist (1991) suggest, enhances nonuse value. Obviously, past use provides information about the good. Furthermore, respondents' plans for future use may have resulted from information gathered about the good. If nonuse WTP is sensitive to information about the good, we would expect users' nonuse WTP to exceed nonusers' WTP to the extent that users have more information than nonusers.

Of course, the converse of this explanation is also feasible -- that those whose nonuse value is higher for a given resource tend to accumulate more information about the resource than those with lower nonuse values. In either case (whether information engenders value or value prompts one to acquire information), the suggestion is that people's nonuse values differ, that the difference is associated with familiarity with the good, and that such differences in value can be measured.

Silberman et al. (1992), however, offer a quite different explanation based on their data about WTP for beach restoration. They concluded that the positive ratios of future users' to future nonusers' nonuse value that they found were attributable to use value being included in users' estimates of nonuse value. Indeed, this possibility is plausible given the methods they used, which required future users, after answering a use value question, to provide a separate estimate of WTP for "simply knowing that more people will be able to use the beach..." (see the earlier discussion of method 6 for more of the statement). Although respondents were told that the payments for "simply knowing" would be added to any entrance pass income to restore the beaches, the respondents were not precluded from or specifically warned against including some use value in their responses. That is, expected future users had the opportunity to double count use value.

Double counting use value could also affect ratios based on method 3 (used by two of the six studies listed in Table 10), where respondents are asked to estimate their WTP under two separate scenarios (zero future use and potential future use). But if responses to the assumed zero future use scenario included some personal use value, the respondents were explicitly ignoring the dictates of the zero use scenario. Double counting use value seems plausible, but it seems less likely than in the Silberman et al. (1992) study, where the nonuse value question did not explicitly state that use value should not be included in the bid.

Unlike in the method-3 studies, the double counting explanation of the higher nonuse bids of users than nonusers could not apply to the ratios based on method 1 (used by three of the six studies listed in Table 10). Method 1 of isolating nonuse value does not allow respondents to double count use value because it requires only one WTP estimate from each respondent.

However, another possible explanation of the positive method-1 ratios reported in Table 10 is that method 1 requires respondents to perform two tasks that may be cognitively unrelated. That is, perhaps respondents, once they have estimated their total WTP, then allocate their WTP to value components based on general affective attitudes that have little to do with actual WTP. If this is the case, then perhaps all respondents (both users and nonusers) would tend to report the same percentages for allocating their total WTP, and the higher total WTP of users would result in greater nonuser WTP for users than nonusers. However, the evidence does not support this explanation. The three method-1 studies that allowed computation of ratios of nonuse value to use value for both user and nonuser subsamples (Table 11) all found quite different ratios across the two subsamples. [Note that "nonusers" can provide estimates of use value as long as nonuse is not defined as "no future use expected."] Two of the studies (Duffield et al. 1993 and Walsh et al. 1985) found considerably lower ratios for users than for nonusers (i.e., users allocated more of their total WTP to use motives than did nonusers). In spite of the lower ratios for users than nonusers, the users' significantly greater total WTP caused their nonuse value to exceed that of nonusers. The third study (Clonts and Malone 1990) reported a higher ratio of nonuse to use value for users than nonusers (both the use value and nonuse value of users exceeded those of nonusers, but the difference in nonuse values was by far the greatest). Perhaps the difference between Clonts and Malone's finding and that of the other two studies is partially explained by the fact that in the former study a respondent was considered a user if a household member (not necessarily the respondent) had used one of the 15 rivers in the past three years, while in the latter studies use was dependent on the respondents' behavior.

The hypothesis that use is preceded by and enhances information, and that information increases nonuse value, is a parsimonious but tentative explanation of the ratios listed in Table 10. If this hypothesis is accepted, method 2 must be rejected as a way to partition total value. If comparisons based on method 2 are removed from Table 9, all but three of the remaining 22 ratios of nonuse value to use value exceed 1. These 22 ratios range from 0.6 to over 10, with a median of 2.56.

Conclusions

This comparison of 31 studies suggests that basing nonuse value solely on the responses of nonusers or uninformed respondents will underestimate nonuse value. Given the opportunity, respondents almost always report that their nonuse WTP exceeds their WTP for personal use. However, while the consistency of this finding across many studies increases its credibility, circumspection is advisable because nearly all of the estimates are based either on self-reported allocations of total WTP or on respondents' estimates of WTP given a hypothetical zero-use scenario. Additional studies of the type performed by Duffield et al. (1993), which do not directly rely on respondent breakdowns of total WTP, are needed.

The comparison of studies suggests that lower estimates are obtained (1) using a mail survey in contrast to personal interviews, (2) using an annual instead of a monthly payment period, and (3) using a contribution payment vehicle rather than increases in taxes and/or prices. Tests of these hypotheses are warranted if contingent valuation of nonuse value is to be used for policy decisions.

The hodgepodge of methods used by the contingent valuation studies summarized herein made comparison of the studies difficult. The assortment of methods is reasonable given the immature state of the field of contingent valuation and the lack of generally accepted guidelines. Nonetheless, the field would benefit from a series of systematic studies to test hypotheses such as those listed above, followed by an effort to standardize contingent valuation methodology.

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Table 1. Type of Goods in Nonuse Value Studies of the 1980s and 1990s	
Good Type	Number of Studies
Wildlife and fish protection	11
Water quality	6
Water flow or lake level	4
Air quality	3
Wilderness preservation	3
Forest quality	2
Wetland preservation	1
Beach restoration	1
TOTAL	31

Table 2. Survey Administration Method	
Method	Number of Studies[1]
Mail	18
Household interview	7
Phone	4
Onsite interview	2
Onsite self-administered	2
Onsite distribute, mail back	2
TOTAL	35

[1]Some of the 31 studies used two survey methods.

Table 3. Elicitation Method	
Method	Number of Studies[1]
Open-ended	12
Dichotomous choice	11[2]
Payment card	5
Bidding game	3
Other	3[3]
TOTAL	34[4]

[1]Studies using the method with a separate sample.
[2]Four of these studies asked an open-ended question after the dichotomous choice question. These studies are not also listed as open-ended in this table.
[3]Hoehn (1991) and Desvousges et al. (1983) used multiple elicitation methods. Clonts and Malone (1990) did not report elicitation method.
[4]Some of the 31 studies used different methods with different samples.

Table 4. Payment Vehicle	
Vehicle	Number of Studies
Contribution to a special fund	15
Increases in taxes and/or prices	6
Special tax	4
Utility bill	4
Payment to a special program	1
Not reported	2
TOTAL	31[1]

[1]In addition to these vehicles, some of the studies also used other vehicles for comparison.

Table 5. Payment Period	
Period	Number of Studies
Annual	22
Monthly	5
Onetime	2
Not reported	2
TOTAL	31

Table 6. Summary of Methods of Isolating Nonuse and Use Values		
Method	Nonuse value	Use value
1	WTP apportioned by respondent	WTP apportioned by respondent
2	WTP of nonuser subsample	WTP of user subsample minus WTP of nonuser subsample
3	WTP when respondents are asked to assume zero use	WTP of actual or potential users minus WTP when respondents are asked to assume zero use
4	WTP of all respondents, who are assumed to be nonusers	na
5	WTP apportioned based on responses to attitude and behavior questions	WTP apportioned based on responses to attitude and behavior questions
6	WTP based on a separate nonuse value question0.00	WTP based on a separate use value question

Table 7. Estimates of Annual Nonuse Value					
Method[1]	Number of Studies	Number of Estimates	Nonuse Value[1]		
			Low	High	Median[3]
1	12	17	$5	$184	$31
2	11	16	1	155	12
3	5	7	17	139	59
4	7	7	7	155	63
5	1	2	8	12	10
6	1	2	18	23	21
All	37[4]	51	$1	$184	$23

[1]Willingness to pay per household per year in 1990 dollars. The one-time payments are included here as annual payments. Where a range is listed in the Appendix table, the midpoint is used here.
[2]Method of isolating nonuse value. See Table 6.
[4]Given an even number of estimates, the median is the midpoint of the median pair.
[4]Some of the 31 studies used more than one method.

Table 8. Characteristics of Studies Producing the Highest and Lowest Estimates of Nonuse Value[1]						
WTP[2]	Study	Good	Administration	Payment period	Payment vehicle	Method[3]
Bottom quartile						
$1	Stoll & Johnson (1985)	crane habitat at Aransas	mail	annual	contribution	2
4	Duffield et al. (1993)	flow in 1-5 MT rivers	mail	annual	contribution	2
5	Gilbert et al. (1992)	Eastern wilderness	mail	annual	contribution	1
6	Whitehead & Blom.(1991)	wetland protection in KY	mail	annual	contribution	2
7	Boyle & Bishop (1987)	shiner habitat in WI.	mail	annual	contribution	4
10	Brookshire et al. (1983)	sheep habitat in WY	mail	annual	contribution	2
11	Silberman et al. (1992)	beach restoration in NJ	interview	one-time	contribution	2
Top quartile						
86	Hagen et al. (1991)	spotted owl habitat in CA	mail	annual	taxes/prices	4
92	Desvousges et al. (1983)	Monongahela R. quality	interview	annual	taxes/prices	3
127	Hoehn (1991)	Grand Canyon air quality	interview	monthly	program	4
139	Greenley et al. (1981)	Platte Basin water quality	interview	annual	tax	3
155	Schulze et al. (1983)	parklands visibility	interview	monthly	utility bill	4
155	Mitchell & Carson (1981)	river water quality in U.S.	interview	annual	taxes/prices	2
184	Loomis (1987a, b)	Mono Lake level/quality	mail	monthly	utility bill	1

[1]To avoid double-counting, no more than one estimate of nonuse value from a given study was included here. [2]WTP per household per year in 1990 dollars. [3] Method of isolating nonuse value. See Table 6.

Table 9. Ratio of Nonuse Value to Use Value[1]

Method[2] Nonuse/Use	Number of Studies	Number of Estimates	Ratio[3]		
			Low	High	Median[4]
1/1	12	17	0.97	10.74	2.56
2/2	9	12	0.11	3.89	0.85
3/3	3	3	0.60	7.57	3.17
5/5	1	2	3.17	7.32	5.25
3/6	2	3	0.85	2.97	1.46
All	23	34	0.11	10.74	1.92

[1]Where a range of WTP is listed in the Appendix table, the midpoint was used here.
[2]Methods of isolating nonuse and use values; see Table 6.
[3]All ratios are listed in the Appendix.
[4]Given an even number of estimates, the median is the midpoint of the median pair.

Table 10. Comparison of Users' and Nonusers' Nonuse Values					
Study	Method[2]	Definition of User Based on:	Nonuse Value[1]		
			Users	Nonusers	Ratio[3]
Clonts and Malone (1990)	1	past 3 years	$50	$28	1.82
Desvousges et al. (1983)	3	last year[4]	66	42	1.57
Duffield et al. (1993)	1	past 3 years	10	7	1.42
Greenley et al. (1981)	3	future	67	42	1.60
Silberman et al. (1992) past users	6/2	future	15	9	1.63
past nonusers	6/2	future	20	10	2.07
Walsh et al. (1985)	1	next year[5]	56	22	2.55

[1] WTP per household per year in nominal dollars. All value estimates are listed in the Appendix.
[2] Method of isolating nonuse value; see Table 6.
[3] Ratio of nonuse value of users to nonuse value of nonusers.
[4] Also considered users were any respondents who provided a use value.
[5] Users were certain of use next year. Nonusers were certain that they would not use the resource next year.

Table 11. Method 1 Ratios of Nonuse Value to Use Value for Users and Nonusers			
Study	Definition of User Based on:	Ratio[1]	
		Users	Nonusers
Clonts and Malone (1990)	past 3 years	2.56	1.72
Duffield et al. (1993)	past 3 years	1.94	2.72
Walsh et al. 91985)	next year	1.06	3.67

[1]Ratio of nonuse value to use value. All ratios are listed in the appendix.

Appendix. Contingent Valuation Studies Since 1980 Measuring Nonuse Value[a]

Author	Good	Sampled population	Survey administration (response rate)	Payment vehicle	Elicitation method	Mean annual nonuse WTP/hh (year of $), method (m),[b] sample size (n)	Mean annual use WTP/hh, method (m),[c] sample size (n)	Ratio: non-use WTP to use WTP[d]
Boyle and Bishop (1987)	maintaining and restoring bald eagle habitat in WI (the eagle would become extinct in WI without this effort)	WI taxpayers who recently contributed to the state's Endangered Resources program[1]	mail (81%)	annual membership in private foundation for this purpose[2]	dichotomous choice	$28.38 (na)[3] m=3 n=86	$46.93[4] m=3 n=99	0.60
"	"	"	"	"	"	$18.02 m=2a[5] n=123	$57.29 m=2[4] n=99	0.31
"	same for the striped shiner (a small, endangered fish)[6]	"	"	"	"	$5.55 m=4[7] n=435	na	--
Brookshire et al. (1983)	improvement of grizzly bear habitat within 15 years (decline in habitat expected without action)[1]	WY hunting license holders who will not hunt grizzly bears[2]	mail (~25%)	annual payment for a grizzly "stamp"[3]	open-ended	$15.20 (na) m=2b[4] n=170	$5.80 m=2[5] n=205	2.62
"	same, but for big horn sheep	same, but for big horn sheep[2]	"	same, but for big horn sheep[3]	"	$6.90 m=2b[4] n=108	$11.10 m=2[5] n=265	0.62

Appendix. Contingent Valuation Studies Since 1980 Measuring Nonuse Value[a]

Author	Good	Sampled population	Survey adminis-tration (response rate)	Payment vehicle	Elicita-tion method	Mean annual nonuse WTP/hh (year of \$), method (m),[b] sample size (n)	Mean annual use WTP/hh, method (m),[c] sample size (n)	Ratio: non-use WTP to use WTP[d]
Clonts and Malone (1990)	preserving 15 Alabama rivers in a free-flowing state	nonuser Alabama residents with phone listings[1]	phone (na)	na	na	\$27.50 (1987) m=1[2] n=630	\$16.00 m=1 n=630	1.72
"	"	user subset from above[3]	"	"	"	\$50.00 m=1[2] n=103	\$19.50 m=1 n=103	2.56
Cronin (1982)[1]	improvement of water quality in Potomac River from useable for recreation to boatable	Washington, D.C. along Potomac	onsite interview (75%)	na	open-ended	\$35.00[2] m=2b n=na	\$9.00[3] m=2 n=na	3.89
Desvousges et al. (1983)	keep the water quality in the Monongahela River from slipping back from level D (boatable) to level E (not usable for recreation)	residents of 5 PA counties in Monongahela watershed who used the river last year[1]	househol d interview (80%)	annual increase in taxes and prices	open-ended[2]	\$65.99[3] (1981) m=3 n=66	\$45.30[4] m=6 n=17	1.46
"	"	same, but nonusers last year	"	"	"	\$42.15[3] m=3 n=139	\$14.20[4] m=6 n=34	2.97

Appendix. Contingent Valuation Studies Since 1980 Measuring Nonuse Value[a]

Author	Good	Sampled population	Survey administration (response rate)	Payment vehicle	Elicitation method	Mean annual nonuse WTP/hh (year of $), method (m),[b] sample size (n)	Mean annual use WTP/hh, method (m),[c] sample size (n)	Ratio: non-use WTP to use WTP[d]
Desvousges et al. (1992)	protecting specified number of waterfowl in the Central Flyway from drowning in waste-oil holding ponds by covering the ponds with wire netting[1]	adult non-student shoppers in Atlanta GA	distribute and complete onsite (na)	annual price increase	open-ended[2]	$59.00 to $71.00[3] (1991) m=4[4] n=398 to 408[5]	na	--
Diamond et al. (1992)	protect specified wilderness areas in CO, MT, ID, or WY from timber harvest, given that 7 of the 57 designated wilderness areas in those states are already proposed for harvest	CO, MT, ID, and WY residents with phone listings	phone (62%)	annual federal income tax surcharge to respondent's household	open-ended	$23.27 to $58.54[1] (1991) m=4[2] n=144 to 151[3]	na	--
Duffield (1992)	support recovery of wolves in Yellowstone National Park (fund essential to recovery)[1]	visitors to the park	distribute onsite, return by mail (31%)	lifetime membership in trust fund	dichotomous choice[2]	$17.39[3] (1990) m=3 n=457	$5.48[4] m=3 n=~450	3.17
Duffield et al. (1993)	buy water to increase summer flows in selected MT rivers, to improve habitat and recreation[1]	residents of 6 urban areas in MT and WA with phone listings	mail (34%)	annual membership in a special trust fund	dichotomous choice[2]	$4.07 (1988) m=2a[3] n=254	$9.97 m=2 n=269	0.41

Appendix. Contingent Valuation Studies Since 1980 Measuring Nonuse Value[a]

Author	Good	Sampled population	Survey adminis-tration (response rate)	Payment vehicle	Elicita-tion method	Mean annual nonuse WTP/hh (year of $), method (m),[b] sample size (n)	Mean annual use WTP/hh, method (m),[c] sample size (n)	Ratio: non-use WTP to use WTP[d]
"	"	"	"	"	"	$7.14[4] m=5 n=554	$2.26[4] m=5 n=554	3.17
"	"	"	"	"	open-ended[5]	$9.33 m=2a[3] n=103	$6.54 m=2 n=124	1.43
"	"	"	"	"	"	$11.35[6] m=5 n=227	$1.55[6] m=5 n=227	7.32
"	"	same, but only users[7]	"	"	"	$10.47 m=1[8] n=124	$5.40 m=1[8] n=124	1.94
"	"	same, but only nonusers[3]	"	"	"	$7.37 m=1[9] n=103	$2.71 m=1[9] n=103	2.72
Gilbert et al. (1992)	protection and management of Eastern wilderness areas (assuming budget cuts eliminated all public funding and protection)[1]	residents of southern Vermont and parts of surrounding states[2]	mail (~27%)	annual contribution to a special trust fund	dichoto-mous choice[3]	$6.40 (1990) m=2a[4] n=78	$7.88 m=2 n=108	0.81
"	"	"	"	"	open-ended[5]	$4.78 m=1 n=195	$2.32 m=1 n=195	2.06

Appendix. Contingent Valuation Studies Since 1980 Measuring Nonuse Value[a]

Author	Good	Sampled population	Survey administration (response rate)	Payment vehicle	Elicitation method	Mean annual nonuse WTP/hh (year of $), method (m),[b] sample size (n)	Mean annual use WTP/hh, method (m),[c] sample size (n)	Ratio: non-use WTP to use WTP[d]
Greenley et al. (1981)[1]	postpone mining that would degrade water quality throughout the South Platte Basin (CO) enough to permanently preclude riparian recreation (3 photos used)	Denver and Fort Collins residents expecting to use sites for recreation in the future	household interview (na)	annual increase in sales tax payments[2]	bidding game	$67.00[3](1976) m=3 n=174	$79.28[4] m=6 n=174	0.85
"	"	same, but future nonusers	"	"	"	$41.95[3] m=3 n=24	na	--
Haefele et al. (1992)	protection programs (against insects and air pollution) for spruce-fir forests along roads and trails in the southern Appalachian Mountains (3 photos used)[1]	residents within 500 miles of Asheville, NC, with phone listings	mail (51%)	annual increase in taxes put into a special conservation fund	dichotomous choice	$50.75 (1991) m=1 n=306	$7.58 m=1 n=306	6.70
"	"	"	mail (53%)	"	payment card	$15.93 (1991) m=1 n=318	$1.48 m=1 n=318	10.74

Appendix. Contingent Valuation Studies Since 1980 Measuring Nonuse Value[a]

Author	Good	Sampled population	Survey administration (response rate)	Payment vehicle	Elicitation method	Mean annual nonuse WTP/hh (year of $), method (m),[b] sample size (n)	Mean annual use WTP/hh, method (m),[c] sample size (n)	Ratio: non-use WTP to use WTP[d]
Hageman (1985)	avoid reduction in California sea otter population from current level to a level that would eliminate chances of seeing the animal and could endanger the population[1]	California residents with phone listings	mail (21%)	annual flat tax paid by all U.S. households, plus an additional personal contribution[2]	payment card	$13.62 (1984) m=1 n=~173	$7.20 m=1 n=~173	1.89
Hagen et al. (1991)	protect northern spotted owls and their old-growth habitat in the Northwest	U.S. residents	mail (39%)	annual increase in taxes and wood product prices	dichotomous choice	$86.32 (1990) m=4 n=394	na	--
Hoehn (1991)[1]	83% improvement in air quality at the Grand Canyon[2]	Chicago residents	household interview (na)	monthly payment to an air quality program	several[3]	$82.79[4,5] (1980) m=4 n=182	na	--
King et al. (1986)	certain survival of a nearby herd of big horn sheep that would be lost with certainty without action	Tucson, AZ residents with phone listings listings	mail (59%)	annual membership in organization that would protect habitat	open-ended	$15.14 (1985) m=3 n=~500	$2.00 m=3 n=~500	7.57
Loomis (1987a)[1]	certain improvement in water level in Mono Lake, with associated water quality and habitat improvements	CA residents with phone listing[2]	mail (44%)	monthly water bill[3]	dichotomous choice	$80.48[4] (1986) m=1 n=~160	$14.20 m=1 n=~160	5.67

Appendix. Contingent Valuation Studies Since 1980 Measuring Nonuse Value[a]

Author	Good	Sampled population	Survey administration (response rate)	Payment vehicle	Elicitation method	Mean annual nonuse WTP/hh (year of $), method (m),[b] sample size (n)	Mean annual use WTP/hh, method (m),[c] sample size (n)	Ratio: non-use WTP to use WTP[d]
"	"	Mono Lake visitors contacted onsite	distribute onsite, return by mail (84%)	"[5]	"	$160.19[4] m=1 n=~100	$41.81 m=1 n=~100	3.38
Mitchell and Carson (1981)[1]	improving water quality in all U.S. river and lakes (from a current unspecified level) to fishable level	national household survey	household interview	annual increases in prices and taxes	payment card	$111.00[2] (1981) m=2a n=?[3]	$126.00 m=2 n=?	0.88
Olsen et al. (1991)	guaranteed doubling of Columbia River Basin salmon and steelhead fish runs	Pacific Northwest residents with phones	phone (72%)	monthly increase in electric bill[1]	open-ended	$26.52 (1989) m=2a,b[2] n=~300[4]	$47.64[3] m=2 n=390	0.56
Rahmatian (1987)	avoid a decrease in air quality and visibility at the Grand Canyon[1]	Denver residents	household interview (na)	monthly increase in electricity bill	open-ended	$47.52[3] (1981) m=1 n=75	$13.44 m=1 n=75	3.54
Rubin et al. (1991)	assure continued existence of the northern spotted owl	WA residents with phone listings	mail (23%)	annual payment[1]	payment card	$49.72 (1987) m=4[2] n=216	na	--

Appendix. Contingent Valuation Studies Since 1980 Measuring Nonuse Value[a]

Author	Good	Sampled population	Survey adminis-tration (response rate)	Payment vehicle	Elicita-tion method	Mean annual nonuse WTP/hh (year of $), method (m),[b] sample size (n)	Mean annual use WTP/hh, method (m),[c] sample size (n)	Ratio: non-use WTP to use WTP[d]
Schulze et al. (1983)	prevent a specified deterioration in visibility from the current average in parklands region (photos used)[1]	residents of Albuquerque, Los Angeles, Denver, and Chicago	househol d interview (na)	monthly increase in electric utility bill	payment card	$101.40[2] (1980) m=4[3] n=448	na[4]	--
Silberman et al. (1992)	quantified restoration and maintenance of selected northern NJ beaches damaged by recent erosion (photo used)	current users of selected nearby beaches, expected future users	onsite interview (na)	one-time contribution to a non-profit foundation, which would also collect entrance fees[1]	bidding game	$15.21[2](1985) m=6 n=1177	na[3]	--
"	"	same, but future nonusers	"	"	"	$9.34[2,4] m=2b n=754	na	--
"	same, but no photo used[3]	Northern NJ and Staten Is. residents who don't use the selected beaches but will in future	phone (na)	"	open-ended	$19.65[5] m=6 n=83	na[6]	--
"	"	same, but future nonusers	"	"	"	$9.51[4,5] m=2a,b n=138	na	--

Appendix. Contingent Valuation Studies Since 1980 Measuring Nonuse Value[a]

Author	Good	Sampled population	Survey administration (response rate)	Payment vehicle	Elicitation method	Mean annual nonuse WTP/hh (year of $), method (m),[b] sample size (n)	Mean annual use WTP/hh, method (m),[c] sample size (n)	Ratio: nonuse WTP to use WTP[d]
Stevens et al. (1991)	preserve and protect bald eagles in New England (extinction in region is assured without action)[1]	New England residents	mail (30%)	annual payment for the next 5 years to a private trust fund[2]	open-ended[3]	$15.81 (na) m=1 n=~85	$3.47[4] m=1 n=~85	4.56
Stoll and Johnson (1985)	effort to preserve essential whooping crane habitat (extinction certain without this effort) & exclusive entrance to refuge areas	visitors to Aransas National Wildlife Refuge in TX	distribute and complete onsite (67%)	annual membership in foundation that would work for crane preservation	dichotomous choice	$9.33 (1983) m=2b[2] n=30	$7.54 m=2 n=351	1.24
"	"	TX residents	mail[1] (36%)	"	"	$1.03 m=2b n=73	$9.64 m=2 n=176	0.11
Sutherland and Walsh (1985)	maintain water quality in Flathead Lake and River (in Montana) at current (pristine) level	residents of 4 Montana cities with phone listings	mail (61%)	annual payment to special fund for protecting water quality in the area	open-ended	$46.25[1] (1981) m=1 n=171	$18.08 m=1 n=171	2.56
Walsh et al. (1984)	protect 10 million acres of potential CO wilderness from certain development, allowing time for an informed decision about wilderness designation	Denver and Fort Collins residents	mail (41%)	annual payment to a special fund[1]	open-ended	$22.60 (1980) m=1 n=195	$23.23 m=1 n=195	0.97

Appendix. Contingent Valuation Studies Since 1980 Measuring Nonuse Value[a]

Author	Good	Sampled population	Survey administration (response rate)	Payment vehicle	Elicitation method	Mean annual nonuse WTP/hh (year of $), method (m),[b] sample size (n)	Mean annual use WTP/hh, method (m),[c] sample size (n)	Ratio: non-use WTP to use WTP[d]
Walsh et al. (1985)[1]	guaranteed protection of 11 specified rivers (development will begin without protection)[2]	CO resident nonusers[3]	mail (51%)	annual payment to a special trust fund	open-ended	$22.00 (1983) m=1 n=40	$6.00 m=1 n=40	3.67
"	"	user subset from above[4]	"	"	"	$56.00 m=1 n=59	$53.00 m=1 n=59	1.06
Walsh et al. (1990)	protection of specified level of live trees from insect damage in 11 CO national forests (3 photos used)[1]	Fort Collins residents with phone listings	househol d interview (~67%)[2]	annual increase in taxes and prices[3]	bidding game	$24.00 (1983) m=1 n=198	$23.00[4] m=1 n=198	1.04
Whitehead and Blomquist (1991)	preserve Clear Creek wetland (a large bottomland hardwood forest wetland in western KY) from potential development for surface coal mining	KY residents with phone listings with prior information about KY wetlands[1]	mail (31%)	annual contribution to the Wetland Preservation Fund for Clear Creek wetland	dichotomous choice	$17.48 (1989) m=2a[2] n=118	na	--
"	"	same, but without prior information	"	"	"	$5.56 m=2a[2] n=96	"	--

Appendix. Contingent Valuation Studies Since 1980 Measuring Nonuse Value[a]								
Author	Good	Sampled population	Survey administration (response rate)	Payment vehicle	Elicitation method	Mean annual nonuse WTP/hh (year of $), method (m),[b] sample size (n)	Mean annual use WTP/hh, method (m),[c] sample size (n)	Ratio: nonuse WTP to use WTP[d]
Whitehead and Groothuis (1992)	reduction in nonpoint source pollution in Tar-Pamlico River (NC) allowing anglers to catch twice as many fish per trip	residents of counties in Tar-Pamlico basin	mail (61%)[1]	annual contribution to a preservation fund for compensating farmers who use pollution control practices	open-ended	\$21.00 (1991) m=2b[2] n=65	\$14.00 m=2 n=26	1.50

"na" indicates not available (not reported); "hh" indicates household

[a] "Nonuse value" refers to WTP for the knowledge that a thing exists (usually called "existence value") and for the desire to make the thing available for others (usually called "bequest value"). "Use value" refers to WTP for current and future use, and for the option of future use (usually called "option value").

[b] Nonuse values were estimated by one of six methods:

1. asking respondents to allocate their total value into categories of value (nonuse value includes existence and bequest value), or asking a separate question about nonuse value;
2. asking total WTP to a sample including only persons who (a) had not used the resource or (b) would not use the resource;
3. asking total WTP to respondents who are asked to assume that they would not use the resource;
4. assuming all respondents are nonusers;
5. statistically separating total value into categories of value based responses about relevant attitudes and behavior;
6. asking separate questions about nonuse value.

[c] Use values were estimated by one of five methods:

1. asking respondents to allocate their total value into categories of value (use value includes option value);
2. the difference: total WTP of the subsample who reported that they had or would use the resource minus total WTP of the subsample who reported they had not or would not use the resource (where the latter is listed in the nonuse column);
3. the difference: total WTP when respondents are assumed to use or have the option to use the resource minus total WTP when respondents are asked to assume that they will not use the resource;
4. there is no value counterpart to nonuse value method 4;
5. statistically separating categories of value based responses about relevant attitudes and behavior;
6. asking separate use value questions.

[d] The components for this ratio are taken from the two columns to the left. Note that the ratios reflect various methods of separating use from nonuse WTP.

Boyle and Bishop (1987)

[1] This subsample is restricted to persons who had made a trip to view bald eagles. Recent noncontributors were also surveyed. Among the past nonviewers who had recently contributed, the mean bid of those who were told they would be able to view in the future was not significantly different from the mean bid of those who were told they would not be able to view the eagles.

[2] Although zero bidders were asked to explain why, all zero bids were included in the analysis.

[3] These respondents were past viewers who were told that the birds' habitat would be in remote parts of the state where viewing was not possible.

[4] These respondents were past viewers who were told that all members would be given information on how to conveniently view bald eagles in WI.

[5] These respondents were past nonviewers (reported they had never made a trip where one of their intentions was to view bald eagles) who were told they would be given information on how to conveniently view bald eagles in WI.

[6] This question followed the bald eagle question.

[7] This small fish is unlikely to be seen or recognized by recreationists.

Brookshire et al. (1983)

[1] The report also contains results for habitat improvements achieved within five years.

[2] The report also contains results for persons who expected to hunt the species.

[3] For future hunters of the species, the report is clear that this stamp was required for hunting. However, for future nonhunters (the results reported here), the report is not clear about what the stamp provided, other than the knowledge that the holder had contributed to a worthy cause. It is assumed herein that purchase of the stamp was similar to a contribution.

[4] Respondents who did not expect to observe the species.

[5] Use in this case was for observation of the species by persons who did not plan to hunt the species. Note that the authors assumed that the bids of users contained no existence value, whereas method 2 herein assumes that users' bids include existence value.

Clonts and Malone (1990)

[1] Subsample who reported that a household member had not used any of the 15 rivers in the last three years.

[2] Each respondent was asked in separate bidding questions to report WTP for use, option value, bequest value, and existence value; then respondents were asked three times to verify the individual and summed values, making this essentially a method 1 study.

[3] Subsample who reported that a household member had used one or more of the 15 rivers in the last three years.

Cronin (1982)

[1] As reported by Fisher and Raucher (1984), who also take some of their summary from another paper by Cronin, a forthcoming (in 1982) paper entitled "Estimating the use, option, and existence values for improved water quality," Pacific Northwest Laboratory, Richland, Washington.

[2] From table 5 of Fisher and Raucher. The nonuse value is for persons who said they would not use the Potomac even if it were as clean as they would like it to be.

[3] Users' total WTP ($44.00) was for persons who are present users or would use a cleaned-up river.

Devousges et al. (1983)

[1] Also included as users are those who provided a use value, even if they did not report using the river in the past year.

[2] For use value, subsamples of approximately equal size responded to (1) bidding game with $25 starting point, (2) bidding game with $125 starting point, (3) open-ended question, and (4) payment card. Responses varied widely depending on vehicle. Only the open-ended responses are reported here because only an open-ended question was used for existence value.

[3] Different categories of value were carefully described to respondents. For existence value, respondents were to assume they "would never use the river."

[4] WTP for future use, described as option price for users and option value for nonusers. The authors caution that some respondents may have failed to distinguish between option value and existence value, including some existence value in the future use value category and/or some future use value in the existence value category. This is in part indicated by the fact that some respondents gave the same estimate for existence value that they had given for option price. See also Fisher and Raucher (1984).

Devousges et al. (1992)

[1] The Central Flyway consists mainly of the Great Plains, from North Dakota and eastern Montana in the north to Texas and eastern New Mexico in the south. Note that this is far from the sampled population in Atlanta. Three subsamples were surveyed about WTP for protecting waterfowl from waste-oil ponds, differing in the number of birds (2,000, 20,000, and 200,000) that died from this in 1989 (and by implication the number of birds that would be saved if the netting were in place). This study also included subsamples that valued reductions in oil spills. Those results are not reported herein.

[2] This study also used a dichotomous choice response format, with the oil spill subsamples.

[3] Mean WTP for the three subsamples were: $59 for 2000 birds protected, $59 for 20,000 birds protected, and $71 for 200,000 birds protected. The differences were found to be not significant.

[4] Because of the long distance from the survey population to the Central Flyway, these responses are assumed here to represent nonuse value.

[5] Sample sizes of the three subsamples were 398, 408, and 399 in order of increasing number of birds protected.

Diamond et al. (1992)

[1] There were five subsamples, differing in the specific wilderness area(s) to be protected. The potentially protected areas and mean WTP follow: Selway Bitterroot ($58.54), Washakie ($23.27), Bob Marshall ($40.69), Selway Bitterroot and Washakie ($44.41), or Selway Bitterroot, Washakie, and Bob Marshall ($46.59). These estimates are with both protest ($0 WTP) and extreme value (>5% of annual income) responses removed. The survey was performed to test for degree of substitutability among goods (i.e., among wilderness areas). Differences in WTP between individual areas or between single areas and groups of areas were not found to be significant.

[2] Although respondents were asked about past use of the wilderness areas, all estimates reported in the paper are for the complete sample. Most respondents of this household survey can be assumed to be nonusers.

[3] The range in size across the five subsamples.

Duffield (1992)

[1] Other trust funds (e.g., the Nature Conservancy, Ducks Unlimited) were mentioned and loss of other animals because of the wolves was mentioned.

[2] Respondents not willing to pay the posited amount were asked if they would pay $1. An open-ended question followed, but only the dichotomous choice results are reported herein.

[3] Respondents were told "Suppose ... that you personally would not have an opportunity to see or hear wolves." Note that median WTP is reported here, not mean WTP.

[4] Respondents were told "Suppose ... that you personally might get to see or hear a wolf in Yellowstone..."

Duffield et al. (1993)

[1] Results presented here are for a combination of three subsamples; one focused on the Bitterroot River, another on the Big Hole River, and the third on a set of five western Montana rivers that included the Bitterroot and Big Hole.

[2] Respondents who were not willing to pay the posited amount were asked if they would pay $1.

[3] Respondents who had not visited the specified river(s) "in the last 3 years."

[4] The entries here assume that 76% of WTP of all respondents (past users and nonusers, and the single and 5-river subsamples combined) was attributable to nonuse interests. The 76% is the midpoint between the high (82.7%) and low (68.1%) estimates among the four equations reported by Duffield et al. (1993) using method 5.

[5] An open-ended question followed the dichotomous choice question.

[6] These entries assume that 88% of WTP was attributable to nonuse interests. This is the average of estimates from three equations using method 5 (86%, 87%, and 91%).
[7] Respondents who had visited the specified river(s) "in the last 3 years."
[8] The users' percentages, not reported in Duffield et al. (1993), are 66% to nonuse and 34% to use.
[9] The users' percentages, not reported in Duffield et al. (1993), are 79% to nonuse and 29% to use.

Gilbert et al. (1992)
[1] A separate sample was asked only about protection and management of Lye Brook Wilderness Area in southern Vermont.
[2] Specifically, the households of this sample were located from 25 to 75 miles from Lye Brook Wilderness Area. The households of the other sample (not reported here), who were asked only about Lye Brook Wilderness Area, were located within 25 miles of Lye Brook.
[3] The dichotomous choice results are medians, not means. Note that the report states that 35% of the responses were "unusable" and that respondents who bid $0 were asked why, but does not indicate what proportion of these were protests.
[4] Respondents who had never visited an Eastern wilderness area.
[5] Respondents were first asked for a dichotomous choice response to a single amount, and then asked the open-ended question.

Greenley et al. (1981)
[1] See also Walsh et al. (1978).
[2] Actual responses were in terms of an increase in sales tax rate, but respondents were told, based on household income and family size, what each 1/4 percent increase in tax rate would likely cost them in dollars per year. A monthly water bill payment vehicle was also used, but is not reported here.
[3] The nonuse value questions were prefaced with "If it were certain you would not use the South Platte River Basin for water-based recreation, would you be willing..." (Walsh et al. 1978:82).
[4] Respondents were asked four separate WTP questions, focusing on current use recreation value, option value, existence value, and bequest value, with the use and option value questions asked first. The report suggests that the four separate responses may have included some overlap. In particular, for users it seems most likely that the earlier responses, dealing with use value, may have included some nonuse value. This is especially likely because the payment vehicle was sales taxes, not recreation use fee.

Haefele et al. (1992)
[1] A second question was asked, about WTP for protection of all remaining spruce-fir forests in the southern Appalachian Mountains.

Hageman (1985)
[1] In addition to sea otters, blue or grey whales, bottlenose dolphins, and northern elephant seals were valued using separate descriptions but identical valuation questions. Results for these other species were similar: total WTP was about $25 for whales and about $18 for the dolphins and seals; percentages allocated to use and nonuse were very similar to those for otters.
[2] Respondents were instructed to "suppose ... the average responses to [the earlier question that determined the nationwide flat tax] did not provide enough funds ... please indicate any additional amount over and above your [earlier] response which your household would be willing to pay ... per year."

Hagen et al. (1991)
[1] Users were not differentiated from nonusers, but most respondents can be assumed to be nonusers.

Hoehn (1991)
[1] See also Randall et al. (1981).
[2] Photos were used to depict differences in air quality. This program was described as the only option for improving air quality.

[3] Randall et al. (1981) state that open-ended questions, payments cards, and bidding games were used. Hoehn does not specify which format was used for the data he presents.

[4] Few of the respondents can be expected to be Grand Canyon visitors, so most of this value can be assumed to be nonuse value.

[5] In 1981, a year after this estimate, 71 Chicago residents were asked to value the same change in Grand Canyon air quality in an "embedded" questioning format (Kahneman and Knetsch 1992). A CVM question about Chicago air quality alone was asked before a question about a combination of Chicago and Grand Canyon air quality. The value of the Grand Canyon air quality improvement is estimated as $11.50, the difference: WTP for the combination of a 100% improvement in Chicago air quality and a 83% improvement in Grand Canyon air quality ($190) minus WTP for a 100% improvement in Chicago air quality ($179). A considerable portion of the total bids is likely to be use value.

Loomis (1987a)

[1] See Loomis (1987b) for the short version.

[2] About one-third of the households had visited Mono Lake sometime, and very few of the respondents were expected to be current or future users.

[3] Two other subsamples used a trust fund payment vehicle or a water bill vehicle with uncertainty about improvement. Twenty percent of the respondents were determined to have protested this form of payment for maintaining lake levels, preferring, for example, that Los Angles residents pay.

[4] WTP for both levels of improvement combined (alternative 3 minus alternative 1).

[5] About 17% protested this payment vehicle.

Mitchell and Carson (1981)

[1] These estimates are reported by Devousges et al. (1983). See Mitchell and Carson (1989) for examples of the payment cards.

[2] Nonusers were those who said they did not participate in in-stream freshwater activities during the past two years (does not preclude future users, and therefore, the bid is likely to include some option price).

[3] Based on a sample of 1,576 respondents, 39% of whom were nonusers; four sets of payment card anchor points were used, and it is unclear whether the results reported here are for all four subsets or only some of the subsets.

Olsen et al. (1991)

[1] About 16% of the nonusers and 19% of the users protested the payment vehicle.

[2] This nonuser subsample reported that they had not fished for these species in the past five years and did not expect to do so in the next five years. A third subsample was also surveyed, consisting of past nonusers who were uncertain about fishing in the next five years. Their mean WTP was $58.56 per year, and was interpreted to contain option price in addition to nonuse value.

[3] The user subsample reported that they had fished for these species in the past two years.

[4] Assumes that roughly 50% of the past nonusers reported in the paper were in the uncertain future user subsample.

Rahmatian (1987)

[1] Photos were used to depict current and the potential lower level of air quality.

Rubin et al. (1991)

[1] A specific payment vehicle was not used. Respondents were asked for the "largest amount that you would be willing to pay per year to be 100% sure that the northern spotted owl will exist in the future."

[2] Users were not differentiated from nonusers, but most respondents can be assumed to be nonusers.

Schulze et al. (1983)

[1] Parklands included the following national parks: Grand Canyon, Zion, Mesa Verde, Bryce Canyon, and Canyonlands. The study also valued increases in Grand Canyon air quality and prevention of plumb blight

seen from Grand Canyon. Several air quality levels were depicted on photos; the results reported here are for avoiding a drop from level C to level B.

[2] A simple average of the WTP of the four sample populations, which ranged from $79 per year for Denver to $116 for Los Angeles. Based on separate questions for the Grand Canyon (simple average of $54 per year) and the other parks ($47).

[3] Some of this bid could be user value. However, use of people living so far from the sites would generally be low. The authors concluded that "visitation plans were not an overwhelming factor in determining bids" and "knowledge acquired through past visits was also of relatively little importance" (p. 168).

[4] The study also estimated a use value using an entrance fee payment vehicle. However, the authors did not report any use rates, so annual WTP could not be computed.

Silberman et al. (1992)

[1] Although 56% of the future nonusers and 36% of the future users bid $0 WTP to the fund, only 7% on average were judged to be protests. However, respondents who bid $0 and indicated "the existence of a new beach would be of value, but it is not fair to ask for contributions to pay for it" were not considered protests.

[2] The nonuse question said in part: "The previous questions were based on your possible use of the new beaches shown in the picture. It may be worth something to you simply knowing that more people will be able to use the beach or because you believe more beaches are good for your community. For example, you might be willing to pay something to maintain a public park even though you won't use it" (p. 227).

[3] Before the existence value question, future users were asked a use value question, but those responses were not reported in the paper.

[4] The authors attribute the difference between future users' and nonusers' bids to improved quality of beach for intended use, and not to differences in personal characteristics of the two subsamples.

[5] Respondents were also told "Remember that there are many worthy causes to contribute to, and that you only have so much money for contributions."

[6] It is not clear from the paper whether these future users were asked about use value as were future users in the onsite survey.

Stevens et al. (1991)

[1] This study also valued wild turkey, salmon, and coyote using a similar approach.

[2] Although 80% reported that bald eagles were "important" to them, 58% reported 0 WTP in protest. Of the protests, 40% were of the payment vehicle (taxes or license fees were preferred, and 25% protested the effort to quantify the economic value.

[3] This open-ended response was preceded by a dichotomous choice response.

[4] The authors report that about 82% of the payment was allocated to existence and bequest value, and that 7% was allocated to use. We assume here that the remaining 11% is option value (to be added to the use category).

Stoll and Johnson (1984)

[1] Results were also reported for a mail survey of non-Texas residents.

[2] Respondents did not anticipate future visitation to Aransas.

Sutherland and Walsh (1985)

[1] This estimate is for users and nonusers combined. The authors report that users' (someone in the household visited the area in the past year) mean WTP exceed nonusers' mean WTP by $8 for option value, $30 for existence value, and $32 for bequest value; actual estimates for these two groups could not be determined.

Whitehead and Blomquist (1991)

[1] Neither this group (with information) or the no-information group had previously visited Clear Creek wetland. "Information" refers to past experience with wetlands by means of onsite use of a KY wetland or offsite use via television, conservation organization literature, and the like. Of course, the survey itself provides some information about wetlands. Note also that lack of prior onsite use of Clear Creek wetland does not preclude future use of the area; the study apparently did not ask for prediction about future use of the site. Also note

that if "information" were considered to be use, then this listing could be changed to show a nonuse value of $5.56 and a use value of $11.92 (using method 2a).

[2] Respondents who had never participated in onsite use of Clear Creek wetland.

Whitehead and Groothuis (1992)

[1] 89% of these answered the contingent valuation question.

[2] Respondents who would not fish the Tar-Pamlico in the future.

Walsh et al. (1984)

[1] About 11% protested.

Walsh et al. (1985)

[1] See also Sanders et al. (1990).

[2] One half the respondents were told to assume that development would begin "next year" without protection, while the other half were told that there was a 50% chance that development would begin next year. Bids of the uncertain subsample were 20% lower than those of the certain subsample. Results reported here are for the combination of the two subsamples.

[3] Subsample who reported 0.0 probability that a member of the household would use of one or more of the 11 rivers "next year".

[4] Subsample who reported 1.0 probability that a member of the household would use of one or more of the 11 rivers "next year". A third subsample, not included herein, reported a probability >0 but <1 of use during the next year.

Walsh et al. (1990)

[1] Six other environmental goods were also valued; WTP responses for these goods were included as independent variables.

[2] Potential respondents were first contacted by mail with a description of the good and the purpose of the study. They were then contacted by phone to arrange a time for the personal interview.

[3] Only 4% protested.

[4] The total value for a specific region of Colorado (northern Front Range) was $61 when only that region was valued and $33 when that region and the rest of Colorado were each independently valued.

The Hedonic Demand Model:

Comparative Statics and Prospects for Estimation

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The Hedonic Demand Model: Comparative Statics and Prospects for Estimation

The hedonic model originally developed by Rosen (1974) has often been cited as a useful framework for understanding consumer demand for non-marketed environmental goods (Ridker, 1967; Freeman, 1979; Palmquist 1990) as well as markets in manufactured differentiated products (Griliches, 1961; Rosen, 1974). Rosen's work, however, contains two related but distinct weaknesses which have generated some confusion among subsequent researchers. These two problem areas are first, the proper definition of the demand functions for the characteristics or attributes of which hedonic goods are composed and second, the econometric identification of the parameters of those attribute demand functions.

Rosen originally defined a class of goods composed of characteristics or attributes (Z) that were demanded by consumers but could only be purchased as tied bundles rather than individually as are ordinary goods. The bundles were sold at a price [Ph(Z)] that varied as a nonlinear function of the quantity of each characteristic in the bundle. The optimal bundle and, hence, the optimal quantity of each characteristic and the price of the optimal bundle are found where the ratio of marginal utilities of all possible pairs of characteristics are just equal to the ratio of the partial derivatives of the price function with respect to the characteristics.

Rosen's suggested econometric procedure for identifying the demand functions for these characteristics first estimates the hedonic price (Ph) as a function of the quantities of the various characteristics (Z) that are bundled together as the hedonic good. Rosen then proposes taking the first derivative of the estimated hedonic price function with respect to each characteristic. These marginal implicit price functions are used to calculate estimated prices for each characteristic. The estimated characteristic prices are to be employed in the same manner as are ordinary goods prices in a "garden variety" simultaneous estimation of supply and demand functions for ordinary goods.

There are several problems with this procedure. First, if the hedonic price function is linear with respect to the characteristics, then there is no variation in the characteristic prices. This is not a serious problem, however, since a linear hedonic price function implies that costless repackaging of characteristics is possible and the hedonic model will therefore offer no improvements over ordinary demand and supply models (Rosen, 1974, and Parsons, 1984).

If, on the other hand, the estimated hedonic price function is nonlinear, demand and supply functions for characteristics will be underidentified unless (possibly arbitrary) restrictions are imposed on the functions.

To see this, consider a simple system with two characteristics. Such a system must have four equations: two for characteristic quantities demanded and two for characteristic quantities supplied. The system can be written as:

(1) $$\text{Demand: } z_i = \alpha \partial Ph(Z)/\partial z_i + \beta D + e_{di} \quad (i = 1,2),$$

(2) $$\text{Supply: } z_i = \gamma \partial Ph(Z)/\partial z_i + \delta S + e_{si} \quad (i = 1,2),$$

with D and S representing demand and supply shifters, e_i representing the error terms, and demand equal to supply as an equilibrium condition. But any supply (demand) shifter will affect demand (supply) through the characteristic price variables $\partial Ph(Z)/\partial z_i$ and therefore all supply (demand) shifters belong in the demand (supply) equation and neither equation is identified.

But it is also unclear if equations (1) and (2) are correct specifications of demand and supply functions for the characteristics. In the hedonic system the marginal price paid for the last unit purchased is an endogenous price that is chosen simultaneously with the quantity purchased. Since Z appears on both the left and right hand sides of equations (1) and (2), we have effectively estimated the quantity demanded as a function of the quantity demanded!

This paper will differentiate itself from Rosen's work by defining the hedonic price function as an n-dimensional surface, measured in dollars, upon which the consumer locates by choosing a vector of characteristics (Z). But the shape of the surface (i.e. the specific dollar value of any given vector of characteristics) is exogenous to the consumer. This implies a new question -- one that has been obscured by defining the hedonic price function surface as a vector of characteristics alone. How does the consumer react to changes in the hedonic price function? We will investigate this question by defining the hedonic price function as [Ph(Z,B)] where (Z) is the vector of characteristics that define a particular bundle and (B) is an associated vector that defines how particular bundles are mapped into a dollar price. Changes in the (B) vector can then be used to define changes in the hedonic price surface and in turn show how the individual consumer's optimal choice of each characteristic in a bundle responds to exogenous changes in the price that must be paid for the bundle.

This approach to the hedonic problem has been previously investigated by Parsons (1984) and Edlefsen (1981). The present paper extends some of their results by defining an additional set of functions that have no real analogue in models with only linear budget constraints and linear prices. These "indirect hedonic price functions" can be used to provide a method for calculating welfare change measures in the absence of specific estimates of the characteristic demand functions.

The first section of the paper discusses the definitions of the Marshallian and Hicksian demand functions and indirect hedonic functions when the hedonic price function is defined as Ph(Z,B). Section 2 investigates the comparative statics of the model. Section 3 examines some of the functions that must be estimated when measuring welfare change in the hedonic context. The final section of the paper discusses the problem of using cross sectional data in the econometric identification of characteristic demand functions in housing markets.

The Demand for Characteristics

The demand side of the hedonic model assumes that when individuals purchase a hedonic good, they are buying a bundle of characteristics that are sold as a package. Consumers do not value the bundle per se, but instead value the characteristics that make up the bundle. This assumption allows the utility function to be written in the form:

$$U[z_1,\ldots,z_n,x_1,\ldots,x_m] \tag{3}$$

where each z_i represents a quantity of one of the n characteristics associated with the hedonic good and each x_j represents a quantity of one of the m other goods available for purchase in the economy.

Where the hedonic model departs from a more traditional model of consumer purchasing is in the treatment of the individual's budget constraint. Here, the characteristics are sold as a bundle and it is the bundle that has an explicit price and not the individual characteristics. These assumptions require the demander's budget constraint to be written as:

$$y = Ph(z_1,\ldots,z_n,\beta_1,\ldots,\beta_n) + \Sigma_1^m p_i x_i, \tag{4}$$

where $Ph(z_1,\ldots,z_n,\beta_1,\ldots,\beta_n)$ is the price of the hedonic good defined by characteristics Z -- to be chosen by the consumer --and parameters (B) which are fixed at any moment by an economy-wide interaction of market supply and demand forces and which are therefore exogenous to any single consumer.

This budget constraint, although nonlinear, does imply two properties that are usually associated only with linear budget constraints: namely the Engel and Cournot aggregation conditions. The Engel aggregation condition is derived by differentiating the budget constraint with respect to income:

(5) $$\sum_{i=1}^{m} p_i \cdot \frac{\partial x_i}{\partial y} + \sum_{j=1}^{n} \frac{\partial Ph}{\partial z_j} \cdot \frac{\partial z_j}{\partial y} = 1.$$

In this case, however, the $(\partial Ph(Z,B)/\partial z_j)$ terms are not explicit and observable prices as are the (p_i) prices for the ordinary goods (Rosen, 1974). They are instead referred to as the marginal implicit prices of the characteristics. Notice that the marginal implicit prices of the characteristics are endogenous functions whose equilibrium values are chosen as the result of the optimal choice of the quantity of each characteristic purchased. The marginal implicit prices are not exogenous variables as are the prices of the other goods in the economy.

Equation (5) can be rewritten in terms of expenditure shares $(\varpi_{i,j})$ and income elasticities $(\epsilon_{iY,jY})$ as:

(6) $$\sum_{i=1}^{m} \omega_i \varepsilon_{iY} + \sum_{j=1}^{n} \omega_j \varepsilon_{jY} = 1.$$

In a similar fashion, the Cournot aggregation condition can be derived by differentiating the budget constraint with respect to any one of the exogenous prices of the ordinary goods (p_i):

(7) $$x_i + \sum_{j=1}^{m} p_j \cdot \frac{\partial x_j}{\partial p_i} + \sum_{k=1}^{n} \frac{\partial Ph}{\partial z_k} \cdot \frac{\partial z_k}{\partial p_i} = 0.$$

Equation (7) can also be rewritten in terms of the budget shares (ϖ) and price elasticities (ϵ):

(8) $$\omega_i + \sum_{j=1}^{m} \omega_j \varepsilon_{ji} + \sum_{k=1}^{n} \omega_k \varepsilon_{ki} = 0.$$

The consumer's utility maximization problem can now be written as:

(9) $$\text{Max } U[Z,X] \text{ subject to } Ph(Z,B) + PX \leq y$$

with choice variables Z (the vector of quantities of the n characteristics associated with the hedonic good) and X (the vector of quantities of the m other goods in the economy).

The Lagrangian for the problem is:

(10) $$L = U[Z,X] + \lambda(y - Ph(Z,B) - PX).$$

The first order necessary conditions are:

(11) $$\partial L/\partial z_i = \partial U/\partial z_i - \lambda(\partial Ph(Z,B)/\partial z_i) = 0 \ \forall i,$$

(12) $$\partial L/\partial x_j = \partial U/\partial x_j - \lambda p_j = 0 \ \forall j, \text{ and}$$

(13) $$\partial L/\partial \lambda = y - Ph(Z,B) - PX = 0.$$

If we compare equation (11) by equation (12), we can derive Rosen's condition that a utility maximizing consumer will equate the ratio of marginal utilities of any pair of goods i and j with their price ratios:

(14) $$[\partial U/\partial z_i]/[\partial U/\partial x_j] = [\partial Ph(Z)/\partial z_i]/[p_j].$$

With the number of equations equal to the number of unknowns, it should in principle be possible to solve equations (11), (12), and (13), for the optimal values of λ, Z, and X as functions of the consumer's income and the exogenous prices. However, since neither the hedonic price Ph(Z,B) nor the marginal implicit prices $\partial Ph(Z,B)/\partial z_i$ are truly exogenous variables, they will not appear in an explicit form in the demand functions for either the characteristics Z or the goods X. Instead, the demand functions for the characteristics as well as the ordinary goods will take the form:

(15) $$z_i = z_i(P, y, B) \text{ and } x_j = x_j(P, y, B)$$

These characteristic demand functions are conditional demands; they are conditioned on the underlying parameter values of the hedonic price function. These parameters therefore play much the same role as do ordinary prices in the demand functions for ordinary goods[1].

If these hedonic demand functions are inserted into the original utility function, an indirect utility function can be defined:

(16) $$V = V[Z(P,y,B), X(P,y,B)] = V[P,y,B].$$

This function shows the maximum utility that the consumer can attain given the exogenous prices (P), income (y), and hedonic price function parameters (B).

Just as it is possible to derive Marshallian demands and indirect utility as functions of (P,y,B) so is it possible to derive Hicksian demand functions and the expenditure function. This approach requires the consumer to minimize expenditures (e) subject to a given utility level and hedonic price function:

(17) $$\text{Min } e = PX + Ph(Z,B), \text{ subject to } U^* = U(Z,X).$$

The Lagrangian for the problem is:

(18) $$\mathcal{L} = PX + Ph(Z,B) + \lambda[U^* - U(Z,X)].$$

The first order necessary conditions are:

(19) $$\partial \mathcal{L}/\partial z_j = \partial Ph(Z,B)/\partial z_j - \lambda(\partial U/\partial z_j) = 0 \quad \forall j,$$

(20) $$\partial \mathcal{L}/\partial x_i = p_i - \lambda(\partial U/\partial x_i) = 0 \quad \forall i,$$

(21) $$\partial\mathcal{L}/\partial\lambda = U^* - U(Z,X).$$

Solving for the choice variables (Z and X) in terms of the exogenous variables (P and U^*) and parameters (B) will yield a set of Hicksian demand functions:

(22) $$z_j = z_j(P,U^*,B) \text{ and } \mathbf{x_i = x_i(P,U^*,B)}.$$

If these demand functions are inserted into the budget constraint, an expenditure function can be defined:

(23) $$e = e[Z(P,U^*,B), X(P,U^*,B)] = e(P,U^*,B).$$

There are two additional functions that appear in the hedonic model that are not found in ordinary demand models. These are the indirect Marshallian and indirect Hicksian hedonic price functions. Each is derived by inserting the appropriate Marshallian or Hicksian demand functions into the hedonic price function. The two functions show the utility maximizing expenditure on the hedonic good as a function of (P,y,B) or (P,U^*,B). For the Marshallian this gives:

(24) $$Ph(Z,B) = Ph[Z(P,y,B),B] = Ph(P,y,B).$$

The Hicksian, on the other hand, may be represented as:

(25) $$Ph(Z,B) = Ph[Z(P,U^*,B),B] = Ph(P,U^*,B).$$

Comparative Statics Results

Given the dependence of each of the functions defined in equations (15), (16), (22), (23), (24), and (25) on the variables (P,y,U^*) and the parameters (B), it is worth investigating the comparative statics effects of changes in (P,y,B) on each of the functions. Roy's identity, the Slutsky equation, and Shephard's lemma are derived below for the hedonic model. An additional identity unique to the hedonic model is also derived below.

In order to derive Roy's identity, begin by differentiating the indirect utility function with respect to one of the hedonic parameters (β_i):

$$(26) \quad \frac{\partial V(P,y,B)}{\partial \beta_i} = \sum_{j=1}^{n} \frac{\partial U[Z(P,y,B),X(P,y,B)]}{\partial z_j} \cdot \frac{\partial z_j}{\partial \beta_i} + \sum_{k=1}^{m} \frac{\partial U[Z(P,y,B),X(P,y,B)}{\partial x_k} \cdot \frac{\partial x_k}{\partial \beta_i}.$$

The first order conditions from equations (11) and (12) imply:

$$(27) \quad \frac{\partial V(P,y,B)}{\partial \beta_i} = \lambda \left(\sum_{j=1}^{n} \frac{\partial Ph}{\partial z_j} \cdot \frac{\partial z_j}{\partial \beta_i} + \sum_{k=1}^{m} p_k \cdot \frac{\partial x_k}{\partial \beta_i} \right).$$

Differentiating the budget constraint with respect to (β_i) gives:

$$(28) \quad 0 = \frac{\partial Ph}{\partial \beta i} + \left(\sum_{j=1}^{n} \frac{\partial Ph[Z(P,y,B)]}{\partial z_j} \cdot \frac{\partial z_j}{\partial \beta_i} + \sum_{k=1}^{m} p_k \cdot \frac{\partial x_k}{\partial \beta_i} \right).$$

Substituting (28) into (27) suggests:

$$(29) \quad \frac{\partial V(P,y,B)}{\partial \beta_i} = - \lambda \frac{\partial Ph}{\partial \beta_i}$$

Differentiating the indirect utility function with respect to (y) gives:

$$(30) \quad \frac{\partial V(P,y,B)}{\partial y} = \sum_{j=1}^{n} \frac{\partial U(Z,X)}{\partial z_j} \cdot \frac{\partial z_j}{\partial y} + \sum_{k=1}^{m} \frac{\partial U(Z,X)}{\partial x_k} \cdot \frac{\partial x_k}{\partial y}.$$

Again, substituting the first order conditions (11) and (12) into equation (30) shows:

$$(31) \quad \frac{\partial V(P,y,B)}{\partial y} = \lambda \left(\sum_{j=1}^{n} \frac{\partial Ph}{z_j} \cdot \frac{\partial z_j}{\partial y} + \sum_{k=1}^{m} p_k \cdot \frac{\partial x_k}{\partial y} \right).$$

But from the Engel aggregation condition of equation (5), the term in brackets on the right hand side of equation (31) is equal to one. This implies that the right hand side of equation (31) is equal to (λ).

Roy's identity for the hedonic model is therefore:

(32)
$$-\frac{\left(\frac{\partial V(P,y,B)}{\partial \beta_i}\right)}{\left(\frac{\partial V(P,y,B)}{\partial y}\right)} = \frac{\partial Ph}{\partial \beta_i}.$$

The Slutsky equation for a change in (β_i) can be derived by differentiating the (n+m+1) first order conditions of equations (11), (12), and (13) with respect to (β_i):

(33)
$$\sum_{k=1}^{n} \frac{\partial(\frac{\partial U}{\partial z_j})}{\partial z_k} \cdot \frac{\partial z_k}{\partial \beta_i} + \sum_{l=1}^{m} \frac{\partial(\frac{\partial U}{\partial z_j})}{\partial x_l} \cdot \frac{\partial x_l}{\partial \beta_i} - \frac{\partial Ph}{\partial z_j} \cdot \frac{\partial \lambda}{\partial \beta_i} -$$

$$\lambda \sum_{k=1}^{n} \frac{\partial(\frac{\partial Ph}{\partial z_j})}{\partial z_k} \cdot \frac{\partial z_k}{\partial \beta_i} - \lambda \frac{\partial(\frac{\partial Ph}{\partial z_j})}{\partial \beta_i} = 0 \quad \forall\, z_{j\,=\,1\ to\ N}\,;$$

(34)
$$\sum_{j=1}^{n} \frac{\partial(\frac{\partial U}{\partial x_k})}{\partial z_j} \cdot \frac{\partial z_j}{\partial \beta_i} + \sum_{l=1}^{m} \frac{\partial(\frac{\partial U}{\partial x_k})}{\partial x_l} \cdot \frac{\partial x_l}{\partial \beta_i} - p_{xk} \frac{\partial \lambda}{\partial \beta_i} = 0 \quad \forall\, x_{k\,=\,1\ to\ M}\,;$$

and

(35)
$$-\sum_{j=1}^{n} \frac{\partial Ph}{\partial z_j} \cdot \frac{\partial z_j}{\partial \beta_i} - \sum_{k=1}^{m} p_{xk} \frac{\partial x_k}{\partial \beta_i} - \frac{\partial Ph}{\partial \beta_i} = 0.$$

Rearranging these equations in matrix form and defining (U_{ij}) as the second derivative of utility with respect to (z_i, z_j, x_i, or x_j), ($Ph_{zi,zj}$) as the second (or first where appropriate) derivative of the hedonic price equation with respect to (z_i and/or z_j), and (p_{xi}) as the price of ordinary good x_i gives:

(36)

$$
\begin{bmatrix}
(U_{z1,z1} - \lambda Ph_{z1,z1}) & \cdots & (U_{z1,zn} - \lambda Ph_{z1,zn}) & (U_{z1,x1}) & \cdots & (U_{z1,xm}) & (-Ph_{z1}) \\
\vdots & \cdots & \vdots & \vdots & \cdots & \vdots & \vdots \\
(U_{zn,z1} - \lambda Ph_{zn,z1}) & \cdots & (U_{zn,zn} - \lambda Ph_{zn,zn}) & (U_{zn,x1}) & \cdots & (U_{zn,xm}) & (-Ph_{zn}) \\
(U_{x1,z1}) & \cdots & (U_{x1,zn}) & (U_{x1,x1}) & \cdots & (U_{x1,xm}) & (-p_{x1}) \\
\vdots & \cdots & \vdots & \vdots & \cdots & \vdots & \vdots \\
(U_{xm,z1}) & \cdots & (U_{xm,zn}) & (U_{xm,x1}) & \cdots & (U_{xm,xm}) & (-p_{xm}) \\
(-Ph_{z1}) & \cdots & (-Ph_{zn}) & (-p_{x1}) & \cdots & (-p_{xm}) & (0)
\end{bmatrix}
$$

$$
\times \begin{bmatrix}
\partial z_1/\partial\beta_i \\
\vdots \\
\partial z_n/\partial\beta_i \\
\partial x_1/\partial\beta_i \\
\vdots \\
\partial x_m/\partial\beta_i \\
\partial\lambda/\partial\beta_i
\end{bmatrix}
=
\begin{bmatrix}
\lambda\partial Ph_{z1}/\partial\beta_i \\
\vdots \\
\lambda\partial Ph_{zn}/\partial\beta_i \\
0 \\
\vdots \\
0 \\
\partial Ph/\partial\beta_i
\end{bmatrix} .
$$

Using Cramer's rule to solve for the effect of a change in (β_i) on (z_j) gives:

(37)

$$\frac{\partial z_j}{\partial \beta_i} = \frac{\begin{vmatrix} (U_{z1,z1}-\lambda Ph_{z1,z1}) & \cdots & (U_{z1,zj-1}-\lambda Ph_{z1,zj-1}) & (\lambda \partial Ph_{z1}/\partial \beta_i) & (U_{z1,zj+1}-\lambda Ph_{z1,zj+1}) & \cdots & (U_{z1,zn}-\lambda Ph_{z1,zn}) & (U_{z1,x1}) & \cdots & (U_{z1,xm}) & (-Ph_{z1}) \\ \vdots & \cdots & \vdots & \vdots & \vdots & \cdots & \vdots & \vdots & \cdots & \vdots & \vdots \\ (U_{zn,z1}-\lambda Ph_{zn,z1}) & \cdots & (U_{z1,zj-1}-\lambda Ph_{z1,zj-1}) & (\lambda \partial Ph_{zn}/\partial \beta_i) & (U_{zn,zj+1}-\lambda Ph_{zn,zj+1}) & \cdots & (U_{zn,zn}-\lambda Ph_{zn,zn}) & (U_{zn,x1}) & \cdots & (U_{zn,xn}) & (-Ph_{zn}) \\ (U_{x1,z1}) & \cdots & (U_{x1,zj-1}) & (0) & (U_{x1,zj+1}) & \cdots & (U_{x1,zn}) & (U_{x1,x1}) & \cdots & (U_{x1,xm}) & (-p_{x1}) \\ \vdots & \cdots & \vdots & \vdots & \vdots & \cdots & \vdots & \vdots & \cdots & \vdots & \vdots \\ (U_{xm,z1}) & \cdots & (U_{xm,zj-1}) & (0) & (U_{xm,zj+1}) & \cdots & (U_{xm,zn}) & (U_{xm,x1}) & \cdots & (U_{xm,xm}) & (-p_{xm}) \\ (-Ph_{z1}) & \cdots & (-Ph_{zj-1}) & (\partial Ph/\partial \beta_i) & (-Ph_{zj+1}) & \cdots & (-Ph_{zn}) & (-p_{x1}) & \cdots & (-p_{xm}) & (0) \end{vmatrix}}{|H|}$$

where | H | is the determinant of the bordered Hessian of second order partial derivatives.

With n characteristics and m ordinary goods, equation (37) can be expanded to give:

(38)

$$\frac{\partial z_j}{\partial \beta_i} = \frac{\lambda \sum_{k=1}^{n} \frac{\partial Ph_{zk}}{\partial \beta_i}(-1)^{(k+j)}|M_{k,j}|}{|H|} + \frac{\sum_{l=1}^{m}(0)(-1)^{(n+l+j)}|M_{n+l,j}|}{|H|} + \frac{\frac{\partial Ph}{\partial \beta_i}(-1)^{(n+m+1+j)}|M_{n+m+1,j}|}{|H|}$$

where | M | is the determinant of the minor formed by the elimination of the kth row and jth column from the determinant in the numerator of equation (37).

Next, the first order conditions for utility maximization are differentiated with respect to y to give:

(39)

$$\begin{bmatrix} (U_{z1,z1} - \lambda Ph_{z1,z1}) & \cdots & (U_{z1,zn} - \lambda Ph_{z1,zn}) & (U_{z1,x1}) & \cdots & (U_{z1,xm}) & (-Ph_{z1}) \\ \vdots & \cdots & \vdots & \vdots & \cdots & \vdots & \vdots \\ (U_{zn,z1} - \lambda Ph_{zn,z1}) & \cdots & (U_{zn,zn} - \lambda Ph_{zn,zn}) & (U_{zn,x1}) & \cdots & (U_{zn,xm}) & (-Ph_{zn}) \\ (U_{x1,z1}) & \cdots & (U_{x1,zn}) & (U_{x1,x1}) & \cdots & (U_{x1,xm}) & (-p_{x1}) \\ \vdots & \cdots & \vdots & \vdots & \cdots & \vdots & \vdots \\ (U_{xm,z1}) & \cdots & (U_{xm,zn}) & (U_{xm,x1}) & \cdots & (U_{xm,xm}) & (-p_{xm}) \\ (-Ph_{z1}) & \cdots & (-Ph_{zn}) & (-p_{x1}) & \cdots & (-p_{xm}) & (0) \end{bmatrix} \times \begin{bmatrix} (\partial z_1/\partial y) \\ \vdots \\ (\partial z_n/\partial y) \\ (\partial x_1/\partial y) \\ \vdots \\ (\partial x_m/\partial y) \\ (\partial \lambda/\partial y) \end{bmatrix} = \begin{bmatrix} (0) \\ \vdots \\ (0) \\ (0) \\ \vdots \\ (0) \\ (-1) \end{bmatrix}.$$

Solving for $\partial z_j/\partial y$ implies:

(40)
$$\frac{\partial z_j}{\partial y} = \frac{(-1)(-1)^{(n+m+1+j)}|M_{n+m+1,j}|}{|H|}.$$

Substituting equation (40) into equation (38) gives:

(41)
$$\frac{\partial z_j}{\partial \beta_i} = \frac{\lambda \sum_{k=1}^{n} \frac{\partial Ph_{zk}}{\partial \beta_i}(-1)^{(k+j)}|M_{k,j}|}{|H|} - \frac{\partial Ph}{\partial \beta_i}\cdot\frac{\partial z_j^M}{\partial y}.$$

In a similar fashion, the (n+m+1) first order conditions for expenditure minimization can be differentiated with respect to (β_i) and placed in a matrix format:

(42)
$$\begin{bmatrix} (Ph_{z1,z1}-\lambda_e U_{z1,z1}) & \cdots & (Ph_{z1,zn}-\lambda_e U_{z1,zn}) & (-\lambda_e U_{z1,x1}) & \cdots & (-\lambda_e U_{z1,xm}) & (-U_{z1}) \\ \vdots & \cdots & \vdots & \vdots & \cdots & \vdots & \vdots \\ (Ph_{zn,z1}-\lambda_e U_{zn,z1}) & \cdots & (ph_{zn,zn}-\lambda_e U_{zn,zn}) & (-\lambda_e U_{zn,x1}) & \cdots & (-\lambda_e U_{zn,xm}) & (-U_{zn}) \\ (-\lambda_e U_{x1,z1}) & \cdots & (-\lambda_e U_{x1,zn}) & (-\lambda_e U_{x1,x1}) & \cdots & (-\lambda_e U_{x1,xm}) & (-U_{x1}) \\ \vdots & \cdots & \vdots & \vdots & \cdots & \vdots & \vdots \\ (-\lambda_e U_{xm,z1}) & \cdots & (-\lambda_e U_{xm,zn}) & (-\lambda_e U_{xm,x1}) & \cdots & (-\lambda_e U_{xm,xm}) & (-U_{xm}) \\ (-U_{z1}) & \cdots & (-U_{zn}) & (-U_{x1}) & \cdots & (-U_{xm}) & (0) \end{bmatrix}$$

$$\cdot \begin{bmatrix} (\partial z_1/\partial \beta_i) \\ \vdots \\ (\partial z_n/\partial \beta_i) \\ (\partial x_1/\partial \beta_i) \\ \vdots \\ (\partial x_m/\partial \beta_i) \\ (\partial \lambda_e/\partial \beta_i) \end{bmatrix} = \begin{bmatrix} (-Ph_{z1}/\partial \beta_i) \\ \vdots \\ (-\partial Ph_{zn}/\partial \beta_i) \\ (0) \\ \vdots \\ (0) \\ (0) \end{bmatrix}$$

where (λ_e) is the LaGrange multiplier from the expenditure minimization problem.

Next, with $|H_e|$ representing the bordered Hessian of the expenditure minimization problem, Cramer's rule is used to solve equation (42) for the change in the Hicksian demand for characteristic (j) with respect to a change in (β_i):

(43)

$$\frac{\partial z_j^{H}}{\partial \beta_i} = \frac{\begin{bmatrix} (Ph_{z1,z1}-\lambda_e U_{z1,z1}) & \cdots & (Ph_{z1,zj-1}-\lambda_e U_{z1,zj-1}) & (-\partial Ph_{z1}/\partial \beta_i) & (Ph_{z1,zj+1}-\lambda_e U_{z1,zj+1}) & \cdots & (Ph_{z1,zn}-\lambda_e U_{z1,zn}) & (-\lambda_e U_{z1,x1}) & \cdots & (-\lambda_e U_{z1,xm}) & (-U_{z1}) \\ \vdots & \cdots & \vdots & \vdots & \vdots & \cdots & \vdots & \vdots & \cdots & \vdots & \vdots \\ (Ph_{zn,z1}-\lambda_e U_{zn,z1}) & \cdots & (Ph_{zn,zj-1}-\lambda_e U_{zn,zj-1}) & (-\partial Ph_{zn}/\partial \beta_i) & (Ph_{zn,zj+1}-\lambda_e U_{zn,zj+1}) & \cdots & (Ph_{zn,zn}-\lambda_e U_{zn,zn}) & (-\lambda_e U_{zn,x1}) & \cdots & (-\lambda_e U_{zn,xm}) & (-U_{zn}) \\ (-\lambda_e U_{x1,z1}) & \cdots & (-\lambda_e U_{x1,zj-1}) & (0) & (-\lambda_e U_{x1,zj+1}) & \cdots & (-\lambda_e U_{x1,zn}) & (-\lambda_e U_{x1,x1}) & \cdots & (-\lambda_e U_{x1,xm}) & (-U_{x1}) \\ \vdots & \cdots & \vdots & \vdots & \vdots & \cdots & \vdots & \vdots & \cdots & \vdots & \vdots \\ (-\lambda_e U_{xm,z1}) & \cdots & (-\lambda_e U_{xm,zj-1}) & (0) & (-\lambda_e U_{xm,zj+1}) & \cdots & (-\lambda_e U_{xm,zn}) & (-\lambda_e U_{xm,x1}) & \cdots & (-\lambda_e U_{xm,xm}) & (-U_{xm}) \\ (-U_{z1}) & \cdots & (-U_{zj-1}) & (0) & (-U_{zj+1}) & \cdots & (-U_{zn}) & (-U_{x1}) & \cdots & (-U_{xm}) & (0) \end{bmatrix}}{|H_e|}$$

Expanding equation (43) while noting that the reciprocal of the LaGrange multiplier (λ_e) from the expenditure minimization problem is equal to the LaGrange multiplier from the utility maximization problem gives:

(44)
$$\frac{\partial z_j^H}{\partial \beta_i} = \frac{\lambda \sum_{k=1}^{n} \frac{\partial Ph_{zk}}{\partial \beta_i}(-1)^{(k+j)}|M_{kj}|}{|H|}.$$

where $|M|$ is the determinant of the minor formed by the elimination of the kth row and jth column from the determinant in the numerator of equation (43) after both the numerator and the denominator have been multiplied by $(1/\lambda_e)$.

But the expression on the right hand side of equation (44) is equal to the first term on the right hand side of equation (41). This implies that:

(45)
$$\frac{\partial z_j^M}{\partial \beta_i} = \frac{\partial z_j^H}{\partial \beta_i} - \frac{\partial P_h}{\partial \beta_i} \cdot \frac{\partial z_j^M}{\partial y},$$

which is the Slutsky equation for the hedonic demand model[2].

Shephard's lemma can be proven for the hedonic case by first noting that:

(46)
$$e(P,U^*,B) \equiv PX(P,U^*,B) + P_h[Z(P,U^*,B),B)].$$

Differentiating (46) with respect to (β_i) gives:

(47)
$$\frac{\partial e}{\partial \beta_i} = \sum_{k=1}^{m} p_k \cdot \frac{\partial x_k}{\partial \beta_i} + \sum_{j=1}^{n} \frac{\partial P_h}{\partial z_j} \cdot \frac{\partial z_j}{\partial \beta_i} + \frac{\partial P_h}{\partial \beta_i}.$$

But it is also true that:

(48)
$$U^* = U[X(P,U^*,B),Z(P,U^*,B).$$

Differentiating this with respect to (β_i) gives:

(49) $$\sum_{k=1}^{m} \frac{\partial U}{\partial x_k} \cdot \frac{\partial x_k}{\partial \beta_i} + \sum_{j=1}^{n} \frac{\partial P_h}{\partial z_j} \cdot \frac{\partial z_j}{\partial \beta_i} = 0.$$

If the first order conditions are substituted into equation (49) and this in turn is substituted into equation (47), Shephard's lemma emerges:

(50) $$\frac{\partial e}{\partial \beta_i} = \frac{\partial P_h^h(P,U^*,B)}{\partial \beta_i}.$$

One final comparative statics result may be derived from noting that in equilibrium the Marshallian and Hicksian indirect hedonic price functions must be equal:

(51) $$P_h^M(P,B,y) = P_h^M[P,B,e(P,B,U^*)] = P_h^h(P,U^*,B).$$

Differentiating the second and third parts of the equality with respect to (β_1) and using Shephard's lemma a type of Slutsky equation in hedonic expenditure can be derived:

(52) $$\frac{\partial P_h^M}{\partial \beta_i} = \frac{\partial P_h^h}{\partial \beta_i} - \frac{\partial P_h^h}{\partial \beta_i} \cdot \frac{\partial P_h^M}{\partial y}.$$

Rearranging this equation gives:

(54) $$\frac{\partial P_h^h}{\partial \beta_i} = \frac{\dfrac{\partial P_h^M}{\partial \beta_i}}{1 - (\partial P_h^M / \partial y)}$$

This equation can be interpreted to mean that the amount of expenditure necessary to hold an individual at a given level of utility when a parameter of the hedonic price function is changed is directly proportional to the change in the Marshallian hedonic price function. The factor of proportionality is simply the reciprocal of the marginal propensity to spend additional income on goods other than the hedonic good.

Welfare Change Estimation

Equation (54) shows the information that is required to obtain estimates of the welfare change to consumers from environmental or other policies. We assume here that the effects of government policy or market forces operate by inducing changes in the (B) vector of the hedonic price function. The new hedonic price function implies a change in the consumer's budget constraint and therefore a possible change in the consumer's welfare. Equation (54) shows that the difference in consumer welfare can be estimated as long as we know the consumer's expenditure on the entire hedonic bundle as a function of the (B) vector and the consumer's income (Y). Note that it is not necessary to estimate the demand functions for the individual characteristics but only $Ph^M(P,Y,B)$, which can be interpreted as a Marshallian demand function for the entire bundle of characteristics[3].

The difficulty with the method lies in our understanding of exactly how policy and other forces affect the (B) vector. Suppose, for example, that we are interested in consumer willingness-to-pay for air quality. If we could observe different vectors of (B) and (Y) with all else in the economy constant except air quality, we could estimate the Marshallian demand for housing (i.e. $Ph^M(P,Y,B)$) and be sure that any differences in housing expenditure that resulted from different values of the (B) vector were due solely to differences in air quality.

Estimation of consumer willingness-to-pay for improved air quality has become a three step process. We must first estimate the (B) vector. This can be done by observing housing prices and characteristic vectors and then using ordinary least squares to estimate the house prices as a function of characteristics. The resulting OLS parameters can be treated as estimates of the (B) vector. By estimating several hedonic price functions, using either cross sectional or time series data, we can obtain variation in the (B) vector.

These estimates will now provide the variation in the (B) vector necessary to estimate $Ph^M(P,Y,B)$[4]. But unless we are confident that all changes in the parameters of the estimated hedonic price function are due to air quality differences, we must also conduct a third round of statistics to isolate the effect that air quality variation has on the (B) vector. How (or if) this can be done requires further research.

Demand Function Estimation

The major implication of the both the demand functions defined in equations (15) and (22) and the Marshallian hedonic function is that estimation of the characteristic demand functions requires variation in the parameters of the hedonic price function. One can attempt to obtain this variation via market segmentation either cross-sectionally (which assumes cross-sectional discontinuities in the housing market) or through time (which assumes intertemporal discontinuities).

Statistical Requirements for Market Segmentation

Neither cross-sectional or time-sectional segmentation have truly solved the "garden variety" identification problem, however. In the very simplest case, the parameters of the hedonic equation enter the demand and supply equations as an arbitrarily imposed single valued function of the (β_i) so that we may write B = f(β_1,β_2,...,β_n). We may then rewrite equations (1) and (2) as:

(55) $$\text{Demand: } z_i = \alpha B + \eta D + e_{di} \quad (i = 1 \text{ to } n),$$

(56) $$\text{Supply: } z_i = \gamma B + \delta S + e_{si} \quad (i = 1 \text{ to } n),$$

Statistical identification of the demand and supply parameters of equations (55) and (56) will still require either a unique demand or supply shifter (D or S) for each equation or the imposition of possibly arbitrary restrictions on one or more equations.

Is Cross-sectional Market Segmentation Justified?

Assume that one is able to obtain a data set large enough to provide the necessary degrees of freedom for the market segmentation approach. It would still be necessary to provide a justification for believing that housing or other markets are in fact segmented.

The usual suspects for the existence of market segmentation are that transaction or moving costs or discrimination prevent individual homeowners from relocating across segment boundaries. While discrimination can certainly assign particular kinds of people to particular market segments, transaction and moving costs are not a satisfactory explanation for market segmentation. Hedonic markets, like other markets, are made by the marginal buyers and sellers, i.e. those who are mobile. Market segmentation can be justified only if there are reasons why the mobile households at the margin can move to some segments, but not to others.

Consider what happens when segments exist (call them "neighborhoods") but individuals are free to choose among segments. Figure 1 shows the hedonic price of the house for two markets ("A" and "B") when the quantities of all characteristics except z_i are held fixed at the same level in both markets. It is easy to see that no type "a" individual will purchase a house in market B since houses are available in market "A" that have larger amounts of z_i for the same price. Similarly, no type "b" individual will purchase a house in market "A." We will never observe more than the outer envelope consisting of discontinuous short sections of intersecting hedonic price functions for the segments. And as the number of segments becomes larger, (or to put it another way, the distinctions between neighborhoods becomes finer) the observed length of each segment becomes shorter. In the extreme, the envelope hedonic price equation segments will look more and more like a single smooth hedonic price function. Since the statistical identification of demand and supply may require a large number of segments, this single smooth hedonic price function envelope is exactly what we are most likely to observe.

Conclusions

Unlike the traditional hedonic model where the price of the hedonic bundle is written solely as a function of the characteristics, the present model has made a clear distinction between those elements of the hedonic price which can be chosen by the consumer (Z) and those which are exogenous to the consumer (B). This distinction has enabled us to develop a set of comparative statics and welfare results that are clear parallels of the comparative statics usually associated only with the linear prices of ordinary demand theory. These results include the Cournot and Engel aggregation conditions, Roy's theorem, the Slutsky equation, and Shephard's lemma. We have shown, too, that the hedonic bundle as a whole has Marshallian and Hicksian demand functions [$Ph^M(P,Y,B)$ and $Ph^h(P,U^*,B)$] that can be manipulated much as other demand functions in order to derive welfare change measures based on consumer expenditure on the entire hedonic bundle.

These functions offer clear possibilities for estimating the value of environmental goods in those cases where the good is purchased as one characteristic in a tied bundle of many different characteristics (e.g. air quality and housing). The theoretical insights offered by the model have provided a shortcut to welfare evaluation by eliminating the need to estimate a demand function for the environmental good. This shortcut instead relies on estimating the Marshallian demand for the entire hedonic bundle.

But practical problems with the model remain. The largest of these involves the correlation between changes in the (B) vector and the supply of the environmental good that is of interest to the benefit-cost analyst. While it may be possible to estimate differences in the (B) vector using either cross-sectional or time series data (and even this may be arduous if markets are well integrated), it may prove difficult to isolate the effect that different levels of the environmental good have had on the (B) vector when many other changes are occurring simultaneously in the economy. These problems offer some interesting research challenges and we hope that the model and the functions that we have developed will spark some renewed interest in the use of hedonic models to identify the values consumers place on environmental goods.

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Notes

1. Parsons (1984) solves several examples of these equations with different functional forms for the utility and hedonic price equations.

2. Edlefsen (1981) demonstrates this results for the general case where there are changes in more than one of the parameters.

3. Parsons (1984) suggests that welfare estimates be obtained by substituting the estimated Marshallian characteristic demands into the utility function and then inverting the utility function to obtain the expenditure function. This, however, requires knowledge of both the utility function and the individual characteristic demand functions as well as the hedonic price function.

4. Parsons (1984) notes that since the hedonic price function parameters are estimates of the true (B), "an error in variables problem must be of concern in the second stage of estimation."

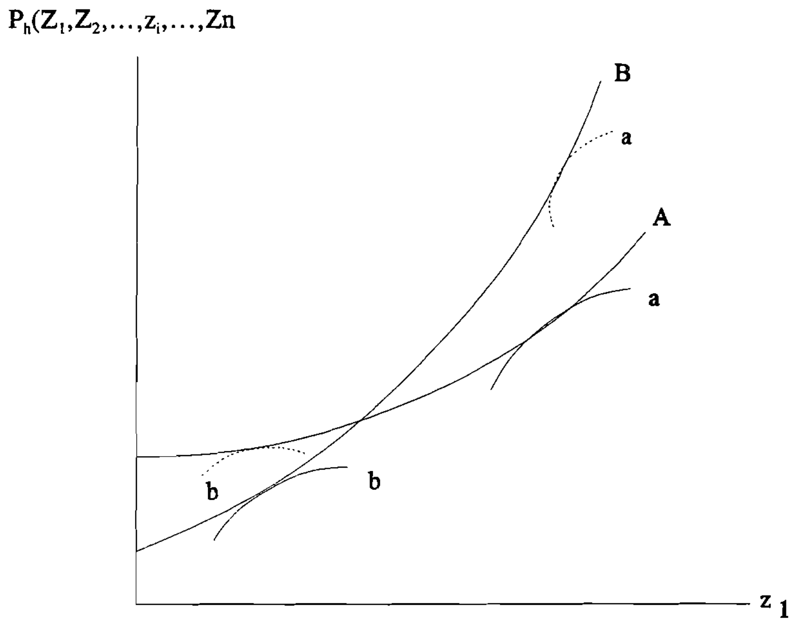

Figure 1. The Hedonic Price Envelope

An Assessment of the Empirical Magnitude of Option Values for Environmental Goods

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Abstract

This paper provides an empirical assessment of the magnitude of option values relative to expected surplus using a model presented by Larson and Flacco (1992). Option values and option prices are computed for both simulated data sets and actual estimates of recreation demands. Results indicate that option values engendered by price and income uncertainty are generally quite a small percent of expected surplus.

An Assessment of the Empirical Magnitude of Option Values for Environmental Goods

The notion of option value has intrigued environmental economists for years and a vast literature now exists defining the concept and predicting its expected sign. See Graham (1981), Bishop (1982), and Smith (1983, 1987, 1990) for reviews and the status of current theoretical thinking. Throughout the development of this literature, empirical work on option value has taken a back seat to theoretical considerations. This has resulted in a dearth of estimates of option value or option price, particularly for studies that infer values from observed behavior. Previous empirical studies of option price or option value have employed contingent valuation to elicit these values (Greenley, Walsh, and Young (1981); Smith, Desvousges, and Fisher (1983); Brookshire, Eubanks, and Randall (1983); Walsh, Loomis, and Gillman (1984); Edwards (1988); Cameron and Englin (1991); Boyle, McCollum, and Teisl (1992)). Many of these studies have yielded sizable estimates of option values, ranging from about 1% (Edwards) to about 98% (Boyle et al.) of option price.

In contrast to previous studies which have employed CVM to infer option values, Larson and Flacco (1992) and Larson (1991) offer a new alternative in the empirical study of option value by demonstrating how to estimate these values using recreation demand models. They present formulas for the computation of option price and option value for the three most commonly used empirical specifications in recreation demand analysis: linear, semilog, and doublelog demands.

A second important insight of the Larson and Flacco work is that, when averaged over a sample, option value can be interpreted as the bias from using ex post consumer surplus rather than the theoretically correct measure under uncertainty, option price. By demonstrating that the sample average of realized consumer surplus is conceptually equivalent to the sample average of expected surplus, they demonstrate that option value can be interpreted as the bias from using ex post surplus. This result has significant implications for applied welfare analysts as ex post consumer surplus is the routinely reported measure in studies of environmental benefits. That is, analysts typically estimate welfare using realized values of explanatory values, without acknowledging that there may have been uncertainty ex ante. Larson and Flacco's result demonstrates that option value can be interpreted as the error or bias between the use of realized values to compute consumer surplus ignoring uncertainty, and the theoretically correct use of option price.

The interpretation of option value as an error between the typical practice of computing ex post welfare measures ignoring uncertainty, and the theoretically correct, but substantially more difficult to estimate measure, option price, creates a new significance for the concept of option value. If the circumstances under which option value is likely to be small can be identified, researchers can continue to ignore uncertainty in computing welfare, safe in the knowledge that the error from so doing will be small. If, on the other hand, the researcher is faced with circumstances that might imply large option values, it will be necessary to characterize the uncertainty explicitly in order to obtain option prices. Thus, questions regarding the size of option value can now be considered as questions concerning whether welfare measurement can be undertaken ignoring uncertainty, or whether it is necessary to explicitly incorporate uncertainty into our models.

Some evidence on this question has been provided by Freeman (1984). He performed an insightful set of simulations using three different utility functions and a variety of parameter values to represent different degrees of risk aversion. He found that in most cases option value is a small fraction of expected consumer surplus. However, relatively large option values are found when there is a low probability of demand for the good, a high degree of risk aversion, and the expected consumer surplus is large. Freeman's results are based on three utility functions that have not been employed in actual demand studies. In contrast, the linear, semilog, and doublelog demand functions examined here and in Larson and Flacco's work have been extensively used in the assessment of recreation benefits.

The purpose of this paper is to employ the findings of Larson and Flacco to examine the magnitude of option value under a variety of assumptions about the distribution of risks from which option value arises. This paper takes two routes in examining these magnitudes. First, a set of simulation experiments is conducted where option price and option value are calculated from demand functions for which the parameters are assumed known and for different assumptions regarding the degree of uncertainty concerning price and income. A range of coefficient estimates are employed in the simulation to examine the effect of relative risk aversion on the size of option value.

The second route taken in examining the likely size of option values for environmental resources is to compute option values based on actual empirical estimates of demand for these resources. Option values are computed at the means of the data from four recent studies of environmental resources: water based recreation (Smith and Desvousges, 1985), Striped Bass angling in Maryland (Bockstael, Strand, McConnell, and Arsanjani, 1990), deer hunting in California (Creel and Loomis, 1990; 1992), and sportfishing in California (Huppert, 1989). Between these four studies, over 30 demand functions for environmental goods are used to examine the size of option value under a range of assumptions about price and income uncertainty.

Option Value from Recreation Demand Models

Examinations of the size of option value are undertaken for each of the three functional forms analyzed by L&F. The three forms are

$$\begin{aligned} &\textit{linear}: x=\alpha+\beta p+\gamma m \\ &\textit{semilog}: \ln x=\alpha+\beta p+\gamma m \\ &\textit{doublelog}: \ln x=\alpha+\beta \ln p+\gamma \ln m. \end{aligned} \tag{1}$$

where x is the quantity of the good (number of recreation trips), p is the price, m is income, and greek letters indicate known parameters.

The now standard definitions of option price and option value due to Cichetti and Freeman (1971) and Schmalensee (1972) are adopted here. Option price is the ex ante payment an individual would make such that the expected indirect utility of retaining access to the good with certainty just equals the expected indirect utility of not retaining access. Using Larson and Flacco's notation, option price can be defined as

$$EV(\tilde{p},\tilde{m}-OP) = EV(\hat{p},\tilde{m}) \tag{2}$$

where V(·) is the indirect utility function, tildes indicate random variables, $\hat{p}$ is the price at which the consumer's demand is zero, and OP is option price. Expected surplus (E(S)) is the expected value of the ex post payments the individual would make under each realization to remain indifferent between receiving the good and not receiving it. Option value is the difference between these two concepts.

Table 1 contains the formulas for option price and expected surplus (compensating variation) associated with the linear, semilog, and doublelog functional forms as presented by Larson and Flacco. The reported formulas are identical to those presented in L&F's paper except for the simplifying omission of qualities and error terms.

An important feature of functional forms used to consider option values is the degree of risk aversion implied by the specification. The relative risk aversion coefficient (r) is a commonly used measure of risk aversion; Freeman (1984) uses it to characterize the degree of risk aversion in his simulations. It is defined as

$$r = -\frac{\partial^2 V(m)/\partial m^2}{\partial V(m)/\partial m} m. \qquad (3)$$

For the linear model, this coefficient is zero, implying risk neutrality. Thus, the linear model is not capable of encompassing a range of risk aversion. For the semilog and doublelog model, the relative risk aversion coefficient is equal to the respective income elasticities. That is, for the semilog, $r=\gamma m$, and for the doublelog, $r=\gamma$, where γ is the respective income coefficient in each case. Thus, these two forms are capable of representing a wide range of risk aversion.

For the case of environmental recreation goods, it has often been noted that the estimated income effects are generally quite small or zero, as the coefficients are often observed to be statistically insignificant. Thus, casual empiricism suggests that risk aversion may be relatively small for recreation goods.

Sources of Uncertainty and the Construction of the Simulation Experiment

In the simulations and actual empirical applications, both price and income uncertainty are examined. To investigate option value arising from price uncertainty, there are assumed to be two possible price outcomes: (1) a price corresponding to a point on the interior of the consumers demand (term it the prevailing price) or (2) the choke price that drives the individual's consumption to zero. To examine the effect of the range of uncertainty on the size of option value, the probability of the price equalling the prevailing price is varied from 0.1 to 0.9. For each set of probabilities, option price, expected surplus and option value are computed.

Initially, a continuous, normal distribution was specified for price. The discrete representation employed here where price has only two possible outcomes was chosen because it is an interesting case for environmental goods; it represents the extreme case of uncertainty over whether the good will continue to be supplied at current conditions or will be eliminated (for example, closure of a recreation site). Also, it represents a higher degree of variation in the price outcomes than a continuous representation. That is, with a continuous distribution about some mean, there are potential price realizations near the mean, whereas with the discrete representation, the only two realizations possible are the prevailing price or the choke price, nothing in between. Thus, continuous price distributions around a mean should generate smaller option values than those presented here.

To examine the magnitude of option value arising from income uncertainty, a set of experiments are undertaken assuming that price is known to the consumer, but that income is uncertain. In this case, the random variable, income, is assumed to be continuous. A mean income is specified and option value is computed under three different assumptions about the size of the standard deviations. In particular, standard deviations of 10%, 20%, and 30% of income are examined. To evaluate the expected values identified in the tables, 1000 draws from a normal distribution for income are taken. These realizations are then used to compute the expected value of interest. So, for the semilog option price formula in Table 1, 1000 draws of income are taken and used to compute 1000 realizations of the denominator. The average of these 1000 then yields the desired expected value.

L&F note that OP is bounded by $OP < \min(m)$, where min(m) is the minimum of the incomes in the income distribution. This bound limits the range of allowable income uncertainty in the simulations since a standard deviation in a normal distribution greater than 0.3 of the mean will generate significant numbers of nonpositive values of m.

The simulation experiments conducted here are similar in spirit to those performed by Freeman (1984). The main difference is that three commonly employed demand functions are examined here, whereas Freeman worked with utility functions that have not been actually used to estimate recreation demand. Also, Freeman examined the case of state dependent preferences.

For simplicity, the additive error term carried through the L&F analysis is omitted since the source of error is not relevant for the computation of option price; hence its inclusion adds unnecessary complexity. Alternatively, the error can be thought of as embedded in the constant term.

The parameter values for each functional form were chosen to generate quantities of trips that would be consistent with those observed in a recreation data set. In particular, quantity was set at 5, price was $15, and income was $40,000. The parameter values and the implied relative risk aversion coefficients are reported in Table 1. In addition to choosing values that resulted in $x=5$, price coefficients were chosen to represent relatively inelastic demands so that welfare changes would be relatively large. A notable exception is for the doublelog where demands must be elastic to imply a non-necessary good[1].

Note that the experiments described here are equivalent to computing option prices and option values for a single individual. That is, the demand functions specified describe a single individual who faces price or income uncertainty. In the case of income uncertainty, the repeated sampling conforms to 1000 realizations of the random incomes for this individual.

[1]With inelastic demands, the area under the doublelog demand curve is unbounded indicating that the good is necessary (Bockstael, Hanemann, and Strand, 1986).

Results of the Simulations

Table 2 reports simulation results under price uncertainty for the three functional forms. The probability of the good being supplied; i.e, Prob(P=\$15), is varied from 0.1 to 0.9. The table reports option value as a percent of expected surplus under these nine probabilities. That is, the table reports

$$\frac{OV}{E(S)}100.$$

Also reported is the relative risk aversion coefficient and the ex post surplus if the good is supplied at P=\$15. The results are quite uniform across functional form and degree of risk aversion. Only when the risk aversion coefficient equals or exceeds 5, does option value exceed one percent of expected surplus. In the semilog case, option value exceeds one percent of expected surplus only when the probability of supply is relatively low (below 0.5). In the doublelog case, option values are slightly larger, but at their largest are only about 3.25% of expected surplus.

Table 3 reports the results of the simulation experiments for income uncertainty. Option value under income uncertainty is zero for the linear case (see Larson and Flacco) so attention is restricted to the semilog and doublelog cases. As mentioned above, the coefficient of variation on income is varied from 0.1 to 0.3. As the standard error increases, so should the size of option value. The table reports option value as a percent of expected consumer surplus.

Computation of option price is more difficult for the doublelog functional form than for the semilog as there is no closed form expression for option price under income uncertainty. Consequently, a numerical procedure was employed to compute option prices. Once the 1000 draws were made, the RHS of the option price formula was computed. Then, option price was set equal to [E(s) - \$.01]. Since option value is negative for income uncertainty for these functional forms (again, see Larson and Flacco), E(s) acts as an upper bound on option price. The means of the LHS were then computed; if the LHS was less than the RHS, option price was decreased by \$.10. This iterative procedure continued until the two sides were equal. The resulting option price is accurate to within \$.10 (or \$1.0 for the r=5). There are no results presented for the r=10 case as computing problems relating to precision prevented the computation of option value when the rate of risk aversion was this great.

In this case, all of the option values are negative. Much larger option values as a percent of expected surplus appear than in the price uncertainty case. With coefficients of variation of 0.1, option value as a percent of expected surplus exceeds 20% when r is greater than or equal to 5.

When the coefficient of variation is 0.3, option value is large even at r=2. This is, however, a rather large coefficient of variation and may not be realistic for most individuals. To put this degree of uncertainty into perspective, an income of $40,000 and a normal distribution with a coefficient of variation of 0.3, implies that there is about a 33% chance that the individual will have income greater than $52,000 or less than $28,000. Though this amount of income uncertainty is not impossible, it would not seem typical of the average individual.

Appealing to the interpretation of option value as a measure of error, these results imply that ex post surplus may be an adequate substitute for option price under price uncertainty, regardless of the probability of supply or the degree of risk aversion. Ex post surplus would also appear to be an acceptable substitute under income uncertainty unless the coefficient of variation is large or the relative risk aversion is large (or both). Since empirical studies in recreation demand often find small income elasticities, it is likely that ex post surplus will provide a good approximation to option price in most actual applications.

Two qualifiers to these generalizations are in order. First, the simulations here correspond to an individual and not to a sample average, thus a few individuals in a sample may exhibit large option values that could increase the sample average. Second, a general difficulty with the use of simulation experiments is that the results of the experiments depend on the particular parameter values chosen. Though the parameters chosen here are intended to be representative of typical recreation demand data sets and were varied to allow large degrees of risk aversion, it is nonetheless possible that they do not adequately represent actual estimates of recreation demand.

Consequently, additional evidence regarding the likely size of option values for environmental goods is sought by estimating the option values from actual empirical studies. Comparisons of that work with the simulation results provides additional evidence regarding the likely sizes of option values relative to expected surplus.

Computing Option Values from Previous Empirical Studies

Four empirical studies of recreation demand were used to compute option values under the same set of price and income uncertainties as examined in the simulation experiments. The first set of demands is from the

Smith and Desvousges (1985) study of demand for water based recreation. They report maximum likelihood estimates of demand for 22 U.S. Army Corps of Engineer recreation sites. The demand functions used constitute the first stage of their varying parameter model. Option values are computed using the mean prices ($\bar{P}$) and incomes ($\bar{m}$) for each of the 22 sites.

The results for price uncertainty are reported in Table 4. The first column reports the relative risk aversion coefficient, which is also the income elasticity. Note that in some cases the income elasticity is negative, implying risk loving behavior. The relative risk aversion is generally small, for 15 of the 22 sites it is less than 0.5. The option values as a percent of expected surplus are reported for probabilities of supply ($P=\bar{P}$) ranging from 0.1 to 0.9. In only one case does option value exceed one percent of expected surplus (Benbrook Lake when the probability of supply is 0.1).

Three other studies are used to compute option values as a percent of expected surplus under price uncertainty. Bockstael, Strand, McConnell, and Arsanjani (1991) report estimates of demand for striped bass fishing in Maryland using data from the 1980 National Survey of Fishing, Hunting and Wildlife Associated Recreation. Estimates of demand using a Tobit estimator, the Cragg model, and OLS are presented in their paper and each set of estimation results is employed here to compute option values under price uncertainty. The option values are generally quite small relative to expected surplus; they are only 3.5% of expected surplus at their largest.

Huppert (1989) presents estimates of the demand for sportfishing in central California. Semilog recreation demand functions are estimated using OLS, non-linear least squares (NLS), and maximum likelihood. In all three cases, option values as a percent of expected surplus are uniformly small.

The final resource examined is from a study of the demand for deer hunting in California by Creel and Loomis (1992). The authors present estimates of the demand for deer hunting estimated with three different estimators. The truncated nonlinear normal model (TNNORM) accounts for sampling truncation, the truncated Poisson (TPOIS) and the truncated Negative Binomial (TNB) also account for the count nature of the data. As for the previous studies, option value as a percent of expected surplus are quite small.

The same set of studies were used to examine the size of option values implied by income uncertainty. The use of the linear demand by Bockstael et al. implies a zero option value from income uncertainty. The results of the Smith and Desvousges study (Table 6) indicate somewhat larger option values than for price uncertainty. For example, for Canton Lake, option value is over 5% of expected surplus in absolute value. This is a direct consequence of the relatively large risk aversion coefficient (2.43) found for Canton Lake. For a coefficient of variation as large as 0.3, option value is quite large (exceeding 10%) in 5 of the 22 cases.

Income uncertainty for the other two studies yields quite small option values in all circumstances (Table 7). An option value of almost 3% of expected surplus (in absolute value) occurs in the Huppert study when the ML estimator is used and the coefficient of variation is 0.3. In all other cases, option values are quite small.

Taken as a whole, the results from these four empirical studies suggest that, while large option values are possible, they do not often occur. Only in the case of income uncertainty are they observed to be larger than 3.5%. In the case of income uncertainty, it requires a fairly large coefficient of variation (0.3) before they are likely to exceed 10% of expected surplus.

A second interesting feature of these results is that the choice of estimator (e.g., ols vs. ML) appears to have little effect on the relative size of the estimated option value. Thus, for example, in the Bockstael et al. study, the use of coefficient estimates from the Tobit, Cragg, or OLS models yield similar option values as a percent of expected surplus.

Observations and Concluding Comments

The simulations and computation of option value from actual studies presented here are intended to extend our current empirical knowledge of the size of option value. Though the results of the simulation experiments depend on the particular parameter values chosen, they are suggestive of the sizes of option value. In particular, they suggest that option value is typically small relative to expected surplus under price uncertainty.

Under income uncertainty, large coefficients of variation can yield quite large option values relative to expected surpluses. In those cases, the use of the theoretically correct measure, option price, rather than expected surplus would be desirable. The results here may help provide a practical guide in this choice. If the estimated income elasticity for the semilog or doublelog is less than or equal to one, the researcher is likely to face an error of substantially less than 10% when ignoring uncertainty, unless he or she believes the coefficient of variation on income is quite large.

The computation of option values from estimated demands for environmental goods confirms these results and, if anything, suggests that option value is likely to be smaller than the simulation results imply, particularly for income uncertainty. In estimates from actual demands, option value only once exceeded 5% of expected surplus when the coefficient of variation was 0.1. With a coefficient of variation of 0.3, option value exceeded 10% of expected surplus for 5 of 28 demand functions.

Two alternate interpretations can be attributed to the set of results reported here with quite different implications for research agendas regarding option value. First, interpreting option value as the error from using the average compensating variation across a sample ignoring uncertainty, the results suggest that this error may be quite small in many circumstances and thus the sample average compensating variation can act as an adequate substitute for option price in welfare analysis.[2]

An alternate interpretation of the results is that the three functional forms studied here do not generally admit large option values, and hence may be inappropriate forms to be used in the study of welfare analysis under uncertainty. However, in this regard it is worth noting the consistency of the findings presented here with those found by Freeman. Combining the results from this study with those from Freeman's work provides evidence from a total of six preference structures, each examined over a range of parameter values. Though there are many other functional forms that could be investigated, the similarity of the findings from these forms suggests that more than just functional form is driving the results.

A final note concerns the use of CVM vs recreation demand models for estimating option prices or option values. The relative magnitude of option values found in this study are consistent with the CVM estimates of Cameron and Englin, who estimate option values to be about 2% of expected surplus, and Edwards, who estimates option values of about 1% of surplus. The difference between these two studies and the other CVM studies which have yielded larger option value numbers (about 15% of expected surplus and up) is that these two studies inferred option values from a particular preference structure. Both studies used CVM to calibrate parameters of a utility function and then used the estimated preference structure to compute option value. Likewise, Larson employed the Larson and Flacco method used here which also uses the formal definition and estimated option value to range from .08% to 2.66% of expected surplus. Thus, the question does not appear to be one of whether to use CVM or recreation demand approaches, but rather whether the formal definitions of the welfare concepts are explicitly used to derive the values or not.

[2]One caveat to these results is that the Larson and Flacco approach does not incorporate state dependent preferences. Thus, this source of option value which is included in Freeman's results is not studied here.

Table 1: Formulas for Computing Option Prices and Surpluses and Parameter Values Employed in Simulations

Functional Form	Option Price, OP	Expected Surplus, $E(\tilde{S})$		Simulation Parameter Values			Relative Risk Aversion
				α	β	γ	r
Linear	$\frac{E\{\tilde{S}e^{-\gamma\tilde{p}}\}}{E\{e^{-\gamma\tilde{p}}\}}$	$E\{\frac{1}{\gamma}(\tilde{x}+\frac{\beta}{\gamma})-\frac{\beta}{\gamma^2}e^{\gamma\tilde{x}/\beta}\}$	5	-.17	.637E-4		0
Semilog	$\frac{1}{\gamma}\ln(1-\frac{\frac{\gamma}{\beta}E\{e^{\alpha+\beta\tilde{p}}\}}{E\{e^{-\gamma\tilde{m}}\}})$	$E\{\tilde{m}+\frac{1}{\gamma}\ln(e^{-\gamma\tilde{m}}-\frac{\gamma}{\beta}e^{\alpha+\beta\tilde{p}})\}$		1.61	-0.033	0.125E-4	0.5
				1.11	-0.033	0.25E-4	1.0
				0.11	-0.033	0.5E-4	2.0
				-2.89	-0.033	0.125E-3	5.0
				-7.89	-0.033	0.25E-3	10.0
Doublelog							
Price	$m-(m^{1-\gamma}+\frac{1-\gamma}{\beta+1}E\{e^{\alpha}\tilde{p}^{\beta+1}\})^{\frac{1}{1-\gamma}}$	$E\{\tilde{m}-(\tilde{m}^{1-\gamma}+\frac{1-\gamma}{\beta+1}e^{\alpha}\tilde{p}^{\beta+1})^{\frac{1}{1-\gamma}}\}$		-0.30	-1.25	0.50	0.5
				-5.71	-1.25	1.01	1.01
Income	$E\{(\tilde{m}-OP)^{1-\gamma}\} =$	same as price case		-16.2	-1.25	2.0	2.0
				-48.0	-1.25	5.0	5.0
	$E\{\tilde{m}^{1-\gamma}\}+\frac{1-\gamma}{\beta+1}E\{e^{\alpha}\tilde{p}^{\beta+1}\}$						
				-101.0	-1.25	10.0	10.0

Table 2: Option Value as a Percent of Expected Surplus under Price Uncertainty
Simulation Results

	Risk Aversion	Probability of Supply at P = $15									Surplus with Supply
		.1	.2	.3	.4	.5	.6	.7	.8	.9	
Linear	r=0	0.17	0.15	0.13	0.11	0.09	0.07	0.06	0.04	0.02	73.42
Semilog											
	r=.5	0.09	0.08	0.07	0.06	0.05	0.04	0.03	0.02	0.01	152.22
	r=1	0.17	0.15	0.13	0.11	0.10	0.08	0.06	0.04	0.02	152.07
	r=2	0.34	0.30	0.27	0.23	0.19	0.15	0.11	0.08	0.08	151.78
	r=5	0.85	0.76	0.66	0.57	0.47	0.38	0.28	0.19	0.09	150.93
	r=10	1.70	1.51	1.31	1.12	0.93	0.75	0.56	0.37	0.19	149.53
Doublelog											
	r=.5	0.17	0.15	0.13	0.11	0.10	0.08	0.06	0.04	0.02	300.58
	r=1.01	0.34	0.30	0.26	0.23	0.19	0.15	0.11	0.08	0.04	298.30
	r=2	0.67	0.60	0.52	0.45	0.37	0.30	0.22	0.15	0.07	297.40
	r=5	1.66	1.47	1.28	1.10	0.91	0.73	0.55	0.36	0.18	291.24
	r=10	3.24	2.86	2.49	2.13	1.76	1.41	1.05	0.70	0.35	281.50

Table 3: Option Value as a Percent of Expected Surplus under Income Uncertainty
Simulation Results

	Risk Aversion	Coefficients of Variation and Expected Surpluses					
		.1	E(S)	.2	E(S)	.3	E(S)
Semilog							
	r=.5	-0.25	152.45	-0.99	153.74	-2.01	153.53
	r=1	-1.01	152.88	-3.96	157.48	-8.66	160.49
	r=2	-3.88	153.75	-14.91	166.71	-28.06	179.34
	r=5	-21.49	168.54	-63.45	254.07	-88.80	410.14
	r=10	-58.40	222.25	-96.60	616.75	-99.86	1246.98
Doublelog							
	r=.5	-0.30	300.50	-1.11	297.19	-9.21	296.62
	r=1.01	-3.01	299.22	-4.40	300.44	-12.24	298.36
	r=2	-4.09	300.88	-17.51	314.70	-95.52	323.30
	r=5	-21.76	321.77	-72.52	398.62	-100.16	546.21

Table 4: Option Value as a Percent of Expected Surplus under Price Uncertainty Smith and Desvousges Results											
Site	Risk Aversion	Probability of Supply at $P=\overline{P}$									Surplus with Supply
		.1	.2	.3	.4	.5	.6	.7	.8	.9	
Arkabutla	0.03	0.01	0.01	0.01	0.00	0.00	0.00	0.00	0.00	0.00	87.00
Lock & Dam No. 2	0.17	0.66	0.58	0.51	0.44	0.36	0.29	0.22	0.15	0.07	909.85
Belton Lake	0.21	0.02	0.01	0.01	0.01	0.01	0.01	0.01	0.00	0.00	28.69
Benbrook Lake	1.59	1.07	0.95	0.83	0.71	0.59	0.47	0.35	0.24	0.12	284.84
Blakely Mt. Dam	-0.16	-.02	-0.02	-0.02	-0.01	-0.01	-0.01	-0.01	-0.00	-0.00	47.66
Canton Lake	2.43	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.27
Cordell Hull Dam	0.06	0.01	0.01	0.01	0.00	0.00	0.00	0.00	0.00	0.00	42.24
DeGray Lake	-0.29	-0.07	-0.06	-0.05	-0.04	-0.04	-0.03	-0.02	-0.01	-0.01	96.82
Grapevine Lake	0.16	0.17	0.15	0.13	0.11	0.09	0.07	0.06	0.04	0.02	206.53
Greers Ferry Lake	0.44	0.12	0.10	0.09	0.08	0.07	0.05	0.04	0.03	0.01	93.70
Grenada Lake	-0.32	-0.17	-0.16	-0.14	-0.12	-0.10	-0.08	-0.06	-0.04	-0.02	111.20
Hords Creek Lake	-1.06	-0.05	-0.04	-0.04	-0.03	-0.02	-0.02	-0.01	-0.01	0.00	15.24
Melvern Lake	1.34	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.01
Millwood Lake	1.38	0.48	0.43	0.37	0.32	0.26	0.21	0.16	0.11	0.05	143.50
Mississippi River No.6	1.14	0.03	0.03	0.03	0.02	0.02	0.01	0.01	0.01	0.00	12.78
New Savannah Bluff	-0.71	-0.22	-0.20	-0.18	-0.15	-0.13	-0.10	-0.07	-0.05	-0.03	89.62
Ozark Lake	0.15	0.05	0.05	0.04	0.03	0.03	0.02	0.02	0.01	0.01	94.93

Philpott Lake	0.31	0.15	0.14	0.12	0.10	0.08	0.07	0.05	0.03	0.02	155.15
Proctor Lake	0.09	0.01	0.01	0.01	0.01	0.01	0.01	0.00	0.00	0.00	52.39
Sam Rayburn Dam	0.20	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	4.06
Sardis Lake	0.20	0.73	0.65	0.57	0.49	0.40	0.32	0.24	0.16	0.80	1077.8
Whitney Lake	0.56	0.05	0.05	0.04	0.04	0.03	0.02	0.02	0.01	0.01	40.16

Table 5: Option Value as Percent of Expected Surplus Under Price Uncertainty
Various Studies Results

Authors	Resource	Form	Estimator	Probability of Supply at $P=\overline{P}$									Surplus with Supply
				.1	.2	.3	.4	.5	.6	.7	.8	.9	
Bockstael et al.	Striped Bass Fishing, MD	Linear	Tobit	1.23	1.09	0.95	0.82	0.68	0.54	0.41	0.27	0.13	16.26
			Cragg	0.82	0.72	0.63	0.54	0.45	0.36	0.27	0.18	0.09	9.04
			OLS	3.48	3.08	2.68	2.29	1.90	1.51	1.13	0.75	0.37	142.86
Huppert	Sportfishing California	Semilog	OLS	0.17	0.15	0.13	0.11	0.10	0.08	0.06	0.04	0.02	816.28
			NLS	0.30	0.26	0.23	0.20	0.16	0.13	0.10	0.07	0.03	497.47
			ML	0.03	0.02	0.02	0.02	0.01	0.01	0.01	0.01	0.00	57.09
Creel and Loomis	Deer Hunting California	Semilog	TNNORM	0.01	0.01	0.01	0.00	0.00	0.00	0.00	0.00	0.00	57.09
			TPOIS	0.07	0.07	0.06	0.05	0.04	0.03	0.02	0.02	0.01	351.05
			TNB	0.07	0.07	0.06	0.05	0.04	0.03	0.02	0.02	0.01	297.73

Table 6: Option Value as a Percent of Expected Surplus under Income Uncertainty Smith and Desvousges Results							
Site	Risk Aversion	Coefficients of Variation and Expected Surpluses					
		.1	E(S)	.2	E(S)	.3	E(S)
Arkabutla	0.03	-0.00	86.34	-0.00	86.34	-0.00	86.32
Lock & Dam No. 2	0.17	-0.03	909.69	-0.12	912.77	-0.25	910.64
Belton Lake	0.21	-0.04	28.70	-0.18	28.67	-0.39	28.75
Benbrook Lake	1.59	-2.36	287.38	-9.69	302.29	-20.43	316.24
Blakely Mt. Dam	-0.16	-0.03	47.69	-0.11	47.69	-0.23	47.81
Canton Lake	2.43	-5.64	0.28	-20.55	0.31	-40.29	0.35
Cordell Hull Dam	0.06	-0.00	42.25	-0.01	42.23	-0.03	42.25
DeGray Lake	-0.29	-0.08	96.70	-0.34	96.90	-0.75	97.39
Grapevine Lake	0.16	-0.13	206.50	-0.47	206.91	-1.04	207.03
Greers Ferry Lake	0.44	-0.19	93.87	-0.78	94.10	-1.76	94.50
Grenada Lake	-0.32	-0.10	111.26	-0.40	111.47	-0.85	111.70
Hords Creek Lake	-1.06	-1.10	15.31	-4.44	15.63	-9.21	16.03
Melvern Lake	1.34	-1.76	0.01	-7.08	0.01	-14.69	0.01
Millwood Lake	1.38	-1.88	144.64	-7.43	152.33	-15.35	157.28
Mississippi River No.6	1.14	-1.28	12.82	-5.10	13.35	-11.26	13.49
New Savannah Bluff	-0.71	-0.54	89.90	-1.94	90.48	-4.35	90.20
Ozark Lake	0.15	-0.02	94.97	-0.09	94.82	-0.21	95.05
Philpott Lake	0.31	-0.10	155.18	-0.39	155.44	-0.86	155.58
Proctor Lake	0.09	-0.01	52.37	-0.03	52.40	-0.07	52.33
Sam Rayburn Dam	0.20	-0.04	4.06	-0.15	4.05	-0.35	4.06
Sardis Lake	0.20	-0.04	1078.57	-0.14	1076.26	-0.35	1082.04
Whitney Lake	0.56	-0.31	40.24	-1.27	40.52	-2.59	40.73

Table 7: Option Value as Percent of Expected Surplus Under Income Uncertainty Various Studies										
Authors	Resource	Form	Estimator	Risk Aversion	Coefficients of Variation and Expected Surpluses					
					.1	E(S)	.2	E(S)	.3	E(S)
Huppert	Sportfishing California	Semilog	OLS	0.13	-0.02	816.17	-0.07	816.25	-0.15	815.73
			NLS	0.38	-0.14	497.99	-0.59	498.83	-1.32	500.14
			ML	0.58	-0.31	28.59	-1.39	28.84	-2.89	29.18
Creel and Loomis	Deer Hunting	Semilog	TNNORM	0.09	-0.01	57.10	-0.04	57.11	-0.08	57.11
			TPOIS	0.18	-0.03	351.31	-0.13	351.51	-0.31	350.62
			TNB	0.21	-0.04	297.65	-0.18	297.93	-0.38	299.00

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Harry Goes to be With Tom and Dick: A Source of Bias in Estimates of Resource Demand

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Abstract

In previous work on non-market demand estimation, the use of recreational resources by others was assumed to have a negative, if any, impact on the demand for the resource through crowding. While recognizing the negative impact that crowding can have, we assert that, in some situations, the use of resources by others has a positive impact on demand. This positive impact results from individuals responding to the popularity of the recreational activity. We demonstrate how bias in welfare measures result when the positive effect of participation of others is ignored. Analysis of data on pheasant hunting quality and hunting participation support the hypothesis that the popularity of the activity directly influences demand.

Harry Goes to be With Tom and Dick:

A Source of Bias in Estimates of Resource Demand

We ask the question: Does an individual enjoy an activity more when others are interested? In situations where the interest of others has lead to crowding, the answer to this question is 'no, on the contrary'. However, can the level of interest shown by others (the popularity of the activity) have a positive affect on participation? We suggest that, in some cases, the popularity of an activity does have a positive effect on the activity's demand. For example, while recognizing that some individuals join fitness centers because of their desire to be fit, we suggest that some are motivated to join because of the popularity of fitness centers provides an opportunity to meet others, an area of interest to share with others, a means of competing with others, etc.

Similarly, popularity can be a determinant of demand for activities associated with non-market resources. That is, in any given year, demands for resources associated with swimming, fishing, hunting, boating, hiking, bicycling, etc., are, in some cases, directly dependent on the popularity of the activity. In these cases, estimates of demand for non-market resources may be biased if effects of popularity are not considered.

Responses to popularity (and crowding) can be instantaneous or lagged. The lagged response represents individuals responding after observing the level of popularity in the previous period. For example, when the public has full knowledge of an increase in the supply of municipal bike paths, the number of people riding bicycles increases in direct response to the additional resource supply. However, in subsequent years, additional people begin or increase bike riding as they observe the added popularity. Increased popularity meabs increased social activities, more frequently a topic of conversation etc. Thus the popularity response is a lagged response to the change in availability of bike paths. The first-year response to the increase in bike paths is only a partial response.

Another example can be made with pheasant hunting. The number of individuals who choose to hunt in a particular year will depend, in part, on hunting quality. When the public has pre-season reports on pheasant populations, responses to quality occur that season. Others choose to hunt to join friends who are (or recently began) hunting, to have experiences to share with friends who hunt, etc. These popularity responses occur in subsequent seasons because most pheasant hunting takes place on the season's opening days when hunting quality is greatest.[1] Thus the immediate response to a change in pheasant hunting quality is only a partial response.

We maintain the assumption made in previous work in that quality/quantity changes in non-market resources directly affect participation. However, from the direct responses to resource quality we suggest that there follows indirect (popularity) responses. Resource demand models are mis-specified when a measure of popularity is not included as an independent variable.

We provide a utility maximization framework of individuals' recreation participation decisions that include effects of popularity and crowding. Time is critical in this framework in characterizing the effects of participation on resource demands. The potential for bias in welfare measures is analyzed in terms of direct and total responses to resource changes in light of the lag that may exist on popularity responses.

We test for existence of a positive effect of participation using data on pheasant hunting from three states: South Dakota, Kansas, and Utah. Results from all three states support the hypothesis that some individuals base their pheasant hunting choice on the popularity of the activity.

Theory

In the discussion below, we first provide a general demand model that includes popularity (or popularity of the associated activity) as a demand determinant. We then outline the limited conditions where the exclusion of the popularity variables will bias demand parameters. We extend these results to two modeling frameworks commonly employed in non-market resource valuation. The first modeling framework includes the household production model and the travel cost model. This framework uses observations on individuals' market expenditures to derive values of non-market resources and thus is an indirect method of resource valuation. The second modeling framework is the contingent valuation approach. This approach directly solicits individuals' willingness to pay for a change in a non-market or environmental resource and thus is a direct method of resource valuation.

Individual i's demand for an item q_i is given by:

[1]Pheasant hunting quality decreases significantly after the season's opening days as kills reduce bird populations and those remaining become more wary.

$$q_i = f_i(P,y,\alpha,A) \quad (1)$$

where P is a vector of prices, y is a measure of income, α is the quality of the good, and A is a vector of "influence" variables (e.g. popularity and crowding). The elements of A (the a_k's) are dependent on total q_i consumed. Because α affects total q_i consumed, the a_k's are dependent on α.

When an a_k is a significant demand determinant, the effect on demand of a change in α is given by:

$$\frac{dq_i}{d\alpha} = \frac{\partial f}{\partial \alpha} + \frac{\partial f}{\partial a_k}\frac{\partial a_k}{\partial \alpha}. \quad (2)$$

That is, demand is composed of a direct response to quality ($\partial f/\partial\alpha$) and an indirect response through the influence of others ($\partial f/\partial a_k$ * $\partial a_k/\partial\alpha$). It is expected that: $\partial f/\partial\alpha > 0$ or quality positively affects demand; $\partial a_k/\partial\alpha > 0$ or quality directly affects the influence variable (e.g. crowding or popularity); and $\partial f/\partial a_k < 0$ or $\partial f/\partial a_k > 0$ or the effect on demand of the influence variable can be negative (crowding) or positive (popularity). While identifying the variable(s) needed to estimate $\partial f/\partial a_c$ * $\partial a_c/\partial\alpha$ may not be easy, understanding the direction of the bias resulting from excluding the demand determinant(s) is important.

Demand models that exclude the significant variable(s) of A are mis-specified. The degree of bias resulting from this specification error depends on the level of correlation between the included and excluded variables and on the relative magnitudes of the true model parameters (Johnston).

In the static (single period) demand case, we assume that price, α, and a_k are the only significant demand determinants so that:

$$q_i = \beta_0 + \beta_1 p_q + \beta_2\alpha + \beta_3 a_k \quad (3)$$

In excluding the significant elements of A (in this case, a_k), the estimated model becomes:

$$q_i = b_0 + b_1 p_q + b_2\alpha \quad (4)$$

From equations 3 and 4, the expected value of b_2, the estimated coefficient on α, is $\beta_2 + \rho\beta_3$ where ρ is the correlation between α and a_k (e.g. the coefficient obtained by regressing a_k on α) [Johnson, 168-9; Pindyck and Rubinfeld, 128-30]. Thus the size and direction of the bias depend on the correlation between a_k and α and the size of β_3. Will exclusion of this variable always generate unsatisfactory estimates? No, not under one set of limited conditions.

The exclusion of a_k from the analysis will not be a problem if: the researcher's interest lies only in knowing the total (direct+indirect) impact on demand of a change in α (i.e. $dq_i/d\alpha$) and the estimated demand model represents a reduced form of actual demand. For example, assume $a_k = \delta_0 + \delta_1\alpha$. Substituting this relationship into equation 3 generates $q_i = (\beta_0 + \beta_3\delta_0) + \beta_1 p_q + (\beta_2 + \beta_3\delta_1)\alpha$. Thus b_0, b_1, and b_2 of equation 4 are estimates of $(\beta_0 + \beta_3\delta_0)$, β_1, and $(\beta_2 + \beta_3\delta_1)$, respectively. This perfect multicolinearity between α and a_k forces us to exclude one of the variables from the estimated model so that neither β_2 or β_3 can be estimated. However, from the estimated reduced form model, both direct and indirect responses are included in $dq_i/d\alpha$. The researcher must be cautious in interpreting b_2 accordingly.

In the dynamic case, the individual's demand is assumed to take the form:

$$q_{i,t} = \beta_0 + \beta_1 p_q + \beta_2\alpha + \sum_{j=0}^{N} \beta_{3+j} a_{k,t-j} \tag{5}$$

where N is the number of time periods in the past where $a_{k,t-N}$ is a significant determinant of current demand and other variables are as defined before. (In the static case, only $a_{k,t-0}$ is significant.) With the exclusion of the significant variables of A (in this case, $a_{k,t-j}$'s), the estimated model is, again, equation 4. The expected value of b_2 is $\beta_2 + \rho_1\beta_3 + \rho_2\beta_4 + \ldots + \rho_{N-2}\beta_N$ where ρ_j is the correlation coefficient between α and $a_{k,t-j}$ [Johnson, 168-9; Pindyck and Rubinfeld, 128-30]. Will the exclusion of the $a_{k,t-j}$'s always generate unsatisfactory estimates? No, not in two cases.

In the first case, the exclusion of the $a_{k,t-j}$'s will not be a problem if (as before): the researcher's interest lies only in knowing the total impact on demand (direct+indirect) of a change in α (i.e. $dq_i/d\alpha$) and the estimated demand model represents a reduced form of actual demand. While both conditions are as in the previous case, the second condition will only hold in more limiting cases. For example, assume $a_{k,t-j} = \delta_{0,j} + \delta_{1,j}\alpha_{t-j}$ and is substituted into equation 5. If, within a cross-product analysis for each product m (or a cross-sectional analysis for each site m), $\alpha_{m,t-j}$ has remained unchanged through the periods $j=0,\ldots,N$, then $q_i = (\beta_0 + \beta_3\delta_{0,0} + \ldots + \beta_{3+N}\delta_{0,N}) + \beta_1 p_q + (\beta_2 + \beta_3\delta_{1,0} + \ldots + \beta_{3+N}\delta_{1,N})\alpha_{m,t}$. Thus b_0, b_1, and b_2 of equation 4 are estimates of $(\beta_0 + \beta_3\delta_{0,0} + \ldots + \beta_{3+N}\delta_{0,N})$, β_1, and $(\beta_2 + \beta_3\delta_{1,0} + \ldots + \beta_{3+N}\delta_{1,N})$, respectively. The perfect multicolinearity between $\alpha_{m,t}$ and the $a_{m,k,t-j}$'s allows only one of these variables to be included in the estimated model. However, from the estimated reduced form model (equation 4), both direct and indirect responses are included in $dq_i/d\alpha$ and b_2 must be interpreted accordingly.

In the second case, the exclusion of the $a_{k,t-j}$'s will not be a problem if: the researcher's interest lies only in knowing the direct impact on demand of a change in α ($\partial f/\partial \alpha$) and the past variation in quality (thus, variations in the $a_{k,t-j}$'s) of product (site) m is uncorrelated with $\alpha_{m,t}$. However, while the researcher obtains an unbiased estimate of β_2, (s)he will not be able to derive the full response to changes in α without explicitly investigating responses to the $a_{k,t-j}$'s.

Under all other conditions, the exclusion of significant $a_{k,t-j}$'s (or appropriate proxies) from the estimated demand model will result in left-out-variable bias.

Estimation of demands for non-market resources requires differs from estimation of demands for market resources because prices of resources are not directly observed. We apply the derivation of the left-out-variable bias discussed above to two modeling frameworks commonly employed in non-market resource valuation. The first modeling framework includes the household production model and the travel cost model. This framework uses observations on individuals' behavior in the market to derive the values of non-market resources and thus is an indirect method of resource valuation. A theoretical foundation for the indirect method of resource valuation, based on the household production (HHP) model, is provided by Bockstael and McConnell (1983) although Clawson and Knetsch provide a less general foundation based on the travel cost model. Subsequent research has extended and applied the HHP approach (e.g. Bockstael and Kling).

Indirect Method of Resource Valuation

The indirect method of non-market resource valuation uses observed expenditures on market goods to derive changes in consumer surplus associated with resource changes. In the HHP model, individuals use time, market goods, and available resources to produce commodities, z_i's, that directly determine utility. For example, an individual can use time, her bicycle, and a bike path to produce a "bike riding" commodity. Another individual can use the same inputs but produce a "social interaction" commodity.

To value changes in α (or a_k) using the HHP model, there must be one or more commodities, Z^α (Z^a), where α (a_k) is a complement in production or enters the utility function directly and utility, u, is unaffected by changes in α (a_k) when Z^α (Z^a) equals zero. Furthermore, there must be a market good, x_1, that is essential in the production of the commodity(ies) in Z^α (Z^a).[2]

[2]Bockstael and Kling (1988) extend this framework by examining the case where no single good but a set of goods is a weak complement.

The market good selected to evaluate the nonmarket good (x_1) is not restricted from being an input in the production of other commodities (Bockstael and McConnel, p. 812); there is no reason why each of two different commodities could not require the same market good but different nonmarket goods as an inputs. Thus the same market good (e.g. travel) can be used to evaluate two different nonmarket goods (e.g. a beach and the crowd at the same beach).

Upon estimation of the compensated demand for x_1 given utility level u^0, $h(P,u^0,\alpha,A)$, the change in welfare (compensating variation) associated with the direct response to a change in resource availability from α_0 to α_1 is given by:

$$CV_\alpha = \int_{p_0}^{p^*} h(P,u^0,\alpha_1,A_0)dp_1 - \int_{p_0}^{p^*} h(P,u^0,\alpha_0,A_0)dp_1 \qquad (6)$$

where CV_α is the compensating variation associated with the change in α, and p^* is the price of x_1 where the compensated demand is zero.

The change in resource availability from α_0 to α_1 will subsequently affect welfare as it drives A_0 to A_1. The welfare affect of the indirect response is given by:

$$CV_a = \int_{p_0}^{p^*} h(P,u^0,\alpha_1,A_1)dp_1 - \int_{p_0}^{p^*} h(P,u^0,\alpha_1,A_0)dp_1 \qquad (7)$$

where CV_a is the compensating variation associated with the change in popularity (or crowding).

The welfare effect of the total (direct+indirect) response to a change in α is the sum of equations 6 and 7 which reduces to:

$$CV_{\alpha+a} = \int_{p_0}^{p^*} h(P,u^0,\alpha_1,A_1)dp_1 - \int_{p_0}^{p^*} h(P,u^0,\alpha_0,A_0)dp_1. \qquad (8)$$

To ensure an unbiased estimate of $CV_{\alpha+a}$, $h(\cdot)$ must be correctly specified unless specific conditions on the relationship between α and A exist. These conditions parallel those discussed earlier.

The earlier discussion on variable relationships and coefficient bias of $f(\cdot)$ remains unchanged when q_i is defined as x_1 and p_i as p_1 in equations 3 through 5. Thus the same conditions on biases of b_0 and b_2 follow as does the unbias of b_1. This approach does not directly measure demand for α but assesses the welfare effects of changes in α by the subsequent shifts in the demand for x_1 as quantified by b_2. Thus, from conditions on biases of b_0 and b_0, we know that when $h(\cdot)$ excludes A:

$$CV_b = \int_{p_0}^{p^*} h(P,u^0,\alpha_1)dp_1 - \int_{p_0}^{p^*} h(P,u^0,\alpha_0)dp_1. \tag{9}$$

which equals equation 6 when α and A are uncorrelated (i.e. b_2 is an unbiased estimate of β_2) and equation 8 when $h(P,u,\alpha)$ is a reduced form of $h(P,u,\alpha,A)$. However, CV_b must be interpreted to reflect the welfare measure derived (CV_α or $CV_{\alpha+a}$). In all other cases the bias of b_2 prevents unbiased assessments of equation 9.

Direct Method of Resource Valuation

The second modelling framework is the contingent valuation approach. This is a direct method for valuing changes in non-market resources. The contingent valuation model (CVM) directly solicits individuals' for their response, q_i, for changes in resource price, p_q, at various levels of quality, α, ceterus paribus. Thus equation 4 applied in the CVM generates a response to resource quality, b_2, that is an unbiased estimate of β_2.

While the CVM provides an unbiased estimate of the direct response to resource quality, no measure of the indirect response (i.e. the response to changes in A) is obtained. Without the indirect response, the total response to a resource change cannot be derived.

Why not directly solicit individuals for their indirect response (i.e. their response to popularity or crowding) so that the β_{3+j}'s can be estimated? While a potential solution, two significant problems must be overcome. First, the description of the level of popularity must provide the respondent a correct perspective. That is, the respondent must clearly understand how various levels of popularity or crowding will make them feel although the respondent may not have had a similar experience. Second, the link between popularity or crowding ($a_{k,t-j}$) and the resource (α_{t-j}) must be known a priori. That is, the respondent can provide information on $\partial f/\partial a_k$ but without information on $\partial a_k/\partial\alpha$, the total response to changes in α cannot be derived.

Thus the CVM allows the researcher to derive unbiased coefficients and the associated welfare measures. However, no indirect response to resource changes is obtained. The HHP model, except in limited cases, generates a bias estimate of the demand effects (and associated welfare changes) to changes in α when A is a significant determinant of demand.

The above discussion has attempted to outline conditions where exclusion of popularity or crowding can bias demand and specifies the relevance of popularity in indirect (HHP) and direct (CVM) models used to estimate resource demands. Below, tests on the significance and estimates of the magnitude of popularity in pheasant hunting are presented. We begin by discussing the model and data.

Testing the Significance of Popularity in Pheasant Hunting

The ring-neck pheasant (*Phasianus colchicus*) was introduce in the United States from Central Asia in the late 1800's and can now be found in approximately 35 states. The ring-neck is appreciated both for its beauty and as a game bird. Its affinity to agricultural lands makes pheasant populations and their associated public benefits sensitive to the private production decisions of farmers.

State wildlife agencies restrict the opening of hunting seasons until late in the year when males of the current year's hatch have their distinctive colors, the stress of winter is yet to come, and most fields are harvested. The first days of the hunting season offers the best hunting as adult populations are at their peak and birds have not learned to be more wary. Thus, most hunting is done in the opening days of the season. Hunters purchase/obtain the necessary stamps, licenses, or permits in advance of the season's opening. Pre-season pheasant counts provide preseason hunting quality information.

Should popularity be a determinant of demand for pheasant hunting, a limited hunting season along with the decrease in hunting quality from the opening days would suggest that responses to popularity occur in the future season(s). That is, after an improvement in pheasant hunting quality results in an increase in the number of hunters and subsequent talk and enthusiasm over hunting, others will respond to this popularity by participating in the following season.

Earlier economic studies on pheasant hunting have found the perceived quality of the pheasant resource to be an important determinant of hunting demand. In one study, a CVM was used to quantify the potential gain in revenue from the institution of user fees to access public pheasant hunting areas (Adams, et al.). In as second study, time series data showed that mercury contamination of pheasants significantly affect the demand for pheasant hunting (Shulstad and Stoevener).

Pheasant Hunting -- Demand and Supply

We use state-level estimates of pheasant populations as a measure of pheasant hunting quality, α. State-level participation provides an aggregate measure of the quantity of pheasant hunting demanded, Q. Thus we aggregate $f(\cdot)$ across individuals to generate the aggregate demand:

$$Q_t = F(P_t,Y_t,\alpha_t,A_t,C_t)$$

where the quantity demanded, Q, is the total number of people hunting pheasants in year t, Y is the average income level of the population, and C is the population. The specification of variables in P and A is discussed below.

The price of pheasant hunting reflects the definition of pheasant hunting supply. Hunting supply is defined as the surrounding rural land since the land can be pheasant hunted every year. Pheasant hunting supply (the amount of rural land as a function of the cost to get there) has not changed significantly over time in the regions where data was available for this analysis. Thus the price of pheasant hunting, p_Q, has not varied and must be dropped from $F(\cdot)$. Lack of price variation does not prevent us from testing hypotheses on the significance of A but does prevent us from valuing changes in α and A.

Measuring Popularity

To test the hypothesis that the popularity variable(s) of A is (are) a determinant of pheasant hunting demand, we use participation in the previous period, Q_{t-1}, as a proxy for popularity. Because participation in the previous period is likely to be directly correlated with the activity's popularity, Q_{t-1} serves well as a proxy to popularity.[3]

The resulting behavioral equation, with variables names that are more specific to this application, becomes:

$$\text{(11)} \qquad \text{Hunters}_t = \alpha_0 + \alpha_1\text{Pheas}_t + \alpha_2\text{Pheas}_{t-1} + \alpha_3\text{Pop}_t + \alpha_4\text{Trend}_t + \alpha_5\text{Hunters}_{t-1}$$

where Hunters, Pheas, Pop, and Trend represent the number of pheasant hunters (Q), pheasant populations (α), state population, and a trend term, respectively, and the t and t-1 subscripts denote the year the variable was observed.

Trend_t is included to account for factors that tend to change over time and affect participation rates. In particular, growth in leisure time and real income over time could lower the resource price, p_i. The inclusion

[3]The variable Q_{t-1} embodies α_{t-1} and all but $a_{k,t-1}$ of the $a_{k,t-j}$'s. Since $a_{k,t-1}$ is a function of α_{t-1}, Q_{t-1} proxy well for the excluded variables.

of Trend reduces the likelihood of a type II error on tests of the hypothesis that popularity is not a significant determinant of demand.[4]

We include the variable $Pheas_{t-1}$ to test the hypothesis that hunters base their expectations of resource quality (pheasant populations) in year t on experiences of hunters in period t-1. Should a portion of hunters form their expectations in this manner, $Pheas_{t-1}$ must be included to reduce the likelihood of a type II error on tests of the hypothesis that popularity is a significant determinant of demand.[5]

Data

Data were gathered from states known to have substantial pheasant populations and hunting activities. The data availability and the methods of collecting and quantifying resource availability and participation varied between states. Of the 19 states contacted, Kansas, Minnesota, South Dakota, and Utah were the only ones time-series data on pheasant populations and the number of pheasant hunters. Minnesota had too few observations thus was dropped from the analysis. The other states contacted were: Colorado, Illinois, Idaho, Iowa, Michigan, Missouri, Montana, Nebraska, New York, Ohio, Oregon, Pennsylvania, Washington, Wyoming.

Few states estimate the actual pheasant population. Rather, they use roadside counts and rural mail carrier surveys to track variations in pheasant populations over time. Wildlife and habitat management experts have found these proxies of pheasant populations reliable.[6]

We used state roadside counts and rural mail carrier surveys as a proxy for pheasant populations dynamics. There is, nevertheless, no a priori reason to believe that these proxies are consistent representatives of pheasant populations across states. We did not, therefore, pool data across states.

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[4]Suppose $Hunters_t = \alpha_0 + \alpha_1 Trend_t + \delta_t$ but one were to estimate the model $Hunters_t = \beta_0 + \beta_1 Hunters_{t-1} + \mu_t$. Since $\alpha_1 Trend_t = \alpha_1 + \alpha_1 Trend_{t-1}$, estimating $Hunters_{t-1}$ would serve as a proxy for the left-out variable, $Trend_t$ so that parameters estimates would be found to be significant.

[5]Suppose that $Hunters_t = \alpha_0 + \alpha_1 Pheas_t + \alpha_2 Pheas_{t-1}$ but one were to estimate $Hunters_t = \beta_0 + \beta_1 Pheas_t + \beta_2 Hunters_{t-1}$. Since $Hunters_{t-1}$ is a function of $Pheas_{t-1}$ ($Hunters_{t-1} = \alpha_0 + \alpha_1 Pheas_{t-1} + \alpha_2 Pheas_{t-2}$) β_2 could be significantly different from zero although popularity (as proxied by $Hunters_{t-1}$) is not a determinant of resource demand.

[6]This point was reiterated by wildlife biologists in several agencies: Bill Baxter at the Nebraska Game and Parks Commission; Steve Riley at South Dakota State Game Fish & Parks; and Terry Riley at the Iowa Department of Natural Resources.

Annual state demographic data were obtained from the U.S. Census Bureau. A linear interpolation was used to estimate state populations between survey years.

Results

Tests of the significance of $Pheas_t$ provide direct tests of the hypothesis that hunting decisions are based on current pheasant populations.

Tests of the significance of the $Hunters_{t-1}$ provide direct tests of the hypothesis that hunting decisions are based on the popularity of the activity. Tests of the significance of $Pheas_{t-1}$ provide direct tests of the hypothesis that decisions to hunt are based on the quality of hunting experienced in the previous year.[7]

Regression results are presented in Table 1. The included variables account for over 80 percent of the variation in the dependent variable as indicated by the R^2's. We reject the hypothesis on the existence of autocorrelation for each state given the significance of ρ. The lagged dependent variable as a explanatory variable invalidates the Durban-Watson test. As an alternative, the least-squares error, e_t, is regressed on e_{t-1} and the explanatory variables so that significant autocorrelation is indicated by the significance of ρ -- the coefficient on e_{t-1} (Johnston, p 313). To ensure confidence in the significance tests and the stability of coefficients, we employ diagnostics for multicollinearity as suggested by Belsley, Kuh, and Welsch (pp 112-13). Tests results indicate that multicollinearity is not a problem in either of the three models.

Results of Hypotheses Tests

The significance of the variable $Pheas_t$ in all states indicates that a significant portion of individuals base their decision to hunt on current pheasant populations. Thus we reject H_o and accept the alternative hypothesis that resource quality, as measured by pheasant populations, is a determinant of demand for pheasant hunting.

Results of t-tests on $Pheas_{t-1}$ are consistent across states in indicating that no significant number of individuals base their expectations of hunting quality on that of the previous year. Thus we fail to reject the hypothesis that the prior year's pheasant population is not a significant determinant of hunting demand.

[7]Non-participants in year t-1 will have information on the hunting quality in year t-1 available to them in year t through contacts with participants, newspaper articles, etc.

The significance of $Hunters_{t-1}$ in all three states leads us to reject the test hypothesis and to accept the alternative -- that popularity (as measured by $Hunters_{t-1}$) is a significant determinant of demand for pheasant hunting. Thus, some individuals base their decision to hunt on the activity(ies) of others.

Results of these hypotheses tests allow us to conclude that resource quality has both direct and indirect effects on the demand for pheasant hunting. The direct effect of resource quality is indicated by the significance of $Pheas_t$. The indirect effect is indicated by the significance of $Hunters_{t-1}$.

Demand Comparisons Across States

We derive demand elasticities to allow cross-state comparisons of results. Cross-state comparisons show that results are consistent with a priori expectations.

The direct response of hunters to 1.0 percent change in pheasant populations is less than 0.4 percent in any of the three states (Table 2). The direct response to changes in hunting quality is greatest in South Dakota and lowest in Utah. Such results are consistent with expected behavior of the marginal hunters of these three states. That is, pheasant hunting quality in South Dakota has resulted in a participation rate of 0.19 but the lower quality in Kansas and Utah has resulted in participation rates of 0.0638 and 0.0592, respectively (Table 2). Those who choose to hunt when quality and, therefore, participation rates are low might be thought of as the "hard core" hunters who demonstrate less sensitivity (e.g. a lower demand elasticity) to quality than those who choose to hunt because of a high level of quality.

Indirect Demand Elasticities

The significance of $Pheas_t$ indicates the importance of pheasant populations on hunting decisions in year t. The significance of $Hunters_{t-1}$ indicates the significant effect that popularity has on hunting decisions in year t. Together these relationships indicate that changes in pheasant populations in year t-1 indirectly impact hunting demand in year t through changes in popularity. Thus changes in pheasant populations in year t have both a direct affect on hunting levels in year t and an indirect affect on hunting levels in subsequent years.

The total indirect effect of a 1 percent change in $Pheas_t$ can be shown to equal $\epsilon_D * \alpha_5/(1-\alpha_5)$, where ϵ_D is the direct elasticity of Pheas, α_5 is the coefficient on $Hunters_{t-1}$, and the term $\alpha_5/(1-\alpha_5)$ quantifies the multiplying effects of popularity. Indirect responses are greatest in Utah and smallest in South Dakota (Table

2). The inverse relationship between indirect responses and participation rates suggests that the marginal popularity response diminishes with increases in participation rates across the range of participation rates in this study.

Direct, indirect, and total (e.g. direct + indirect) demand elasticities with respect to $Pheas_t$ are all inelastic. The relative magnitudes of the direct and indirect elasticities indicate that, in the long run, the indirect response can exceed the direct response to quality changes in areas where participation rates are low. This relative magnitude of the indirect effect indicates the potential for left-out variable bias, mis-interpretation of results, and under-estimation of the full effect of resource changes in recreational demands models where the relevant popularity variable(s) are not included in the analysis.

Summary

In previous work on non-market demand estimation, the use of recreational resources by others was assumed to have a negative, if any, impact on the demand for the resources through crowding. In this paper we describe conditions underwhich the use of recreational resources by others has a positive impact on demand for the resources. We demonstrate how demand for the resource or the associated recreational activity is biased when estimated without accounting for the effect(s) of resource use by others. We focus our attention on the "popularity" effect on demand associated with resource use by others. We recognize the popularity response as an indirect response to resource quality in that quality directly affects participation and any change participation also implies a change in popularity.

We develop a recreational resource demand model that incorporates the effect of popularity and demonstrate biases in demand estimates and subsequent welfare measures that result when the popularity variable is excluded. Within our examination of specific approaches to non-market demand estimation, we show that the household production model can result in an upward bias of the coefficient on the resource quality variable. The contingent valuation approach is shown to provide an unbias estimate of the direct effect of resource quality, however, is poorly suited to estimating popularity responses. Subsequent bias in welfare estimates for each model is also discussed.

Popularity is found to be a significant determinant of resource demand in an application to pheasant hunting. Hunting quality, as indicated by pheasant populations in the current year, is also found to be significant. Results suggest that, in the long-run, the popularity response from a given resource change may be greater than the direct response thus emphasizing the importance of considering effects of popularity as a determinant of resource demand.

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Table 1 - Regression Results

State	Constant	$Pheas_t$	$Pheas_{t-1}$	Pop[1]	Trend	$Hunters_{t-1}$	R^2	n	ρ
Kansas	219,000 (201,000)[1]	8,430 (1,800)	-4,150 (2,490)	-0.0891 (0.0947)	1,610 (1,130)	0.639 (0.197)	0.79	27	0.247 (0.387)
S.Dak.	42.9 (76.5)	9.95 (1.33)	-2.27 (1.82)	-0.0000408 (0.000123)	0.789 (0.323)	0.458 (.136)	0.77	43	0.172 (0.292)
Utah	22,600 (28,400)	11,200 (5,710)	-5,820 (5,340)	-0.00605 (0.0388)	-105 (1,420)	0.744 (0.157)	0.75	27	-0.140 (0.319)

[1]Standard error

Table 2 - Elasticities and Participation Rates

	----------Elasticities---------			
State	Direct	Indirect	Total	Partic. rate
Kansas	0.264	0.420	0.684	0.0638
S.Dak.	0.377	0.247	0.624	0.194
Utah	0.158	0.420	0.578	0.0592

Comparing Observed and Multiple-Scenario Contingent Behavior:

A Panel Analysis Utilizing Poisson Regression Techniques*

Jeffrey Englin

Trudy Ann Cameron

Abstract

Recently, contingent *behavior* survey questions have arisen as an alternative to the more-traditional contingent *valuation* approach to non-market valuation. In this analysis we estimate the demand for recreational angling by combining contingent behavior and observed behavior data. We begin with simple econometric specifications of demand and progress to fixed effects panel specifications. Our specifications include both OLS and Poisson count data models. The count data models are used to capture the non-negative integer nature of the demand data. In this case, the results show a significant difference between the demand relationships implied by the contingent behavior and observed behavior data. The differences in implied consumer surplus exceed 50% in several models.

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Comparing Observed and Multiple-Scenario Contingent Behavior

A Panel Analysis Utilizing Poisson Regression Techniques

Introduction

For the valuation of non-market resources, contingent behavior questions provide a potentially attractive alternative to contingent valuation questions. An example of a contingent behavior question would be to ask a respondent: "How many times would you visit this site if travel costs doubled?" Contingent behavior questions focus on hypothetical *behavior* rather than hypothetical *prices*. It is possible that people are better able to predict what they would do in a hypothetical situation than whether they would pay some hypothetical price in a referendum survey. It may also be easier for respondents to predict prospective behavior than it is to provide an estimate of their total willingness-to-pay for a resource. Given this possibility, it is particularly important to evaluate the consistency of welfare estimates derived from contingent, as opposed to observed behavior. It is also important to determine whether apparent differences between welfare estimates from these complementary forms of demand information could be an artifact of the econometric specification chosen by the investigator.

This research offers several innovations in contingent behavior analysis. Using current trips as a reference point, this study is based upon a survey instrument that asks repeated questions about recreational trip taking behavior under ever-rising travel costs. The responses by each individual are used to construct a panel. By exploiting the panel aspect of the data we are able to estimate fixed effects models of demand. These models are based upon combining observed behavior and the responses to the contingent behavior questions. Adopting a fixed effects approach allows us to "net out" otherwise unquantified individual heterogeneity that might compromise our ability to isolate unbiased price effects. Rather than controlling for myriad individual-specific attributes by attempting to measure them accurately and incorporate them explicitly into the demand specification as control variables, the fixed effects model allows systematic differences to enter implicitly as an individual-specific difference in the intercept term. Extracting panel data from each individual respondent may reduce the need for surveys to pose extensive questions about sociodemographics. This less-invasive questioning may increase response rates.

The estimates from our models also allow us to test explicitly the hypothesis that the responses from the contingent behavior questions reflect the same underlying demand relationship as the observed behavior. Our observed and contingent behavior questions elicit responses that are empirically compatible and can be analyzed using identical specifications. The panel approach forms a revealing test of the consistency between observed behavior and hypothetical behavior in this sample. This endeavor complements earlier research, such as Dickie, Fisher and Gerking (1987) where independent models are used for observed and hypothetical market data. We also draw upon prior research (e.g. Cameron, 1992, Adamowicz, Louviere, and Williams, 1992, and Jakus, 1993) where other models have been designed to impose and/or test consistency between observed and a single hypothetical behavior scenario.

Two different classes of econometric panel data models are utilized in this analysis. The first is the classic fixed effects ordinary least squares (OLS) panel estimator based upon the assumption of normal error distributions. However, there are two characteristics of the data that suggest that the conventional fixed effects model may be inappropriate. One is that the trip data are non-negative integers, generally less than ten. Second, a feature of the contingent behavior questions in this study is that many respondents reach their choke price (the lowest price at which they choose to purchase zero recreational trips) as trip costs are increased during the contingent behavior part of the survey. To address these complications we also utilize a Poisson fixed effects specification. This second approach employs the framework of Hausman, Hall and Griliches (1984) to develop a Poisson panel model.

The paper proceeds in the following way. The second section outlines the basic model of demand we adopt in this study. The third section provides an overview of the data. The fourth section presents and discusses the results of our analysis. The final section concludes the paper and discusses some of the limitations and prospects of our analysis.

Models

We begin with a stylized individual's demand curve for a recreational site. This demand is derived from the classical constrained utility maximization problem wherein utility is twice differentiable and there is a linear budget constraint. Maximizing this utility gives the true underlying Marshallian[1] demand;

$$Q = F(P,X) \tag{1}$$

where Q is the quantity demanded, P is the travel cost to the site and X is a vector of individual attributes. In principle, the true demand for a site by an individual (1) should be reflected in both her observed behavior (revealed preferences) and her responses to the contingent behavior questions posed in the survey (stated preferences). The modelling approach described in this paper can be used to examine the empirical consistency of demand information from the revealed preference question and demand information contained in the answers to the contingent behavior questions.

Possible differences between the observed and contingent behavior demand models can be accommodated straightforwardly in this setting. This issue is whether quantity demanded, Q, is systematically different depending on whether the price is a contingent behavior one, $P|_{cb}$, or an observed price, $P|_{ob}$.

$$Q = F(P|_{cb}, P|_{ob}, X) \tag{2}$$

Given repeated observations on Q for individuals, equation (2) could be estimated using classical OLS fixed effects models. However, the dependent trips variable in equation (2) contains many zeros and consists of non-negative integers. This suggests a second approach.

Poisson regression techniques deal explicitly with these data attributes (see Hausman, Hall and Griliches, 1984; Hall, Griliches and Hausman, 1986; Cameron and Trivedi, 1986; and Gray and Jones, 1991). The basic Poisson regression is found by maximizing the likelihood shown in equation 3.[2]

$$\log L = \sum_i [-\lambda_i + Q_i X_i \beta - \ln(Q_i!)] \tag{3}$$

where X_i is a vector of explanatory variables including only a constant term and price P in our pooled data models, β is a two-element vector of estimated coefficients, $\lambda_i = \exp(X_i\beta)$ and ! denotes factorial. In our differentiated models, the contingent behavior scenarios are treated as the base case ($D_i = 0$) and a dummy variable for observed behavior ($D_i = 1$) is included by itself and interacted with price P. This renders β a four-element vector in the differentiated models.

In the simplest Poisson regression models, all randomness is assumed to be from the Poisson process. This specification presumes that all systematic variation is captured in the independent variables, X. This seems unlikely in a multiple-scenario contingent behavior context. It seems more plausible that there are important permanent, unmeasured, and systematic differences among the respondents. These differences could include ability to answer contingent behavior questions, personal beliefs about nature, or expectations about future recreational desires.

These considerations can be addressed with the fixed effects generalization of the Poisson model. Following Hausman, Hall and Griliches (1984) we introduce respondent-specific effects through the use of conditional maximum likelihood. In the conditional likelihood method we try to explain each observation on number of trips given knowledge of the total number of trips for that respondent, $Q_i = \Sigma_j Q_{ij}$. Fortunately, the Poisson distribution belongs to the exponential family of distributions. Hausman, Hall and Griliches (1984, p. 919) show that log likelihood of the Poisson fixed effects model is:

$$\begin{aligned} \text{LogL}(\beta) = & \sum_{i=1}^{N} \log\Gamma((\sum_{j=1}^{M} Q_{ij}) + 1) - \sum_{i=1}^{N}\sum_{j=1}^{M} \log(\Gamma(Q_{ij}+1)) \\ & - \sum_{i=1}^{N}\sum_{j=1}^{M} Q_{ij} \log \sum_{s=1}^{M} \exp[-(X_{ij}-X_{is})\beta] \end{aligned} \tag{4}$$

where Γ is the mathematical gamma function, i denotes individuals and j denotes within-individual observations.[3] The variables represented by the X vector depend upon whether we differentiate between contingent and observed responses. As Hausman, Hall and Griliches (1984) have shown, this specification is analogous to the conventional OLS fixed effects technique.

Available Data

The data were collected in a mail survey of ten thousand Nevada anglers who held fishing licenses during 1988.[4] 2002 anglers returned questionnaires. The survey consisted of two parts. The first part asked about all angling trips during 1988 and collected demographic information. The second asked three contingent behavior questions.

The bulk of the questionnaire focused on actual trips. The information obtained included: demographics, total trips during the year, and an allocation of the trips on a site-by-site basis. Travel cost to the average site was calculated in the following way. First the average distance traveled was measured on state maps. Then (assuming an average speed of 50 miles per hour) a combined travel time/out-of-pocket cost calculation was made. The travel time was valued at one third the hourly wage and out-of-pocket costs priced at 25 cents per mile.[5]

The contingent behavior portion of the survey was structured by first asking about total actual trips in 1988, and then asked the following three open ended contingent behavior questions. The precise questions were:

We would like to find out how many fishing trips you might have taken if the trips had been more expensive.

*a. If the cost was 25 percent more, how many trips would you have taken?*_____

*b. If the cost was 50 percent more, how many trips would you have taken?*_____

*c. If the cost was 100 percent more, how many trips would you have taken?*_____

The contingent behavior questions directly elicit the *number of trips*, Q, each respondent would have made under the three price scenarios. The increased cost for each respondent was inferred from the original travel cost calculation for the average trip. One limitation of our data is that we do not know exactly which trips would be foregone as the cost increases.

In this analysis, the data on observed trip-taking behavior and the data from the contingent behavior questions are pooled. This approach results in *each individual* being represented in four observations. A panel is formed by ordering the individual responses from the cheapest (the observed behavior), to the most expensive (the contingent behavior answer corresponding to a doubling of cost). To simplify the analysis the sample is restricted to those respondents who provided a complete set of four observations on the demand data. This results in a balanced panel data set with a total of 5580 observations for 1395 individuals. The mean levels of the primary variables across the 5580 observations were: trips (6.97) and costs ($157.86). The means across just the 1395 observed data points were: trips (10.60) and cost ($109.81).

Econometric Analysis

The results reported in this section include standard non-panel estimates and fixed effects panel data estimates for both OLS and Poisson distributional assumptions. In any analysis of panel data, an important consideration is the choice between a fixed effects specification and a random effects specification. The fixed effects specification offers two important advantages in this context. Since we are dealing with individuals cross-sectionally, the fixed effects approach allows us to net out the individual effects. In essence, each person has an implicit individual dummy variable that corrects for their particular personal characteristics and angling opportunities. This allows us to focus on the only variables that do change across the within-individual responses: the trip cost and whether that cost is observed or hypothetical. Second, the fixed effects approach does not assume that individual effects are uncorrelated with other regressors as does the random effects model. Applying a random effects model when individual effects are correlated with other variables in the model results in inconsistent estimates.

Ordinary Least Squares Regression Results

The first set of results is shown in Table 1. Table 1 contains the results for both a standard OLS specification and the conventional OLS fixed effects specification (see Greene 1990). Since there are many respondents reporting zero trips during the 1988 season under one or more of the hypothetical scenarios, we are more or less limited to a linear functional form (among the standard alternatives in the literature). For each type of model (without and with fixed effects), two different specifications are reported in Table 1. The first specification (denoted "pooled") includes a single cost variable and does not differentiate between the actual costs associated with the observed behavior and the three levels of contingent behavior costs proposed to each individual. The second specification (denoted "differentiated") allows the coefficient on the observed cost variable to differ from the coefficient on the cost contingent data variables.

The distinction between observed and contingent data is an important issue in this study. Since our concern focuses on contingent behavior's potential deviation from observed data, we choose to analyze the results at the means (over the 1395 respondents) of the *observed* data rather than at the means of the pooled contingent and observed data. Where cost and trip data are required, the econometric results use the observed mean cost of $109.87 (versus the higher pooled mean cost of $157.86) and observed mean trips of 10.60 (versus the lower pooled mean trips of 6.97). Clearly, all of the elasticity results described below could readily be evaluated at the means of the pooled data rather than at the means of observed data.

Most of the parameters in Table 1 are precisely estimated, and many are significant well beyond the 1% level. The common-intercept OLS specification suggests no difference between the demand curves implied by the observed and contingent behavior data. An F-test shows that the generalization to include a distinct observed travel cost intercept term and price does not make a significant contribution to the simple pooled specification. The value of the test statistic is 8.2 which is not significant at conventional levels. This result does not extend to the fixed effects models, where the demand associated with the contingent data is significantly less elastic than that associated with the observed data. The value of the F-test for the joint significance of the differentiated price parameter and intercept is 37.33 which is significant at the 1% level.

To see the differences between the different OLS-based specifiations, first consider the demand elasticities implied by the pooled data specifications. The common-intercept OLS pooled model produces a demand elasticity of -0.61 when evaluated at the means of the observed data (computed as (109.81/10.6)*(-0.0595)). The fixed effects OLS pooled model shows a demand elasticity of -0.63 ((109.81/10.6)*(-0.0612)). Now consider the demand elasticities implied by the differentiated models. The common-intercept OLS differentiated model implies a demand elasticity of -0.48 (109.81/10.6)*(-0.0466)) for the contingent data but -0.39 (109.81/10.6)*(-0.0466+0.0088)) for the observed data. For the fixed effects OLS differentiated model, the estimated demand elasticity for the contingent data is -0.50 (109.81/10.6)*(-0.049))and for the observed data it is -1.00 (109.81/10.6)*(-0.049-0.0482)). Keep in mind that all elasticities are being evaluated at the means of the observed data.

Point estimates of seasonal consumer surplus implied by each of the different specifications explored in this paper are summarized in Table 3. Bockstael, Hanemann and Strand (undated) have derived consumer surplus formulas corresponding to linear demand specifications. For a linear specification such as the one used in our OLS-based models, they show that gross seasonal consumer surplus can be calculated as $(\text{trips})^2/-2\beta$, where trips is the quantity of trips in a season and β is the estimated coefficient on the price (travel cost) variable. Using this formula, the pooled standard OLS demand model provides a point estimate of the seasonal consumer surplus to anglers of \$944 (10.6^2 /(-2)*(-0.049-0.0482)). The pooled data fixed effects model produces a point estimate of seasonal consumer surplus of \$917, which is quite close to the simple OLS estimate of \$944. In contrast, the differentiated OLS fixed effects models shows a sizable difference between the contingent and observed behavior consumer surplus point estimates. According to the differentiated model contingent behavior demand parameters, consumer surplus is estimated to be \$577. The observed data increases the estimated consumer surplus well above the simple OLS or the contingent behavior estimates to \$1146. This is more than double the contingent data estimate. Moving to a fixed effects framework that accounts for individual heterogeneity provides sharply different welfare estimates from the observed and contingent behavior models.

Maximum Likelihood Poisson Regression Results

The Poisson-based regression results are displayed in Table 2. Poisson regression offers several advantages over OLS in this situation. One is that the Poisson distribution incorporates non-negative integers as outcomes. Another is that the Poisson distribution allows for zeros in a natural manner as well. Zeros are especially important in this context. Any well designed contingent behavior study of the type we are analyzing will force many people to their choke price. In the limit, everyone in the sample could be forced to zero visits by scenarios with sufficiently high travel costs.

All of the parameters in Table 2 are precisely estimated. Two general points are worth mentioning. First, as in the OLS case, the differentiated models perform better than the pooled models. Secondly, the fit of the fixed effects models is markedly better than the standard models. The improvement in the maximized value of the log likelihood exceeds 22,000. Clearly, the fixed effects models have sharply improved explanatory power.

Like their OLS counterparts, the demand elasticities associated with the Poisson models fall within conventional ranges for travel cost models of demand for fishing trips. The pooled standard Poisson has a demand elasticity of -1.05 (since the specification is inherently log-linear, the elasticity computation involves only mean observed costs: -0.0096*109.81). The differentiated standard Poisson suggests a demand elasticities of -0.96 for the contingent behavior portion of the model, but a lesser elasticity (-0.40) for the demand function implied by the observed data of the model. Unlike the case for the analogous OLS models, the Poisson specifications do show a significant difference between the observed and contingent data. The value of the $\chi^2(2)$ test statistic is 50 (for the hypothesis that the simple Poisson contingent and observed demand functions are different) which is significant well beyond the 1% level. The corresponding value of the $\chi^2(2)$ test for the fixed effects Poisson is 26, also significant well beyond the 1% level. The pooled fixed effects Poisson model implies a demand elasticity of -1.07. The demand elasticities of the differentiated fixed effects Poisson specification are consistent with the estimate from the differentiated fixed effects OLS specification. The contingent behavior portion has a demand elasticity of -1.01 for the Poisson versus -0.63 for the OLS. The observed data suggests elasticities of -1.54 for the Poisson and -1.00 for the OLS. At the means of the data the estimated demand relationships based on fixed effects specifications seem to be reasonably stable across specifications.

Since all of the Poisson models impose a semi-logarithmic functional form, consumer surplus is calculated as (trips)/β (this formula is also provided by Bockstael, Hanemann and Strand (undated)). Consumer surplus point estimates for our Poisson specifications are also displayed in Table 3. The pooled Poisson without the observed data dummy variable and interaction term produces a point estimate of the seasonal consumer surplus of $1104 (10.6/-0.0096) at the mean of the observed data. The differentiated specification of the ordinary Poisson suggests that the consumer surplus is $2864 for the contingent data, but dramatically smaller for the observed data at $1204. The fixed effects Poisson pooled model provides an estimate of seasonal consumer surplus of about $1081. In the differentiated model the contingent behavior consumer surplus is estimated to be $1152, with the differential for observed behavior decreasing the estimate to $751.

Conclusion

In this example, differences in demand implied by contingent behavior as opposed to observed data persist across alternative estimating specifications. In all cases except one, the contingent behavior questions imply much higher consumer surplus values than does observed behavior. The exception is the simplest pooled OLS analysis where we do not find any statistically significant difference between the demands implied by observed and contingent behavior data. For the fixed effects OLS model, evaluated at the means of the observed data, the contingent behavior consumer surplus is nearly double the observed behavior estimate. This difference is reduced by switching to Poisson distributional assumptions. The fixed effects Poisson specification suggests that the contingent behavior estimates of consumer surplus may be only 50% higher than the observed data estimates.

An important limitation to the empirical results of this analysis is that we do not know from the data exactly which trips are foregone as costs increase. Therefore, we must maintain the hypothesis that the quality of the trips remains constant as costs increase. This assumption, however, is purely the result of the particular data set we examine in this study. While this is a strong assumption, it is less strong in the context of Nevada anglers who have few angling choices. With fewer choices, it is harder to substitute among sites than in most situations.

Perhaps the clearest finding of this analysis is that the panel estimators provide a powerful tool when analyzing contingent behavior data. By using a fixed effects approach we are able to "net out" the individual respondent heterogeneity. We also find that Poisson models that naturally account for the non-negative integer nature of the dependent variable (trips) show much smaller differences in demand between contingent and observed behavior than do OLS models.

Fixed effects estimators always involve a tradeoff for the researcher. They allow for latent individual-specific intercept terms but generally preclude the explicit estimation of the effects upon demand of any regressor that does not vary across observations for a particular individual. If the influence of sociodemographic factors upon demand for resource is important to policy decisions then the use of fixed effects models becomes problematic. But in other welfare applications, it is beneficial to be able to net out sociodemographic factors by accommodating diverse forms of individual heterogeneity in a fixed effects model in order to concentrate exclusively on the effects of prices upon quantities demanded. For example, as long as the sample of people is random the results of a fixed effects analysis can be expanded to provide population welfare estimates in the conventional manner. This is usually the case in cost-benefit analysis. Contingent behavior scenarios on survey instruments provide a valuable opportunity for the researcher to explore variations in quantities demanded as a function of price under conditions of ceteris paribus *for each respondent.*

Some insights gained from the conduct of this study are worth noting for the benefit of future studies. First, it would have been helpful if the contingent scenarios regarding travel costs could have departed from the strict 25%, 50% and 100% increases used on all questionnaires in our present survey data. The greater the number of design points for the contingent scenarios, the easier it would have been to explore alternative specifications that require greater resolution regarding the functional form of demand over this range of alternative travel costs.

The mixed contingent/observed behavior approach combined with panel data techniques would appear to have much to offer for other traditions of econometric analysis in environmental valuation. For example, random utility models are often estimated in a logit framework. Fixed effect logit regression techniques are well established. In a fixed effect logit RUM framework it would certainly be possible to ask contingent behavior questions that address quality changes rather than simply the access questions analyzed in this study. The fixed effects approach would allow researchers to net out individual heterogeneity in order to focus more clearly upon the likely consequences of changes in site-specific attributes.

Endnotes

1. In this analysis we focus on Marshallian demand and consumer surplus because our scenarios cause only marginal changes in household income. As a result income does not change across scenarios for any given individual. The panel estimation approach we develop in subsequent sections will not serve to identify demand coefficients for independent variables that do not exhibit variation within individuals. If we were dealing with a case where there were non-marginal changes in income across the contingent scenarios, income could be handled independently, and a Hicksian analogue to our approach could be developed.

2. In this paper, we examine only the Poisson regression model. We do not pursue the issue of possible overdispersion that occasionally necessitates generalization (e.g. to a negative binomial specification). Gourieroux, Monfort, and Trognon (1984) show that the parameter vector β is consistently estimated provided that the mean, $\lambda_i = \exp(X_i\beta)$, is correctly specified. If the λ_i are distributed Poisson the estimates are also efficient.

3. The details of the Poisson fixed effects conditional log-likelihood function are also given in Gray and Jones (1991).

4. Issues of sample selection are ignored in this analysis. A more thorough assessment of representative demand function would have to address sampling corrections in conjunction with the models discussed here.

5. Our qualitative findings are not sensitive to these assumptions.

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Table 1. Ordinary Least Squares (OLS) Results[a] (n = 1395*4 = 5580)				
Variables	**Standard OLS**		**Fixed Effects OLS**	
	Pooled	Differentiated	Pooled	Differentiated
Constant	16.3828*** (0.5793)	13.8860*** (0.8501)		
Observed Data Dummy (D)		0.8729 (5.8237)		6.9937** (3.3513)
Travel Costs (P)	-0.0595*** (0.0354)	-0.0466*** (0.0047)	-0.0612*** (0.0018)	-0.0490*** (0.0024)
Observed Data × Travel Costs (DP)		0.0088 (0.0526)		-0.0482* (0.0305)
R-Squared	0.0478	0.0503	0.8169	0.8193

***significant at the 1% level or beyond
**significant at the 5% level or beyond
*significant at the 11% level or beyond

[a] standard errors in parentheses

Table 2. Poisson Regression Models[a] (n = 1395*4 = 5580)				
Variables	Standard Poisson		Fixed Effects Poisson	
	Pooled	Differentiated	Pooled	Differentiated
Constant	3.3909*** (0.0206)	3.2454*** (0.0334)		
Observed Data Dummy (D)		-0.4816*** (0.1686)		0.6021*** (0.1892)
Travel Costs (P)	-0.0096*** (0.0001)	-0.0088*** (0.0002)	-0.0098*** (0.0001)	-0.0092*** (0.0002)
Observed Data × Travel Costs (DP)		0.0051*** (0.0015)		-0.0049*** (0.0017)
Maximized Log-Likelihood	-38,453	-38,428	-16,133	-16,120

***significant at the 1% level or beyond

[a] standard errors in parentheses

Table 3. Summary of Consumer Surplus Point Estimates: Annual Benefits

	OLS		Poisson	
	Standard	Fixed Effects	Standard	Fixed Effects
Pooled	$944.20	$917.97	$1104.16	$1081.63
Differentiated Model: Observed Behavior	-[a]	$577.98	$2864.86	$751.77
Differentiated Model: Contingent Behavior	$1205.57	$1146.53	$1204.54	$1152.17

[a] no statistically significant difference between the contingent and observed welfare estimates

Valuing Public Goods: Discrete Versus Continuous Contingent-Valuation Responses[1]

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Abstract

We compare open-ended and dichotomous-choice contingent-valuation questions. Responses to open-ended questions are taken as "true" values, and dichotomous-choice bid amounts are applied to these values to simulate respondents' answers to a dichotomous-choice question. The null hypothesis of no difference in the synthetic and actual dichotomous-choice estimates is rejected.

Valuing Public Goods: Discrete Versus Continuous Contingent-Valuation Responses

Introduction

Economic analyses of environmental issues have maintained a high profile in the national policy arena through the 1970s, 1980s, and into the 1990s (Cropper and Oates, 1991). Throughout this period economists have continued to develop methodologies for assessing the benefits and costs of environmental regulations. One approach, contingent valuation, has received extensive consideration because of advantages over revealed-preference techniques when choice data are limited or unavailable. Contingent valuation uses survey-research methods to elicit individuals' expressed Hicksian surplus for a specified change in the environment (Mitchell and Carson, 1989).

Contingent valuation is most useful in situations where behavioral measures of Hicksian surplus are lacking. Therefore, it often is not possible to validate contingent-valuation by comparisons with market or quasi-market estimates of value for the same commodity. In such cases comparing value estimates derived from different survey-instrument designs, including differences in question format, provide a basis for tests of convergent validity (Carmines and Zeller, 1979). A number of different formats have been used to ask contingent-valuation questions, and the common formats, in alphabetical order, are dichotomous choice, iterative bidding, open ended, and payment cards (Boyle and Bishop, 1988; Smith, Desvousges, and Fisher, 1986; Sellar, Stoll, and Chavas, 1985). Currently, open-ended and dichotomous-choice questions appear to be the most popular formats.[1] Open-ended questions were one of the earliest questioning formats employed in contingent-valuation studies (Hammack and Brown, 1974). Dichotomous-choice questions were developed later in the evolution of contingent valuation (Bishop and Heberlein, 1979), and are the most commonly employed questions in the empirical literature today.[2]

In practice, open-ended and dichotomous-choice questions are intended to measure the same theoretical construct. If these two question formats provide statistically congruous estimates of Hicksian surplus for the

[1]Iterative-bidding questions are not widely used due to the potential anchoring (starting-point bias) of valuation responses on the initial bid posited to respondents (Boyle, Bishop, and Welsh, 1985; Smith, Desvousges, and Fisher, 1986). Payment cards, which were developed by Mitchell and Carson, also have not received widespread acceptance among contingent-valuation practitioners due, we believe, to concerns that respondents' valuation responses may be influenced by subjective decisions regarding the information presented on the payment cards.

[2]Dichotomous-choice questions have been variously referred to as "take-it-or-leave-it," "closed-ended," or "referendum" questions in the literature.

commodity being evaluated, procedural invariance holds (Tversky, Sattah, and Slovic, 1988; Kahneman and Tversky, 1984) and convergent validity is established (Carmines and Zeller, 1979). Procedural invariance implies that either questioning format is equally appropriate for contingent-valuation applications. However, Hoehn and Randall (1987) have argued that open-ended questions are likely to yield smaller estimates of central tendency than are dichotomous-choice questions. Although this proposition is generally supported by the literature, empirical tests of procedural invariance have been hampered by methodological and experimental design limitations.

For example, dichotomous-choice means are nonlinear functions of estimated coefficients from a structural model, which complicates the derivation of standard errors of estimates of central tendency. Therefore, most studies that have presented dichotomous-choice and open-ended estimates of central tendency have not conducted statistical tests (Bishop and Heberlein, 1979 and 1990; Bishop, Heberlein, and Kealy, 1983; Johnson, Bregenzer, and Shelby, 1990; Sellar, Stoll, and Chavas, 1985). Sellar et al. present confidence intervals for their dichotomous-choice estimates, but these bounds were based on an *ad hoc* procedure. More recently, Milon (1989) recovered estimates of standard errors for dichotomous-choice estimates from within-sample heterogeneity of characteristics of survey respondents. None of these studies provides a rigorous test of congruence between dichotomous-choice and open-ended question formats.

A second limitation of the existing comparisons is that the same respondents have been asked both open-ended and dichotomous-choice questions (Bishop and Heberlein, 1979 and 1990; Bishop, Heberlein, and Kealy, 1983; Kealy and Turner, 1993). Because responses to subsequent valuation questions are contaminated by the respondents' answers to prior valuation questions, this nonindependence prohibits the application of classical statistical tests of congruence. Although, Kealy and Turner develop a statistical test to be applied when respondents answer both dichotomous-choice and open-ended questions, it is nearly impossible to control for all of the effects of an initial valuation question on respondents answers to a subsequent valuation question. Consequently the desirable experimental design is to apply each valuation question to independent subsamples drawn from the same population.

An important contribution to the contingent-valuation literature is the adaptation of bootstrapping procedures to dichotomous-choice valuation data by Park, Loomis, and Creel (1991). This procedure allows for direct comparisons of discrete-choice estimates of central tendency with estimates of central tendency from continuous (open-ended) response data. Statistical tests of means from independent samples can be performed using established statistical procedures.

We employ the Park, Loomis, and Creel adaptation concurrently with a new approach for comparing Hicksian surplus estimated from independent dichotomous-choice and open-ended data sets. We use open-ended response data to construct a synthetic dichotomous-choice data set from which comparisons can be made with the actual dichotomous-choice data. The essence of this approach is to apply dichotomous-choice offer amounts to data generated from respondents' answers to an open-ended question to simulate how they might have answered a dichotomous-choice question evaluating the same increment of an environmental commodity. Taking the open-ended responses as "true" reflections of the values respondents hold, the synthetic dichotomous-choice data mimics the behavioral response pattern of how these respondents would have responded if they had received a dichotomous-choice question. Convergent validity, or procedural invariance, is established if the actual and synthetic dichotomous-choice estimates are statistically congruous.

Modeling Framework

The valuation problem can be formalized as:

$$V(p, y - t) = V(p^c, y) \qquad (1)$$

where V(●) is an indirect utility function, p is an implicit price for access to an environmental asset, y is income and t is a measure of Hicksian compensating variation to ensure access. The prevailing level of the implicit price is p, and p^c is at or above the choke price such that this person would choose not to use the resource in question.

The wording of a generic, open-ended valuation question to estimate t might be posed as: "What is the most that you would pay per year for access to this resource?"

Responses to this question generate a continuous distribution that typically includes a probability mass at $0. Analyses generally consist of tests of sample means and perhaps the estimation of tobit equations to identify variables that significantly explain variation in expressed willingness to pay across respondents.

The generic, dichotomous-choice question to estimate t might be posed as: "Would you pay $x per year for access to this resource?" Responses to this question generate a binomial distribution of "yes"/"no" answers, and an estimate of t cannot be obtained directly from these responses. The traditional form of analyzing these data follows Hanemann (1984) where equation (1) is converted in the following manner:

$$V(p, y - x) + e_1 \; O(^{>},_{<}) \; V(p^c, y) + e_2 \qquad (2)$$

where x $O(^{<}{}_{,>})$ t and the e_i are random errors.[3] The basic presumption is that respondents know their preferences with certainty, but there are components that are unobservable to empirical investigators. Subsequently, either a probit or logit model traditionally has been fitted to discrete-response data (Bowker and Stoll, 1988):

$$\begin{aligned} Prob(yes) &= Prob(x<t) \quad (3) \\ &= Prob[V(p, y - x) - V(p^c, y) > e_2 - e_1] \\ &= F(t) \end{aligned}$$

where F(t) is a cumulative distribution function with a parametric specification defined by the assumed distributions of the e_i's. Estimated willingness to pay is derived as:[4]

$$E(t) = \int O(^{00}{}_{,00}) [1 - F(t)]dt. \quad (4)$$

The variance of this estimate is commonly derived from bootstrapping procedures (Park, Loomis, and Creel, 1991; Krinsky and Robb, 1986).

Experimental Design

Our experimental design avoids question-order effects by having independent samples of respondents answer either an open-ended or a dichotomous-choice valuation question, with the format of the valuation question being the only difference between survey instruments. We then randomly applied dichotomous-choice offer amounts ($x in equation (2)) to the open-ended responses in the same way they were applied to the respondents in the dichotomous-choice experiment. That is, if an open-ended response was $z and $z < $x, we assigned a response of "no." Conversely, if $z > $x, the assigned response would be "yes." This procedure is comparable to the approach employed by Cameron and Huppert (1991) to derive a synthetic dichotomous-choice data set from responses to a valuation question employing a payment card to elicit valuation statements. However, Cameron and Huppert only knew the intervals from the payment card in which respondents valuation statements occurred (i.e., they did not have a continuous response surface), and they did not have responses from an actual dichotomous-choice question with which to make com arisons. Our research not only derives synthetic dichotomous-choice value estimates from open-ended response data, but also includes a direct comparison of

[3]An alternative approach to analyzing these data has been proposed by Cameron (1988) and Cameron and James (1987). McConnell (1990) has demonstrated that the choice between the Hanemann and Cameron interpretation of dichotomous-choice valuation data is a matter of style and is not based on purported technical merits of either approach. Within this manuscript we will follow Hanemann's formulation, however analyzing the data with the Cameron formulation would not change any of our statistical results.

[4]Boyle, Welsh, and Bishop (1988) and Hanemann (1989) have discussed alternative specifications of the expected value when these bounds of integration are not satisfied.

synthetic value estimates with value estimates derived from independently collected dichotomous-choice data. Taking open-ended data as the "truth" allows us to explore whether the differing behavior structures of open-ended and dichotomous-choice questions result in statistically congruous estimates of value.

The Data

We applied the comparisons of synthetic and actual dichotomous-choice data to three data sets. The first two data sets involve moose hunting in Maine; one sample hunted moose in 1989 and the other sample applied for, but did not receive, a permit for the 1989 hunt.[5] We will refer to these as the "permit" and "no-permit" samples hereafter. In both cases the valuation issue was the value hunters place on moose hunting.[6] For the permit sample, *ex post* Hicksian compensating surplus is estimated by surveying hunters after the hunt. These valuation responses are based on actual hunting experience. The same value was estimated for the no-permit sample by asking applicants to assume they had the opportunity to hunt, and then asking the same valuation question posed to members of the permit sample.

Comparisons of synthetic and actual dichotomous-choice data for each of these treatments is important. Cummings, Brookshire, and Schulze (1986) argue that contingent-valuation respondents must be "familiar" with the commodity being evaluated and must have "valuation/choice experience" with relevant increments of the commodity (p. 105). *Ex post* estimates of Hicksian surplus from the permit sample satisfy all of the Cummings, Brookshire, and Schulze "Reference Operating Conditions" (ROCs) and, consequently, represents our "cleanest" comparison between synthetic and actual dichotomous-choice data. If these estimates are found to be statistically congruous, the subsequent question is whether this result can be extended to the no-permit sample where one of the ROCs is violated. Respondents in the no-permit sample have some choice experience because they applied for a permit to hunt, but they do not have familiarity because they have not experienced a moose hunt. Alternatively, if synthetic and actual dichotomous-choice estimates are not statistically congruous for the permit sample, the question is whether this incongruity is exacerbated when we compare data from the no-permit sample.

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[5]The moose hunter data were collected via mail survey in the Fall of 1989. The response rates, as a percent of the number of surveys deliverable by the U.S. Postal Service, were 92 percent for the permit sample and 84 percent for the no-permit sample.

[6]Dichotomous-choice bid amounts in these experiments were assigned from a continuous distribution using the protocol outlined by Boyle, Bishop, and Welsh (1988). The distribution of bid amounts was derived from a dichotomous-choice valuation question applied in a survey of moose hunters after the 1988 hunt. This same distribution was used to generate dollar amounts for the permit and no-permit samples and for the actual and synthetic dichotomous-choice applications.

The third data set involves the provision of response centers to prevent environmental damage from oil spills in the U.S.[7] Comparisons of synthetic and dichotomous-choice oil spill data continues the progression of the investigation outlined above.[8] The valuation responses reported here are *ex ante* estimates of option price (Bishop, 1982; and Smith, 1983).[9] Not only are the familiarity and choice experience conditions violated, a third ROC is violated in that the policy to be valued involves uncertain outcomes.

Results

Most contingent-valuation studies employing open-ended questions generally do some censoring of response data by removing protest zeros, trimming high observations and statistically searching for data outliers (Desvousges, Smith, and Fisher, 1987; Mitchell and Carson, 1989; and Reiling et al., 1989). Similar exclusions are not possible with dichotomous-choice questions because of the discrete-response data. To make a fair comparison of the open-ended and dichotomous-choice data we include responses of all individuals who answered either question format.

We assume normal error distributions for equation (3) and analyze the actual and synthetic dichotomous-choice data using probit models. We also assume a linear specification of the indirect utility function, resulting in income canceling out of estimated probit equations (Hanemann, 1984). The variables included in the moose-hunter and oil spill equations are defined below. We present univariate statistic for these variables and the estimated probit coefficients.

[7]The oil spill data were collected in a self-administered survey conducted in Southlake and Lakeshore Malls in Atlanta, Georgia. It was not our intention to develop value estimates that could be extrapolated to any specific population of respondents. Rather, the objective was to conduct a test of theoretical validity on a sample of individuals who might be asked to respond to any contingent-valuation survey of adults. Therefore, it was not necessary to know the population from which the sampling frame was drawn. The survey research literature states that an experiment needs only to be internally valid as long as the results will not be generalized to a larger population. Internal validity can be achieved for any population when random sampling allows for comparable experimental and control groups (Babbie, 1979; Sellitz, Wrightsman, and Cook, 1979). Other experimental applications of survey research in malls include Viscusi and O'Connor (1984) and Viscusi, Magat, and Huber (1985). Mall intercept surveys have also been deemed sufficiently reliable to be admitted as legal evidence (McCarthy, 1984).

[8]Six bid amounts were used in this experiment ($10, $25, $50, $100, $250, and $1,000). The distribution of bids was uniform across these six dollar amounts, and the same distribution of bids was employed in the actual and synthetic dichotomous-choice experiments. Using six dollar amounts is consistent with the evolving literature on the selection of dichotomous-choice dollar amounts (Alberini, 1991 a and b; Kaninnen, 1992; Cooper, 1992).

[9]This experiment was conducted for response centers for small spills (less than 50,000 gallons) and response centers for all spills. We only report small-spill comparisons here to streamline the exposition. Our empirical findings for the small-spills and the all-spills treatments result in identical conclusions (Desvousges et al., 1992).

Moose Hunter Data

Univariate statistics for each variable in the moose hunter equations are presented in Table 1. The BID variable is a randomly assigned dollar amount used to develop the synthetic and actual dichotomous-choice valuation responses, respectively, for the open-ended and dichotomous-choice samples. The means of the other explanatory variables, HUNT, BAG, and BAG BULL, all suggest that the assignment of experimental treatments was random across respondents. The BAG and BAG BULL variables were not included for the no-permit sample because these respondents did not have the opportunity to hunt.

The estimated probit equations for the moose hunter data are presented in Table 2. Estimated coefficients for the permit sample are presented in the second and third columns, and coefficients for the no-permit sample are presented in the fourth and fifth columns. For the permit sample, all coefficients are significant except for the coefficient on the BAG BULL variable with the actual data. Only the coefficient on the bid variable is significant in the synthetic data equation. All significant coefficients in both equations have the expected signs. Moving to the no-permit sample, only the coefficients on the bid variable are significant, and these coefficients have the expected signs. The HUNT variable is omitted from the synthetic data equation because all respondents in this sample hunted other game during 1989 (see Table 1).

Oil Spill Data

Univariate statistics for each of the variables in the oil-spill equations are presented in Table 3. These data, as with the moose hunter data above, indicate the assignment of open-ended and dichotomous-choice treatments was random.

Estimated probit coefficients for the actual and synthetic oil-spill data are presented in Table 4. For the actual data, BID, READR, and NORGS are significant, and all coefficients have the expected signs except the coefficient for READR. Only BID has a significant coefficient in the synthetic data equation.

Comparisons of Actual and Synthetic Data

Estimates of central tendency for the open-ended data, and actual and synthetic dichotomous-choice data are presented in Table 5.[10]

The standard errors for the actual and synthetic dichotomous-choice estimates were derived by resampling from the original data and estimating the models. One thousand estimation iterations were conducted for each model.

The first comparison is between the open-ended and actual dichotomous-choice data. For the moose hunter data, the open-ended means are less than the means derived from the actual dichotomous-choice data. However, a significant difference only occurs for the no-permit sample ($z = -2.26$). In the oil-spill experiment the open-ended mean exceeds the actual dichotomous-choice mean, and this difference is significant ($z = 1.75$). The large open-ended mean in this sample is a result of a few respondents who provided extremely high valuations (e.g., $5,000, $10,000, $20,000, $25,000, $30,000, $35,000, $50,000), which also contribute to the relatively large standard error. According to the Cummings, Brookshire, and Schulze ROCs, hypothesized procedural invariance should hold for the moose hunter permit sample, and we could not reject the null hypothesis of no difference in the estimated means. Procedural invariance did not hold when one or more of the ROCs were violated, as demonstrated by the rejection of the null hypotheses of no difference in the estimated means for the no-permit and oil-spill samples.

The other key question of the experiment is whether the open-ended data, when treated as being reflective of respondents' true values in the derivation of synthetic dichotomous-choice estimates, are statistically congruous with the valuation estimates derived from the actual dichotomous-choice data. As can be observed from the third and fourth columns of Table 5, the synthetic means are all less than the comparable actual means, and each of the differences is significant. In turn, we conclude that procedural invariance does not hold when dichotomous-choice and open-ended data are compared using common statistical analysis procedures.

The difference between the actual and synthetic dichotomous-choice estimates is not surprising for the no-permit and oil-spill samples, since the initial open-ended estimates where different from the actual dichotomous-

[10]Trimming the open-ended moose hunter data if respondent's bids were greater than $10,000 or 25 percent of respondent's incomes, and removing outliers according to the Belsley, Kuh, and Welsch (1980) procedure, used by Desvousges, Smith, and Fisher (1987) and Reiling et al. (1989), reduces the mean for permit sample to $638 with a standard error of $44, and did not change the mean for the no-permit sample. No screen for protest zeros was included in the survey instrument. The resulting synthetic mean for the permit data is $436 with a boot-strapped standard error of $76. Applying the same screens to the oil-spill data and screening for protest zeros reduced the open-ended mean to $144 with a standard error of $239 The resulting synthetic oil-spill mean is $108 with a boot-strapped standard error of $38. As stated above, these censored data are not used in the comparisons because comparable screens cannot be applied to the dichotomous-choice data.

choice estimates. An interesting finding for both of these samples is that the synthetic means were less than the respective univariate means for the open-ended data. This result occurs because there is a probability mass at $0 and nonzero responses are clustered at low dollar amounts in the distribution of the open-ended data. Thus, the synthetic response data contains a large proportion of "no" responses, resulting in the lower estimated synthetic means relative to the open-ended data. This result, as noted above, is particularly true for the oil-spill data where a few extremely large valuation responses skew the open-ended mean upward. These insights also help to explain the difference between the actual and synthetic means for the permit sample. The synthetic data contain a relatively larger proportion of "no" responses leading to a lower, and a significantly different, mean.

Discussion

The research presented here constitutes a rigorous comparison of responses to dichotomous-choice and open-ended contingent-valuation questions. The procedure not only compares estimates of central tendency, but also allows for a behavioral comparison of answers to different contingent-valuation questioning formats. That is, taking responses to an open-ended question as being indicative of respondents "true" values, synthetic dichotomous-choice responses were developed to simulate how these respondents might have answered if they had of received a dichotomous-choice question. Although we conducted these comparisons in the context of dichotomous-choice and open-ended questions, this approach can be employed to compare dichotomous-choice responses to any other contingent-valuation questioning format that results in a continuous response distribution.

The findings from our statistical comparisons lead us to reject procedural invariance with respect to the application of dichotomous-choice and open-ended contingent-valuation questions, and this conclusion appears to include applications where Cummings, Brookshire, and Schulze's (1986) ROCs suggest contingent valuation works best—*ex post* estimates of use values. Our results support the Hoehn and Randall (1987) proposition that dichotomous-choice estimates exceed open-ended estimates. This relationship is true for all three comparisons of actual and synthetic dichotomous-choice means, and is true for two of the three comparisons of the open-ended and actual dichotomous-choice data. The one exception is a result of a few large and extremely influential responses in the open-ended oil-spill response data.

The notion of convergent validity, as proposed by Carmines and Zeller (1979), suggests that competing empirical methodologies can be accepted as valid if they provide statistically congruous estimates. This convergence did not occur in our study, implying that the choice of a contingent-valuation questioning format is not simply a matter of style or of convenience. Hoehn and Randall argue that dichotomous choice is a more appropriate format for contingent-valuation questions. However, in the absence of empirical tests of the validity of dichotomous-choice estimates, this recommendation, based on theoretical arguments, is premature. Such investigations should include tests of criterion validity as applied by Bishop and Heberlein (1979), but also must include more rigorous tests of formal statistical hypotheses.

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A Contingent Valuation Test of the Prominence Hypothesis

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Abstract

The prominence hypothesis suggests that choice questions tend to induce lexicographic preferences relative to matching questions. To investigate this hypothesis in a contingent valuation setting, we collect data from independent samples receiving dichotomous choice and payment card questions. By comparing actual with projected responses across individuals we conclude that purported "yea-saying" in dichotomous choice CV experiments may result from truthful revelation of preferences regarding the "right thing to do" rather than deliberate strategic behavior. By creating an adjusted set of responses we illustrate that the choice of value elicitation method influences marginal as well as expected willingness to pay.

Key words: payment card, discrete choice, yea-saying, lexicographic preference, forests.

A Contingent Valuation Test of the Prominence Hypothesis

The contingent valuation method (CVM) is routinely used to elicit individual willingness to pay (WTP) for improvement in an environmental good or service. WTP is theoretically defined as the point at which an individual is indifferent between an option defined by clearly specified levels of environmental quality and wealth and a second option defined by a more preferred level of environmental quality and a less preferred level of wealth. The accuracy and reliability of values obtained using this method rest on the premise that the value of a good revealed through a hypothetical transaction is no different than the value that would be expressed for that good in an actual transaction.

Recently, a number of experimental results have been reported that bring into question the reliability and accuracy of the CVM for making liability assessments and policy decisions. The basic issue is that individuals appear to behave in a non-economic fashion in the public good for income trade-off decision problems posed by the experimenters. Apparently anomolous results have been reported regarding issues of embedding (Kahnemann and Knetsch), marginal willingness to pay (Desvouges et al.), and procedural variance (Desvouges et al.; McFadden and Leonard).

Such assessments have lead some to argue that CVM is fatally flawed (Anonymous). However, we argue more optimistically that the above studies point out some of the issues that CVM practitioners need to address through careful, well-designed studies. In particular, we suggest that it is useful to reconsider how transactions, whether actual or hypothetical, can be logically interpreted. As pointed out by Fischhoff and Furby and Brown and Slovic, transactions depend on a variety of conditions regarding how individuals perceive the good in question, the value measure used and the social context, all of which may influence the assignment of value. Consequently, the possibility arises that substantive differences in assigned value for identical goods may be understood as functions of identifiable factors and that such functions may be used to explain apparent disparities in value.

Given the fundamental nature of the arguments pro and con concerning the validity of CVM, our ambitions in this paper are rather modest. We specifically address the third issue mentioned above - the premise that variation in value elicitation methods should not influence the measurement of value. Our basic premise is that WTP values are constructed by respondents during the course of a CV experiment and that values are sensitive to the value elicitation measure used. Interpretation of value, then, depends on the ability to understand and predict the cognitive process used to assign a value (Schkade and Payne). If analysts understand in a quantitative sense how wording influences cognitive process and value formation, then bias reduction adjustments may be derived. For example, if the ratio of values obtained using two different procedures were always constant, then obvious adjustments could be made to eliminate the bias. While that fortunate circumstance is not likely, we suggest a formal test of the hypothesis that the propensity for bias induced by procedural differences is randomly distributed - i.e. pure noise. Rejection of this hypothesis raises the possibility that descriptive information can be used to explain induced by procedural diferences.

This paper proceeds as follows. In Section II we present a behavioral model elucidating the concepts of procedural variance and the prominence hypothesis. In Section III we present our empirical model and Section IV describes our experimental design and empirical data. Section V presents our results and in Section VI we discuss the implications of this research.

The Prominence Hypothesis

A growing body of psychological literature argues that preferences are constructed, not simply revealed, in the process of value elicitation (see Payne, Bettman, and Johnson for a cogent review). This argument is based on the idea that core values (or "held" values) are translated into assigned values within a decision context (Brown). Recent research suggests that the process of translating core values into assigned values proceeds by the use of heuristic processes that are sensitive to the method of elicitation (Brown and Slovic; Tversky, Sattath, and Slovic). Consequently, variation in procedures such as how questions are asked may lead to the use of different heuristics and different assignments of value.

The prominence hypothesis postulates that individuals consistently discriminate between the more important and less important attributes of a decision problem, and that the prominent attribute is weighted more heavily in choice than in matching. A justification for dependence on the primary attribute in a choice task is provided by Tversky, Sattath, and Slovic (henceforth TSS):

> Because it is often unclear how to trade one attribute against another, a common procedure for resolving conflict in such situations is to select the option that is superior on the more important attribute. This procedure, which is essentially lexicographic, has two attractive features. First, it does not require the decision-maker to assess the trade-off between the attributes, thereby reducing mental effort and cognitive strain. Second, it provides a compelling argument for choice that can be used to justify the decision to oneself as well as to others. (p.372).

The propensity for yea-saying in dichotomous choice CV experiments has been noted and considered as a starting point bias problem (Mitchell and Carson). Rather than attributing yea-saying to anchoring on starting values, the prominence hypothesis suggests an interpretation based on a broader view of the nature of utility functions. For example, we consider Sen's argument that utility can be viewed not only in terms of individual well-being but also in terms of the "promotion of certain causes and the occurence of certain things, even though the importance that is attached to these developments are not reflected by the advancement of his or her well-being, if any, that they respectively cause" (p.41). Given an interpretation of utility that includes individual *agency* and *well-being*, the prominence hypothesis suggests that choice questions provide individuals with the opportunity to express their agency for "the right thing to do".

These ideas may be made more apparent by introducing some notation. Let $P=\{p_1,p_2,\ldots,p_n\}$ represent a vector of primary attributes and let $S=\{s_1,s_2,\ldots,s_n\}$ represent a vector of secondary attributes. For simplicity we consider the vector dimensions $n=2$. The object set is then defined by the product SxP with elements p_1s_1 and p_2s_2. Let $p_1s_1 \geq_c p_2s_2$ indicate that bundle p_1s_1 is at least as good as bundle p_2s_2 in a choice setting, and let $p_1s_1 \geq_m p_2s_2$ indicate that bundle p_1s_1 is at least as good as bundle p_2s_2 in a matching setting. Because indifference maps do not exist for lexicographic preferences (i.e. each composite good has no points other than itself to which it is indifferent) the tendency for choice tasks to induce lexicographic preferences as infered by the prominence hypothesis is developed by introducing the notion of context-specific (contingent) weights.

The existence of a utility function given the standard axioms of choice (reflexivity, completeness, transitivity, and continuity) depends upon the existence of numbering functions so that bundles with higher numbers are preferred to bundles with lower numbers (e.g., Deaton and Muellbauer). A preference function can then be generally linked to a utility function as

$$p_1s_1 \geq_i p_2s_2 \quad iff \quad U_i[F_i(p_1),G_i(s_1)] \geq_i U_i[F_i(p_2),G_i(s_2)] \tag{1}$$

where F_i and G_i are numbering functions, U_i is monotonically increasing in each of its arguments and $i=c,m$ (choice, matching).

Equation (1) suggests the existence of two families of indifference curves, one for choice problems and one for matching problems.

By assuming that utility is additively separable, a simple context-specific trade-off model can be derived from the general utility function in equation (1):

$$p_1 s_1 \geq_i p_2 s_2 \quad \textit{iff} \quad F_i(p_1) + G_i(s_1) \geq_i F_i(p_2) + G_i(s_2)\ ,\ i=c,m. \tag{2}$$

The TSS contingent weighting model is derived from equation (2) by extracting context-specific weights from the numbering functions F_i and G_i:

$$p_1 s_1 \geq_i p_2 s_2 \quad \textit{iff} \quad \alpha_i F(p_1) + \beta_i G(s_1) \geq \alpha_i F(p_2) + \beta_i G(s_2). \tag{3}$$

The marginal rate of substitution (MRS_i) between the attributes P and S in the contingent weighting model can readily be seen to be $MRS_i = \alpha_i F'(P)/\beta_i G'(S)$ (where the prime denotes the first derivative). The ratio of MRS_m to MRS_c can therefore be written:

$$\frac{MRS_m(PS)}{MRS_c(PS)} = \frac{\alpha_m F'(P)/\beta_m G'(S)}{\alpha_c F'(P)/\beta_c G'(S)} = \theta. \tag{4}$$

In the standard economic model, the ratio of MRS_m to MRS_c is unitary because only one family of indifference curves exists. In the contingent weighting model, however, it is postulated that $\alpha_m/\beta_m \neq \alpha_c/\beta_c$ due to differences in the context-specific weights. In particular, the prominence hypothesis proposes that choice problems are more likely to induce lexicographic preferences for the prominent attribute, or $\alpha_m/\beta_m < \alpha_c/\beta_c$.

We view θ as an index of the degree of choice-matching discrepency. As choice-based decisions become lexicographic (i.e. as $\alpha_c/\beta_c \rightarrow \infty$) the ratio of MRS_m to MRS_c approaches zero ($\theta \rightarrow 0$). In their experiments, TSS report θ values ranging from 0.19 for a public safety program to 0.86 for a profit sharing program. Their results confirm what we would expect *a priori* - the propensity for choice-based measures to induce lexicographic preferences is higher for public goods than for private goods. Indeed, this behavioral pattern has been recently observed and reported in other research (Boyle et al.; Kealy and Turner) and is consistent with our conjecture that choice based measures induce the agency aspects of individual utility to be expressed.

Our analysis is significantly different than earlier research in that we view θ as a propensity which varies over individuals rather than goods. What we are suggesting is that a referendum question causes some individuals to include agency aspects of their preferences in their response whereas such aspects would be ignored by the same people in a different decision context. Although we are not aware of a complete theoretical model that could be used to exactly specify critical individual characteristics, we expect *a priori* that variables related to the individual's budget constraint may play a role in explaining procedural variance.

Empirical Considerations

The method we use to test the prominence hypothesis in a CV setting is contingent upon having observations from independent samples of respondents. The only difference in the survey instruments we use is the form of the value elicitation question. For the "matching" question we use the payment card format and for the "choice" question we use a dichotomous choice question.

Our method consists of five steps. First, we estimate a model to explain WTP payment card responses using individual characteristics as explanatory variables. Second, using the explanatory variables and parameter estimates, we project the payment card model onto the sample receiving the dichotomous choice question to create a *projected dichotomous choice* variable. That is, projected willingness to pay is compared with the actual dichotomous choice offer amount and the consistent response is recorded. Third, we empirically investigate the propensity for procedural variance across individuals by constructing an indicator variable. The indicator we use is a dummy varible that is set equal to one if there is a discrepency between the actual and projected dichotomous choice responses. Otherwise the indicator is set equal to zero. Fourth, we estimate a maximum likelihood probability model that explains variation in the indicator variable by a set of individual characteristics. Finally, we construct an *adjusted dichotomous choice* variable based on the estimated procedural variance probability model that allows us to address issues regarding model specification in cases where paired observations are not available.

It should be noted that our method for creating the *projected dichotomous choice* variable differs from the method used by Cameron and Huppert and Boyle et al. to create *synthetic dichotomous choice* variables. In those studies, dichotomous choice offer amounts were randomly applied to individuals responding to either payment card (Cameron and Huppert) or open-ended (Boyle et al.) questions and consistent synthetic responses were directly derived. Because we want to be able to identify which individuals in our discrete choice sample answered the actual discrete choice WTP question in a way not predicted by our payment card observations, it is necessary to project expected WTP values based on individual characteristics.

Because our indicator variable is one-dimensionsal (procedurally variant/invariant) and the underlying WTP value is two-dimensional ($MRS_c > MRS_m$, $MRS_c < MRS_m$) for procedurally variant responses, it is critical to partition the data based on the actual dichotomous choice response. That is, for individuals responding to the actual dichotomous choice question with a yea, variance with the projected dichotomous choice response (i.e. nay) implies that $WTP_c > WTP_m$. Conversely, for individuals responding to the actual dichotomous choice question with a nay, variance with the projected response (i.e. yea) implies that $WTP_c < WTP_m$. Consequently, in order to detect underlying differences in WTP with our indicator variable, we estimate two maximum likelihood models of procedural variance using subsets of the data based on actual responses.

Payment Card Analysis. The first step in the development of our empirical analysis is to specify a model of respondent behavior that explains observations induced by a WTP "matching" problem. Following the strong recommendation of Cameron and Huppert, we use the completely censored regression model to analyze our payment card data. The basic assumption of this model is that the respondent's true valuation lies within an interval defined by upper and lower limits specified by adjacent payment card values. In essence, the model maximizes the likelihood that observations fall within the appropriate intervals conditional on a set of explanatory variables.

Along with others, we note that the distribution of WTP values is skewed to the right in a fashion that may be approximated by a lognormal distribution. Consequently, we assume that ln(WTP) is normally distributed and estimate our completely censored regression model after first taking the natural log of the left hand side variable Y. As noted by Cameron and Huppert, if ln(Y) has a normal distribution with mean $(x_i'\beta)$ conditioned on the explanatory variables x_i, then the predicted median of Y is $\exp(x_i'\beta)$. The predicted mean of Y is $\exp(x_i\beta)\exp(\sigma^2/2)$ where σ^2 is the estimated error variance.

Because both the median and mean are legitimate measures of central tendency for WTP values, we use both measures to create projected WTP and projected dichotomous response to actual dichotomous choice offer amounts.

A Conditional Probability Model. In the standard referendum model, a yea-response is elicited if the individuals WTP exceeds the dichotomous choice offer amount. The probability of a yea-response is modeled as:

$$P_1 = Pr(WTP_c > offer) = 1 - F(x\beta_1) \tag{5}$$

where F (in our case) is the logistic distribution function, x is a vector of explanatory variables, and β is a vector of parameters. If responses to referendum questions are conditioned by a propensity for procedural variance, then the parameter estimates in equation (5) are likely to be biased. We estimate the propensity for procedural variance by the following equations:

$$\begin{aligned} P_2 &= Pr(actual\ choice \neq projected\ choice \mid WTP_c > offer) = F(x\beta_2) \\ P_3 &= Pr(actual\ choice \neq projected\ choice \mid WTP_c < offer) = F(x\beta_3). \end{aligned} \tag{6}$$

Because P_2 and P_3 are symmetrical, we expect that the signs on β_2 are opposite the signs on β_3.

If we can explain P_2 and P_3 by the vector x, then we suggest creating a vector of adjusted dichotomous choice responses as follows:

$$\begin{aligned}
&\textit{if } P_2 > 0.5 \textit{ then } r = 0 \\
&\textit{if } P_2 \leq 0.5 \textit{ then } r = \textit{actual response} \\
&\textit{if } P_3 > 0.5 \textit{ then } r = 1 \\
&\textit{if } P_3 \leq 0.5 \textit{ then } r = \textit{actual response.}
\end{aligned} \tag{7}$$

In other words, if the probability of a variant response exceeds 0.5, a synthetic response that is consistent with the payment card model is created. Otherwise, the actual response is used. By comparing the model estimated by equation (5) with the same model estimated using the vector of adjusted responses r, we can examine implied biases in parameter estimates for the unadjusted model.

Expected WTP. Once the parameters of the sequential probability model have been estimated, we compute expected willingness to pay using the methods proposed by Hanemann (1984, 1989). It is well known that expected willingness to pay in a dichotomous choice experiment can vary depending on how one treats negative WTP values (Johansson, Kristom, and Maler). Because negative WTP does not make sense in the context of our experiment, we compute expected willingness to pay for the referendum data using the following formula:

$$E(WTP_c) = \int_0^\infty [1 - F(x\beta_1)]dx. \tag{8}$$

Experimental Setting

Our experiment focuses on protection of the boreal montane forest ecosystem in the southern Appalachian Mountains. This ecosystem occurs as a series of island-like patches on the mountain tops and high ridges in Virginia, North Carolina, and Tennessee and is highly valued both for recreational opportunities and the diverse array of plant and animal life that occurs there. Since the 1950's there has been a dramatic increase in spruce-fir mortality occurring in this region. For example, one-quarter of this ecosystem is classified as having severe mortality (greater than seventy percent of the standing trees dead) (Dull et al.). Decline of the spruce-fir forest is highly visible from roads and trails. The cause of this rapid forest decline is generally attributed to the balsam wooly adelgid, a non-indigenous forest pest accidently introduced from Europe. Also, there is evidence that air pollution is a factor in the decline of these forests.

The format of the survey and its implementation closely followed the Dillman method. To test for procedural variance, the sampled households were randomly assigned to two groups. Half of the sample received

CV questions with a payment card answer format and half received questions with a discrete choice answer format. Otherwise, the questionnaires were identical.

The valuation questions were framed in terms of individual willingness to pay to prevent anticipated future decline in the boreal montane forest area currently free of decline symptoms. Consequently, our Hicksian measure is of equivalent surplus. Stages of forest decline were conveyed through a series of color photographs (and a map showing the location of the study area) included with the questionnaire.

Results

The overall response rate was 52 percent of delivered surveys with a two percent difference across the two versions of the survey. This resulted in observations on 486 households. There were 23 protest bids for the discrete choice treatment and 17 protest bids for the payment card experiment. For the results reported below, protest bids were included in the analysis. Descriptive statistics for the variables used in our analysis are presented in Table I.

The first step in our analysis is to estimate a completely censored regression model explaining WTP as measured by payment card responses. As can be seen in Table II, income has a positive influence on WTP, suggesting that forest protection is a normal good. The number of days spent in recreating outdoors has a positive influence on WTP and age was found to have a negative effect on WTP. The parameter estimate for BUGS is positive and indicates that individuals who reported having knowledge of severe insect damage to the boreal montane forest in the southern Appalachians prior to receiving the survey have a higher WTP than individuals who did not have such information. Given that the media typically attributed forest decline to air pollution, and given that most scientists would agree that the decline was primarily due to insect damage, such individuals could be considered to be well-informed regarding the good being valued.

Using the results of the completely censored regression model, we can predict the dichotomous choice response for the sample receiving the actual dichotomous choice question. Table III reports the number of total responses that were positive and negative for the actual dichotomous choice question and the projected dichotomous choice responses using the median and mean projected values. As can be seen, about 41 percent of the respondents indicated that they were actually willing to pay the offered bid amount. Further, it can be seen that the proportion of yea-responses remains relatively constant between the $15 and $100 offer amounts. In contrast, the proportion of yea-responses using median WTP projections falls off very rapidly above $10 offer amounts. Although the total proportion of yea-responses predicted using mean WTP projections is about the same as the actual proportion of yea-responses, it can be seen that the proportion of yea-responses using mean projections falls off rapidly above $20 offer amounts.

Next, we test the hypothesis that the propensity for a procedurally variant response can be predicted by individual characteristics. Table IV presents the results of our maximum likelihood estimation. Models 1 and 3 are estimated using the subset of observations where the actual dichotomous choice is yea. Model 1 uses mean predicted values and Model 3 uses median predicted values. Models 2 and 4 are estimated using the subset of observations where the actual dichotomous choice is nay. Model 2 uses mean predicted values and Model 4 uses median predicted values.

The first thing to notice in Table IV is that the signs on the significant variables in Models 1 and 3 are opposite their sign in Models 2 and 4, respectively. As expected, this is because the subsetted data is symmetrical regarding implied underlying values attributable to a procedurally variant response. To facilitate discussion of these results, we focus on Model 1. The other Models can then be appropriately interpreted.

In Model 1 we see that the propensity for a variant response decreases with income. A variant response in this case implies that $WTP_c > WTP_m$. Therefore, the probability of revealing a higher valuation to a referendum CV question than to a payment card CV question decreases with income. This implies that people with lower incomes may be viewing their expenditure function differently when faced with a referendum CV question than when faced with a payment card question. In reference to equation (4), this result suggests that the weight placed on forest protection (α_c) relative to the weight placed on income (β_c) is inversely related to income (holding α_m/α_c constant).

We can also see that the probability of revealing a higher valuation to a referendum question than to a payment card question increases with the actual offer amount (but at a decreasing rate). This also suggests that individuals view their expenditure function differently when receiving a high offer amount, or that α_c/β_c increases with the offer amount. Of course, we cannot tell whether individuals place a greater weight on forest protection (e.g., it is the "right thing to do") or less weight on the numeraire, only that the ratio changes.

We can also see that the likelihood for procedural variance increases with age. This result may be interpreted to mean that individuals with fixed incomes are more likely to increase the weight given to forest protection or decrease the weight given to other expenditures in expressing their preference.

Individuals who reported having prior knowledge of insect damage to the forests in the study area were less likely to express a variant response than individuals not expressing such knowledge. This result can be interpreted to mean that prior knowledge regarding the good being valued decreases the likelihood of a procedurally variant response. This result is consistent with the studies demonstrating that procedural variance is less likely to occur for private than for public goods (Boyle et al., Kealy and Turner).

Finally, we note that goodness-of-fit of these models is quite high as measured by t-statistics, percent correct predictions, and McFadden's R^2. We also mention that the specification was modified in Model 4 because only 3 procedurally variant responses were identified. Full specification of this model resulted in 100 percent correct predictions.

Table V presents our maximum likelihood models of willingness to pay for forest protection using actual and adjusted (via equation (7)) dichotomous choice data. For comparative purposes, WTP in these models is specified as it was in the completely censored regression (Table 2). The first thing to notice is that the overall fit as measured by t-statistics, percent correct predictions, and McFadden's R^2 is better for the adjusted choice than the actual choice model. This is not surprising given that the completely censored regression model was used to adjust the response data. However, we note that the parameter estimates for the two models differ greatly, suggesting that the set of variables considered to explain WTP as measured by the payment card may not be considered to explain WTP as measured by a referendum model. This is important because policy-makers may like to be able to identify stake-holders that would be impacted by policy decisions regarding the public good in question.

Finally, notice that the estimate of expected willingness to pay for forest protection using the actual dichotomous choice model is roughly five times as high as was estimated using the payment card data. However, expected willingness to pay using the adjusted choice data is quite similar to the payment card measure. This is further evidence that bias introduced by differences in value elicitation procedures can be severe, and that models to explain such differences are required.

Implications

The results of this study strongly reject the hypothesis that commonly used CVM value elicitation procedures do not result in different estimates of expected willingness to pay. This result is in conformance with recent CVM studies and psychological research (Boyle et al.; Kahnemann and Knetsch; McFadden and Leonard; Tversky, Sattath, and Slovic).

Our results are distinct from previous research. Whereas earlier studies demonstrate procedural variance across goods (Boyle et al.; Kealy and Turner; Tversky, Sattath, and Slovic) we demonstrate that the propensity for choice-matching discrepency can vary systematically across individuals. Consequently, our methods and data provide support to the conjecture that choice questions tend to induce lexicographic preferences relative to matching questions. Our results suggest that the disparity in WTP measured using dichotomous choice and payment card methods may result from truthful reporting of preferences regarding "the right thing to do" rather than willful misrepresentation of true preferences as suggested by the strategic bias hypothesis (Kealy and Turner).

Given our results and the current concern regarding the reliability and accuracy of values estimated using the contingent valuation method, we strongly recommend that statistical experiments of the type reported here be embedded in CV experimental design. This approach can increase the informational efficiency of WTP studies, especially if field experiments are based on insights provided by laboratory-based findings (Fienberg and Tanur).

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Table 1. Descriptive Statistics

Variable acronym	Description	Mean (Standard Dev.)
AMOUNT	Value circled on payment card	$20.86 (2.48)
WTP	Dichotomous choice response, "yes" = 1	0.412
VARIANT	= 1 if dichotomous choice response is different than payment card response	0.350
OFFER	Offered amount for dichotomous choice	$111.84 (148.56)
INCOME	Household income	$43,523. (29,784)
DAYS	No. days recreate more than 10 mi. from home per year	22.959 days (36.884)
BUGS	= 1 previous knowledge of insect damage	0.232
EDUC	Highest level of education attained	13.931 (3.00)
AGE	Respondents age	46.99 years (15.288)

Table 2. Completely Censored Regression Model of Willingness to Pay for Forest Protection

Variable	Coefficient	t-stat.	prob.
Constant	-3.563	-1.702	0.089
ln(INCOME)	0.605	3.235	0.001
DAYS	0.010	2.358	0.018
AGE	-0.035	-3.601	0.000
BUGS	0.792	2.371	0.018
σ	1.840	15.135	0.000
N	184		
Average median WTP	4.21		
Average mean WTP	22.86		

Table 3. Positive and Negative Responses for Actual and Projected Dichotomous Choice at Different Offer Amounts

Offer amount	Actual choice (Yes, No, %yes)	Projected choice, median (Yes, No, %yes)	Projected choice, mean (Yes, No, %yes)
$ 5	19, 7, 0.73	8, 13, 0.38	20, 1, 0.95
10	11, 5, 0.69	6, 8, 0.43	13, 1, 0.93
15	11, 14, 0.52	1, 19, 0.05	17, 3, 0.85
20	11, 10, 0.52	1, 17, 0.06	12, 6, 0.67
30	13, 14, 0.48	0, 19, 0.00	5, 14, 0.26
50	12, 13, 0.48	1, 22, 0.04	6, 17, 0.26
100	10, 16, 0.38	0, 21, 0.00	2, 19, 0.10
150	5, 22, 0.19	0, 23, 0.00	1, 22, 0.04
250	3, 15, 0.17	0, 13, 0.00	0, 13, 0.00
500	1, 20, 0.05	0, 20, 0.00	0, 20, 0.00
TOTAL	96, 132, 0.41	17, 175, 0.09	76, 116, 0.40

Table 4. Maximum Likelihood Model of the Propensity for Procedural Variance Across Individuals

Variable	Model 1	Model 2	Model 3	Model 4
Constant	-5.92 (0.610)	6.54 (0.49)	2.91 (0.37)	1.60 (1.00)
ln(INCOME)	-2.46*** (2.46)	3.26** (1.94)	-2.51*** (2.50)	
EDUC	-0.26 (1.42)	-0.20 (0.91)	-0.25 (1.31)	
OFFER	0.17*** (3.88)	-0.15*** (2.86)	0.17*** (2.52)	-0.35* (1.86)
$OFFER^2$	-0.0003*** (3.67)	0.0003** (2.32)	-0.0003** (2.31)	
ln(AGE)	8.29*** (3.49)	-8.86*** (2.96)	7.71*** (2.69)	
BUGS	-4.93*** (3.11)	3.81** (2.14)	-3.86*** (2.92)	
N	86	106	86	106
% correct predict	0.90	0.98	0.91	0.97
McFadden's R^2	0.69	0.81	0.60	0.51

Note: t-statistics in parentheses. *** denotes significance at the 0.01 level, ** denotes significance at the 0.05 level, and * denotes significance at the 0.10 level.

Table 5. Maximum Likelihood Models of Willingness to Pay for Forest Protection Using Actual and Adjusted Dichotomous Choice Data

Variable	Actual Choice	Adjusted Choice
Constant	-4.04	-27.59**
	(1.64)	(2.13)
OFFER	-0.009***	-0.36***
	(4.15)	(3.20)
ln(INCOME)	0.48**	5.41***
	(2.12)	(2.84)
DAYS	0.004	-0.008
	(0.84)	(0.94)
AGE	-0.01	-0.43***
	(1.23)	(2.92)
BUGS	0.51	10.81***
	(1.41)	
N	193	192
% correct predictions	0.69	0.97
McFadden's R^2	0.18	0.90
WTP	$110.27	$32.46

Note: t-statistics in parentheses. *** denotes significance at the 0.01 level, ** denotes significance at the 0.05 level, and * denotes significance at the 0.10 level.

Bias in Discrete Response Contingent Valuation

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Abstract

The empirical literature on discrete response contingent valuation has found that seemingly innocuous changes in the statistical models estimated result in significantly different point estimates of willingness to pay (Boyle et al 1993; Cooper and Loomis 1992; McFadden and Leonard 1992). This paper hypothesizes and examines several potential explanations for these results. First it investigates and compares the biases inherent in SB and DB maximum likelihood estimation procedures and how they react to various bid designs and sample sizes. Then it examines the presence and identification of "outliers" in binary choice data and how these outliers influence estimation. Finally, it presents an alternative approach to addressing the issue of outliers which explicitly acknowledges the possibility of upwardly biased response probabilities.

Bias in Discrete Choice Contingent Valuation

Several recent papers focus on showing that point estimates of willingness to pay (WTP) are sensitive to seemingly innocuous changes in the statistical model estimated when using discrete response contingent valuation (CV). Cooper and Loomis (1992) show empirically that point estimates of WTP depend systematically on the length of the bid vector used. They conclude that it is essential to include bid values in the tails of the WTP distribution in order to obtain an unbiased point estimate of WTP. Similarly, McFadden and Leonard (1992) find that their point estimate of WTP is sensitive to the inclusion or exclusion of a bid value in the upper tail of the WTP distribution. Boyle et al (1993) find that their point estimates of WTP, estimated using "synthetic" discrete response CV data, created by comparing open-ended responses to a set of bid values, are significantly different from the point estimates of WTP estimated using the actual open-ended data.

These results are disturbing. Maximum likelihood estimation of discrete response models results in point estimates that are consistent. This means that if the sample size is large, the model is correctly specified and the data is reliable, statistical perturbations such as those described above should not greatly affect the point estimates. Several of these conditions are violated in the studies being discussed; for example, Boyle et al (1993) use a sample size of 139; they also find that their open-ended responses indicate a mixed WTP distribution, with a spike at zero, but they do not specify their model this way in the synthetic data experiment. Furthermore, McFadden and Leonard (1992) and Boyle et al (1993) suggest that their data may not be completely reliable; they each allude to the possibility of "yea-saying," or the possibility that some respondents claim they would pay any bid amount offered to them, no matter how high.

This paper concentrates on the statistical issues associated with discrete response CV, and attempts to identify explanations for the results cited above. First it investigates and compares the bias inherent in SB and DB maximum likelihood estimation procedures and how it reacts to various bid designs and sample sizes. It is found that a poor bid design does bias WTP estimation. This suggests that the sensitivity of WTP estimates is indeed partially caused by the CV researcher's choice of bid design; but the magnitude of the bias identified here is not nearly as large as that claimed in the earlier papers, nor is it systematically dependent on bid design in the same way that these papers find. This suggests that the sensitivity of WTP estimates must be caused by some other conditions such as those mentioned in the previous paragraph. This paper explores the identification of "outliers" in binary choice data and how these outliers influence estimation. It then presents an alternative approach to addressing the issue of outliers which explicitly acknowledges the possibility of upwardly biased response probabilities, or yea-saying.

Bias in the Logit Model

In this section, the influence of bid design on bias is examined. In order to do this, it must be noted that although maximum likelihood estimation results in consistent parameter estimates, it does not necessarily result in unbiased parameter estimates. Indeed, numerical simulations have shown that estimating logit or probit models using small samples can result in substantially biased parameter estimates (Anderson and Richardson 1979; Griffiths, Hill and Poper 1987).

Copas (1988) has shown that the bias can be analytically derived for the logit model. The expression for the bias can be applied to the SB and DB models. In this section, it is shown that DB parameter estimates are less biased than SB. Furthermore, it is shown that a poor bid design causes biased as well as less-efficient estimates. The DB procedure is less sensitive to a poor bid design than the SB procedure, and is therefore less biased than the SB. In addition, the sample size plays a different role in decreasing SB and DB bias and asymptotic variance. The results in this section however, are not sufficient to explain the large differences in point estimates found in the literature cited above.

The bias in maximum likelihood estimation can be calculated explicitly for the logit model. Following Copas (1988), the Taylor expansion of the score vector of a likelihood function is:

$$0 = S_j(\hat{\theta}) \doteq S_j(\theta) + (\hat{\theta} - \theta)'H_j + \frac{1}{2}(\hat{\theta} - \theta)'L_j(\hat{\theta} - \theta) \qquad \textbf{(1)}$$

where H_j is the jth column of H, the Hessian matrix and L_j is the Hessian matrix of the score function, S_j. Taking the expectation of equation (1), and using the fact that $E[S_j(\theta)] = 0$ gives:

$$0 = E(\hat{\theta} - \theta)'H_j + \frac{1}{2}E(\hat{\theta} - \theta)'L_j(\hat{\theta} - \theta)\,. \qquad \textbf{(2)}$$

The quadratic term in equation (2) can be rearranged to be:

$$\frac{1}{2}E[(\hat{\theta} - \theta)'L_j(\hat{\theta} - \theta)] = \frac{1}{2}E\{tr[(\hat{\theta} - \theta)(\hat{\theta} - \theta)'L_j]\}. \quad (3)$$

In general, H depends on the response vector, y which means that the expectation of equation (3) involves the covariance of H and $\hat{\theta}$. For the logit function and only for the logit function, the expectation of H_j is constant which simplifies the calculation tremendously. Letting the bias b equal $E(\hat{\theta} - \theta)$ therefore gives:

$$b'H_j = E(\hat{\theta} - \theta)'H_j \simeq \frac{1}{2}tr(H^{-1}L_j) = h_j. \quad (4)$$

Letting h be the vector of h_j's gives

$$b \simeq H^{-1}h \quad (5)$$

and letting H^{jk} be the inverse of $H = \{H_{jk}\}$, the sth element of b is:

$$b_s \simeq \frac{1}{2}\sum_j \sum_k \sum_l H^{sj}H^{kl}L_{jkl} \quad (6)$$

which can be calculated using the maximum likelihood estimate, $\hat{\theta}$ and the bid values.

The bias typically has positive sign so that $\hat{\theta}$ is overestimated. In the case of a simple logit regression: $F(x) = 1/(1+\exp(\alpha - \beta x))$, both $\hat{\alpha}$ and $\hat{\beta}$ will be biased upward. The biases in the estimated values of mean and median WTP are unclear as both estimators involve the ratio of $\hat{\alpha}$ and $\hat{\beta}$.

The Effect of Bid Design on Bias

The expression for the bias in equation (6) is useful because it can be calculated using different bid points, but it does not involve the response vector, y. The bias can therefore be calculated analytically as a function of bid points but without using actual data. Previous studies of the bias in WTP estimation have involved empirical examples using actual data (Cooper and Loomis 1992) and Monte Carlo simulations (Kanninen and Kriström 1993; Cooper and Loomis 1993).

Cooper and Loomis (1992) compared WTP estimates for ten data sets using different bid ranges. They found that their WTP estimates were extremely sensitive to the bid ranges used for all ten data sets. In particular, their WTP point estimates decreased as they dropped observations with bid values in the upper tails of the WTP distributions and increased as they dropped observations with bid values in the lower tails. The problem with using real data however, is that the experiment is not pure; there are so many unknown factors such as the true parameter values and the correct WTP distribution; and so many possibilities for measurement error and bias. Any explanation given for their results must therefore be speculative.

Kanninen and Kriström (1993) and Cooper and Loomis (1993) test the impact of bid ranges on simulated data sets. This approach avoids the problems of unknown parameter values and distributions as well as the problems of measurement error and bias. It is also asymptotically quite accurate. Still, the results are dependent on the noninfinite number of samples drawn. The analytical approach to calculating bias presented here avoids the problems associated with drawing actual or simulated sample responses because it does not require them in the calculation. The effects of different bid designs on bias can therefore be isolated.

In Table 1, the results of several calculations using different bid ranges are presented. The bias of $\hat{\alpha}$ and $\hat{\beta}$ is calculated using equation (6) for both the SB and the DB model. The bias of estimated mean or median WTP[1] is calculated as $(\hat{\alpha}/\hat{\beta}) - (\alpha/\beta)$, where $(\hat{\alpha}, \hat{\beta})$ are equal to $(\alpha + \text{bias}(\hat{\alpha}), \beta + \text{bias}(\hat{\beta}))$; and the asymptotic variance of estimated mean WTP is calculated using the delta method for a function of two normal random variables.

The base set of bids used are the set used in Cooper and Loomis (1992) which has twenty bids ranging from \$3 to \$700.[2] The parameters (α, β) are assumed to be equal to (1.6, .01) which is similar to the parameter estimates Cooper and Loomis obtained in their analysis. In the second calculation, the bid range is expanded so that the highest bid is \$2000 (still with 20 different bids). This bid value is far into the right tail of the WTP distribution and holds less than a $1.0\text{x}10^{-8}$ probability of obtaining a positive response. The third calculation drops the upper bids so that the highest bid is \$250 which holds a .29 probability of obtaining a positive response; and the fourth calculation uses a bid design with bids ranging from \$250 to \$500. Since

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[1] In this paper, mean and median WTP are equal due to the symmetry of the logit distribution. Bias can also be calculated for the truncated mean of WTP where WTP is assumed to be greater than zero for all individuals. The results in this case are qualitatively similar to those presented here for the untruncated mean WTP.

[2]Citing Cooper (1993), Cooper and Loomis (1992) state that this type of bid design is nearly optimal. This result contrasts sharply with the results of Kanninen (1993) which shows that C-optimality places all bid points at the median value for the SB logit model, and initial bids at the median value with follow-up bids at the conditional median values for the DB logit model. D-optimality places the bid points half at the .18 and half at the .82 percentiles for the SB logit model and at the median with follow-up bids at the .12 and .88 percentiles for the DB logit model. Optimal design results for the probit model are presented in Alberini and Carson (1993).

mean WTP is equal to $160, this bid range covers only part of the upper half of the WTP distribution and is considered to be a poor bid design.

As Table 1 shows, the bias in mean WTP is fairly small for the SB model (-2.98% for the base case) and smaller yet for the DB model (-.76% for the base case). Expanding the bid range to reach 2000 makes the bias worse. For the SB case, the bias is -8.43%, while for the DB case, the bias is -1.57%. Bid values so far into the tails can create one of two potential problems: either the bid receives a "no" response, and the observation is wasted, essentially reducing the sample size, or worse, the bid receives a "yes" response and distorts the estimation. Either event worsens the bias. Note that the DB model is less sensitive to this problem because it does gain information from the lower follow-up bids. Decreasing the bid range decreases the bias: -.59% for the SB model and -.67 for the DB model. This of course is because the bids are closer to the middle of the distribution where they are more informative than the bids in the tails of the distribution.

The above result is similar to that of Kriström (1990, chapter 6). He finds that "An almost unbiased estimate is obtained by fixing the maximum percentile to 79% ..." The optimal experimental design results cited in footnote 1, together with Kriström's result suggest a general rule of thumb that bid points should not be placed outside the .18 and .82 percentiles for the SB model and .12 and .88 percentiles for the DB model.

An important attribute of the DB model is its ability to adapt to an otherwise poor bid design. The fourth calculation uses bids ranging from $250 to $500. The SB model overestimates α by 20.64%, β by 12.95%, and WTP by 10.89%. The DB model on the other hand, does surprisingly well, overestimating α and β by less than 3%, and overestimating WTP by only .64%. This is because the follow-up lower bids, assigned to be equal to half the initial bids are generally in the appropriate range. The initial bids are too high on average, but this just means that the majority of respondents will respond "no" to the initial bid, and they will be offered a lower follow-up bid that is quite informative. The DB model offers this second chance which makes it much more robust to poor bid designs than the SB model.

The final calculation places all bid values at the C-optimal bid points: the median value, $160, for the SB case and $160 $\pm$ 110 for the DB case. The bias in each case is zero, and the asymptotic variances are the Cramer-Rao lower bounds, $4/n\beta^2$ for the SB case and $3.2/n\beta^2$ for the DB case.

From a statistical perspective, the magnitude of the variance of estimated WTP is more important than the magnitude of the bias of estimated WTP. The point estimate represents the middle of the confidence interval of estimated WTP, but it is the variance that determines the length of the confidence interval. As the length of the confidence interval indicates the degree of uncertainty about the estimated welfare benefit measure, CV researchers should be more concerned about minimizing this length than about obtaining a more precise point estimate. It is the variance of estimated WTP that should be of most interest.

The behavior of the asymptotic variance is similar to the behavior of the bias in each example. The DB model is consistently more efficient than the SB model. The sample size and value of β play crucial roles in the value of asymptotic variance. As shown in Kanninen (1993), the asymptotic variance is inversely proportional to $n\beta^2$. So as expected, as sample size increases, the asymptotic variance decreases proportionately. Using this information, it is possible to compare the gain from increasing sample size for the SB model and the gain from moving from the SB to the DB model. If for example, we doubled the SB sample size using the base case bid values, then the SB asymptotic variance of WTP would be approximately 365. This same asymptotic variance could be achieved by increasing the DB sample size by only 30%. Also, as β increases, the asymptotic variance decreases. This is because β is inversely proportional to the standard deviation of the WTP distribution. If β is small, a large asymptotic variance of WTP is unavoidable.

The results of the calculations presented in this section conflict with some of the empirical results cited above. The bias inherent in maximum likelihood estimation causes mean WTP to be underestimated to a greater extent when higher bid values are added to the bid range. Cooper and Loomis (1992) and McFadden and Leonard (1992) claim to obtain the opposite result empirically. On the other hand, Boyle et al (1993) do find that their synthetic discrete response model underestimates WTP.

Influential Points in Binary Regression

The biases resulting from maximum likelihood estimation are smaller than those found in the empirical literature, and in some cases the directions of the biases do not agree. The large empirical differences in point estimates must therefore be explained some other way. In this section, the possibility of the existence of outliers is discussed.

Day and Kerridge (1967) have shown that unlike ordinary regression, with binary regression the bid points on the edge of the design space have relatively little influence on the fit of the model. That is, observations taken at bid values located in the tails of the distribution, provided they are not outliers, do not have much influence on the estimated parameter values. It is actually the "doubtful" cases, those with probability values near .5 that exert the most influence in binary regression. This is precisely why the SB minimum variance optimal design points are all equal to the median value.

The clause in the previous statement: "provided they are not outliers" is extremely important. If observations in the tails are influential, then they must be outliers. As Copas (1988) points out, outliers in binary regression can take only one of two forms: either there is a high probability of obtaining a 0 response at the design point, and a 1 response occurs, or there is a high probability of obtaining a 1 response and a 0 response occurs. If either of these phenomena occur, the observation is an outlier and it exerts high leverage on the fit.

So a simple test for outliers in the case of binary regression is to test for the degree of influence observations in the tails have on parameter estimation. This can be done by estimating the model both with and without the observations in the tails. If observations in the tails exert high influence on the parameter estimates, then these observations are not standard observations, but are actually outliers.

McFadden and Leonard (1992) observe this phenomenon in their data. They obtained a positive response rate of 15.4% at a bid value of $2000 for preserving the Selway Bitterroot Wilderness. The bid value $2000 is probably several standard deviations away from the sample mean.[3] When they dropped these observations from their sample, so that sample size was reduced from 365 to 322 observations, their mean WTP estimate was reduced by 46.1% from $489 to $263. Cooper and Loomis (1992) obtain qualitatively similar results when they drop observations at the higher bid points from their samples. These empirical findings conflict with the fact that observations in the tails of the distribution exert relatively little influence on parameter estimates; logically, this leads to the conclusion that these observations are outliers.[4]

In fact, the high positive response rates these studies obtain in the tails indicate that there is more than an occasional outlier problem with the data. Recent empirical evidence supports the notion that initial positive response rates are higher than they actually should be (Duffield and Patterson 1992). There appears therefore to be a need for an estimation procedure that accounts for the unexpectedly high positive response rates in the tails.

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[3]McFadden and Leonard do not provide enough information in their paper to verify this, but they imply it. With a normal distribution, the probability of obtaining an observation more than 3 standard deviations from the mean is minute.

[4]Researchers have dealt with the possible presence of outliers in several ways. The simplest way is to drop any observations that seem unreasonable. An alternative is to use a resistant fitting technique where outliers are downweighted relative to the other observations in the sample. As Copas (1988) shows analytically and Pregibon (1982) shows empirically, resistant fitting techniques can substantially bias maximum likelihood parameter estimates.

Modelling Response Bias

The results cited in the previous section suggest that there is systematic bias in responses to binary choice contingent valuation. Positive response bias can be explicitly modelled using a model similar to that proposed by Copas (1988) to address the problem of contaminated binary data. If it is assumed that the probability of wrongly responding "yes" to a bid value that exceeds WTP is γ ($\gamma > 0$), then the probability of obtaining a yes response is:

$$P^{*}(yes) = P(yes) + \gamma P(no) \tag{7}$$

and the probability of obtaining a no response is:

$$P^{*}(no) = (1 - \gamma)P(no) . \tag{8}$$

This specification is equivalent to assuming that there is a fixed probability γ that a respondent will respond "yes" to any bid amount offered. In other words, there is a fixed percentage of the population that can be classified as "yea-sayers." It might be more elegant to specify yea-saying behavior as a function of explanatory variables such as demographic or attitudinal variables; most CV surveys collect such information. The model presented here is only a simple example of the type of model that can be developed to explicitly account for systematic bias or yea-saying behavior. Equations 7 and 8 can be entered into a log-likelihood function just as the standard probabilities functions are.

The DB case can be modelled in several ways. Assuming upward bias in all responses results in the following probabilities:

$$P^{*}(no, no) = P(no, no)(1 - \gamma) \tag{9}$$

$$P^{*}(no, yes) = P(no, yes)(1 - \gamma) \tag{10}$$

$$P^{*}(yes, no) = P(yes, no)(1 - \gamma) \tag{11}$$

$$P^{*}(yes, yes) = P(yes, yes) + \gamma[1 - P(yes, yes)] . \tag{12}$$

Essentially, this specification assumes that there is a probability γ that an individual will respond "yes-yes" to any set of bid offers.

Estimation results for a wetlands improvement program in the San Joaquin Valley, California are presented in Table 2.[5] Both the SB and DB models are estimated, and the DB model is estimated with and without γ. In the SB case, the inclusion of γ without the inclusion of additional explanatory variables makes the model inestimable because the SB model is unable to distinguish between a biased upward response and a true positive response; there is insufficient information to distinguish the two phenomena. The DB model however, does have this capability as there is more information per observation than in the SB case.

The DB model without γ estimates a lower WTP than the SB model, presumably because the upper follow-up bids received more negative responses than did the initial bids. The results of DB estimation with γ perform slightly, but not significantly, better than the DB model without γ in terms of the likelihood function. The estimated value for γ is .20 and is statistically significant. This indicates that yea-saying behavior, or some observationally equivalent behavior is present in the data. Mean WTP estimated with γ is significantly lower than mean WTP estimated without γ.

The estimated model accords with the evidence presented in this paper. The bias parameter accounts for respondents overstating their willingness to pay which explains the upward bias in the tails of the distribution. Without accounting for this bias, the observations in the tails exert too much influence on the model estimation. As expected, this model estimates a lower WTP than the model that ignores the potential for yea-saying; the difference between estimates is 25%. If the yea-saying hypothesis is true, then conventional models overestimate WTP.

Conclusion

This paper shows that statistical bias in WTP estimation does not accord with empirical examples that have shown wide but systematic variation in WTP estimates when different models or design points have been used. It shows that observations in the tails of the distribution increase the bias in maximum likelihood estimation and furthermore, that observations in the tails of the distribution are more likely to accentuate any yea-saying behavior existing in the sample population. If this behavior is not explicitly accounted for in the estimation procedure, WTP estimation can be significantly distorted. A model is presented which explicitly accounts for this behavior and is estimated using data from a contingent valuation study for a wetlands

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[5]The data are from a contingent valuation study conducted for the San Joaquin Valley Drainage Program. The study focused on WTP for protecting wildlife and wetlands habitat in the San Joaquin Valley, California. See Hanemann, Loomis and Kanninen (1991) for more details about the study.

improvement program in the San Joaquin Valley. In this simple example, yea-sayers are estimated to make up 20% of the sample population. Accounting for this behavior substantially reduces the mean WTP estimate.

The results of the experiments conducted in this paper suggest that we take another look at an old debate in the CV literature: the use of mean or median WTP as the primary welfare benefit estimator. Hanemann (1984) argued for using median WTP on the basis of its robustness to alternative functional forms and its applicability to median voter theory. On the other hand, mean WTP is the appropriate welfare measure for consistency with the Pareto-efficiency criterion (Johansson, Kriström and Mäler 1989). The general consensus has favored using mean WTP. This has led to discussions of the importance of tacking down the tails of the WTP distribution, as the width of the tails can greatly influence the estimate of mean WTP. This paper suggests that we cannot accurately tack down the tails with a finite sample size; in fact, placing bid points in the tails of the distribution tends to harm estimation of mean WTP, not improve it. Given the strong dependence of the estimate of mean WTP to the width of the tails, this result suggests that we take another look at using median WTP. Although it may not be as theoretically appealing as mean WTP, median WTP is a value we can estimate without tacking down the tails. By not soliciting WTP responses in the tails of the distribution, we improve WTP estimation.

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Table 1: Analytical Bias and Asymptotic Variance of WTP — Logit Model

$\alpha = 1.6, \quad \beta = .01, \quad n = 100$

Single-Bounded Model				
Bid Ranges	% Bias α	% Bias β	% Bias mean WTP	Var(WTP)
3-700[1]	3.89%	5.86%	-2.98%	729.37
3-2000[2]	6.95%	12.9%	-8.43%	1321.27
3-250[3]	2.44%	2.82%	-.59%	594.38
250-700[4]	20.64%	12.95%	+10.89%	4648.71
160 only	-	-	0	400.00

Double-Bounded Model				
Initial Bid Ranges[5]	% Bias α	% Bias β	% Bias mean WTP	Var(WTP)
3-700	2.19%	2.68%	-.76%	471.77
3-2000	2.64%	3.66%	-1.57%	613.23
3-250	1.91%	2.34%	-.67%	449.60
250-700	2.82%	2.41%	+.64%	541.39
160 w/ follow-up ± 110	0	0	0	320.00

[1] The bid values are: 3,5,10,20,30,40,50,60,70,80,90,100,120,150,200,250,300,400,500,700.

[2] The bid values are: 3,5,10,20,30,40,50,60,70,80,90,100,120,150,200,500,700,1000,1500,2000.

[3] The bid values are the same as in footnote 1 with all bids greater than 250 replaced with the value 250.

[4] The bid values are: 250,300,400,500,700.

[5] Follow-up bids were equal to $(1 \pm .5)$*(initial bid).

Table 2: Estimation Results - Logit Model Wetlands Improvement Program, n=558

	α	β	γ	Mean(WTP)	Var(WTP)	F
SB	1.60 (15.21)[1]	.0079 (10.14)		202.53	199.91	-.64
DB	2.01 (22.99)	.012 (22.99)		167.50	38.13	-1.40
DB	2.36 (14.42)	.0189 (13.85)	.20 (7.39)	124.87	33.54	-1.36

[1] Numbers in parentheses are t-statistics

Multiple Bounded Discrete Choice Models

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Introduction

In recent years economists have turned to the use of discrete choice models as a framework to both collect and analyze contingent valuation data. First employed in a resource valuation context by Bishop and Heberlein in 1979, the discrete choice model has been modified and expanded in several significant ways. First, formal links between microeconomic theory and the discrete choice questioning format were developed (Hanemann 1984). Other authors developed additional perspectives linking discrete choice models and distributions of consumer surplus for non-market goods (Cameron 1988). Once the theory and practice of discrete choice models was well established, attention was turned to understanding the variance of measures of willingness to pay. Several perspectives have been offered, including procedures to calculate analytic estimates of variance (Cameron 1991) and estimates of variance based on resampling or Monte Carlo techniques (Duffield and Patterson 1991, and Poe et al. 1992). Several researchers have explored the implications of the bids on the estimates of mean willingness to pay and the variance of mean willingness to pay (Kanninen 1992, and Cooper and Loomis 1992). One of the most recent developments has been the double bounded logit model (Hanemann et al. 1991).

The double bounded logit approach represents perhaps the most sophisticated way to collect and analyze contingent valuation data. This is not to say that all issues associated with discrete choice models have been settled. First, while nearly all would agree that the double bounded procedure provides increased precision (both for parameter estimates, and for estimates of central tendency) there has been some concern that the double bounded procedure may in fact introduce some bias into the valuation process. In addition, the double bounded procedure requires the use of personal or telephone interviews. While personal interviews may be desirable from a methodological point of view, personal or telephone interviews can be much more expensive to administer than mail surveys.

In this paper we propose a multiple bounded model. The multiple bounded model is the generalization of the double bounded model. However in the multiple bounded model, survey respondents are asked discrete choice willingness to pay questions about a wide variety of bids. In the next section, we develop the statistical framework required to implement a multiple bounded analysis. In the following section we present examples of questions that can be used to collect data required to estimate a multiple bounded model and in the final section we present the results of various simulations that explore the properties of the multiple bounded model relative to double bounded and single bounded models in the context of a logit analysis.

Statistical Framework for the Multiple Bounded Model

While discrete choice models are increasingly the choice of many contingent valuation practitioners, the actual mechanics of estimating discrete choice models are less well understood. Since the interplay between theory and estimation techniques is critical to both understanding and implementing the multiple bounded model we are proposing, in this section we will first review the typical application of the maximum likelihood method to the estimation of single and double bounded logit models and then modify this approach to facilitate the discussion of multiple bounded models.

The process of estimation by maximum likelihood is one in which the parameters to be estimated are chosen in such a way that the estimated probability of the sample actually observed is as large as possible. For discrete choice models, this means selecting the parameters of a cumulative density function to make the predicted probabilities of the actually observed series of yeses and nos as large as possible. The typical assumption is that the cases upon which the discrete choice model is based, represent independent random draws from some unknown distribution. The probability of a series of independent random occurrences is simply the product of the probabilities of each of the observed outcomes. The probability of a given sample is simply the product of the estimated probability of each of the observed outcomes. Let $p_i(\beta,X)$ represent the probability that respondent i says yes to the bid presented to her. In this formulation, p_i depends on the characteristics of the person and the amount of the offer, X, as well as the parameters to be estimated, β. We will also let y_i represent a binary variable taking the value of one if respondent i says yes to the bid, and zero otherwise. The probability of observation i is simply p_i if the respondent accepts the bid, and $(1-p_i)$ if not. Regardless of the response, the probability of observation i can be written as:

(1) $$p_i^{y_i} * (1-p_i)^{(1-y_i)}.$$

When observation i is a "yes", the multiplicand on the right of equation (1) takes the value of one, and when observation i is a "no" the multiplicand on the left takes the value of one. Given that the probability of a series of independent observations is simply the product of the probabilities of the individual outcomes, the probability, or likelihood, of the entire sample can be written as:

(2) $$\text{Likelihood} = \prod_{i=1}^{n} p_i^{y_i} * (1-p_i)^{(1-y_i)}.$$

The problem solved by maximum likelihood estimation is to select β in such a way as to maximize the quantity defined in equation (2). Following standard maximization procedure, maximizing equation (2) with respect to β requires:

(3) $$\frac{\partial \text{Likelihood}}{\partial \beta} = \frac{\partial \left[\prod_{i=1}^{n} p_i^{y_i} * (1-p_i)^{(1-y_i)} \right]}{\partial \beta} = 0.$$

Analytic solutions defining the β satisfying equation (3) typically do not exist. In such cases equation (3) is solved by using any of a variety of numeric analysis techniques. A typical numeric analysis technique is Newton's method. This method is often used when it is desired to find the value of X that satisfies an implicit function of the form:

(4) $$f(X) = 0.$$

If the implicit function has a solution, an approximation to this solution can be found by using the following iterative process:

(5) $$X_{n+1} = X_n - f(X_n) * \frac{\partial f(X_n)}{\partial X}.$$

This iterative process continues until the second term on the right hand side of equation (5) is arbitrarily small. In the case of a discrete choice model, we are looking for the value of β that solves the implicit function defined in equation (3). Since equation (3), is itself a first derivative, equation (5) can be rewritten as:

(6) $$\beta_{n+1} = \beta_n - \frac{\partial Likelihood(\beta_n)}{\partial\beta} * \left[\frac{\partial^2 Likelihood(\beta_n)}{\partial\beta^2}\right]^{-1}.$$

Equation 6 simply says that the β vector maximizing the likelihood function can be found by an iterative process in which each successive approximation of the solution vector is adjusted by the ratio of the first derivative and second derivatives of the likelihood function. If β is a vector, the β vector is adjusted by the product of the gradient of the likelihood function and the hessian of the likelihood function. In any case, if the underlying likelihood function is universally concave (that is to say if the matrix of second order derivatives of the likelihood function with respect to the estimated parameters is negative definite) it will have a single maximum, and this unique maximum can be found using the iterative procedure just outlined.

Because equation (2) presents the likelihood as a product of n terms, where n is the sample size, the derivatives of equation (2) with respect to β can be very cumbersome to evaluate. This difficulty is alleviated somewhat by considering the log of equation (2), the log likelihood. The log likelihood can be written as:

(7) $$Log(Likelihood) = \sum_{i=1}^{n} (y_i * \ln(p_i) + (1-y_i) * \ln(1-p_i)).$$

Writing the derivatives of equation (7) with respect to β is often much simpler than writing the derivatives of equation (2). Since the logarithmic transformation is a monotonic transformation, the β vector maximizing equation (2) also maximizes equation (7). At the heart of most discrete choice estimation programs are derivatives based on equation (7). Indeed, the gradient and hessians for the multiple bounded model presented in this paper are developed for the log likelihood function as opposed to the likelihood function.

While most discussions of discrete choice models focus on the likelihood as specified above, for the development of the multiple bounded model it is important to focus on a slightly different specification of likelihood for a discrete choice model. This alternate specification focusses on the contribution of each observation to the value of the likelihood (or log likelihood) function. To illustrate, let X_i represent the bid presented to a respondent and CS_i represent the consumer surplus held by the ith respondent. A response of yes to X_i implies that $CS_i > X_i$. Likewise, a response of no implies $CS_i < X_i$. What does this individual observation contribute to the value of the likelihood function for a given value of β? From equation (1), if the respondent answers yes, the contribution is p_i and if the individual responds "no" the contribution is $(1-p_i)$. If CS_i is taken to be a random variable with a cdf F( ), then $p_i = 1-F(x_i)$. Next, note that p_i is the probability that $CS_i > X_i$. If the respondent says yes to X_i several facts follow: 1) CS_i is revealed to lie on the interval of the real line from X_i to ∞; 2) p_i is the probability that CS_i lies on the interval from X_i to ∞; and 3) p_i is added to the likelihood. On the other hand, if the ith individual says no to X_i: 1) CS_i is revealed to lie on the interval of the real line from 0 to X_i; 2) $1-p_i$ is the probability that CS_i lies on the interval from 0 to X_i; and 3) $1-p_i$ is added to the likelihood. In other words, the contribution of any single observation to the likelihood is the probability associated with the line interval on which CS_i is revealed to lie.

Keeping in mind the fact that the contribution of any observation to the likelihood function is simply the probability associated with the line interval on which CS_i is revealed to occur, it is easy to reformulate the single bounded model in terms of a double bounded model. In the double bounded model, respondents are asked whether they would pay a specified amount. If they answer yes to the first amount, they are asked about a higher amount. If they respond no to the first amount, they are asked about a lower amount. Again, let CS_i represent the consumer surplus of the ith individual. Now let X_{i1} represent the first amount that will be asked of the ith individual, X_{iL} represent the lower amount that will be asked if the respondent says no to the initial amount, and X_{iU} represent the amount that is asked about if the individual responds yes to the initial amount. These three ordered amounts break the real line into four segments: 0 to X_{iL}, X_{iL} to X_{i1}, X_{i1} to X_{iU}, and X_{iU} to ∞. The pattern of responses to the two bids that are actually asked about is sufficient to make an inference about which of the four line segments contains CS_i. In particular, the following response patterns support these inferences:

(8) $$\text{YES-YES} \Rightarrow X_{iU} < CS_i < \infty.$$

(9) $$\text{YES-NO} \Rightarrow X_{i1} < CS_i < X_{iU}.$$

(10) $$\text{NO-YES} \Rightarrow X_{iL} < CS_i < X_{i1}.$$

(11) $$\text{NO-NO} \Rightarrow 0 < CS_i < X_{iL}.$$

Using the usual perspective, for a given β, the probability of the ith observation in a double bounded model can be written as:

(12) $$r_{NN_i}^{D_{NN_i}} * r_{NY_i}^{D_{NY_i}} * r_{YN_i}^{D_{YN_i}} * r_{YY_i}^{D_{YY_i}}.$$

In equation (12), r_{NNi} represents the probability of observing a no-no response pattern from the ith individual. The probability of a No-No pattern is simply the probability that $CS_i < X_{iL}$ which is evaluated as $1 - p(X_{iL})$. Likewise, r_{NYi} represents the probability of observing a No-Yes response pattern and can be written as $p(X_{iL}) - p(X_{i1})$. D_{NN} is a dummy variable taking the value of 1 if the observed response pattern was No-No and takes the value of 0 otherwise and D_{NY} is a dummy variable taking the value of 1 if response pattern was No-Yes and the value of 0 otherwise. The other variables are defined in a similar manner. The likelihood function for the double bounded model can be written as follows:

(13) $$\text{Likelihood} = \prod_{i=1}^{n} r_{NN_i}^{D_{NN_i}} * r_{NY_i}^{D_{NY_i}} * r_{YN_i}^{D_{YN_i}} * r_{YY_i}^{D_{YY_i}}.$$

The similarities of the double bounded model to the single bounded model are clear. First, while four probability terms appear in the likelihood function for each observation, only one of the probability terms actually contributes to the value of the likelihood. Furthermore, the probability contributed to the value of the likelihood by any particular observation is simply the probability associated with the portion of the real line on which the response pattern implies CS_i must lie. Second, while the gradient and hessian of the likelihood function are a bit more complex to evaluate, once they are specified, routines that would estimate the single bounded model (such as Newton's method described above) could also be used to estimate the double bounded model. Finally, the output from the single bounded and double bounded models are the same, namely a parameter vector, a variance covariance matrix, and the value of the log likelihood function. The primary advantages associated with the double bounded models are increases in the precision of the estimated parameters or increases in the precision of estimates of central tendency. These gains in precision, in essence, are associated with decreases in the size of the region in which CS_i is revealed to occur.

The expansion of the double bounded model to the multiple bounded case should now be relatively straight forward. For example, if respondents are asked to respond to a total of 10 bids, the 10 bids would divide the real line into 11 regions. The response pattern would identify which of the eleven regions contained CS_i. The generalization of equation (13) to the case where the research design is one in which the respondent is asked about k bids, is as follows:

$$(14) \qquad \text{Likelihood} = \prod_{i=1}^{n} r_{1i}^{D_{1i}} * r_{2i}^{D_{2i}} * \ldots * r_{ki}^{D_{ki}} * r_{(k+1)i}^{D(k+1)i}.$$

In equation (14), r_{1i} represents the probability that CS_i falls on the interval from 0 to the lowest bid, in other words the probability that respondent said no to all bids asked about, r_{ki} represents the probability that CS_i falls on the interval between the two highest bids, (in other words the probability that the respondent says yes to all but the highest bid), and $r_{(k+1)i}$ is the probability that the respondent would say yes to all of the bids offered. The log likelihood corresponding to equation (14) can be written:

$$(15) \qquad \ln(\text{Likelihood}) = \sum_{i=1}^{n} [D_{1i} * \ln(r_{1i}) + D_{2i} * \ln(r_{2i}) + \ldots + D_{ki} * \ln(r_{ki}) + D_{k+1,i} * \ln(r_{k+1,i})].$$

While straightforward from a theoretical view, implementation of a computer program to estimate the multiple bounded perspective as illustrated in equations (14) and (15) is quite difficult. The major difficulty is that as the number of bids increases, so does the number of intervals for which probabilities (and eventually derivatives) must be evaluated.

This problem can be solved by recalling that regardless of whether a single, double, or multiple bounded model is being estimated, the probability each observation contributes to the likelihood is the probability associated with the interval on which CS_i is revealed to lie. Recall that p has been defined as the probability the respondent i would agree to pay a randomly selected bid X. As defined above, p_i is the probability the $CS_i > X_i$. Now, if we let Z represent the probability from the cdf for CS, Z_i will represent the probability that $CS_i < X_i$. Obviously p_i and z_i are related as follows: $p_i = 1 - Z_i$. In essence, p represents the probability that CS falls above a specific point while Z represents the probability that CS falls below a specific point. It becomes somewhat easier to complete the exposition of the multiple bounded model using Z rather than p (although either could be used, and in the case of a discrete choice model based on a logistic distribution for willingness to pay, the change from Z to p simply involves a change in the sign of all the estimated parameters.)

Recall that for each observation, only one of the k+1 terms in equation (15) is not equal to zero. In particular, the only non-zero term is associated with the interval in which the respondent changes from a yes response to a no response. We now define X_{iL} as the highest amount the individual said they would be willing to pay, and X_{iU} as the lowest amount the respondent would not be willing to pay. The interval on which CS_i must fall has a lower end of X_{iL} and an upper end of X_{iU}. For any observation, the contribution to the likelihood function is the probability associated with this interval. In terms of Z, this probability can be written as $Z_{iU} - Z_{iL}$. Regardless of how many offers might be asked, the only two that matter are the two comprising the endpoints of the interval on which CS_i is revealed to lie. Keeping this in mind, equations (14) and (15) can be rewritten as:

(16) $$\text{Likelihood} = \prod_{i=1}^{n} \left[Z_{iU} - Z_{iL} \right].$$

(17) $$\ln(\text{Likelihood}) = \sum_{i=1}^{n} \ln\left[Z_{iU} - Z_{iL} \right].$$

It is relatively simple to have a computer program scan the bids offered to each respondent and determine the two bids that define X_{iL} and X_{iU} when the response pattern contains some yes and some no responses. In the case where the respondent rejects all bids, there is no bid associated with Z_{iL} and when the respondent accepts all bids there is no bid associated with Z_{iU}. This small difficulty is addressed by recalling that when the respondent rejects all bids, CS_i is revealed to be less than the lowest bid. Since Z_{iL} is the probability associated with the lower end of the interval on which CS_i is revealed to occur, when the respondent rejects all bids, Z_{iL} can be set to zero. Likewise, since Z_{iU} is the probability associated with the upper end of the range on which CS_i is revealed to lie, when the respondent accepts all bids, Z_{iU} can be set equal to 1. Given these minor considerations, the only problem is to calculate the gradient and hessian of equation (17) to carry out the process of determining the β that maximizes the value of the likelihood function.

To this point everything that has been said applies to qualitative response models in general. The particular form of the gradient and hessian will depend on the form of the density function that is assumed to describe the distribution of CS_i. For a logit model, the underlying density function and the derivative can be written as:

$$Z(X) = \frac{1}{1 + \exp(-\beta * X)}. \tag{18}$$

$$\frac{\partial Z(X,\beta)}{\partial \beta} = X*Z*(1-Z). \tag{19}$$

Given these results, the gradient of the log likelihood function can be written as:

$$\frac{\partial \ln(\text{Likelihood})}{\partial \beta} = \sum_{i=1}^{n} \frac{1}{Z_{iU} - Z_{iL}} * \left[\frac{\partial Z_{iU}}{\partial \beta} - \frac{\partial Z_{iL}}{\partial \beta} \right]. \tag{20}$$

$$\frac{\partial \ln(\text{Likelihood})}{\partial \beta} = \sum_{i=1}^{n} \frac{1}{Z_{iU} - Z_{iL}} * [\, X_{iU}*Z_{iU}*(1-Z_{iU}) - X_{iL}*Z_{iL}*(1-Z_{iL})]. \tag{21}$$

And the hessian can be written:

$$\frac{\partial^2 \ln(\text{Likelihood})}{\partial \beta^2} = \sum_{i=1}^{n} \left(-1 * \left[\frac{1}{Z_{iU} - Z_{iL}} \right]^2 * \left[\frac{\partial Z_{iU}}{\partial \beta} - \frac{\partial Z_{iL}}{\partial \beta} \right]^2 + \left[\frac{1}{Z_{iU} - Z_{iL}} \right] * \left[\frac{\partial^2 Z_{iU}}{\partial \beta^2} - \frac{\partial^2 Z_{iL}}{\partial \beta^2} \right] \right). \tag{22}$$

$$\begin{aligned} \frac{\partial^2 \ln(\text{Likelihood})}{\partial \beta^2} = & \sum_{i=1}^{n} -1 * \left[\frac{1}{Z_{iU} - Z_{iL}} \right]^2 * [\, X_{iU}*Z_{iU}*(1 - Z_{iU}) - X_{iL}*Z_{iL}*(1 - Z_{iL})]^2 \\ & + \sum_{i=1}^{n} \left[\frac{1}{Z_{iU} - Z_{iL}} \right] * \left(X_{iU}^2*Z_{iU}*(1 - Z_{iU}) - 2*X_{iU}^2*Z_{iU}^2*(1 - Z_{iU}) \right) \\ & - \sum_{i=1}^{n} \left[\frac{1}{Z_{iU} - Z_{iL}} \right] * \left(X_{iL}^2*Z_{iL}*(1 - Z_{iL}) - 2*X_{iL}^2*Z_{iL}^2*(1 - Z_{iL}) \right). \end{aligned} \tag{23}$$

Multiple Bounded Questioning Format

The questioning format used to gather data for a multiple bounded model was developed by sociologists to support the development of what has become to be known as a "return potential curve". The typical application of this technique has been to explore the strength of social norms (Jackson 1965). For example, in the context of exploring the norm associated with crowding, survey respondents might be asked how crowded they would feel during a river trip if they encountered various numbers of other boating parties (Heberlein and Vaske, 1977, Shelby 1981). An example of this question illustrating the exploration of norms about optimal flow rates on the part of professional guides on Grand Canyon River trips (figure 1). Adapting the return potential questioning format to that of a contingent valuation discrete choice framework merely involves describing a referendum over whether or not to pursue public provision of a non-market good. The dimension on which the respondents are asked to make multiple evaluations is the dollar amount they would be required to pay if the referendum passes. For each dollar amount, the respondent is asked how he would vote if passage of the referendum meant that he would have to pay the amount being asked about. Figure 2 illustrates the questioning format in a contingent valuation context. In this example, the respondent is asked to indicate how likely it is that they would quote "Yes" on a referendum.[1]

In many ways this questioning format is very similar to a payment card. For example, both the payment card and the multiple bounded procedure require the respondent to consider a wide range of values. However, the payment card seems to be much more similar to open-ended contingent valuation techniques in which the respondent is asked to provide a point estimate of their individual willingness to pay. The multiple bounded technique maintains the discrete choice format in that the respondent is asked to indicate whether or not they would be willing to pay each of the amounts asked about.

There are several potential advantages of the multiple bounded questioning format. One advantage is that while the double bounded procedure requires personal interviews, the multiple bounded approach can be implemented using either personal interviews or mail surveys. The multiple bounded approach offers other, perhaps more practical, advantages over single bounded and double bounded approaches. As anyone who has carried out a single or double bounded model knows, the selection of the bids to be offered is an important

[1]In this example the researcher would use a specific response level to define the discrete choice. For example, all respondents with C, D, or E circled could be coded as no votes. This example also suggests that one could develop a multinomial multiple bounded model.

and often difficult task. The uncertainty of the appropriate bid design is typically resolved using one of two alternate approaches. First, extensive pretests can be used to assist in the design of the bids. Secondly, samples can be split into several groups. For example, with two groups, an initial bid design is used to implement the study using the first group. Data collected from the first group is reviewed and if necessary, the bid design is then modified. While both of these procedures provide the researcher with more confidence in the bid design, they do so at the expense of increasing effort devoted to pre-testing and/or increasing the time required for completing data collection. By allowing the researcher to ask discrete choice questions about a wide variety of bids, the multiple bounded model may substantially alleviate the difficulties of selecting a bid distribution.

Performance of the Multiple Bounded Model Relative to Single and Double Bounded Model

We explored the properties of the multiple bounded model using a simulated data set in which willingness to pay was drawn from a logistic distribution having the form:

$$z = \frac{1}{1+\exp(6-.04*\$)}$$

This distribution was chosen for several reasons. First, nearly all of the mass of this distribution lies on the positive portion of the real line. This avoids difficulties associated with choosing appropriate measures of central tendency that occur when the model "predicts" a significant proportion of individuals having willingness to pay of zero or less than zero. Secondly this distribution is such that the probability of observing a yes to bid greater than $300 is very small. This means that problems with "thick tails" should not be severe in this simulated data set. Identical samples were analyzed using a multiple bounded, double bounded and single bounded model. In the single bounded model, offers were selected at random at $10 intervals from $90 to $210. The end points of this range were selected so that offers covered the distribution from approximately the 10% level to the 90% level. In the double bounded model, bids were set at $120, $150 and $180. These amounts approximately corresponded to the quartiles of the distribution from which the data were drawn. The multiple bounded model employed 13 bids for each observation. These bids were set at $10 increments that ranged from $90 to $210. Models were estimated for samples sizes of 200, 100, 50, and 25. The results of these estimations are presented in tables 1 - 4.

For sample sizes of 200, all three methods produced reasonable models in the sense that all coefficients are of the sign expected and are highly significant. However, both the double bounded and multiple bounded models produce results that are substantially more precise than the results from the single bounded model. The double bounded model produces estimates that are slightly more precise for estimates of the mean, while the multiple bounded model produces a higher level of precision for the estimated parameters. This slight advantage of the double bounded model over the multiple bounded model in estimating mean willingness to pay is to be expected, since the design of the double bounded bids is close to the C-Optimal bid design to minimize the variance of estimated mean·willingness to pay (Kanninen 1992). For all sample sizes, the multiple bounded model produces smaller variances for the estimated parameters than the double bounded model. For all sample sizes, the single bounded logit model produced the lowest estimates of mean willingness to pay. The double bounded model usually produced higher estimates of mean willingness to pay than the multiple bounded model. While all four of the simulated data sets were drawn from a distribution with a mean of $150, the actual willingness to pay in the four samples deviated slightly from $150. In the samples of 200, 100, 50 and 25, the mean willingness to pay was $148.41, $147.71, $152.16, and $154.95 respectively.

The previous examples were designed to explore the efficiency of the various models when the researcher has sufficient prior information to implement a nearly C-optimal bid design for a double bounded model. In practice, since the purpose of a contingent valuation exercise is to estimate average willingness to pay, it would be unusual that a researcher would actually be able to implement a C-optimal bid design for a double bounded model. To address the relative efficiency of the double bounded model in the presence of non-optimal bid designs, three additional double bounded models were estimated. The first additional double bounded model was estimated using bids of $90, $150, and $210. The second additional double bounded model was estimated using a bid structure of $140, $180 and $220. In the third additional double bounded model the bids were set at $100, $140, and $180. When non-optimal bid designs are used, the multiple bounded model typically provides better precision for mean willingness to pay than the double bounded models.

Conclusion

From a statistical point of view the multiple bounded discrete choice model is a rather simple extension of single and double bounded model. From a computational point of view, the only difficult task is to write the gradient and the hessian in terms of the upper and lower interval on which CS_i is revealed to occur. When compared to a double bounded model based on an optimal bid structure, the multiple bounded appears to provide slightly less precise estimates of central tendency and small improvements in the precision of estimated parameters. Given this result one might ask whether it is worthwhile to implement a multiple bounded model. We believe there are several compelling arguments in support of the multiple bounded model. First, we believe that the multiple bounded model may reduce or eliminate starting point like biases that may be present in the double bounded model. Second, we believe that use of the multiple bounded model substantially reduces the practical difficulties associated with designing bids for either the single or double bounded model. Third, we believe that in cases where the researcher is unsure about what would constitute an optimal design to estimate mean willingness to pay in a double bounded model, a multiple bounded model is likely to provide more precise estimates of central tendency than the double bounded model. Finally, we hold out the possibility that the multiple bounded model would allow a researcher to obtain the levels of precision associated with double bounded models without employing costly personal interviews that are required for the double bounded model. In summary we believe that in many practical applications, the multiple bounded model may be cheaper to implement than the double bounded model, and will provide precision (for estimated parameters and for measure of mean willingness to pay) similar to and probably greater than that which would be obtained from the double bounded model.

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Figure 1. Example of Question Format Used to Generate a Return Potential Curve for Evaluation of In-stream Flow.

CONSTANT FLOW LEVELS

1. How would you, as a commercial river guide using the boat you usually pilot, evaluate each of the following water levels for a commercial Grand Canyon river trip? Assume the water level would be constant for the entire trip. (CIRCLE ONE NUMBER FOR EACH WATER LEVEL)

Flow Level	Very Satisfactory	Somewhat Satisfactory	Neutral	Somewhat Unsatisfactory	Very Unsatisfactory
2,000 cfs	1	2	3	4	5
3,000 cfs	1	2	3	4	5
4,000 cfs	1	2	3	4	5
5,000 cfs	1	2	3	4	5
7,500 cfs	1	2	3	4	5
10,000 cfs	1	2	3	4	5
15,000 cfs	1	2	3	4	5
20,000 cfs	1	2	3	4	5
25,000 cfs	1	2	3	4	5
30,000 cfs	1	2	3	4	5
40,000 cfs	1	2	3	4	5
50,000 cfs	1	2	3	4	5
60,000 cfs	1	2	3	4	5
80,000 or more	1	2	3	4	5

Figure 2. Example of a Multiple Bounded Contingent Valuation Question.

HOW WOULD YOU VOTE ON THIS ISSUE?
(Circle one letter for each amount)

How would you vote if passage meant your taxes next year would increase by . . .?	Definitely Yes	Probably Yes	Not Sure	Probably No	Definitely No
10¢	A	B	C	D	E
25¢	A	B	C	D	E
50¢	A	B	C	D	E
75¢	A	B	C	D	E
$1	A	B	C	D	E
$2	A	B	C	D	E
$3	A	B	C	D	E
$4	A	B	C	D	E
$5	A	B	C	D	E
$6	A	B	C	D	E
$7	A	B	C	D	E
$8	A	B	C	D	E
$9	A	B	C	D	E
$10	A	B	C	D	E
$15	A	B	C	D	E
$20	A	B	C	D	E
$25	A	B	C	D	E
$30	A	B	C	D	E
$35	A	B	C	D	E
$40	A	B	C	D	E
$45	A	B	C	D	E
$50	A	B	C	D	E
$75	A	B	C	D	E
$100	A	B	C	D	E
$150	A	B	C	D	E
$200	A	B	C	D	E

Table 1. Single, Double, and Multiple Bounded Models for a Sample Size of 200

	Actual Parameter	Single Bounded	Double Bounded	Multiple Bounded
α	6	6.92 (.9442)	6.75 (.5471)	5.88 (.4030)
β	-.04	-.04719 (.006306)	-.04518 (.003557)	-.03952 (.002589)
Median[a]	$150	$146.60 (3.89)	$149.43 (2.81)	$148.82 (3.09)
Analytic 95% Confidence Interval for Median[b]		[138.97, 154.23]	[143.93, 154.94]	[142.76, 154.89]
Simulation 95% Confidence Interval for Median[c]		[138.60, 154.28]	[144.10, 155.12]	[142.80, 154.80]
Mean[d]	$148.41	$146.62	$149.45	$148.89
Simulation 95% Confidence Interval for Mean[e]		[138.71, 154.36]	[144.15, 155.14]	[142.94, 154.89]

( ) Numbers in parentheses are estimated standard errors.

[a] Standard errors for the median were developed using the procedures suggested in Cameron, 1991.

[b] Analytic confidence intervals calculated using the estimated standard error for the median.

[c] Simulation confidence intervals calculated by dropping the 25 highest and 25 lowest observations from a series of 1,000 medians generated using procedures suggested in Krinsky and Robb, 1986.

[d] Mean estimates from the models were calculated as $(\ln(1+\exp\alpha)/-\beta$.

[e] Simulation confidence intervals calculated by dropping the 25 highest and 25 lowest observations from a series of 1,000 means generated using procedures suggested in Krinsky and Robb, 1986.

Table 2. Single, Double, and Multiple Bounded Models for Sample Size of 100

	Actual Parameter	Single Bounded	Double Bounded	Multiple Bounded
α	6	5.61 (1.1768)	6.27 (.7399)	5.58 (.5504)
β	-.04	-.03914 (.007946)	-.04224 (.004827)	-.03761 (.003536)
Median[a]	$150	$143.41 (6.21)	$148.39 (4.24)	$148.33 (4.59)
Analytic 95% Confidence Interval for Median[b]		[131.24, 155.58]	[140.07, 156.70]	[139.33, 157.33]
Simulation 95% Confidence Interval for Median[c]		[129.59, 155.94]	[139.34, 156.25]	[139.21, 157.40]
Mean[d]	$147.71	$143.50	$148.43	$148.43
Simulation 95% Confidence Interval for Mean[e]		[129.79, 156.10]	[139.46, 156.27]	[139.48, 157.52]

( ) Numbers in parentheses are estimated standard errors.

[a] Standard errors for the median were developed using the procedures suggested in Cameron, 1991.

[b] Analytic confidence intervals calculated using the estimated standard error for the median.

[c] Simulation confidence intervals calculated by dropping the 25 highest and 25 lowest observations from a series of 1,000 medians generated using procedures suggested in Krinsky and Robb, 1986.

[d] Mean estimates from the models were calculated as $(\ln(1+\exp\alpha)/-\beta$.

[e] Simulation confidence intervals calculated by dropping the 25 highest and 25 lowest observations from a series of 1,000 means generated using procedures suggested in Krinsky and Robb, 1986.

Table 3. Single, Double, and Multiple Bounded Models for Sample Size of 50

	Actual Parameter	Single Bounded	Double Bounded	Multiple Bounded
α	6	6.59 (1.8409)	5.95 (1.0188)	4.86 (.7310)
β	-.04	-.04379 (.01210)	-.03857 (.006485)	-.03138 (.004458)
Median[a]	$150	$150.48 (8.09)	$154.19 (6.58)	$154.84 (7.78)
Analytic 95% Confidence Interval for Median[b]		[134.62, 166.34]	[141.30, 167.08]	[139.59, 170.08]
Simulation 95% Confidence Interval for Median[c]		[131.68, 169.46]	[141.55, 168.58]	[138.65, 170.38]
Mean[d]	$152.16	$150.51	$154.26	$155.08
Simulation 95% Confidence Interval for Mean[e]		[133.97, 169.64]	[141.90, 168.77]	[139.14, 170.69]

( ) Numbers in parentheses are estimated standard errors.

[a] Standard errors for the median were developed using the procedures suggested in Cameron, 1991.

[b] Analytic confidence intervals calculated using the estimated standard error for the median.

[c] Simulation confidence intervals calculated by dropping the 25 highest and 25 lowest observations from a series of 1,000 medians generated using procedures suggested in Krinsky and Robb, 1986.

[d] Mean estimates from the models were calculated as $(\ln(1+\exp\alpha)/-\beta$.

[e] Simulation confidence intervals calculated by dropping the 25 highest and 25 lowest observations from a series of 1,000 means generated using procedures suggested in Krinsky and Robb, 1986.

Table 4. Single, Double, and Multiple Bounded Models for Sample Size of 25

	Actual Parameter	Single Bounded	Double Bounded	Multiple Bounded
α	6	9.25 (3.4593)	6.64 (1.5172)	5.69 (1.1176)
β	-.04	-.05954 (.02218)	-.04233 (.009696)	-.03642 (.006970)
Median[a]	$150	$155.41 (9.43)	$156.77 (8.60)	$156.17 (9.54)
Analytic 95% Confidence Interval for Median[b]		[136.92, 173.90]	[139.92, 173.62]	[137.48, 174.86]
Simulation 95% Confidence Interval for Median[c]		[130.16, 185.00]	[140.15, 176.73]	[134.46, 176.20]
Mean[d]	$154.95	$155.41	$156.80	$156.26
Simulation 95% Confidence Interval for Mean[e]		[131.94, 190.89]	[140.31, 176.79]	[135.35, 176.22]

( ) Numbers in parentheses are estimated standard errors.

[a] Standard errors for the median were developed using the procedures suggested in Cameron, 1991.

[b] Analytic confidence intervals calculated using the estimated standard error for the median.

[c] Simulation confidence intervals calculated by dropping the 25 highest and 25 lowest observations from a series of 1,000 medians generated using procedures suggested in Krinsky and Robb, 1986.

[d] Mean estimates from the models were calculated as $(\ln(1+\exp\alpha)/-\beta$.

[e] Simulation confidence intervals calculated by dropping the 25 highest and 25 lowest observations from a series of 1,000 means generated using procedures suggested in Krinsky and Robb, 1986.

Table 5. Comparisons of Multiple Bounded and Non-Optional Double Bounded Models, Sample Size 200

	Multiple Bounded	Double Bounded Bids at $90, $150, $210	Double Bounded Bids at $140, $180, $220	Double Bounded Bids at $100, $140, $180
α	5.88 (.4030)	5.64 (.4239)	5.39 (.5256)	6.19 (.4739)
β	-.039517 (.002589)	-.03757 (.002689)	-.03652 (.003294)	-.04216 (.003127)
Median[a]	$148.82 (3.09)	$150.09 (3.50)	$147.56 (3.72)	$146.90 (3.05)
Analytic 95% Confidence Interval for Median[b]	[142.76, 154.89]	[143.23, 156.95]	[140.27, 154.85]	[140.92, 152.88]
Simulation 95% Confidence Interval for Median[c]	[142,80, 154.80]	[143.16, 156.79]	[139.82, 154.44]	[140.82, 152.44]
Mean[d]	$148.64	$150.18	$147.68	$146.95
Simulation 95% Confidence Interval for Mean[e]	[142.94, 154.89]	[143.41, 156.90]	[140.00, 154.58]	[140.89, 152.47]

( ) Numbers in parentheses are estimated standard errors.

[a] Standard errors for the median were developed using the procedures suggested in Cameron, 1991.

[b] Analytic confidence intervals calculated using the estimated standard error for the median.

[c] Simulation confidence intervals calculated by dropping the 25 highest and 25 lowest observations from a series of 1,000 medians generated using procedures suggested in Krinsky and Robb, 1986.

[d] Mean estimates from the models were calculated as $(\ln(1+\exp\alpha)/-\beta$.

[e] Simulation confidence intervals calculated by dropping the 25 highest and 25 lowest observations from a series of 1,000 means generated using procedures suggested in Krinsky and Robb, 1986.

A Convolutions Approach to Measuring the Differences in Simulated Distributions: Application to Dichotomous Choice Contingent Valuation*

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Abstract

Resampling or simulation techniques are now frequently used in applied economic analyses, but previously developed significance tests for differences in empirical distributions have either invoked normality assumptions or used non-overlapping confidence interval criteria. This paper demonstrates that such methods will generally not be appropriate, and presents an exact empirical test, based on the method of convolutions, for assessing the statistical significance between approximate empirical distributions created by resampling techniques. Application of the proposed convolutions approach is illustrated in a case study using empirical distributions from dichotomous choice contingent valuation data.

A Convolutions Approach to Measuring the Differences in Simulated Distributions: Application to Dichotomous Choice Contingent Valuation

Introduction

Resampling or simulation techniques are increasingly applied to estimate standard deviations and confidence intervals for welfare measures (Kling; Kling and Sexton; Adamowicz, Fletcher and Graham-Tomasi; Creel and Loomis), elasticities and flexibilities (Dorfman, Kling and Sexton; Marquez), economies of size and scope (Eakin, McMillan and Buono; Schroeder), travel cost models (Loomis, Park and Creel), and contingent valuation (Park, Loomis and Creel; Duffield and Patterson; DesVousges et. al., 1992a, 1992b). While considerable effort has been focused on motivating, developing and comparing alternative methods of approximating distributions, very little attention has been given to developing formal statistical tests of the difference between approximate empirical distributions generated by these techniques. Such assessments are essential to applied economic and policy analyses in which comparison of point estimates are needed across policy alternatives, population and commodity groups, inputs, and levels of provision of non-marketed goods.

Using the dichotomous choice contingent valuation method (DC-CVM) as an example, this paper presents a statistical test, based on the method of convolutions, to evaluate the significance of the difference between approximate empirical distributions and illustrates how to apply this test to actual DC-CVM data. Because the convolution formula provides an exact measure of the statistical significance of the difference between empirical distributions, it is preferable to previous techniques that either impose restrictive assumptions of normality or adopt a non-overlapping confidence interval criterion. Moreover, the non-parametric nature of this test is a logical extension of the motivations for using empirical distributions in the first place.

The DC-CVM was chosen as a vehicle for demonstrating this approach because: 1) bootstrapping and other resampling techniques are widely being adopted to approximate distributions of DC-CVM benefit measures; 2) comparisons of benefit measures for different quality levels and scenarios is a fundamental objective of DC-CVM (Cummings, Brookshire and Schulze; Mitchell and

Carson); 3) benefit comparisons are also essential to assessing the validity and reliability of the contingent valuation method (Bishop and Heberlein; Loomis); and 4) DC-CVM is an area in which statistically biased or otherwise inappropriate techniques for comparing approximated distributions have been used and reported by some researchers. It is essential to note, however, that the discussion that follows is not limited to the particular estimation approach applied in this example. The criticisms and suggested techniques developed in this paper with respect to DC-CVM are generalizable to any simulated distributions of economic parameters for which it is reasonable to ask "Is the difference between distributions significantly different from zero?".

The remainder of this paper is organized as follows. A critique of the methods currently being used to evaluate the significance of the difference between empirical distributions is provided in the following section. The third section presents the convolutions method. The mechanics of this technique are demonstrated using simple hypothetical distributions in the fourth section, and the convolutions technique is applied to DC-CVM data in the fifth section.

A Critique of Past Methods for Evaluating Differences in Simulated Distributions

Instead of providing a detailed review of resampling techniques currently being used in the economic literature, this paper assumes that two approximate empirical distributions of point estimates, such as those presented in Figure 1, have already been created[1]. Interpretation of Figure 1 is as follows: $f_X(X)$ and $f_Y(Y)$ are the simulated probability density functions for parameters X and Y; the shaded area represents an approximate $(1\text{-}\gamma)$ confidence interval; and $L_{1-\gamma}(.)$ and $U_{1-\gamma}(.)$ depict the lower and upper bounds of this confidence interval. Although the two distributions lie on the same number line, they are separated in the figure in order to isolate the degree of overlap between the two confidence intervals.

The dichotomous choice format asks individuals if they would be willing to pay a specified amount, or bid value, for a public good. Bid values (A) are randomly assigned across survey participants, and the yes/no (1/0) responses across participants and bid values can be modeled using a random utility framework (Hanemann, 1984). The following linear logit distribution is frequently used to model the cumulative distribution function (G(A;θ)) of willingness to pay

$$G(A;\theta) = \Pi^N(A) = [1+e^{\theta}]^{-1} \tag{1a}$$

where,

$$\theta = \alpha - \beta A + \xi X \tag{1b}$$

$\Pi^N(A)$ is the probability of a 'no' response to bid value A, X is a vector of other explanatory variables, and α, β and ξ are coefficients to be estimated (Hanemann (1984); Bowker and Stoll; Boyle and Bishop). Approximate empirical distributions of the mean that correspond to those presented in Figure 1 may then be calculated using resampling techniques (e.g. bootstrapping, Krinsky and Robb, monte carlo) for the estimated coefficients and the following closed-form solution presented in Hanemann (1989)

$$E(WTP) = \frac{1}{\beta}\ln(1+e^{\alpha+\xi\bar{X}}) \tag{2}$$

In the above equation, E is the mathematical expectation operator and $\bar{X}$ is the mean value vector corresponding to X.

Two techniques for evaluating the significance of the difference between these distributions have been proposed in the literature. The first, as implicitly suggested by Krinsky and Robb and applied by Desvousges et al. (1992b), is that if the simulated distributions are approximately normal then classical statistical procedures for estimating differences can be applied. For instance, assuming equal and known standard errors (STDERR) for two normal distributions, the null hypothesis that the 'true' mean of the first distribution is equal to the 'true' mean of the second distribution is tested using the following difference formula,

where Z is the test statistic and $\bar{X}_i$ are the sample means (Snedecor and Cochrane, p. 101). As noted, the Z value has a standard normal distribution. In the bootstrapping framework, the standard error

$$Z = \frac{(\bar{X}_1 - \bar{X}_2)}{\sqrt{2} * STDERR} \qquad \sim N(0,1) \tag{3}$$

of the mean is given by the estimated standard deviation of the empirical distribution of the mean estimate.

Objections to this normality assumption occur at both a theoretical and empirical level. First, much effort has been given to developing these empirical approaches in order to capture non-linearities, and subsequent non-normalities, of the functions of parameters used to calculate the desired distribution. It would seem counterproductive to impose unneeded parametric assumptions at this stage. Second, our experience suggests that empirical distributions estimated following the method associated with Equations (1) and (2) are skewed, and the assumption of 'approximately normal' is often inappropriate. More generally, there is little reason to assume that non-linear functions of normal parameters will approximate a normal distribution, a fact that is equally relevant to elasticities, flexibilities, and welfare measures.

Park, Loomis and Creel (hereafter PLC) avoid the assumption of normality by employing a non-overlapping confidence interval criterion to evaluate differences in point estimates. That is, PLC judge the differences in mean willingness to pay across estimates to be statistically significant at the 10 percent level if their empirical 90 percent confidence intervals do not overlap. This approach is also used in Desvousges et al. (1992a) who state that "overlapping confidence intervals imply that significant differences do not exist...between WTP estimates" (p. 22). With respect to Figure 1, the distributions are judged, using this criterion, to be significantly different at the 5 percent level if $U_{0.95}(Y)$ lies to the left of $L_{0.95}(X)$ on a number line. If $L_{0.95}(X)$ lies to the left of $U_{0.95}(Y)$ then the two central confidence intervals overlap and the distributions are not judged to be significantly different at the 5 percent level using the non-overlapping confidence interval criterion.

In general, the actual significance of this non-overlapping confidence interval approach will not correspond to the stated level of the test. This point is demonstrated most simply for normal distributions using the analytical solution presented in Equation (3). Recall that for a single normal distribution the 95 percent confidence interval for the mean of an estimate is defined as $\overline{X}_i$ $\pm$1.9600*(STDERR). Again assuming that the standard errors for both distributions are known and equal, this implies that the critical difference in means, ($\overline{X}_1$ - $\overline{X}_2$), associated with the non-overlapping 95 confidence intervals would have to be at least 3.9200 standard errors apart before they would be judged to be significantly different. Making this substitution, Equation (3) becomes

$$Z=\frac{(3.9200*STDERR)}{\sqrt{2}*STDERR}=\frac{3.9200}{\sqrt{2}}=2.772 \quad (5)$$

The estimated z value of 2.772 corresponds to a significance level (which shall be referred to as γ') of 0.0048 rather than the stated value of γ=0.05.

Conversely, Equation (4) can be rearranged and solved for the difference between two means that corresponds to a non-overlapping confidence interval for γ=0.05. Simple algebra and a critical value of 1.9600 indicate that the point where the two means is significantly different occurs when the means are approximately 2.772 standard errors apart. At this distance, the non-overlapping two-sided confidence intervals only encompass about 87 percent of their respective distributions.

Clearly the non-overlapping confidence interval criterion given by (1-γ) confidence intervals does not correspond to the γ level of significance for the normals case. In general, a lack of correspondence between γ and γ' is expected. For the normal distribution above, the significance level is understated (i.e. $\gamma > \gamma'$) and the test is more conservative than indicated. The degree of this difference between γ and γ' will depend upon the shape of the empirical distributions that are being compared.

In sum, the two methods currently being applied in DC-CVM either involve inappropriate assumptions or are statistically biased.

The Method of Convolutions

Another alternative - one that accommodates any distributional form - is based on the method of convolutions. This technique is used in statistics and mathematics to evaluate the sum of distributions of random variables and series (Feller; Mood, Graybill and Boes).

Let X and Y be independent random variables[2], with respective probability density functions $f_X(x)$ and $f_Y(y)$. Then, for all values of X and Y

$$f(x,y) = f_X(x)f_Y(y) \tag{6}$$

Define the difference V = X-Y to be a new random variable. The probability of the event V=v is defined as the union of all the possible combinations of x and y which result in a difference of v. For continuous functions this relation is given explicitly as

$$f_V(v) = f_{X-Y}(v) = \int_{-\infty}^{\infty} f_Y(x-v)f_X(x)\,dx = \int_{-\infty}^{\infty} f_X(v+y)f_Y(y)\,dy \tag{7}$$

which is a variant of the convolution formula (Mood, Graybill and Boes). Using only the far right hand side of Equation (6), the cumulative distribution function $F_V(v^\circ)$ of the difference of X and Y is

$$F_V(v^\circ) = \int_{-\infty}^{v^\circ} f_V(v)\,dv = \int_{-\infty}^{v^\circ}\int_{-\infty}^{\infty} f_X(v+y)f_Y(y)\,dy\,dv \tag{8}$$

For empirical applications with discrete observations, the dimensions of Equation (7) can be reduced substantially. If $f_Y(y)=0$ or $f_X(v+y)=0$ then $f_V(v)=0$ also. This implies that the range of the first integrand can be bounded by the minimum of the ordered y vector and the value of y for which (v+y) exceeds the range of the empirical distribution of $f_X(x)$. These values shall be denoted infy and supy, respectively. Similarly the second integral can be bounded from below by the minimum possible value for X-Y, denoted here as infv. In this manner Equation (7) can be restated for discrete probabilities obtained from simulation procedures as

$$\hat{F}_V(v^\circ) = \sum_{infv}^{v^\circ}\sum_{infy}^{supy} \hat{f}_X(v+y)\hat{f}_Y(y)\Delta y\Delta z \tag{9}$$

where $\hat{F}_V(v^o)$, $\hat{f}_X(x)$ and $\hat{f}_Y(y)$ are discrete approximations of $F_V(v^o)$, $f_X(x)$ and $f_Y(y)$. The incremental values for y and z are defined by the desired level of precision and computational power.

The above equations can be directly applied to the information provided from the simulated distributions. As in the simulation methods, the distribution of the differences will generally not be known, and an empirical approach to estimating confidence intervals is necessary. Adopting a 'percentile approach' (Efron) the lower bound and upper bound of the 1-γ confidence intervals are respectively defined as

$$\hat{L}_{1-\gamma}(Z)=\hat{F}_Z^{-1}(\gamma/2) \qquad \hat{U}_{1-\gamma}(Z)=\hat{F}_Z^{-1}(1-\gamma/2) \tag{10}$$

And,

$$[\hat{L}_{1-\gamma}(Z),\hat{U}_{1-\gamma}(Z)] \tag{11}$$

is the approximate (1-γ) central confidence interval for Z. This range will often be non-symmetric around the mean.

Combining the principle of the two sided difference in means test with a percentile approach, the null hypothesis that the difference between X and Y equals zero is accepted at the γ level of significance if the approximate (1-γ) confidence interval of the convolution includes zero and rejected otherwise. Alternatively, assuming that the distributions are ordered in a descending fashion, the approximate significance of the difference between distributions is determined by twice the value of the cumulative distribution function at the convoluted value of zero.

A Simple Demonstration of the Convolutions Technique

This section demonstrates the application of the discrete convolution formula presented in Equation (8) and the suggested statistical test for estimating the significance of the difference between two approximate empirical distributions. Suppose that we are interested in evaluating the difference between the two approximate empirical distributions presented in Table 1. The probability density function (pdf), cumulative distribution function (cdf), and the calculations required to generate a convolution of these distributions are demonstrated in Table 2, where $f_V(.)$ and $F_V(.)$ are the pdf and cdf respectively and only the values that lie within the bounds set by infy, supy and infv are reported. Evaluating $F_V(0)$ indicates that the two distributions are different at the 17 (=2*0.085) percent level.

Application of the Convolutions Approach

This section applies the convolutions technique to evaluating differences in compensating variation associated with two different water flow levels in the Grand Canyon. In addition, this section further demonstrates that the normality based approach and the non-overlapping confidence interval criterion are inappropriate and may lead to misguided conclusions about the significance of the difference between distributions in policy relevant applications. In order to focus on the convolutions technique, the model presented in this example is intentionally simplistic -- only the bid value and cost of the trip are included as explanatory variables in the statistical analysis. More sophisticated models and a greater description of the study are presented in Bishop et al. [1987], Bishop et al. [1989], and Boyle, Welsh and Bishop.

Flow level in the Grand Canyon is a decision variable for the Glen Canyon dam, which generates electricity and regulates flows below the dam. These flow levels, measured in cubic feet per second (cfs), are outside the control of boaters but do affect the quality of whitewater rafting trips in the Grand Canyon.

> "Time at attraction sights, such as Indian ruins and side canyons with pleasing scenery, and for layovers, depends on the speed of the current. The size and the number of rapids are affected by dam releases. Boaters, particularly those on commercial trips, enjoy fairly large rapids that depend on substantial flows. At relatively low flows and flood flows, passengers, particularly those on commercial oar powered trips, may have to walk around rapids. This is generally considered undesirable by passengers" (Bishop et al., 1987, p. 11-12)

Given these considerations, Hicksian surplus values for different flow levels are implicitly defined as

$$V(P,Y-H_j;f_j) = V(P^m,Y;f_j) \qquad (12)$$

where V(.) is an indirect utility function, P is the price, Y is income, f_j is the jth flow, H_j is Hicksian compensating surplus (WTP) for the jth flow, and P^M is the choke price at or above which the trip would not be taken (Boyle, Welsh and Bishop). In this simplified analysis other trip attributes and personal characteristics are assumed to be constant, and are subsumed here for notational convenience.

Two different flow ranges are considered in this analysis: 0 to 25,000 cfs and 26,000 to 33,500 cfs. These flow ranges are termed low and high flows respectively. The value of 33,500 cfs corresponds to the maximum flows that can be used to generate electricity by the dam, and thus represents the maximum of the policy relevant range. The 25,000 cfs cut-off point approximates the mean of the flow levels experienced by participants in the survey sample, and was used as an ad hoc division between low and high flows.

The linear specification of the logit model detailed in Equation (1) above was used to evaluate the distribution of willingness to pay from bid values and responses, with

$$\theta_j = \alpha_j - \beta_j A + \xi_j P \qquad (13)$$

In the above equation α, β and ξ again represent coefficients to be estimated, j indicates the range of flows experienced, P is the price of the trip taken, and A is the dichotomous choice bid value. Respondents were grouped into flow level categories based on their mean flow level experienced during their trip taken from hydrological data. As presented in Table 3, the estimations are fairly robust. Although some individual coefficients are not significant, each estimated equation has highly significant χ^2 values.

In addition, log-likelihood ratio tests indicate that the estimated distribution of WTP for the low and the high flow levels are significantly different at the 10 percent level (LR=8.27 >

$\chi^2_{3,10}$=6.25). Thus, we can conclude that, over these flow ranges, flow levels do have a significant effect on the distribution of WTP.

Whether there are significant differences in Hicksian surplus values is a different question, one that can only be answered by comparing distributions of mean willingness to pay estimates. Formally the hypothesis being tested is that

$$H_L = H_H \tag{14}$$

where H_L and H_H are the Hicksian surpluses associated with low and high flow conditions.

Estimated means and their distributions for each scenario were created by applying the Krinsky and Robb technique to the closed-form solution presented in Equation (2) above[3]. Critical points on these distributions are presented in Table 4 and the distributions themselves are presented in Figure 2.

Evaluation of Table 4 and Figure 2 indicate two points of interest. First, the distributions evaluated here are significantly skewed and apparently deviate from normality. As a result of this observation, classical difference tests based on normality assumptions are not relevant here. The second interesting point is that in spite of the statistical significance between WTP distributions for low and high flows, their distributions overlap considerably. Most notably, their 90 percent confidence intervals do overlap and thus, the application of the non-overlapping confidence interval criterion would lead to the conclusion that the mean WTP distributions are not significantly different at the 10 percent level.

A different conclusion is reached with the convolutions method detailed in this paper, for which the distribution is plotted in Figure 2 and critical points are identified in Table 5[4]. In contrast to the non-overlapping confidence interval criterion, the mean willingness to pay values for low and high flows are judged to be significantly different at the 4.2 percent level and the 90 (and 95) percent confidence intervals for the difference do not include zero. This level of significance is clearly less than 10 percent, indicating that the non-overlapping confidence interval criterion would lead to erroneous conclusions of the level of significance in policy relevant situations. In this instance, the deviation between the significance judgement of the non-overlapping confidence interval and the actual level of significance is substantial.

Summary and Conclusions

Economists have increasingly turned to resampling or simulation techniques to explore the variability in a wide range of estimated economic parameters, including elasticities, flexibilities and various welfare measures. Undoubtedly resampling and simulation techniques are a valuable tool for exploring the inherent variability of estimated parameters for which it is difficult (if not impossible) to develop analytic variance estimates. However by themselves, these techniques do not provide a way to compare the distributions that arise from applying the techniques in various contexts. Often it is this comparison that is of most interest. For example, is the price elasticity significantly different between two demographic groups? Do any of the various policy options result in a higher value of the estimated welfare measure?

Appropriate answers to these types of questions require appropriate statistical tests. The two approaches that have been used and reported by researchers to compare approximate empirical distributions may be inappropriate in many specific applications. The method of convolutions, as presented in this paper, does provide a proper statistical test for assessing the significance of the difference between two distributions and represents a logical extension of resampling techniques.

Application of the convolutions approach is not costless, however. While some computing packages offer routines that will perform a convolution of two distributions[5], the actual calculation of the convolution can be computationally intensive if the distributions have many points.

The decision of whether to adopt the convolutions approach will depend upon the objective of the research and the nature of the distributions. With respect to the normality assumption the decision is obvious. For those cases in which the hypothesis of normality is rejected, then using a normality based approach is wrong. Under those circumstances a convolutions approach would seem justified. The answer is less clear when considering the non-overlapping confidence interval criterion. The criterion is conservative in the sense that if two differences are found to be non-overlapping at the 5 percent level the difference between the two distributions is certainly significant at that level. However it is possible that the 95 percent confidence intervals will overlap, and yet, the distributions will actually be different at the 5 percent level. If the consequences of declaring a significant difference insignificant is of little importance then the non-overlapping confidence criteria might be deemed an acceptable test. Yet, if one is interested in reducing the chance that a significant difference is missed or if the researcher desires to report the actual level of significance, then the convolutions approach may prove advantageous.

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Endnotes

1. A critical review and comparison of the three techniques currently being used in DC-CVM is provided in Poe. The techniques themselves are developed separately in Park, Loomis and Creel, Duffield and Patterson, and Desvousges et al. [1992b].

2. Independence is not necessary for the convolution formula itself, but this assumption facilitates the empirical application of this method. This is not meant to imply that the assumption of independence is inconsequential. Indeed, for contingent valuation it implies that these estimates should be derived from separate samples or by some other means that assures independence. The need for independent samples is shared by other statistical approaches. For example, in applying the classical techniques based on normality assumptions Desvousges et al. (1992b, p. 30) note that "using independent samples....is essential for the hypothesis testing".

3. Estimates of the significance using methods suggested in Duffield and Patterson and Desvousges et al. [1992b] provide similar results and are available from the authors.

4. In calculating the empirical distribution of the convolution, Δy and Δv were set at 1.

5. The convolutions program used in this paper was performed in GAUSS, making use of the CONV routine. It is our understanding that the option of programming a convolution exists in other matrix based languages (e.g. SAS-IML).

Table 1. Hypothetical Distributions

Range	$f_x(.)$	$F_y(.)$
0	0.00	0.05
1	0.00	0.30
2	0.10	0.60
3	0.40	0.05
4	0.40	0.00
5	0.10	0.00

Table 2. Demonstration of convolution for Simple Distributions

			$F_v(-2)$	= 0.000
$f_v(-1)$=	$f_x(2)f_y(3)$	= 0.005	$F_v(-1)$	= 0.005
	(.1)(.05)			
$f_v(0)$ =	$f_x(2)f_y(3)$	= 0.080	$F_v(0)$	= 0.085
	(.1)(.6) + (.4)(.05)			
$f_v(1)$ =	$f_v(2)f_y(1) + f_x(3)f_y(2) + f_x(4)f_y(3)$	= 0.290	$F_y(1)$	= 0.375
	(.1)(.3) + (.4)(.6) + (.4)(.05)			
$f_v(2)$ =	$f_x(2)f_y(0) + f_x(3)f_y(1) + f_x(4)f_y(2) + f_x(5)f_x(3)$	= 0.370	$F_v(2)$	= 0.745
	(.1)(.05) + (.4)(.6) + (.4()(.5) +(.1)(.05)			
$f_v(3)$ =	$f_x(3)f_y(0) + f_x(4)f_y(1) + f_x(5)f_y(2)$	= 0.200	$F_y(3)$	= 0.945
	(.4)(.05) +(.4)(.3) +(.1)(.6)			
$f_v(4)$ =	$f_x(4)f_y(0) + f_x(5)f_y(1)$	= 0.050	$F_x(4)$	= 0.995
	(.4)(.05) + (.1)(.3)			
$f_v(5)$ =	$f_x(5)f_y(0)$	= 0.005	$F_v(5)$	= 1.000
	(.1)(.05)			

Table 3. Estimated Logit Equations for Different Flow Levels for Commercial White Water Boaters

Flow Conditions[a,b]	Low	High
Constant	-1.368*	0.418
	(0.766)	(0.730)
Cost	-0.000373	-0.00133***
	(0.000436)	(0.000423)
Bid	0.00380***	0.00226***
	(0.000944)	(0.000732)
Model x^2	21.78***	22.16***
n	98	128

[a]Asymptotic standard errors in ().
[b]Significance levels *(10%), **(5%), ***(1%)

Table 4. Empirical Mean Willingness to Pay for Different Flow Ranges for Commercial White Water Boaters Using Krinsky and Robb Simulation Technique

Flow Level	Calculated from Parameter Means	Based on 1000 Draws: Lower Tail		Median	Mean	Upper Tail		Standard Error	Skew[a]
Low Flow	536.06	443.13	457.60	540.73	548.23	643.76	679.89	79.02	6.07
High Flow	735.14	609.10	623.22	745.75	782.77	1049.57	1221.05	171.49	4.18

[a]Skewness test: $g_{0.01,\ 1000} = 0.180$ [Table 34b; Tables for Statisticians and Biometricians]

Table 5. Approximate Significance Levels and Confidence Intervals of Difference Between Mean Willingness to Pay Estimates for Different Flow Ranges for Commercial Whit Water Boaters Using Convolutions Technique

Estimated Significance Level of Difference	Confidence Interval Bounds			
	Lower Tail		Upper Tail	
	5%	10%	10%	5%
0.043	7	41	526	696

Figure 1: Simulated Distributions and the Non-Overlapping Confidence Interval Criterion

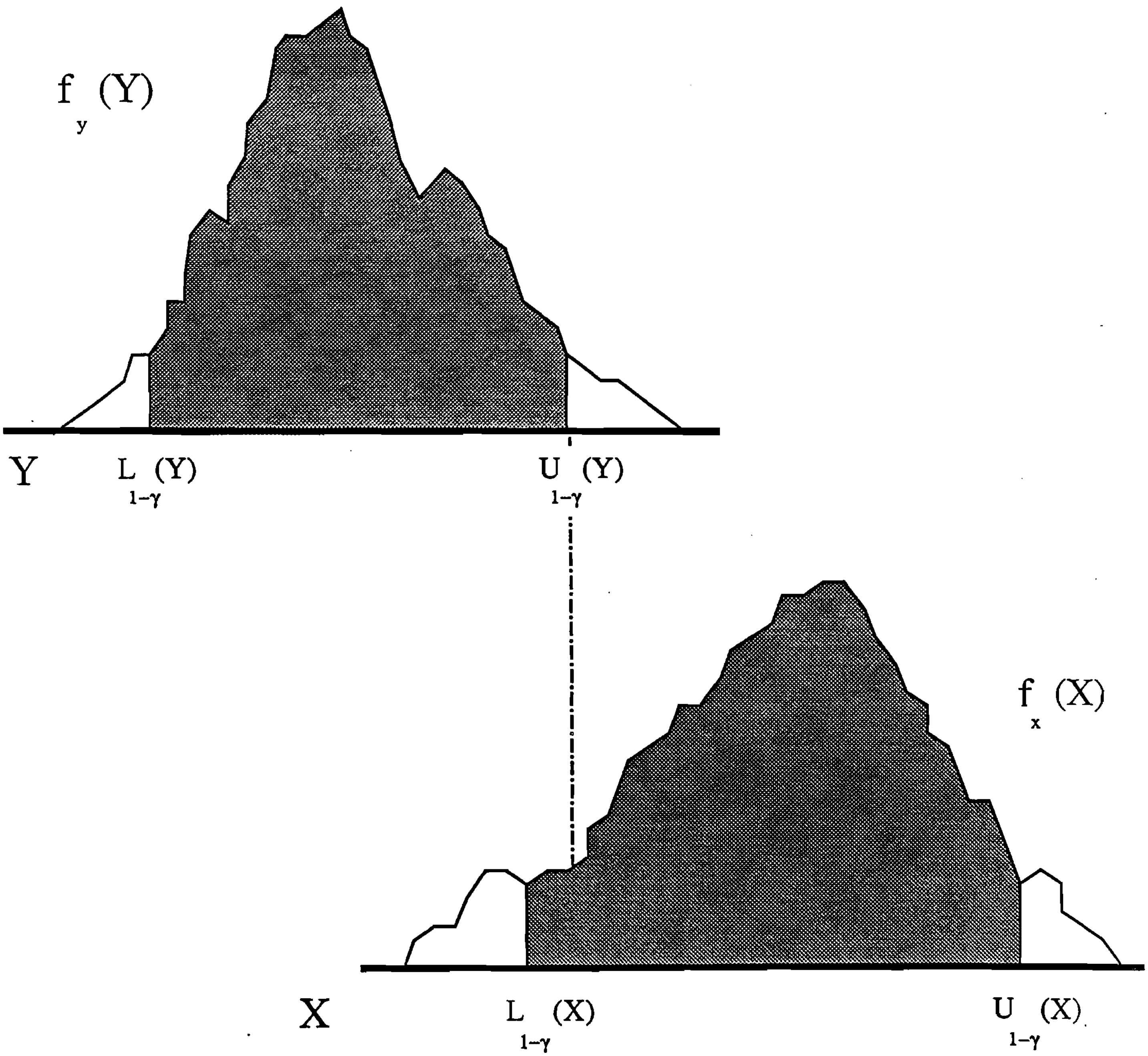
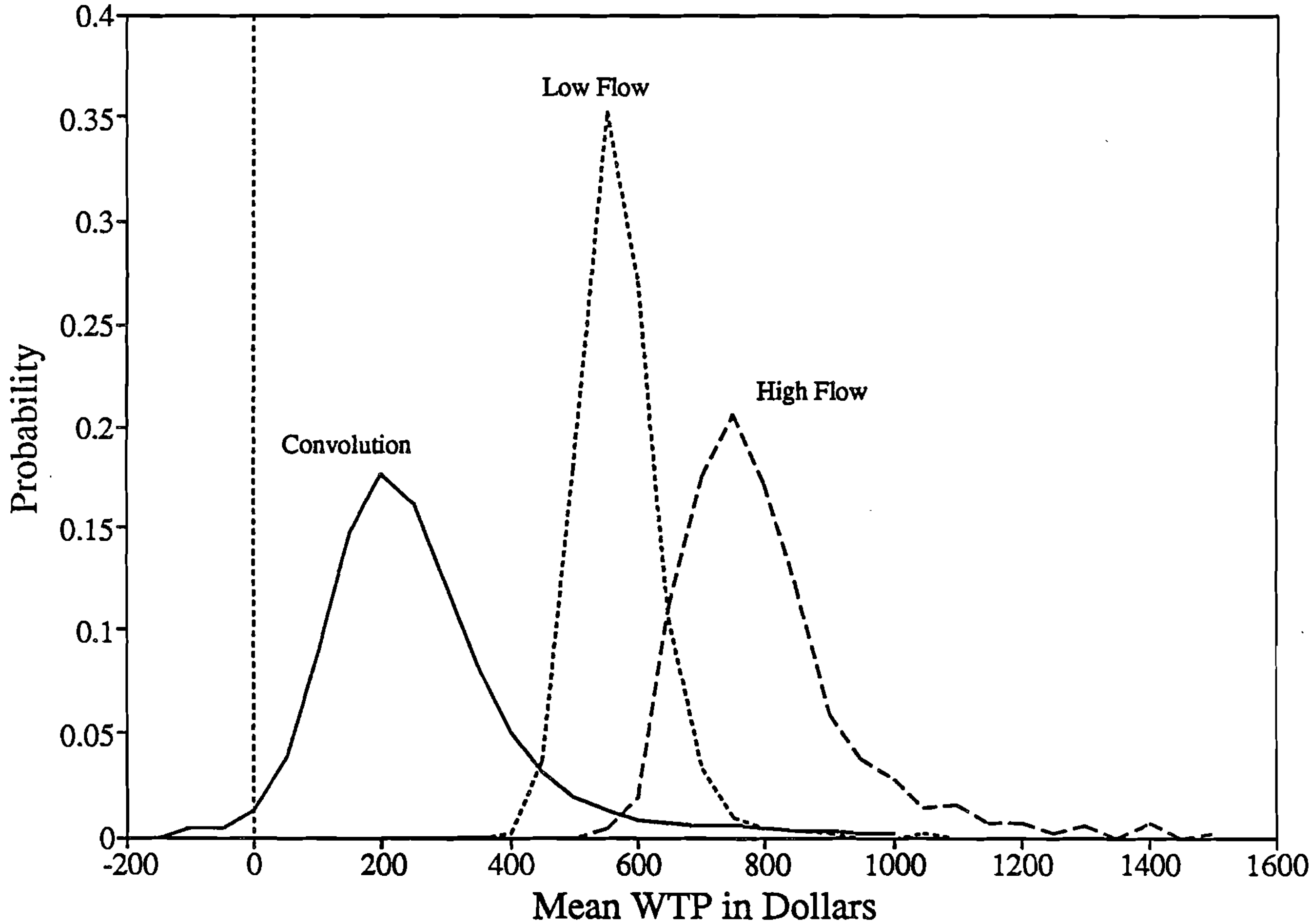


Figure 2: PDF and Convolution of Mean WTP for Low and High Flows



Intellectual Capital and the Transfer Process

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Intellectual Capital and the Transfer Process

The argument we want to make seems to be particularly appropriate on the occasion of the first meeting of rechartered W-133, retitled as Benefits and Costs Transfer in Natural Resource Planning (emphasis added). This paper contains a review and evaluation of procedures to achieve Objective 2 of rechartered W-133: To Develop Protocols for Transferring Value Estimates to Unstudied Areas. The expectation is that developing an understanding of the important variables explaining observed difference in values can provide an empirical basis for adjusting values to make the accumulation of nonmarket valuation studies useful to future policy decisions. The problem is to discover ways to integrate or synthesize the findings of past research with those produced currently and in the future. We intend to show that economic theory provides a model that possibly could lead to improvement in the way we talk about and think about the endowment of past studies in relation to current research.

Capital Theory

It may be possible to treat past nonmarket valuation studies as a potentially useful "stock," a kind of intellectual capital. Then some of the general principles and framework could apply as to other forms of capital. The findings of nonmarket valuation studies may "depreciate" through time and the stock may be "invested in" by new research activities. Of particular concern to value transfer is technological change because the history of nonmarket valuation research has been dominated by rapid technological innovation in theory, econometrics, and research procedures. Solow (1992) recently suggested that technology "could be accommodated in the theoretical picture by imagining that there is a stock of technological knowledge that is built up by scientific...research and depreciates through obsolescence." Technological innovation in theory and research method is expected to improve reliability which has the side effect (external cost) of speeding up "depreciation" by increasing the obsolescence of studies using less advanced theory and procedures. The rate of obsolescence can be reduced by (1) meta-analysis to discover improved bases for adjusting transfer values; and by (2) increasing the rate of investment in new dual research projects that are designed (a) to fill the gaps in knowledge of the value of unique environmental resources, and (b) to assess the validity of previous studies by replication and by experiments to test the effect of changes in conditions including technological innovation.

Capital theory may help us respond to the recharter issue when USDA administrators urged W-133 members to reform their independent ways and conduct truly cooperative research. Why should society invest in W-133 member research if the stock of intellectual capital on the value of environmental resources is conceived as decreasing with new investment, because of the high rate of obsolescence?[1] The benefit and cost transfer problem, in the long run, may be viewed as a concern for sustainability. Perhaps we need to re-evaluate past and current research to see if, in fact, each generation of nonmarket valuation economists leaves behind it a larger stock of intellectual capital than it inherited. Perhaps for each generation, to increase the sustainable level of knowledge can only be done at the expense of some of its own independence. Obviously, that is what most past generations of agricultural and resource economists have done (Marion Clawson, John Krutilla, Bill Brown, etc.).

It is our experience in the past that W-133 often functioned as a community of independent scientists from each of the states represented. The rechartered W-133 project statement appears to be an attempt to shift the balance somewhat toward more emphasis on community and less on independence. We choose the phrase "shift the balance somewhat toward community" carefully, because we do not believe the intent is to reduce independence to zero. Obviously, the ongoing competition between independent researchers for RFP contract awards and publication leads to innovation in theory and research methods and technological progress in the science of nonmarket valuation. However, it is significant that for a quarter century, USDA administrators have strongly recommended every five years that W-133 scientists learn to function more as a community of cooperative researchers contributing to the stock of policy-relevant information.

Perhaps rechartered W-133 provides an opening or opportunity to begin redirecting what appears to be a sharp rise in competitive rivalry among nonmarket valuation researchers in the past decade. The RFP system provides an incentive to criticize the research program at all but one experiment station in order to justify awarding a competitive contract to the chosen station. Far too often, when past research has been reviewed, there has been an attempt to find some "fatal flaw" that can be used to discredit nonmarket valuation work as committing an irredeemable mistake in theory or method, often without sufficient empirical evidence to support the criticism (Cummings and Harrison, 1992; Cambridge Economics, 1992; Freeman, 1993). We believe that this one-liner or "gotcha" approach is extreme and will eventually be shown to be basically dishonest, unfair, and wasteful of research dollars.

The fact is that all papers and studies have limitations imbedded in the models, econometric methods and other relevant conditions of the particular case. Most of the W-133 work we have read appears to contain mostly

[1] In the worse case scenario, without transfer research, the rate of obsolescence of past studies may become so high (approaching 100 percent) that the only reliable source of knowledge is the flow of new research. There is no useful stock to draw upon, as alleged, for example, in the case of acid rain (Brown and Calloway 1990).

positive and reasonably reliable contributions but with some negatives. The transfer approach emphasizes the positive contributions of past studies and adjusts for negatives where possible. Also, the proper language of the transfer approach should usually be nonjudgmental. Necessary limitations of all studies become omissions rather than failures. Omissions are usually not sufficient grounds for obsolescence of the entire study ignoring its mostly positive contributions. This is perhaps the most difficult aspect of the transfer approach to adopt when lawyers increasingly recommend court litigation.

In an adversary proceedings, the transfer approach is equivalent to the expert witness and attorney on one side arguing for the best features of the evidence for the case on the other side and to support it just as strongly as their own. This is not likely to occur since consulting and expert witness time is paid to support only one side of an adversary proceedings. This has several potential effects. First, a disincentive to reallocate economists' time to the value transfer problem is the high opportunity cost of foregone consulting and expert witness time paid to support one side of an adversary proceedings. Second, one requirement of work as an expert witness is that past professional studies and papers cannot be contrary to the special interest of the prospective client. This means that as more of us rely on such part-time employment, special interests may come to have more influence on the content of professional studies and journal papers. Also there is an incentive for comments in peer reviews to depend, in part, on the special interest served by the reviewer.

Damage assessment of rare and disruptive events may come to dominate the agenda for contingent valuation, particularly in estimation of passive use value. Recent history of nonmarket valuation appears to have been dominated by a single violent event, the Alaska oil spill. The $3 million cost of the Alaska state CVM study appears to be more than justified by the reported $1.2 billion settlement, and its subsequent effect on damage assessment in future oil spill cases. But we should not confuse the intensity of the few history setting violent events with the frequency of many ordinary events that have much more effect on total human welfare (full income is estimated as 2-3 times regular income). We believe that the center of nonmarket valuation by W-133 should be rooted in the many onsite and offsite recreation uses and services of environmental resources that define the daily life of people in ordinary situations most of the time.

Our own experience in adversary proceedings has been contrary to a more scientific or balanced approach by a community of cooperative scholars. Still, it is necessary that economists participate as expert witnesses in court cases to provide the necessary bases for efficient resolution of disputes. Perhaps it is unrealistic, but the science of economics might better serve the law as "friend of the court" to provide original research or benefit transfer evaluations for the judge rather than for one side of the case. Another alternative recommended by the NOAA panel (Arrow, 1993) is for economic consultants on both sides of an oil spill case to enter into an agreement on a single CVM survey instrument. The NOAA panel also indicated that economists for both sides might negotiate pretrial agreements as to suitable adjustments to facilitate the transfer of values from reference studies to each new case.

The point is that if we approached the applications to policy of past and current research in a more positive and accurate way, we might be better able to contribute to rational policy in natural resource planning. Randall (1992) argues that when studying complex phenomena such as nonmarket valuation of environmental resources, appropriate reliability tests are multi-faceted rather than simple (as a litmus test for acidity). "Modern philosophy of science" he says, "has moved away from the claim that there are simple and clear-cut tests for valid scientific methods and warrantable scientific knowledge. Current thinking tends more toward tentative judgments based on a preponderance of the evidence. ...there remains scope for differences of opinion among experts as to exactly what is the best way to do some of the tasks involved. This provides the opportunity for...experiments to map the responsiveness of...values to changes in valuation conditions. The results of such tests, while seldom individually conclusive, do contribute to the larger preponderance of the evidence test."

The Transfer Process of Adjusting Values

The value transfer process hinges on the reallocation of resources to developing an empirical basis for adjusting values from original studies to different conditions at other times and places. But individual professional incentives have traditionally favored original research over literature review necessary to adjust values in the transfer process. Funding agencies have favored original research in the allocation of scarce contract and grant dollars. In graduate student supervision, a literature review has been acceptable for the M.S. degree but original research is usually required for a Ph.D. The tradition is also evident in the private sector. Through 1990, U.S. industry spent 70 percent of its research budget on new products while 30 percent went to the process of information transfer. In Japan, the numbers are reversed. The Japanese view, to a large extent, has been to let someone else innovate the basic product technology. The real key to their success has been information transfer. Similarly, the adjustment and transfer of nonmarket values should be an accepted, even encouraged, practice to general economists seeking to increase the stock of policy-relevant models and research.

To begin planning a program of research on the sensitivity of TCM and CVM value to changes in model, method, and other relevant conditions, it is important to explore the state of the art. What do we know from existing literature about adjusting values for transfer? The potentially useful literature on recreation and environmental quality includes (1) two-way tests of the responsiveness of value to change in conditions; (2) meta analysis of pooled data from past studies to statistically estimate the effect on average value of a range of conditions; (3) nearly all original nonmarket value studies contain regressions that test the effect of a few potentially important variables, and for some notable studies, this represents a major objective (Carson, et al. 1992); (4) The recreation unit-day value approach of the water agencies reflects common sense of the panel of experts who developed it, even though they provide no empirical tests of significance. It is interesting that Randall suggested in a paper, some time past, that the unit value approach might someday be routinely applied to existence values as well as use values.

A number of useful two-way tests of significant difference between two treatments have been conducted as part of original research designed as dual purpose studies to have a direct use in policy application at the study site and an indirect use in adjusting results of past studies to answer policy questions at other times and places. The outline of a program of necessary experiments to map the responsiveness of nonmarket value to change in conditions is evident in recent work, particularly by many members of W-133 (Ward and Loomis, 1986; we apologize for not citing many other authors). These studies, while not conclusive, have begun to test the significant difference between values reported when conditions change: site quality, substitution possibility, econometric model, treatment of monetary and time cost, level of information, open-ended compared to dichotomous choice,[2] TCM compared to CVM estimates, single- compared to multiple-site, onsite use and passive use values, etc.

Preliminary trials indicate that meta-analysis may provide another tentative basis for adjusting past studies. Most notably, Smith and Kaoru (1990) pooled 734 benefit estimates from 77 TCM studies (1967 to 1986) in statistical analysis of variables that explain difference in reported benefits averaging $73 per day or trip. The authors made an important contribution to understanding the effects of alternative methods of estimating travel time cost, presence of a substitute price term, use of a regional model, type of site studied, statistical models (linear, log-linear, or semilog), and estimators (ordinary least squares, generalized least squares, or maximum likelihood-tobit) used in TCM studies. They conclude that these methodological variations significantly affect benefit estimates. The question remains whether these variables would have as much effect in a regression model holding constant the effects of other important variables including recreation activity, time on site, quality of the site, location of the site, variable cost per mile, travel time cost per hour, income and other socioeconomic variables, or sample size and

[2]The Exxon study of wilderness areas in the northern Rocky Mountains contains useful information for benefit transfer (Cambridge Economics, Inc., 1992). Their phone survey results of $79 (± 26) total value for 13 million acres can be compared to the value per million acre of open-ended and dichotomous choice mail survey and iterative bid personal interview results of previous research ($77 for 10 million acres in one case, Walsh, 1986).

coverage. Also, use of limited dependent variable techniques, such as poisson regression, and modeling for disequilibrium labor markets in the estimation of opportunity time cost may be more important in the future. More recently, Smith and Huang (1991) conducted a meta-analysis of 37 hedonic studies of how air quality affects property values.

Smith (1993) also reports that differences in assumption about the geographic extent of the market largely explained the difference in plaintiff and defendant estimates of damages as $15 million vs. $140,000 from contamination of a five-mile stretch of a river by mine wastes. Both side's CVM surveys were very close ranging from $5 to $8 per household. He concludes that we have little basis for estimating how damages to a resource will affect aggregate levels of use.

In a related meta analysis of TCM and CVM user studies, we (Walsh, et al. 1992) reviewed 120 studies from 1968 to 1988 with 287 benefit estimates averaging $34 per recreation day. We evaluated the omission of travel time; use of individual observation, zonal or hedonic price approach; instate sample coverage or including out-of-state users; recreation activity, whether specialized or general; household survey or site-specific study; site administration, public or private; site resource quality, low, ordinary or high; method, CVM, TCM or other; inflationary adjustment effect; open-ended, iterative, or dichotomous choice question; and site location, and FS region of U.S. Potentially important variables we omitted include: starting point; payment vehicle; direct travel cost per mile; travel time cost per hour; income and other socioeconomic variables; sample size, coverage and randomness; statistical model and estimator used; time onsite; specific site quality variables; and possible interaction between some variables.

One study of onsite recreation use values relied on the judgment of a panel of experts to adjust values for benefit transfer. Detailed descriptions and evaluations of the design aspects of nonmarket value studies completed from 1968 to 1982 were prepared by Sorg and Loomis (1984). The authors increased the reported TCM values by 30% for the omission of travel time; both TCM and CVM values were increased by 15% for omission of out-of-state users; and TCM values were decreased 15% for application of the individual observation approach. Although there is unanimity in the need to include travel time, the exact size of the correction of benefit estimates when travel time is not included is difficult to determine. Similarly, omission of out-of-state users tends to understate the number of visits to most resource-based sites at relatively higher travel costs. The individual observation approach uses annual trips per participant as the dependent variable. Although this is statistically more efficient than the zonal approach, it omits the effect of travel costs on the probability of participating unless both are included in the model.

There is a need for a thorough review of all original empirical work, the roughly 100 TCM and 100 CVM studies, to identify and catalogue the effect of potentially important variables on value estimation. Sensitivity analysis has increased in recent studies. Most notably, the Alaska state oil spill study (Carson, et al. 1992) based on personal interviews with 1,043 U.S. households appears to have been designed primarily to test the effect of a large number of variables on a median one-time WTP of $31 to prevent an Exxon Valdez size oil spill for the next 10 years. In passive use[3] value studies, emphasis has noticeably shifted to asking a large number of questions about what the respondent and interviewer think about the survey, similar to a college course evaluation. Why did they answer yes, no, or don't know, to the WTP amount? Did they understand and believe the information presented? Were they bored, distracted, careful in their answers? Were the questions and information biased in any direction? The answers to similar questions were coded by category and their effect on WTP of $31 were estimated, as plus $7, minus $1, etc. This may be the kind of information that administrators, judges, and juries need to evaluate the reliability of CVM studies. It also may be useful in adjusting results for value transfer.

The NOAA Nobel Panel (Arrow, et al. 1993) on CVM passive use values recommended standard procedures for oil spill damage studies. For the most part, their conclusions appear to be similar to the procedures used by the Alaska state study (Carson, et al. 1992). See summary in Table 1. Of special interest here, they address the problem of adjusting values for transfer: (1) original studies should be compared to average values of other independent studies at different times, and (2) government should fund reference surveys of small to large oil spills to calibrate (or adjust) the results of other surveys that do not fully meet the standards recommended.

Finally, we should re-evaluate the unit day approach to benefit transfer recommended by federal guidelines (Water Resources Council, 1983) as providing acceptable economic estimates of the value of recreation opportunities and resources. The approach may be used if the cost of original TCM or CVM studies would exceed budget constraints and if the recreation site studied is relatively small, with fewer than 750,000 recreation days per year. The method relies on expert judgment to develop an approximation of the average willingness to pay for recreation use. The values selected are considered to be equivalent to consumer surplus, i.e., net of travel cost or price. The guidelines recommend a range in value, adjusted to fiscal year 1993, of about $8.90 to $26.10 per day of specialized recreation including wilderness use. General recreation values are much lower, $2.20 to $6.60 per day. Initially

[3]Carson, et al. (1992) state: "Passive use values encompass what economists refer to as option values, existence values, and other nonuse values." (p. 1 note).

based on a survey of entrance fees at private recreation areas in 1962, unit day values have been adjusted for changes in the consumer price index to the present.

The council guidelines recommend five criteria to rate particular sites: (1) quality of the recreation experience as affected by congestion; (2) availability of substitute areas (in hours of travel); (3) carrying capacity as determined by level of facility development; (4) accessibility as affected by road and parking conditions; (5) environmental quality, including forests, air, water, wildlife, pests, climate, adjacent areas, and aesthetics of the scenery. Individual sites are rated on a 100-point scale, in which recreation experience is assigned a weight of 30, availability of substitutes 18, carrying capacity 14, accessibility 18, and environmental quality 20 points. See Walsh (1986; 232). These attributes and their weights will be candidates for measurement and evaluation in value transfer applications. Also, the distribution of prospective users by travel distance from the study site should be estimated. With a nonlinear demand curve, average surplus per visitor will be affected if the distribution of distance traveled varies between sites (McKean and Revier, 1990).

Conclusions

This paper addressed the problem of information transfer, that is, the possiblity of adjusting past studies to estimate benefits for long-run policy analysis. The process involves developing an understanding of the important factors that explain the observed differences in benefit estimates. The new problem becomes to design dual purpose studies, with a direct use in policy application at the study sites and an indirect use to answer policy questions at other times and places.

Past studies can be treated as empirical intellectual capital that is "depreciated" by obsolescence and "invested in" by new research. New innovations in theory and research procedure speed up "depreciation" by increasing the obsolescence of less advanced studies. Objective 2 of W-133 seeks to slow down "depreciation" in capital stock by adjusting results of past studies.

All studies have limitations—in the model or theoretical framework, in the econometrics or statistics, and/or in the research procedures, the survey questions and data used in the analysis. Previously, some variations in these conditions were considered deficiencies and grounds for rejection of study results. Focusing on value transfer procedures represents a fundamental change in how we treat limitations in published research. Now when limitations are identified, the objective becomes to learn how to adjust for them. When we learn how to adjust previous studies for known limitations, perhaps the accumulated stock of empirical intellectual capital can better serve the public purpose in efficient environmental decisions.

There is a limit, of course, to how far W-133 should go in identifying conditions that affect nonmarket value estimates to adjust values for transfer (Stigler, 1961). Searching past studies is time consuming and costly. Analysts may expect to find a more acceptable basis for adjusting values to a new site if they review 100 studies than if they review only a few, but at some point the gain from additional canvassing would be too small to be worth the cost in time and other resources. The point is that it pays to spend additional time reviewing past studies when the benefit exceeds the cost. Also, the W-133 community has to be careful that the adjustment devices themselves do not become a vehicle for attempts by one side of adversary proceedings to adjust past nonmarket value estimates to zero. But rechartered W-133 should help to neutralize the very strong, and understandable, pressures to increase the obsolescence of the stock of intellectual capital in nonmarket valuation.

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Table 1. Some NOAA Panel Recommended CVM Procedures, 1993

Report sample size and sampling frame, overall nonresponse rate and its components (e.g., refusals) and nonresponse on all important questions.

Accurate description of the program or policy and base case.

Reminder of availability and price of undamaged substitute resources introduced directly prior to main valuation question.

Reminder that payment would reduce expenditures for private goods or other public goods.

Public referendum format, willingness to pay added tax.

A "Don't Know" or "No Answer" should be explicitly allowed.

Follow with an open-ended question: "Why did you vote yes, no, or don't know?" Answers coded to show how values change:

> It is (or is not) worth it
> Don't know
> The oil companies should pay
> Indifferent between a yes and a no vote
> Inability to decide without more time or information
> Preference for some other mechanism; do not believe scenario
> Bored by survey and anxious to end it; inability

Conservative survey design and statistical analysis that tends to underestimate WTP eliminating extreme responses.

Open-ended check on the degree of understanding and acceptance of information presented.

Test for interviewer effects (encouraging favorable response).

Test effect of photographs.

Allow adequate time lapse after accident before survey.

Average values across independent studies at different times.

Government fund reference surveys of small to large oil spills to reduce embedding and to calibrate surveys that do not fully meet these conditions.

Some Problems with Deriving Demand Curves from Measure-of-Use Variables in Referendum Contingent Valuation Models

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Some Problems with Deriving Demand Curves from Measure-of-Use Variables in Referendum Contingent Valuation Models

Introduction

The use of referendum-style, dichotomous choice (DC) questions is popular in recent contingent valuation (CV) studies of nonmarket goods and services. Econometric advances afford the opportunity to review and scrutinize earlier studies. There are some unresolved points in the current literature. One important issue centers around the inclusion of "measure-of-use" variables as regressors in explanatory models. The use of such quantity information in valuation functions facilitates the derivation of demand curves. However, some authors counsel against the use of such variables because of endogeneity concerns, and avoid their inclusion in model specifications. Others continue to discuss the derivation of demand curves, with the focus on econometric approaches for doing so. The result is a lack of clear guidance for variable selection in *de novo* research, and for assessing the validity of previously estimated models. The need to derive demand curves and other per-unit-of-use welfare measures is increasingly motivated by the objectives of benefit transfer.[1]

The contribution of this paper is to examine this issue in greater detail. Grogger's specification test provides a technique for addressing endogeneity questions on measure-of-use variables in DC-CV models. A typology is presented and used to distinguish among different types of CV models and how they incorporate such variables. Empirical results from applying the specification test provide initial confirmation for the proposed typology. The results offer direction for variable selection in designing original models. Juxtaposed against this technical issue is an emerging policy question -- the transferability of DC-CV models out of their original setting. The opportunity to conduct specification tests is unlikely to be available in a benefit transfer exercise. Thus, the typology may also serve as a screening tool in the emerging protocol for acceptable benefit transfers. Caution is urged in reconstructing demand curves from "off-the-shelf" DC-CV models.

Background on DC-CV

The dichotomous choice contingent valuation (DC-CV) approach for referendum data was introduced by Bishop and Heberlein, and is "emerging as the preferred methodology" (Duffield and Patterson) in many

[1]Smith (1992a, p. 1083) discusses the "need for marginal values" in the transfer exercises involved in environmental costing for agricultural programs. See also Morey (p. 2).

applied studies. In the DC-CV format, the individual is queried for a yes or no response to a specific payment level (bid). With sufficient variation in the payment levels across the sample, and information on the probability distribution of acceptance/rejection, it is possible to estimate willingness to pay (WTP), or willingness to accept (WTA) compensation via statistical inference. Hanemann provides a utility-difference motivation for interpreting DC-CV in a random utility maximization framework; he also identifies a tolerance distribution or threshold motivation approach. This important alternative interpretation for DC-CV is fully defined in the development of the censored regression models (Cameron and James; Cameron, 1988, 1991). McConnell regards these alternative interpretations to be the "dual" to each other in economic utility theory.

The censored regression approach allows direct estimation of a valuation function, which can be interpreted as an expenditure-difference function. This function may be obtained either through general optimization procedures, or by simple transformation of the probit or logit probability results (as estimated for the utility-difference model). This conditional statement of WTP (or WTA) as a function of (presumably) exogenous variables can be interpreted as a valid money measure of welfare change; it may be either the Hicksian compensating or equivalent measure, depending upon the question asked, and the implied property right. Directly obtaining the valuation function also facilitates determination of the marginal valuation function (Hicksian compensated demand).

The computational ease of the censored regression approach facilitates the review of previous DC-CV studies. As Cameron (1988, p. 378) states:

> The logistic censored regression procedure also allows us to go back and reinterpret the results generated by other researchers, since the derivation of this model brings out a more appropriate interpretation of the referendum data parameter estimates yielded by simple logit discrete choice models. It is easy to **recover the underlying demand functions with no more than just the fitted models** in published versions of these papers. [Bold emphasis added]

This opportunity is demonstrated in Cameron (1988) by a reinterpretation of the DC-CV results of Bishop and Heberlein, and Sellar et al.[2]

Recent research on the DC-CV format is quite comprehensive, focusing on functional form (Bowker and Stoll; Boyle), experimental design (Cameron and Huppert; Cooper; Cooper and Loomis; Duffield and Patterson), and the development of confidence intervals around welfare estimates (Cameron, 1991; Park et

[2] Bishop and Heberlein's CV model was for a single trip, where the relevant use variable is the length of stay. Sellar et al.'s application was for a season, where the relevant use variable is the number of trips.

al.). However, the question of recovering demand curves from fitted probability models raises an important issue with variable selection that is discussed in the next section. While the focus is on deriving demand curves from the antecedent valuation function, endogeneity bias in estimated coefficients also affects other derived welfare measures, such as the commonly used "consumer surplus per-unit-of-use" (e.g. WTP per trip, evaluated at the sample means).

Addressing the Endogeneity Problem

This section explores the issue of possible endogeneity in the specification of the logit or probit probability models. This issue was raised by McConnell and focused on the use of "quantity demanded" or measures-of-use variables as explanatory regressors.

While endogeneity is a general econometric concern, the following quote from McConnell (p. 30) addresses the issue in relation to DC-CV:

> Whether one deals with utility differences or cost differences, the arguments of the function ought to be exogenous to the consumer, not consumer choice variables. There are several compelling reasons why exogenous variables work better. The basic problem with including quantity demanded in the valuation function is the endogeneity. The quantity of a good changes when exogenous variables change, but a ceteris paribus change in quantity is contrary to the spirit of economics, unless the quantity is rationed. Since the quantity of the good is chosen optimally, its marginal value is zero.

In response to these concerns over endogeneity, some researchers have counseled against, or avoided including measure-of-use variables in DC-CV models (see Cameron, 1992, p. 305). For example, Park, Loomis and Creel in their discussion of both linear and logarithmic DC-CV models note:

> Both specifications examined here are consistent with McConnell's demonstration that endogenous variables such as the number of trips must be omitted from the valuation function.

This quote demonstrates a distinct shift in variable selection protocol; e.g., in an earlier study, using the same elk hunting data set, Loomis et al. previously estimated a set of DC-CV logit models that included measure-of-use variables.[3]

Is it unacceptable to include measure-of-use variables in the DC-CV variable selection process? The one argument is that because of potential endogeneity, you should not. Others might point to the available literature, where such variables are commonly seen, as support for either estimating or interpreting models

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[3] In this case, the variable of concern was total trips to the site, while the frame of reference for the valuation was a single trip.

which include such variables, or possibly transferring models out of their original context. A further counterargument is that these measure-of-use variables are often better interpreted as measures of "avidity", thus, leaving them out may induce omitted variable bias. In the logit probability model, the exclusion of relevant variables "biases the estimates of the remaining slope coefficients toward zero" (Cramer, p. 36). Clearly, there is a need for an empirical test, and a set of guidelines for using previously estimated results.

Consideration of model misspecification in the DC-CV format is only just beginning (Ozuna et al.). Grogger's specification test for exogeneity in the logit and probit models offers the opportunity to address this issue in the DC-CV context.

Grogger motivates his Hausman-like specification test by considering the problem in a nonlinear least squares framework. The test has the advantage of being computationally convenient and is robust to departures from normality. It can be applied to either probit or logit models estimated through standard maximum likelihood (ML) estimation procedures. Furthermore, the nonlinear instrumental variable (NLIV) estimator used in conducting the test is "consistent in the presence of endogenous regressors." The test statistic **h** is given by:

$$h=(\hat{\gamma}_{NLIV}-\hat{\gamma}_{ML})' \cdot [VC(\hat{\gamma}_{NLIV})-VC(\hat{\gamma}_{ML})]^{+} \cdot (\hat{\gamma}_{NLIV}-\hat{\gamma}_{ML}) \tag{1}$$

where:[4]

$$\begin{aligned} \hat{\gamma}_j &= \textit{coefficients on } 1\times G \textit{ vector of possibly endogenous variables} \\ VC(\hat{\gamma}_j) &= \textit{a } G\times G \textit{ block of the var-covariance matrix on } \gamma_j \\ [A]^{+} &= \textit{the Moore-Penrose inverse for any matrix } A \end{aligned} \tag{2}$$

Under the null hypothesis of exogeneity (no misspecification), **h** follows a chi-squared distribution with **G** degrees of freedom; the NLIV estimator is consistent under both the null and the alternative hypothesis (misspecification due to endogeneity of one or more independent variable). A significantly large chi-squared test statistic indicates the presence of endogeneity. This test helps to fill a gap in the DC-CV literature.

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[4] The Moore-Penrose inverse or generalized inverse of any matrix A, is another matrix A^{+} that satisfies the following: (i) $AA^{+}A=A$, (ii) $A^{+}AA^{+}=A^{+}$, (iii) $A^{+}A$ is symmetric, and (iv) AA^{+} is symmetric. These conditions held for all empirical applications discussed in this paper. In the special case of an overdetermined system of equations, the formula for finding the Moore-Penrose inverse is given by: $A^{+}=(A'A)^{-1}A'$ (Greene, p. 38).

Several applications of this test are given later, using a published DC-CV study on pheasant hunting (Adams et al.), which incorporated a measure-of-use variable. The authors viewed this variable as a measure of hunting "avidity" or intensity of preferences[5]; they made no attempt to derive the Hicksian demand curve.[6]

The Policy Context: Benefit Function Transfer

Benefit transfer refers to the transfer of some existing benefit estimate from its study setting to some alternative policy setting. Benefit transfer has been practiced on an ad hoc basis in legal and policy settings; the issue lies in developing acceptable protocol for doing so (Smith).[7] *Benefit function transfer* refers to the transfer of an existing benefit function rather than simply a point estimate or confidence interval for WTP (Loomis); it has been described as an "ideal" form of benefits transfer (Desvouges et al.). An estimated function provides a policy analyst with greater flexibility in calibrating a transferred value to a policy setting.

With growing interest in the topic of benefit transfer, and increasing use of the DC-CV format, an important question is whether one could use the estimated model from an existing study to derive a valuation function, and if desired, derive the demand curve? Further, should these demand functions and associated welfare measures per-unit-of-use be part of the accepted protocol for benefit transfer. Discussion of the use of DC-CV models in a benefits transfer context has begun (Downing and Ozuna.; Duffield et al.). It seems likely that the censored regression approach, with its emphasis on covariates and the ordinary regression analogy, will be a particularly attractive candidate for benefit function transfer.

Consider a simple example. A natural resources agency may be interested in transferring a valuation estimate for upland bird hunting from the study site to an alternative setting. Some proposed change would

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[5] Quantity/quality ambiguity over the interpretation of such variables dates to Davis (p. 396), who states: "The length of time one stays in the area appears to measure the quantity of the good consumed but also reflects a quality dimension, suggesting that longer stays probably reflect a greater degree of appreciation for the area."

[6] The desire to develop a demand curve from the Adams et al. results was motivated by practical considerations. The original survey was conducted in 1986. The stocking program was dropped in 1987, and an annual fee-access "put-and-take" hunt initiated in 1989. Thus, several years of actual price-quantity information offered a unique opportunity for a "ground truth" check on a CV survey. The full comparison required that a demand curve be derived from the DC-CV probability results, if possible.

[7] The topic of benefits transfer has received increasing professional attention. For further discussions see the compilation of articles in volume 28 of *Water Resources Research* (e.g. Brookshire and Neil).

negatively affect wildlife habitat which currently supports significant hunting activity. As is common, the agency may have projections on the changes in the number of trips and be interested in obtaining a per-trip measure of consumer surplus or more preferably, a demand curve. From the limited set of nonmarket valuation studies available, a particular study may be the most appealing alternative. It would be instructive to agency analysts to know if they could validly reconstruct per-unit welfare measures from the targeted study.

Deriving Demand Curves from the DC-CV Model

Our focus here is on the review and possible reinterpretation of the DC-CV results from a study (Adams et al.) in a situation similar to the above example. The objective is to explore the suggested derivation of the inverse Hicksian demand curve from the censored logistic regression approach to DC-CV data. In Adams et.al. [hereafter referred to as ABMJM], the welfare measure was the WTP to avoid the loss of a pheasant stocking program. Its format follows that of Sellar et. al. (1985,1986) [hereafter referred to as SSC], who valued the loss of access to a recreational boating site. Both studies:

(1) elicit the Hicksian equivalent surplus measure of welfare change, the WTP to avoid a loss,
(2) utilize the utility-difference (or random utility) model for DC-CV,
(3) utilize the log-linear specification of the logit probability function with the logarithm of the fee used as an explanatory variable, and a logarithmic transformation of the number of trips (hunting) or launches (boating) as an explanatory variable

Since its original publication the SSC study has been subjected to additional scrutiny and professional discussion, especially with reference to the development of the censored logistic regression interpretation of DC-CV (Cameron, 1988; McConnell; Patterson and Duffield). Given the general comparability of the format between the two studies, we extend this discussion to the ABMJM study.

The discussion centers around the "measure-of-use" or quantity of visits variable.[8] Following Cameron's notation (1988) we denote this measure-of-use as q. The two motivating questions are: Can we use this variable to construct a marginal valuation function or Hicksian demand curve? and; If this is a quantity demanded variable, is there potential endogeneity in including it in the logit probability function and the resultant valuation functions? The first question is addressed below.

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[8] Thus, our concern is with variable selection; important considerations with functional form (Hanemann; Boyle; Cameron, 1991) are not addressed. The so-called "log-logistic" has tended to provide the best empirical fit in DC-CV models, but cannot be derived from any valid utility function. It has been shown that it can be traced to first order approximation of such a function; it is commonly accepted in the threshold interpretation of DC-CV as the best statistical approximation. For further discussion see Cameron (1991).

A preliminary step is to establish the general correspondence of the SSC and the ABMJM studies. First, define the familiar "log-odds ratio" or logit index from the logistic probability model:

$$LO_i = f(X_i, q_i) \tag{3}$$

where LO_i is the logarithm of the odds ratio of the ith individual responding "yes" to the offered bid or fee, X_i, which is included as an explanatory variable and assumed to vary across the sample. The "crucial" additional explanatory variable is again q, the number of trips, and f(.) is the general functional form for the assumed utility-difference in the random utility framework. SSC utilize the "so-called" log-linear specification:

$$f(X_i, q_i) = \gamma_1 + \gamma_2 \cdot \log(q_i) + \alpha \cdot \log(X_i) \tag{4}$$

The results of two site-specific logit models from SSC are given below:

$$\begin{aligned} &\textit{Livingston}: \quad LO_i = 3.06 - 1.37\log(X_i) + 0.67\log(q_i) \\ &\textit{Somerville}: \quad LO_i = 4.78 - 1.26\log(X_i) + 1.75\log(q_i) \end{aligned} \tag{5}$$

From the study site in ABMJM:

$$\textit{E.E.Wilson}: \quad LO_i = 4.88 - 2.25\log(X_i) + 0.664\log(q_i) \tag{6}$$

All other explanatory variables in ABMJM have been evaluated at their sample means and collapsed into a "grand intercept" for conformity between studies. Hereafter, the subscript i will be dropped for simplicity of notation.

To obtain the Hicksian demand curve, SSC first estimate the logit probability model, and numerically integrate the cumulative distribution function of the assumed error on the utility-difference to obtain the expected value of the conditional willingness to pay, E(WTP|q). The SSC formula for the demand curve is given by:

$$\frac{\partial E(WTP|q)}{\partial q} = - \int_0^{X_{max}} \frac{f_q \exp[-f(X,q)]}{(1+\exp[-f(X,q)])^2} dX \tag{7}$$

The formula does not have a closed-form solution and is evaluated numerically. While not reproduced here, the formula for the slope of the demand curve is even more complex, and again must be evaluated numerically.

In noting SSC's failure to integrate the expression in (7) to infinity, Patterson and Duffield correct it to be:

$$\frac{\partial E(WTP|q)}{\partial q} = (1/q)\cdot [\partial E(WTP|q)/\partial \log(q)] = \frac{\gamma_2}{-\alpha}\cdot(1/q)\cdot E(WTP|q) \tag{8}$$

Equation (8) represents the expression for Hicksian demand from the utility-difference interpretation.

The censored logistic regression approach for deriving the Hicksian demand curves and the resultant price elasticities of demand, begins by transforming the original logit probability coefficients to obtain the underlying valuation function (without the fee variable after the reparameterization). Specifically, from (3):

$$\begin{aligned} \log WTP &= (\gamma_1/\alpha) + (\gamma_2/\alpha)\cdot\log(q) \\ &= \beta_1 + \beta_2\cdot\log(q) \end{aligned} \tag{9}$$

Where $(-1/\alpha) = K$ is the alternative dispersion parameter from the logit model; K is used in reparameterizing the original coefficients to obtain the underlying valuation function. With the exponential transformation, this function is expressed as:

$$WTP = e^{\beta_1 + \beta_2\cdot\log(q)} \tag{10}$$

To obtain the expected WTP requires multiplication of (10) by an exponentiation correction factor, C:

$$C = \frac{(\pi\cdot K)}{\sin(\pi\cdot K)} = \Gamma(1-K) \times \Gamma(1+K) \tag{11}$$

where Γ is the Gamma function (Duffield and Patterson).[9] Cameron (1988) reparameterizes four site-specific logit equations from SSC. Below are several of the resultant valuation functions:

$$\begin{aligned} &\textit{Livingston}: \log(WTP)=2.23+0.489\log(q) \\ &\textit{Somerville}: \log(WTP)=3.79+1.389\log(q) \end{aligned} \tag{12}$$

The equivalent function from ABMJM is:

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[9] The objective is to obtain E(WTP); however, it is usually lnWTP that has been estimated. By Jensen's Inequality, it can be generally stated that, E(f(x)) ≠ f(E(x), implying in this case, E(ln(WTP)) ≠ ln(E(WTP)). Thus we can not simply take the anti-log of E(ln(WTP)) to obtain E(WTP). Specifically, E(ln(WTP|q)) provides the conditional median, and is not equal to the conditional mean, given the logarithmic transformation. The relationship between the two can be determined through the moment generating function (in this case the for the log-logistic distribution). The full derivation of the correction factors in (equation 11) can be found in Johnson and Kotz (p. 4).

$$E.E.Wilson\text{:}\quad \log(WTP) = 2.17 + 0.294\log(q) \tag{13}$$

To derive the Hicksian demand curve in this censored logistic regression format note that:

$$\partial\, logWTP/\partial \log(q) = \beta_2 \tag{14}$$

Then it can be shown that:

$$\partial WTP/\partial q = \beta_2 \cdot [(\exp(\beta_1 + \beta_2 \log(q)) \,/\, q)] \tag{15}$$

which follows from the generalized exponential-function rule for taking derivatives (Chiang, p. 293) for any function f(t) of a random variable t:

$$\frac{d}{dt}e^{f(t)} = f_t \cdot e^{f(t)} \tag{16}$$

This application of the chain rule clarifies the correspondence between (8) and (15), and thus between the utility-difference and censored regression approaches (Patterson and Duffield). Specifically, the censored regression approach does not avoid the truncation point issue of the utility-difference approach; rather, it implicitly assumes the upper limit to be infinity (also see the discussion by Carson, p. 144).

If we identify marginal willingness to pay as the implicit price of a trip q, $\partial WTP/\partial q = p(q)$, then the presumed demand equation can be expressed as:

$$\begin{aligned} \log p(q) &= \log\beta_2 - \log(q) + \beta_1 + \beta_2 \cdot \log(q) \\ \log \mathrm{p(q)} &= (\beta_1 + \log \beta_2) + (\beta_2 - 1) \cdot \log(\mathrm{q}) \end{aligned} \tag{17}$$

Rearranging to isolate log(q) on the left-hand side:

$$\log(\mathrm{q}) = [(\beta_1 + \log(\beta_2))/(1-\beta_2)] - [1/(1-\beta_2)] \cdot \log \mathrm{p(q)} \tag{18}$$

Cameron (1988) presents the implied Hicksian demand functions for SSC in algebraic form for four separate locations. Two of which are shown below: one adheres to the theoretical notion of a downward-sloping demand curve and one does not:

$$\begin{aligned} &Livingston\text{:}\quad \log(q) = 2.96 - 1.96 \log p(q) \\ &Somerville\text{:}\quad \log(q) = -10.96 + 2.57 \log p(q) \end{aligned} \tag{19}$$

Similarly, the inverse Hicksian demand for the ABMJM model is:

$$\textit{E.E. Wilson:} \quad \log(q) = 1.35 - 1.42 \cdot \log p(q) \tag{20}$$

The slope coefficient in (20) can be interpreted as the price elasticity of demand, $\partial log(q)/\partial logp(q) = -1.42$. The estimated Hicksian demand curve for ABMJM is thus downward-sloping and relatively price elastic.

The first concern is potential fragility in this result. One issue is that the absence or presence of the exponentiation correction factor (11) may influence the result. Cameron (1988, 1991) asserts that it will not affect the elasticity. This conclusion can be verified more explicitly as follows:

$$E(WTP) = e^{\beta_1 + \beta_2 \cdot \log(q)} \cdot C \tag{21}$$

From a minimal extension of (16):

$$\frac{d}{dt} e^{f(t)} \cdot C = C f_t \cdot e^{f(t)} \tag{22}$$

where the estimated C is taken as a multiplicative constant, and which accordingly changes (17) to:

$$\log p(q) = \log\beta_2 - \log(q) + \beta_1 + \beta_2 \log(q) + logC \tag{23}$$

and modifies (18) to:

$$\log(q) = [(\beta_1 + \log(\beta_2) + \log(C))/(1-\beta_2)] - [1/(1-\beta_2)] \cdot \log p(q) \tag{24}$$

Thus, while the intercept of the Hicksian demand function is impacted by the exponentiation correction factor in the so-called log-linear model, the slope coefficient (elasticity) is not. With an estimated K=0.44 and C=1.416, we can apply this result to the ABMJM study; the revised Hicksian demand function becomes:

$$\textit{E.E.Wilson:} \quad \log(q) = 1.84 - 1.42 \cdot \log p(q) \tag{25}$$

The final result appears to be an appealing looking downward-sloping demand curve. Following Cameron (1988, p. 363) this can be described as a "per unit" demand curve. Such per-unit demand curves are discussed in the applied DC-CV research (Duffield and Allen; Loomis et al.).

In summary, this section traced out the mechanics of deriving a "per-unit" demand curve from the DC-CV format. The correspondence between the utility-difference and the censored regression approaches was discussed. The computational convenience of the latter, which can easily be applied to any fitted logit (or probit) model, may facilitate the recovery of demand curves from historical models (e.g., ABMJM). However, because of potential endogeneity concerns, interpretation of such demand curves is required.

Interpreting the Result

Putting the endogeneity question aside for the moment, are there other concerns with the above approach to deriving demand curves? First, despite its appearance, one interpretation of the ABMJM result is that you really don't have a demand function; the hypothetical valuation exercise is lumpy, it was intended to elicit the value of the hypothetical change for the entire period. Second, it might also be argued that the number of trips is a measure of avidity or the intensity of preferences; the marginal value is inversely related to the intensity of historical preferences for pheasant hunting at the site. But does this mean that we can interpret this marginal relationship as a Hicksian demand curve where hypothetical price and quantity combinations are identified? The quantity of trips in ABMJM or boat launches in SSC were not goods sold in the hypothetical market; they were measures of use under a previous set of circumstances. They reflect choices where no payment was required, hypothetical or otherwise. These measure-of-use variables are historical, or what Prince and Ahmed alternatively refer to as **experience-specific.**

Deriving Marginal Valuation Functions

The approach taken in SSC and the reinterpretation by Cameron (1988) and others (Duffield and Patterson) is different from that of eliciting WTP (or WTA) for successive increments or decrements in a hypothetical quantity or quality of environmental services, and then deriving a marginal valuation function. As introduced into the CV literature by Randall et al., the Bradford bid curve is obtained for a set of increments or decrements in a quality or quantity variable. The theoretical Bradford bid curve approach was laid out in detail by Brookshire et al.

For the Bradford bid/valuation function approach, marginal valuation functions can be obtained from contingent markets, provided that a set of hypothetical increments or decrements are presented for valuation in that market. Consider the following equations taken from Brookshire et al. in their iterative bidding CV study of elk hunting near Laramie, Wyoming:

$$\begin{aligned} a{:}\,WTP &= 59.701 + 8.705 \cdot ENC - 0.284 \cdot ENC^2 \\ b{:}\,\ln WTP &= 4.362 + 0.142 \cdot \ln ENC \end{aligned} \qquad \textbf{(26)}$$

The variable ENC is defined as the frequency of elk encounters. The key point is that ENC represents increments (0.1, 1, 5, or 10) that are exogenously provided by the researcher in the hypothetical market.

Taking the derivative of WTP of the quadratic equation (a) in (26) with respect to ENC gives, ***p(ENC)*** = ∂***WTP/***∂***ENC = 8.705 - 0.568 ENC;*** which can be manipulated to provide, ***ENC = 15.325 - 1.76 p(ENC).*** For the logarithmic specification (b) in (26) we follow the procedure outlined in equations (16-18) to obtain the marginal valuation function for the logarithmic model, ***log(ENC) = 2.811 - 1.17 log p(ENC).***

The record for using the Bradford bid curve approach versus experience-specific variables to obtain marginal valuations of quality changes is mixed. In an early study on recreation congestion, Cichetti and Smith utilized hypothetical combinations of congestion and levels of use as explanatory variables in their WTP function. As reviewed by Prince and Ahmed, subsequent recreation congestion studies tend to rely instead on experience-specific variables. Several early CV studies on waterfowl hunting (e.g. Hammack and Brown) derive marginal values from valuation functions using experience-specific variables. In a more recent CV study on wetlands protection and waterfowl hunting a Bradford bid curve approach is adopted (Bergstrom et al.).

Thus, both experience-specific variables and hypothetical increments or decrements are used in the derivation of marginal values or marginal valuation functions for quality changes. The interest here lies in extending this concept of obtaining a marginal function onto measure-of-use variables. Marginal valuation functions (with respect to changes in use) could then be interpreted as demand curves.

While the valuation of a set of increments and decrements has been focused on quality variables, it is possible to theoretically construct demand curves for measure-of-use variables, provided that such information is collected in contingent behavior questions. The contingent behavior responses must be elicited in congruence with the elicited valuations for the set of increments or decrements of quality changes. However, valuation functions should not use this contingent behavior information directly as an explanatory variable; it was elicited as an endogenous response to the hypothetical market (McConnell). And therein lies the rub. We would like to be able include measure-of-use variables in our valuation functions and then take a derivative to obtain a demand curve. But measures of hypothetical use (contingent behavior) introduce endogeneity, and measures of experience-specific use were not chosen in the context of the hypothetical market.

A Twist to the Discussion

Measure-of-use variables can also enter into valuation functions in more disguised forms, such as part of a combined variable. One example can be found in the DC-CV study of wetlands protection by Bergstrom et al.. In their logit function they utilize a set of total annual harvest variables as explanatory variables. As one example, **TWFBAG** is a constructed variable representing the annual number of waterfowl bagged:

$$TFWBAG = q \cdot bag \cdot p_j \tag{27}$$

where: q = annual waterfowl hunting days (historical); bag = average bag per day (historical); p_j = a multiplicative factor for percentage of maintained catch levels for the jth scenario.

The survey presented three scenarios **(j=1,2,3)** to each individual. Yes/no responses were elicited to a DC valuation question for annual site access given current catch levels **(p_1=1.0)**. Additionally, binary responses were obtained for two decrements in quality, a 50 percent decrease **(p_2=0.5)**, and a 75 percent decrease **(p_3=0.25)** in current catch levels. These three valuation responses were then stacked in the data set according to the appropriate **p_j**.

The TWFBAG variable is used as an explanatory variable in the logit function. Bergstrom et al (p. 138) argue that this probability function:

> ...can be used to derive a bid function for wetlands-based recreation. This bid function can then be used to derive a demand function for wetlands-based recreation (Sellar, et al., 1986).

In another presentation of their research (Stoll et al.), they develop such a quality demand curve.

This formulation is more appealing in that it incorporates the Bradford bid curve approach in using hypothetical decrements in environmental services. However, q is an experience-specific variable; and there is no guarantee that such included historical information would remain constant over the set of hypothesized increments or decrements in environmental services. In other words, the model does not predict the level of use decision in response to the hypothesized changes in environmental services. Whenever use is revised significantly, a bias may be introduced into any estimated demand functions or welfare measures that do not explicitly model this change.

In conclusion, there is room for improved discussion of acceptable principles and procedures for deriving marginal benefit (demand) curves from CV valuation functions. To this end the following section presents a typology of CV models which incorporate measure-of-use variables.

A Typology of CV Models with Measure-of-Use Variables

The following typology is proposed as an aid in sorting out the several types of contingent valuation surveys with respect to measure-of-use variables. The typology will be used to generate several hypotheses which will later be tested empirically using the original data from ABMJM.

In the first type, the constructed market is a counterfactual market. The ith individual might be asked the question: "Think back on your visits, assuming nothing was changed, would you have been willing to pay X_i dollars for access to the site last season?" The only thing that changes in the counterfactual is the presence of the market itself. Such a question would typically be preceded by a measure-of-use question. The collected information on the measure-of-use variable is experience-specific, but is assumed to be incorporated into the statement of the counterfactual market. It is part of the contingent scenario. The quantity of use was user chosen; however it will be predetermined endogenous or exogenous to the actual contingent valuation choice(s). The maintained hypothesis in such approaches is that this level of use is fixed.

The good example of the type I format can be found in the open-ended valuation of Hammack and Brown.[10] The SSC study is also of this type. Other DC-CV examples include Loomis et al., and Duffield et al.. Given the maintained hypothesis of a fixed level of use, it may be acceptable to derive a demand curve.

In the remaining three types, the constructed market is referred to as hypothetical to emphasize that it is forward looking. An individual might be asked the question: "If a seasonal pass were to be sold for access to the site would you be willing to pay X_i dollar for such a pass?" What distinguishes these three types is the description and status of q.

In type II the experience-specific or historical quantity variable is used. This is a common approach; examples include ABMJM and Boyle.[11] Deriving demand curves may introduce considerable bias if the level of use is revised significantly in response to the contingent scenario.

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[10] Hammack and Brown (p. 23) use the following open-ended question to elicit WTP: "Suppose that your waterfowl hunting costs for 1968-69 season were greater than you estimated in Question 7. Assume these increased costs in no way affected general hunting conditions. ABOUT HOW MUCH GREATER DO YOU THINK YOUR COSTS WOULD HAVE HAD TO HAVE BEEN BEFORE YOU WOULD HAVE DECIDED NOT TO HAVE GONE HUNTING AT ALL DURING THAT SEASON?" They estimated valuation functions that included the historical level of use as an explanatory variable. Derived marginal valuations for quality changes were conditional on the assumption that there would be no changes in the level of use in response to the quality change.

[11] Cameron and Huppert use the following question to elicit WTP: " What is the MOST you would be willing to pay each year to support hatcheries and habitat restoration that would result in a doubling of current salmon and striped bass catch rates in the San Francisco Bay and Ocean area if without these efforts your expected catch in this area would remain at current levels?" The question was originally structured as a payment card, but then also used in constructing DC-CV models through simulation. Their valuation functions include a TRIPS variable which represents the number of salmon and striped bass fishing trips in the past 12 months; this experience-specific measure-of-use is used in deriving marginal valuations.

In type III, a contingent behavior variable is used. In this case we are soliciting the expected level of use in the contingent market, simultaneously chosen with the valuation response. While this type of information is often collected, it is not commonly used in estimating the valuation function. Since it is user chosen in response to the contingent scenario, we expect that it will be an endogenous variable. Incorporating it into the model requires some sort of joint estimation process.

Finally, we consider the *plausibility* of a fourth type. An individual might be asked as part of either a unidimensional or multidimensional contingent scenario to value a set of increments of decrements in the level of use. We expect this measure-of-use variable to be exogenous to the model, and that a Hicksian demand curve (as a function of q) could be constructed provided sufficient variation in q. Type IV adheres to the traditional notion of the Bradford bid curve approach, as typically applied for increments or decrements in a quality variable.

In summary, four separate types of CV models have been identified. In doing so a potential distinction has been drawn between the SSC study and the ABMJM study. Both appeared to provide a direct avenue for deriving Hicksian demand curves, this is now called into question by the typology.

Empirical Evidence

In this section we put our typology to work. We would expect that the measure-of-use variable in ABMJM should show no statistical evidence of endogeneity since it follows the type II format. However, if any contingent behavior information is introduced into the DC-CV estimation model, we expect that endogeneity will be an econometric concern. To test these hypotheses we apply Grogger's "simple test for exogeneity."

Testing for Endogeneity on an Experience-Specific Variable

The first task is to test for endogeneity on the measure-of-use variable, (number of pheasant hunting trips taken) in the original ABMJM model. Conducting this test requires a prediction for this variable from a set of instruments. This was done with a log-linear OLS model whose explanatory variables included the set of exogenous regressors from the original logit model, plus additional variables collected in the survey. (The results are presented in Appendix A.) The predictions from this OLS regression are then utilized in the specification test. The basic inputs for the test are shown in Table 2, which provides logit estimation results for both the original model[12] and with the inclusion of predicted trips as an explanatory variable.

The chi-squared test statistic is **h=0.069**; thus, there is no statistical evidence ($p<0.001$) to support the hypothesis that q is an endogenous variable.[13] [14] McConnell's legitimate concerns with endogeneity cannot be given blanket application to the inclusion of measure-of-use variables.

Testing For Endogeneity on a Contingent Behavior Variable

Although unreported in the original study, the ABMJM survey also collected a contingent behavior response. After the dichotomous choice valuation question, respondents were asked the following Yes/No question: "If the stocking program were to be eliminated, would you stop hunting pheasants in western Oregon?" The dummy variable CB is used as an indicator of this contingent behavior; where a 1 indicates that all pheasant hunting trips would be eliminated, and a 0 indicates otherwise (either fewer or same number trips).

Performing the specification test requires that we estimate a model that can predict the CB response, the decision to revise the level of use. In this case probability of a yes or no response is modeled with a logit function. (Estimation results for predicting CB are given in Appendix A.)

Table 3 provides the estimation results. Model One adds the CB dummy variable as a regressor on the probability of accepting the offered fee. Model Two includes the predicted CB as a dummy variable,

[12] The signs on the estimated coefficients are reversed from ABMJM to reflect modeling the probability of a yes response, rather than a no, to the offered fee level. In the threshold interpretation context, it is more appealing to model the probability that WTP>Fee as a yes response. The estimated coefficient on LNFEE of -2.25 corrects a typographical error. This particular error is of some import; given $\alpha < 1$, implying $K > 1$, the mean value would be undefined for the log-logistic model (Duffield and Patterson).

[13] As a check on the robustness of this conclusion, a Tobit model also was used to predict LNVIS, and thus account for possible censoring bias (trips ≥ 1, implying LNVIS ≥ 0). Substantively equivalent results were obtained; the estimated regression coefficients were quite close between the two models (OLS and Tobit), and the same conclusion obtained for the specification test. Estimation results for the Tobit model are available in an extended version of this paper.

[14] The LNVIS variable merits additional discussion. As is common with intercept (on-site) surveys, LNVIS is a constructed variable which combines actual trips taken prior to the survey plus expected additional trips for the remainder of the season. The inclusion of this expectation heightens initial concern with potential endogeneity on the measure-of-use variable. However, the expectation was not elicited in reference to any hypothetical market scenario, and as noted, there is no evidence to support endogeneity

where the predictions were converted into a 0,1 value. Model Three utilizes the predicted probabilities directly, with a suggested weighting correction for possible heteroskedasticity (Grogger). [15]

In Model One, the CB variable has no appreciable effect on the coefficients or goodness-of-fit statistics from the original ABMJM model. However, including the predicted probabilities (in either form) from the NLIV estimator changes model results. Notably, the income effects, which drove the ABMJM policy conclusions, are muted.

As expected, the evidence from the specification test supports the conclusion that CB is an endogenous variable. The chi-squared test statistic is **h=6.36** using the Model Two predicted CB coefficient as the NLIV estimator; The chi-squared test statistic is **h=15.686** using Model Three. The results are significant at the 0.025 and 0.001 levels, respectively.

Potential Policy Implications

Given the available evidence for endogeneity, Models Two and Three are of interest, in that they provide consistent estimators. Thus, we might consider that the dichotomous choice valuation and the trip revision decisions are jointly determined in response to the contingent scenario. Models Two and Three are estimated in a nonlinear instrumental variables framework, where there is more than one available instrument for the single dummy endogenous variable. The models produce consistent (but inefficient) parameter estimates. The potential policy impacts of such a result can be explored further. Figure 1 presents the estimated Hicksian demand curves from the original ABMJM model (the first model in Table 2) and the revised models (Models Two and Three in Table 3) using the unweighted and weighted NLIV estimator .

The impact on the expected conditional willingness to pay is seen in Table 4. All estimates were calculated using equation (21). The first column gives the expected conditional WTP from the ABMJM model. This value of $21.36 is approximately 10-20 percent higher than the expected WTP's from the two revised models. Disaggregating the sample into those who would (CB=1) and would not (CB=0) completely stop pheasant hunting in Western Oregon with elimination of the stocking program shows two distinct groups

[15] The weighting w_i used on each predicted probability was $w_i=[p_i(1-p_i)]^{-1/2}$, where p_i is the predicted probability for the CB model, and may help to correct for potential heteroskedasticity, but will not provide the most efficient estimator (Grogger).

($23.28 vs $14.85).[16] The lower of these two values, $14.85, falls outside of the 95% confidence interval (CI) around the estimated mean WTP value of $21.36 for the original model. The 95% CI of $16.27 to $24.75 was calculated using the analytical formulas from Cameron (1991) for recovering the variance-covariance matrix for the set of transformed parameters from the information matrix of the original logit estimation.[17]

The direction of the change and the size of the policy impacts are specific to the ABMJM survey data. They should not be generalized. However, it does seem likely that demand and valuation per-unit-of-use estimates will be biased upward for hypothesized losses in environmental services, and downward for hypothesized gains.

Discussion and Comparisons with Other Research

Joint estimation procedures for valuation functions that include endogenous explanatory variables are a fruitful avenue of CV research. To my knowledge this paper is the first to address such concerns for a measure-of-use variable and the derivation of demand curves. For completeness, several related studies should be discussed.[18]

Whittington et al. utilized a joint estimation procedure to analyze the effect of "time to think" on CV results. The binary decision to revise the initial open-ended bid is treated as jointly determined with the final amended bid. This is a similar situation to the one addressed in this paper for the CB variable -- the valuation function is believed to have a dummy endogenous variable -- albeit not for a measure-of-use variable. In the first stage, a probit model is estimated to describe the determinants of revising the bid. The

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[16] Alternatively, one might argue that those who answered "no" to the trip revision/contingent behavior question really did not understand the valuation question or the importance of the stocking program, and thereby contaminated the sample. However, this position is difficult to defend. In the year following the ABMJM survey, the pheasant stocking program was dropped. (See footnote 6.) Total visits to the site for the next two seasons were about one-third of previous visitation levels.

[17] Verification for this result comes from the unpublished work of Bergland et al., who obtained similar 95% CI results using several alternative simulation/bootstrapping approaches applied to the same data set and logit model. For example, they obtain a 95% CI of $15.36 to $24.07, in one representative result.

[18] For a single iteration dichotomous choice format, Cameron and Quiggin lay out an estimation procedure that accounts for the endogeneity inherent in the second level fee. Their empirical results demonstrate a statistically significant difference in variance across the two single referendums; additionally, they provide a precise estimate of the correlation across the two binary valuation responses. Park and Loomis use a joint estimation procedure to account for interdependencies across alternative DC-CV valuation scenarios.

final valuation function is estimated with nonlinear two-stage least squares with the predicted revision decision as significant explanatory variable.

In an open-ended valuation study on congestion, Prince and Ahmed treat both WTP for a hiking trip and length of stay, their measure-of-use variable q, as endogenous variables. A recursive system argument is used to provide a generalized least squares (GLS) estimate of WTP, where predicted q is included as a regressor. Although q is experience-specific, it is argued that it may not represent an optimal choice because of potentially unrealized expectations about congestion. While no empirical test for endogeneity was conducted, the predicted q was statistically significant in the valuation function.

In contrast to these procedurally-related works, Morey and colleagues (Morey; Morey et al.) provide a conceptually-related argument from a discrete choice revealed-preference standpoint and the modeling of participation levels.[19] In discrete choice random utility models, the failure to model the participation decision can bias welfare measures. When nonparticipation is not one of the available choices, the derived welfare measures are referred to as consumer surplus "per-trip" or "per-unit-of-use." These per-trip valuations must be combined with some independent estimate of use levels; there is no guarantee that this can be done in a logically consistent manner. Through empirical applications and simulation models, it has been demonstrated that the bias involved can be considerable.

Models which fail to consider the decision to participate and changes in the level of use, will produce biased welfare measures of changes in environmental or resource quality. In their application of a random utility model, Morey et al. (p. 15) state:

> For each supply scenario, one could estimate demand for trips...conditional on the total number of trips to all sites not changing when the supply conditions change. However, since the change in supply conditions will likely cause a change in the participation rate, these conditional demand estimates will, in most cases, be biased upward for deteriorations and biased downward for improvements.

This same type of phenomenon has been demonstrated here in the DC-CV context. It must be recognized that hypothetical changes in the access to, or quality of, environmental services can be used to elicit valuation responses; but these valuation responses may be conditional on concomitant changes in expected use levels of the respondent.

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[19] Morey does reference Cameron (1988) and Cameron and James, and their DC-CV models; however, his discussions and applications are otherwise geared to the discrete choice RUM's of revealed preference/travel cost methods.

Final Comments and Conclusions

A primary purpose of this research is to sound a note of caution to future DC-CV researchers and policy analysts in reconstructing demand curves from "off-the-shelf" DC-CV models for benefit transfer purposes. The findings suggest that no blanket prescriptions are available concerning the endogeneity of measure-of-use variables. Grogger's test for probit and logit models helps to fill a gap in evaluating the econometric specification of DC-CV models. It provides a technique for resolving endogeneity questions on measure-of-use variables. Such variables can enter into CV models in a variety of ways. Absent endogeneity, there is still the opportunity for considerable bias in demand curves or other derived per-unit-of-use welfare measures. For example, a measure of historical avidity should not be misinterpreted as the chosen level of use in the hypothetical market.

The results demonstrate potential policy implications from the incorrect application or interpretation of measure-of-use variables. Whenever a proposed change can impact behavior, the modeling structure must explicitly account for these changes in valuation functions and derived welfare measures.

A typology was presented and used to generate several hypotheses. It distinguishes among different types of CV models and how they incorporate measure-of-use variables. The empirical results from applying Grogger's test provide initial confirmation for the proposed typology. The development of the typology, and any future refinements, are particularly important in a benefit transfer context. The opportunity to conduct specification tests will not always be available for a benefits function transfer exercise. Policy analysts require accessible tools for discriminating among the many available, published and unpublished, DC-CV studies. This typology may provide an initial screening tool, concerning the incorporation of measure-of-use variables, in the emerging protocol for acceptable benefit transfer.

Finally, the development of joint estimation techniques appears to provide a fruitful area for future CV research. In particular, it may facilitate the incorporation of contingent behavior responses in total and marginal valuations.

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Table 1. The Proposed Typology

Type	Description of Constructed Market	Description of q	Status of q to the Constructed Choice	Can a Valid Hicksian Demand Function Be Estimated?
I	counterfactual (backward looking)	experience-specific	exogenous	YES; directly under the maintained hypothesis of fixed level of use
II	hypothetical (forward looking)	experience-specific	exogenous	NO; unless it holds that the hypothetical mkt induces no changes in use
III	hypothetical (forward looking)	contingent behavior	endogenous	CONCEPTUALLY YES, but not directly, must be done in some sort of joint estimation process
IV	hypothetical (forward looking)	increments or decrements in the contingent scenario	exogenous	YES; directly provided sufficient variation in q across the sample

Table 2. Model Estimation Inputs for Endogeneity Test on the Experience-Specific Use Variable

Variable	Coefficient	Coefficient
INTERCEPT	3.752 **(2.48)	3.436 *(1.809)
LNFEE	-2.253 ***(-4.081)	-2.148 ***(-3.981)
D1	0.994 (1.345)	0.934 (1.266)
D2	2.017 ***(2.758)	1.938 ***(2.664)
LNVIS	0.663 *(1.972)	
PREDICTED LNVIS		0.718 (0.970)
Likelihood Ratio Test	***38.865	***35.5804
Maddala R^2	0.330	0.307
McFadden R^2	0.292	0.267
% Correct Predictions	0.784	0.753

The numbers in parentheses are the asymptotic standard errors; *, **, and *** indicate significance at the 0.05, 0.025, and 0.01 levels, respectively. LNFEE=the natural logarithm of the offered fee or bid; D1=a dummy variable for the $15-30,000 income group; D2=a dummy variable for the $30,000+ income group;LNVIS=the natural logarithm of the total visits.

Table 3. Model Estimation Inputs for Endogeneity Test on the Contingent Behavior Variable

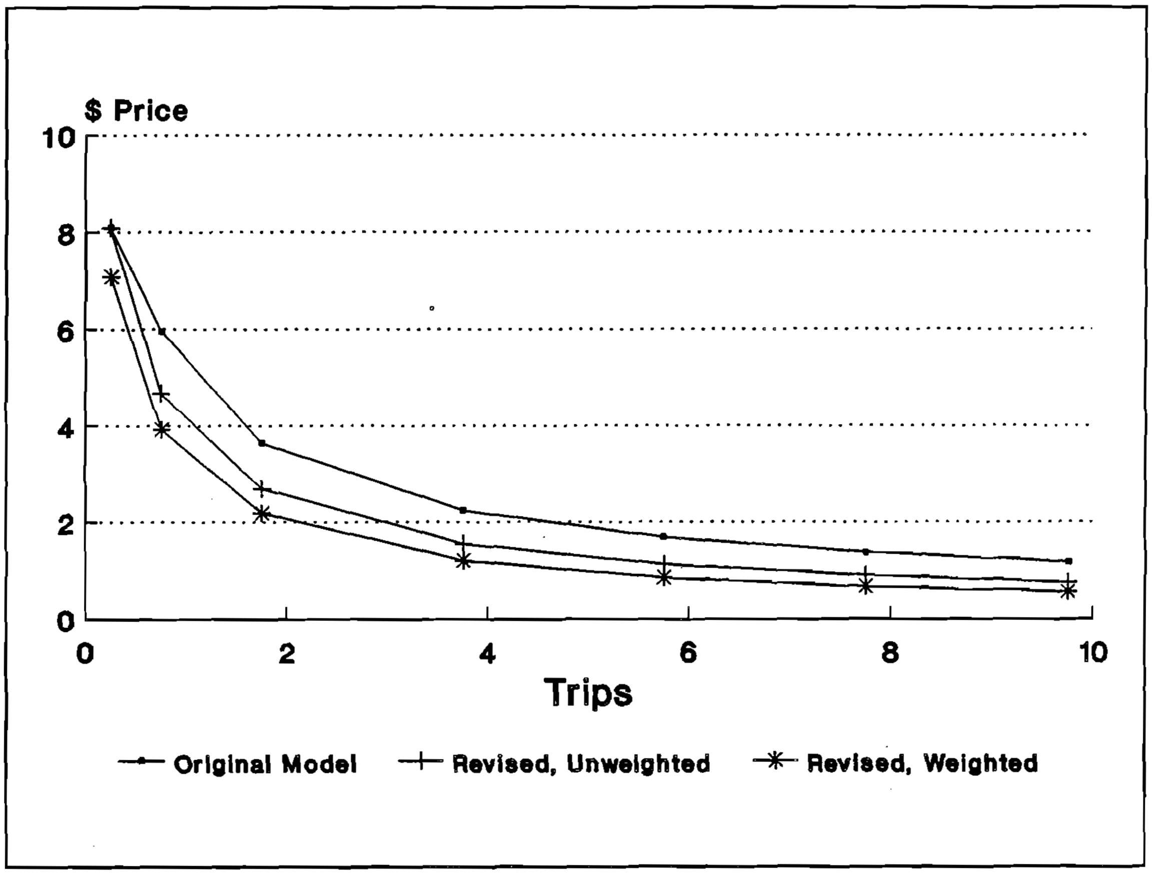
Variable	Model One Coefficient	Model Two Coefficient	Model Three Coefficient
INTERCEPT	3.726 **(2.477)	4.379 **(2.593)	4.262 **(2.45)
LNFEE	-2.222 ***(-3.995)	-2.495 ***(-4.003)	-2.680 ***(-3.903)
D1	1.061 (1.395)	0.294 (0.329)	-0.080 (-0.085)
D2	2.040 ***(2.788)	1.698 **(2.215)	1.673 **(2.216)
LNVIS	0.678 **(1.998)	0.518 (1.471)	0.393 (1.091)
CB	-0.198 (-0.365)		
Predicted CB From NLIV		0.968 (1.360)	
Predicted CB Probability From NLIV, With Weightings			1.203 *(1.859)
Likelihood Ratio Test Statistic	***38.998	***40.723	***42.685
Maddala R^2	0.331	0.343	0.356
McFadden R^2	0.293	0.306	0.320
% Correct Pred.	0.784	0.763	0.742

Numbers in parentheses are asymptotic standard errors. *, **, and *** indicate significance at 0.05, 0.025, & 0.01, respectively.

Table 4. Comparison of Expected Willingness to Pay Results

Original Model	Model Two evaluated at the sample means	Model Two, evaluated at the sub-sample means, with CB=0	Model Two, evaluated at the sub-sample means, with CB=1	Model Three, evaluated at the sample means
$ 21.36	$ 19.27	$ 14.85	$ 23.28	$ 17.50

Figure 1. Hicksian Demand Curves



Appendix A

Table A.1. Estimation Results for the Log-Linear OLS Model (Predicted Trips)

Variable	Coefficient	T-statistic
INTERCEPT	1.452	***7.817
BAG1	0.503	***2.834
BAG2	0.932	***4.114
WND	0.048	0.294
MILES	-0.029	*-1.828
D1	-0.306	-1.434
D2	-0.112	-0.558

Dependent Variable = LNVIS (the natural logarithm of total visits); R^2=.2064; ST. Error of the estimate=0.774; *, **, and *** indicate significance at the 0.05, 0.025, and 0.01 levels, respectively; n=97; BAG1 is an indicator variable (1=average daily harvest rate >1, 0 otherwise); BAG2 is an indicator variable (1=average daily harvest rate <1, 0 otherwise), there is a daily bag limit of two at the site; WND is a dummy variable that indicates weekend use (1= yes, 0 otherwise); MILES = total trip miles to the site.

Table A.2. Logit Estimation Results for Contingent Behavior

Variable	Coefficient	T-statistic
INTERCEPT	-2.316	**2.392
BAG1	0.063	0.115
BAG2	0.281	0.363
EO	1.376	**2.171
WND	0.799	*1.658
LNFEE	0.294	0.921
D1	1.034	1.541
D2	0.297	0.470
LNVIS	0.402	1.276

Dependent Variable = CB (1= yes, would eliminate all trips in response to elimination of stocking program, 0 otherwise); *, **, and *** indicate significance at the 0.05, 0.025, and 0.01 levels, respectively; n=97; EO is a dummy variable indicating whether substitute sites in eastern Oregon were visited (1=yes, 0 otherwise); Likelihood Ratio Test Statistic = 19.5***; McFadden R^2 = 0.15; Percentage Correct Predictions = 0.76.

Reservoir Recreation Demand and Benefits Transfers: Preliminary Results

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Abstract

This paper reports on tests of the interchangeability of travel cost demand models for U.S. Army Corps of Engineer reservoirs in the Sacramento, Little Rock, and Nashville districts. Statistical tests of coefficient equality suggest rejecting a transferable model among all three districts. However, the Little Rock and Nashville models were similiar enough to fail to reject the hypothesis of equal coefficients at the 0.01 level. Despite this finding, interchanging the Little Rock and Nashville demand models produces visitor use and total benefit estimates that are more than 100% too high. However, interchanging the Little Rock and Nashville demand coefficients resulted in average consumer surplus estimates which are quite close to their own site estimates. This is due to similiarity of the Little Rock and Nashville price coefficients. Thus, a more limited form of transferrability which focuses on average benefit per day, rather than on predicting total use and and total benefits, appears promising.

Reservoir Recreation Demand and Benefits Transfers: Preliminary Results

Necessity is the Mother of Benefit Transfer

Introduction to Benefit Transfer Issues

Agency decision makers and analysts often need three types of information to evaluate policy or budget proposals at recreation sites: (1) value per recreation day; (2) an estimate of use; (3) knowledge of how (1) and (2) change with changes in the quality or quantity of site characteristics.

Internally consistent answers to all three of these questions would be ideally obtained from a recreation demand equation. A zonal recreation demand equation using aggregate data is typically of the following form:

(1) $$DAY\_T_{ij}/POP_i = f(TOTCOST_{ij}, SUBS_i, SQ_j, DEMO_i)$$

where

DAY_T_{ij} = total annual day trips from origin i to site j.

POP_i = population of origin i.

$TOTCOST_{ij}$ = round trip travel cost from origin i to site j. $TOTCOST_{ij}$ should also include the opportunity costs of travel time.

$SUBS_i$ = a measure of the price and quality of substitute sites k (k=1,..n) available to origin i

SQ_j = a measure or index of the quality of recreation site j.

$DEMO_i$ = demographic characteristics of origin i.

For recreation sites where information on DAY_T_{ij} is available, the estimation of such a model is relatively straightforward. Collection of such information usually requires an expensive survey of recreation behavior. In our context, benefit transfer involves using data on recreation visitation at sites in one region (surveyed region) to predict recreation behavior, DAY_T_{ij} and benefits, at sites in a separate region for which visitation data is unavailable (target region).

Mechanics of Demand Transfer

If recreation behavior as measured by the coefficients is the same in the surveyed region and the target region, then a model estimated for the surveyed region predicts how recreation use, benefits per day, total recreation benefits and the marginal value of changes in site characteristics would change in the target region. This is accomplished by inserting the values of the independent variables for the target region into the surveyed region's estimated equation.

We make the distinction between transfers that are geographic interpolations, where the target site is within the geographic area where the demand equation was estimated, and geographic extrapolations. The latter refers to transferring the coefficients to a target site that is located outside the original market area, where the target region shares no common origins or destinations which might act as substitutes.

Our purpose is to test whether recreation behavior is similar enough across different regions to allow model transfer. Our approach is to estimate the same recreation demand model for three different regions, and test the equality of the estimated coefficients across the three regions using a Chow test. The resulting test statistic has an F-distribution. The test compares the error sum of squares of the three models estimated individually versus a pooled model. The pooled model imposes the restriction of coefficient equality. If this restriction is incorrect, the pooled error sum of squares will be much larger than the sum of the error sum of squares from the three models estimated individually.

Other Types of Benefit Transfer Approaches

There are two circumstances when such a complete benefit transfer is either not recommended or not required. First, a transfer is not recommended if the recreation behavior is not the same in the surveyed and target regions. Second, there are times when the agency or analyst has a reasonable estimate of current total recreation use at the target site and only needs a benefit estimate per visit because no origin-destination data are available to estimate a demand equation for the target site. In essence, all that needs to be transferred in this case is the price coefficient. If the price coefficient is equal, benefits per trip will be equal under several common functional forms. For example, with a semi-log demand model the value per trip (or day) is simply 1 divided by the price coefficient. This paper will explore both the more complete and this partial type of transferability.

Data Sources

The dataset was develop using a variety of sources. Exit surveys of day use visitors to U.S. Army Corps of Engineer (COE) reservoirs in three Districts, Sacramento, (SAC), Little Rock (LR) and Nashville (NASH), in 1983-1986 gave a tally of the origin zip codes of visitors. Given the nature of the data, a zonal rather than individual observation TCM is most appropriate (Ward and Loomis, 1986). With a zonal model, the dependent variable is defined as the number of visits per capita from each zone of origin.

Data on facilities at each reservoir were obtained from the COE's Natural Resource Management System dataset. All demographic data on visitor zones of origin were obtained from 1980 census reports. Information on fish stocking was obtained by contacting individual state fish and game agencies. Total surface area at available substitutes were determined from contacts with numerous fish and wildlife departments, water resource agencies, power companies, irrigation districts and conservation agencies.

Defining Zones of Visitor Origins

There are at least two primary approaches to defining visitor zone of origins: zip codes or counties. The advantage of using zip codes is that travel distances can be precisely determined. However, defining origin zones by zip code results in a majority of origins sending zero visitors. This causes statistical estimation problems. Also, zip code origins results in a very large dataset.

Using counties as origin zones simplifies statistical estimation and results in a more manageagble datset, but travel distances are approximate. The use of counties is typical in many zonal TCM's (Ward and Loomis, 1986) and it facilitates the use of published demographic data. Therefore, we chose to use county zones of origin. Errors from aggregating travel distances for each county were minimized by using the largest city in the county as the common origin point. Errors in using aggregate travel distances are most apparent in large counties, such as some found in Southern California. However, these counties tend to be located in desert or mountainous terrain and most of the population in these large counties are concentrated in a few urban areas. Since the counties within the LR and NASH districts tend to be relatively small, the largest city assumption will result in minimal error.

Aggregation of Raw Data into Visitor Origin Zones

The zip code data were aggregated to the county level using the computer program TRANSCAD. The dataset was further divided by each year and each destination site. Valid zero visitation totals were included since this provides useful information. Failing to include these zeros truncates the sample and can lead to overestimation of use and benefits when applying the model to new areas.

Unfortunately, the COE exit surveys did not ask visitors whether they were on multi-destination trips. After inspecting the maps surrounding the recreation sites and using our own familiarity with previous recreation visitation patterns, a 250-mile radius was considered sufficient to capture the majority of single destination day trip travellers. Visitors from origins more than 250 miles away, approximately five hours driving time one-way, were assumed to be on multi-destination trips. These visitors were excluded from the estimated model since it is not possible to accurately determine the correct round trip travel cost to visit one COE reservoir for persons visiting several destinations on one large trip.

Expanding Sample Use Estimates to Annual Site Visitation Estimates

An important step in defining the dependent variable was to determine the appropriate sample expansion factors to extrapolate from the survey sample to the population. The ratio of sample visitation to COE estimates of total site visitation was taken as the sample expansion factor. However, inspite of the aggregation of visitor zip codes by county, some counties still had zero visits. Of course, multiplying by a sample expansion factor still produces a population estimate of zero total visits from that county. This result that all such counties sent zero visitors during a recreation season is likely to be false. Some counties had zero sampled visits one year but positive sample visits other years. With sample expansion factors ranging from around 50 to above 1000, multiplication by these factors produced estimated total visits which jumped from zero one year up to several hundred or more the next year. Moreover, the population values of zero visits fail to reflect different sampling rates across reservoirs.

The general method for dealing with these zeros was to adopt a Bayesian view of how the zeros were generated. While we retained the population zeros for some model specifications, the zeros were modified using sample informaiton for others. We assumed that the zero sample values reflect a small sample problem. If survey rates were incrementally increased, eventually positive visitation would be recorded from all counties in the market area. Assuming a random sample, in any case were the expected (but unobservable) value of sampled visits was 0.5 or less, observed sample visits are zero. We assume in these cases that the true population value of total visits must lie between 0 and 0.5 times the sample expansion factor. We dealt with this problem by selecting the midpoint of this region, equal to 0.25 times the sample expansion factor. This approach also considers the variable sampling rates at different reservoirs.

After this adjustment was made for dealimg with the zeros, the visitation data were put on a per capita basis by dividing by the number of people living in county i. The population of the county is part of the overall demographic data assembled for each county.

Assembling Demographic Data on Visitor Origins

The county demographic data was also used to develop a list of demographic independent variables. The list below includes the demographic variables which were consistently significant in regression equations. The variable name are in parentheses.

1. Per capita annual income of county i (INCOME)

2. Average annual wage rate of workers in county i (WAGE_RT)

3. The median age in county i (MED_AGE)

Additional demographic variables were collected and may be used in future analyses.

Assembling Corps of Engineers Reservoir Site Characteristics

Another group of independent variables dealt with the characteristics of each site. We determined important site characteristics by reviewing the literature on reservoir recreation demand models (Wade, et al., 1989; Ward and Fiore, 1991), and considering the applications to COE policy which the model must be able to address. The following variables were deemed important and were available from for each reservior from the C.O.E. N.R.M.S. dataset:

1. The elevation of the reservoir (in feet above sea level) at the recreation or summer pool (ELEV).
2. The storage capacity (in acre-feet) at the recreation or summer pool (STORAGE).
3. The surface area (in acres) at the recreation or summer pool (SUR_AC).
4. The number of land acres at the project under jurisdiction of the C.O.E. (LAND).
5. The mean depth (in feet) at the recreation or summer pool (DEPTH). This variable is equal to STORAGE divided by SUR_AC.
6. The number of shore miles at the recreation or summer pool (SHORE).
7. The number of parking spaces (PARKING).
8. The number of picnic tables (PICNIC).
9. The number of boat launch lanes (LANES).
10. The number of swimming beaches (BEACHES).
11. The number of full-service marinas (MARINAS).
12. The number of private docks (DOCKS).

Most of these variables describe the size of the reservoir or the available facilities. Conversations with Jim Henderson, U.S. Army Corps of Engineers suggested that DOCKS would be an accurate proxy for the amount of private development around each reservoir. Preliminary analysis showed most of these site characteristics where highly correlated with each other. Therefore, to minimize multicollinearity problems, this paper uses actual surface area as a proxy for site quality. As discussed in the conclusions, construction of a facilities index is presently underway and may be used in future versions of this model.

To obtain the actual surface acres of a reservoir during a recreation season, we obtained daily reservoir levels for all sites from each C.O.E. district. Area-capacity tables were used to convert elevation readings into surface acres.

The next step was to determine a recreation season average surface acres (REC_SA). We determined that a weighted average by monthly visitation was most appropriate. Thus, reservoir levels were most important during the peak recreation season.

While fishing quality measured by the number of species and amount of stocking and water quality are also likely to be important site quality variables, they proved relatively unimportant in our initial analyses and are not discussed further at this time.

Measuring Vehicle Travel Distances and Times

Travel distances and times were calculated using the computer program PCMiler. Each origin was defined as the largest city in each county within 250-miles of each Corps site. Destination points were towns closest to each j reservoir. We assumed the visitors travelled to one of these nearby towns and then continued on to the nearest major recreation site on the reservoir. The additional distance from the nearby town to the major recreation area was added to total travel distance. This was done for up to four towns surrounding each site and for each origin the shortest distance (MILES) and associated travel time (TIME) was chosen.

A total travel cost variable (TOTCOST) was calculated using MILES and TIME. The total vehicle cost was defined as round-trip miles times the per mile variable vehicle cost. We obtained data on intermediate-size vehicle operations costs from the U.S. Department of Transportation (1990). This cost was then divided by the average number of people per vehicle according to the Corps exit surveys. An estimate of the opportunity cost of time was then obtained by multiplying the round-trip travel by one-third the county wage rate. To reflect trip preparation time, $1 was added to the sum of vehicle and time costs to obtain total travel costs (TOTCOST).

Incorporating Substitute Sites

The availability of substitute recreation sites has been shown to significantly affect visitation to C.O.E. reservoirs (Rosenthal, 1987). Thus, we need to formulate a measure to describe the number and quality of substitutes for each C.O.E. reservoir available to residents of each county i. Following Knestch, et al. (1976), a substitute index was developed to convey the relative distance and quality of the substitute sites. Quality of substitute reservoirs and lakes was based on their surface area. A site would be considered a substitute if its surface area exceeded 500 acres. The substitute index was constructed as the sum over all substitute sites of the ratio of surface acres divided by travel distance. Thus, any zone of origin with larger and closer substitute sites has a higher substitute index.

Another substitute variable was defined as the travel distance from county i to an ocean or one of the Great Lakes. This was performed using PCMiler. For the Nashville and Little Rock districts, the travel distances from each county i to six potential major shore recreation areas were calculated. From these six areas, the minimum of the distances for each county was chosen for the variable (OCEAN). For the Sacramento district, the travel distance from county i to the nearest beach access was determined for each California county. The beach accesses were determined from a description of California's state beach system.

Figure 1 summarizes the various sources of data and data compilation techniques used to assemble the dataset for the three COE Districts.

Initial Model Specifications

A necessary condition for accurate benefit transfer is that the appropriate demand model has been estimated in the first place. There are several approaches to aggregate or zonal TCM's. One important choice relates to definition of the dependent variable and functional form. Historically, semi- or double-log models have been used extensively due to their desirable properties on the trip per capita specification of the dependent variable. For example both Vaughan, et al. (1982) and Strong (1983) found that taking the log of trips per capita minimized the heteroskedasticity frequently found in zonal TCM's. As discussed in Stynes et al. (1986), there is a bias in retransforming the log estimates back. While we have adjusted our visitation estimates for this translation bias, this bias can be avoided by directly estimating the exponential model using non-linear least squares. Weighting of the variables by population (Hellerstein, 1992) or the square root of population (Bowes and Loomis, 1980) to account for heteroskedasticity is another frequently used remedy. Recently Hellerstein (1992) has attempted to estimate aggregate count data models that use total zone trips as the dependent variable and weights the independent variables by the zone population. This yields some statistical efficiencies when most origins take very few trips per origin and allows the zeros to be counted in an exponential model without the need to add a positive constant to the dependent variable in order to take the log of the zeros.

Bockstael, et al. (1990) has suggested Tobit and Heckman sample selection models be appropriate for modelling recreation participation and trip frequency. Since our datasets reflect all of the counties within the market area, many of them (upwards of 40% in some COE districts) make no sample visits. As such, the Heckman two stage modelling process may be desirable: (1) the first model estimates the likelihood that county will have at least one visitor, (2) the second model estimates the number of visits for the counties that take positive visits. The second stage implicitly accounts for the selection process of the visit sample
by adjusting the error distribution for being truncated at one trip.

Finally, Rosenthal (1987) and Knetsch, Brown and Hansen (1976) use total trips from an origin as the dependent variable and compensate by specifying population as an independent variable. This avoids some of the statistical problems associated with the trips per capita formulation.

Refining the Market Area

One difficulty in both initial model estimation and later model transfer is defining the market area. Since our data does not code whether the individual was on a single destination trip or not, we have tried several definitions of the market area. We originally designed the datset with a 250-mile one way limit. However, subsequent analysis shows that few Corps reservoirs are sufficiently attractive to visitors to warrant such drives on day trips. Plotting the data and estimating several regressions at varying distance cut-offs led us to conclude that 150 miles one-way is likely the maximum distance most people in these three Corps districts travel for single destination reservoir recreation.

Demand Model Selection Criteria

Several alternative modelling strategies were evaluated against four successively stricter criteria. The first criterion was the model needed to have theoretical and empirical properties consistent with the travel cost method and the particular structure of our data. The second criterion related to theoretically expected signs and statistical significance of the variables. Thus, price should be negative, as should our substitute index. Recreation surface acres should have a positive influence on visitation.

A third criterion related to how well estimated models predicted recreational use in each respective Corps district. To assess this criterion, a performance ratio was constructed as follows: predicted visitation across all sites in the district was divided by actual visitation across all sites in the district. A perfect model would have a ratio of one. We selected the most robust model specifications for further comparison and benefit estimation according to which specification have the smallest deviations from one across the three districts.

A fourth evaluation criterion was whether the estimated demand equation did a reasonable job of estimating existing use of sites within the COE District. Since the each regional model contained 8-9 sites, each with numerous origins, estimating each site's use accurately can be difficult. This is particularly true with a per capita specification of the dependent variable and the fact that a single set of coefficients is applied to multiple sites in the region. Even if the Corps district-wide value of the error term is zero, when the observations are grouped by site, one site will be overestimated and another underestimated.

Evaluating Transferability

Transferability of recreation benefits can be evaluated at three levels. First, a transfered demand function can be used to predict total visitor use. Second, a model transfered from another site can be used to produce a benefit estimate per visit and compared with an own-site model. Third, formal statistical tests of structural difference in the coefficients can be conducted. If a least squares estimation method is used, the appropriate test is a Chow test comparing the restricted residuals of a pooled regression that imposes coefficient equality across the three COE districts versus the sum of the unrestricted residuals from estimating the demand functions for the three districts independently. For demand estimation approaches involving maximum likelihood estimation, such as the Heckman sample selection approach, a likelihood ratio test is used to compare the restricted and unrestricted models.

Statistical Analysis

Preliminary Models that Were Rejected

Weighted Regressions. Two types of weighted regression method were tried initially. The weighted Possion model of Hellerstein (1991) was estimated on the 1985 Sacramento data. This model was estimated using Limdep's Poisson model with zone population as weights. The weighting appears to have inflated the t statistics, such they were all extremely significant. The ratio of predicted to actual visitation was .35. This is unacceptable and is too far out of line with other demand specifications. This approach is not pursued further.

Weighted least squares was also tried on both data sets. Per research by Bowes and Loomis (1980), the weights were the square root of the zone population, to correct the higher residual variance from zone of origins with lower populations. For the Sacramento District, the equation yielded theoretically correct signs and generally statistically significant coefficients. The model predicted about 75% of observed total visits. However, weighted least squares performed worse for the Little Rock district. The sign on substitutes was positive and insignificant. The price coefficient was negative and the recreation surface acres was positive, and both were significant. The use prediction from the weighted least was poor in the Little Rock district. The other drawback to the weighted least squares approach is the possibility of negative predicted visitation from distant orgins. This is counter-intuitive and makes benefit estimation more complicated and somewhat arbitrary depending on how the negatives are treated. This approach was not pursued any further.

Tobit Models. Tobit models have the desirable property of accounting for the censored nature of the dependent variable. For the Tobit model (and the following Heckman model) the original zero visits were retained. The estimated Tobit models for the Sacramento and Little Rock districts had the expected signs, but this was not the case in Nashville. The main failing of the Tobit model was its poor prediction of total use across sites in all three districts. Even though the appropriate unconditional expectation was used, the models overpredicted by a factor of two in Sacramento and Little Rock and three in Nashville. This may, in part, be an artifact of the Tobit restriction that both the probability of visiting and number of trips are a function of the same estimated coefficients. The Heckman sample selection model avoids this restrictions and, as described later, performed much better.

Candidate Demand Models. Three demand specifications were given careful evaluation based on their statistical structure being more consistent the nature of our data, their theoretically consistent signs and significance and performance in predicting overall site use in each of the three COE districts.

Double Log Demand Models. A double log or Cobb-Douglas type demand model is a commonly used TCM demand specification and alleviates the prediction of negative visits. The double log model cannot accomodate origins with zero visits. For double log estimation zeros were replaced with .25 times the sample expansion factor, as discussed earlier.

As summarized in Table 1, double log models using visits per capita and total visits as the dependent varible both yielded the correct signs on total cost, recreation surface acres, and substitutes for all three districts. Substitutes was insignificant only in the Nashville District. Figure 2 shows the double log model for total visits predicted visits accurately only for the Sacramento District. Little Rock was substantially over-predicted with a ratio of predicted to actual of nearly 2 and Nashville substantially under-predicted with a ratio less than .5. Figure 2 shows the double-log visits per capita model fared somewhat better with less overprediction for Little Rock and slightly less underprediction in Nashville.

Nonlinear Least Squares. A refinement of the double log model is the non-linear least squares (NLS) estimator. The NLS model is same functional form as the double log model with a multiplicative instead of additive error term. Table 1 shows the visit per capita NLS model has correct signs on all of the variables in the Little Rock and Nashville District, but one sign was reversed (though insignificant) in the Sacramento District. As shown in Figure 2, the NLS on visits per capita had the closest overall correspondence between predicted and actual visits of any of the model structures although just slightly better than the more complicated Heckman model. Thus, the NLS on visits per capita will be carried forward for cross-district comparison via the Chow test and calculation of average consumer surplus later in this paper.

Heckman Sample Selection

The Heckman sample selection model involves a two step procedure: a probit model is estimated to predict the probability of positive versus zero visitation; then a double-log demand equation is estimated on the log of total trips of counties that actually visited with the addition of a truncation variable. This variable is calculated from the first stage probit and is called the inverse mills ratio. The ratio reflects the fact the second step sample is restricted to counties with positive visits. We chose to use the same independent variables to explain the decision of whether to visit a particular reservoir as well as to explain the number of trips. This need not always be the case but we felt it was appropriate in this case.

The probit part of these models fit fairly well in terms of theoretically expected signs and statistical significance. The signs and significance level of the variables in the continuous part of the model are shown in Table 1. All variables had the expected signs, although the substitute variable was insignificant in the Nashville District. The continuous part of the Heckman models consistency underpredicted visits, but in general was quite close to actual visits across the three districts. Given the Heckman approach explicitly models the zero visitation phenomenon and its uniformly correct signs and its relative accuracy, it will be the other model brought forward for model transfer testing.

Transferability Evaluations of NLS and Heckman Models

Statistical Tests of Transferability

The separately estimated coefficients for both the NLS and Heckman (continuous portion) are show in Table 2. As is evident, there is quite a bit of variability in the constant term and slope coefficients between the three district in both demand specifications.

For both the NLS model and the continuous portion of the Heckman model, a Chow test can be performed to test the statistical equality of coefficients between all three districts. In particular the null hypothesis is:

Ho: $\beta^{SAC} = \beta^{LR} = \beta^{NASH}$

Where β_i is a vector of coefficients for each independent variable in each district.

Comparing the sum of squared residuals from a NLS model pooled over the three districts to the sum of square residuals for the three NLS equations estimated separately yielded a F statistic of 4.74. Given the critical $F_{(5, 2215)}$ of 2.21 at the 0.05 level, we reject equality of coefficients across the three districts. Therefore, transferability to estimate use and benefits across all three districts is not defensible on statistical grounds. However, the Chow-test between just the Little Rock and Nashville districts has an F of 2.3, just slightly above the critical $F_{(5, 2215)}$ of 2.21. Using a more stringent F for the .01 level fails to reject the hypothesis that the coefficients are the same across the two districts. Thus, the least model transferrability appears to be between Sacramento and the two other districts. Below we check to see the error in performing the transfer of coefficients to estimate average consumer surplus per visit.

Performing the same Chow test on the continuous portion of the Heckman demand equation results in an even stronger rejection of equality of coefficients. The calculated F is 23.14. As suggested by Greene (1992) a likelihood ratio (LLR) test of the combined probit and continuous part may be a more comprehensive way to test similarity of two step models of sample selection. The logic behind the LLR is similar to the Chow test. The LLR of the pooled model and the sum of the independently estimated models will be statistically different if the imposition of coefficient equality in the pooled model is inconsistent with the data. The LLR test is two times the difference between the pooled LLR minus the sum of the individual LLR. The calculated chi-square is 196, far greater than the critical chi-square at the 0.01 level (23.2). Hence, neither of the two equations corresponding to the two stages of the Heckman model are transferrable. Nonetheless we wish to investigate the degree of error from transferring the average consumer surplus under the conditions.

Evaluation of Equation Transferability for Predicting Use Across Districts

Given the Chow test results, it is not surprising that the application of the Little Rock NLS coefficients to Sacramento considerably overpredicted visits. Application of Sacramento NLS coefficients to Little Rock likewise resulted in a very large overestimate of visitation. Transferring Sacramento coefficients to Nashville results in even larger overprediction, off by two orders of magnitude. However, the Nashville and Little Rock models transfered better. Application of the Little Rock coefficients to Nashville yielded visitation estimates by site that were in fact better for six of the eight sites than Little Rock model itself. The better correspondence between Little Rock and Nashville may be due to far greater geographic, hydrological, and cultural similarity of these two districts as compared to Sacramento. Application of Nashville NLS coefficients to Little Rock resulted in over-estimation of visits by about 100% greater than what the Nashville model predicted, which itself overestimated site visits by about 40%.

Comparison of Average Consumer Surplus Estimates

In some benefit transfer applications the analyst knows the total visitation at their target site, but does not have a value per visitor day to apply. Thus, a less ambitious form of benefit transfer is compare the own model average consumer surplus to the consumer surplus from transferring the coefficients. Table 3 provides this comparison for the NLS model. Application of the Little Rock and Nashville's coefficients to the Sacramento District yields values of per day 3-4 times higher than directly using Sacramento's own coefficients. This pattern holds in reverse for transferring Sacramento's coefficients to Little Rock and Nashville. The Sacramento coefficients estimate a much lower value for consumer surplus than if the own-district coefficients are used. The reservoirs in the Little Rock and Nashville districts appear to produce higher per-visit consumer surplus estimates than the Sacramento district reserviors. This makes sense since the Little Rock and Nashville reservoirs are large major attractions while the Sacramento reserviors are mostly small and located in the low elevation foothills of the Sierra Nevada Mountains which tend to be quite hot during the recreation season. Recreators in the Sacramento district may prefer a wide range of substitute recreation opportunities, such as Lake Tahoe, the Pacific Ocean, three National Parks and the high elevations of the Sierra Nevada Mountains.

Application of the Nashville coefficients to Little Rock yields benefits per visitor day quite close in both absolute size and in terms of ranking of sites from most valuable to least valuable. Application of the Little Rock coefficients to Nashville yield estimates per day that are also comparable in both absolute size and in ranking.

Future Research

We see two main avenues for future research. First, we plan to apply this same basic research protocol to camping data at the same three COE districts. While the camping samples are smaller, the greater specificity of the activity and greater presumed forethought for undertaking a camping trip are expected to yield more similarity in coefficients. Second, we need to incorporate more site characteristics into our demand specification. Including only surface acres as a proxy for site facilties ignores the probable importance of boat ramps, picnic tables, parking, etc. Unfortunately, these variables appear to be highly correlated with surface acres making their individual coefficient estimates highly imprecise. Use of principal components regression, ridge regression, or other methods for dealing with colinearity is high on our research agenda. Adequately addressing the colinearity may help to improve the transferability of estimated demand equations.

Conclusions

Across the best specified and predicting models, our evidence to date rejects the statistical equality of the three recreation demand equations for U.S. Army Corps of Engineer reservoirs. Comparing predicted use obtained from interchanging the three districts' demand coefficients further reinforces the view they are not completely transferrable, in that the price and quality elasticities are significantly different. In comparing the average consumer surplus, the data shows that Sacramento's coefficients transferred poorly in predicting average consumer surplus or visitor use in the other two districts. Nashville's coefficients did a much better job predicting recreation use and average consumer surplus in the Little Rock district and as did the Little Rock's coefficients in predicting at Nashville.

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Table 1. Comparison of Signs and Statistical Significance of Alternative Demand Model Specifications

VARIABLE	SACRAMENTO		LITTLE ROCK		NASHVILLE	
	Sign	Signif	Sign	Signif	Sign	Signif
DBLOG(VCAP)						
LTOTCOST(-)	-	***	-	***	-	***
LRECSA(+)	+	***	+	***	+	***
LLOGSUB(-)	-	***	-	***	-	***
LMEDAGE(+/-)	+	***	-	**	+	***
DBLOG(TOTVIS)						
LTOTCOST(-)	-	***	-	***	-	***
LRECSA(+)	+	***	+	***	+	***
LLOGSUB(-)	-	***	-	***	-	n.s.
LMEDAGE(+/-)	+	***	-	n.s.	+	*
LPOP(+)	+	***	+	***	+	***
NLS(VCAP)						
LTOTCOST(-)	-	***	-	***	-	***
LRECSA(+)	+	***	+	n.s.	+	***
LLOGSUB(-)	+	n.s.	-	n.s.	-	n.s.
LMEDAGE(+/-)	+	***	-	**	+	***
NLS(TOTVIS)						
LTOTCOST(-)	-	***	-	***	-	n.s.
LRECSA(+)	+	***	+	n.s.	+	n.s.
LLOGSUB(-)	-	***	-	***	-	n.s.
LMEDAGE(+/-)	+	**	-	*	-	n.s.
LPOP(+)	+	***	+	***	+	n.s.
HECKMAN(TOTVIS)						
LTOTCOST(-)	-	***	-	***	-	***
LRECSA(+)	+	***	+	***	+	***
LLOGSUB(-)	-	***	-	***	-	n.s.
LMEDAGE(+/-)	+	***	+	n.s.	+	n.s.
LPOP(+)	+	***	+	***	+	***
INV-MILLS		**		***		***

Where *, ** and *** indicates significance at the .1, .05 and .01 levels respectively.

Table 2. Regression Coefficients from NLS and Heckman Sample Selection

VARIABLE	SACRAMENTO	LITTLE ROCK	NASHVILLE
NON-LINEAR LEAST SQUARES-Visits Per Capita			
CONSTANT	-36.24	18.22	95.26
(Std Error)	(18.5)	(28.3)	(92.9)
LRECSA	1.05	.115	.42
	(.27)	(.10)	(.10)
LTOTCOST	-3.69	-1.69	-1.72
	(.23)	(.09)	(.09)
LOGSUB	4.44	-2.65	-16.03
	(3.33)	(4.07)	(14.15)
LMEDAGE	2.91	.75	2.95
	(.99)	(.42)	(1.07)
F	298	120	135
R^2	.77	.52	.27
N =	348	447	1430
HECKMAN-SAMPLE SELECTION, CONDITIONAL DEMAND-Total Day Trips			
CONSTANT	75.39	111.1	69.5
(Std.Error)	(42.92)	(36.58)	(85.26)
LTOTCOST	-4.74	-2.96	-3.39
	(.74)	(.24)	(.52)
LRECSA	1.24	.51	1.01
	(.33)	(.11)	(.14)
LOGSUB	-19.31	-16.93	-12.28
	(8.16)	(5.44)	(13.20)
LMEDAGE	6.65	.08	.87
	(2.17)	(.78)	(1.19)
LPOP	1.15	1.13	1.34
	(.33)	(.14)	(.22)
INV-MILLS	3.87	.97	1.51
	(1.55)	(.34)	(.48)
F	59.62	57.02	53.32
R^2	.57	.55	.52
N (>0)	267	279	296

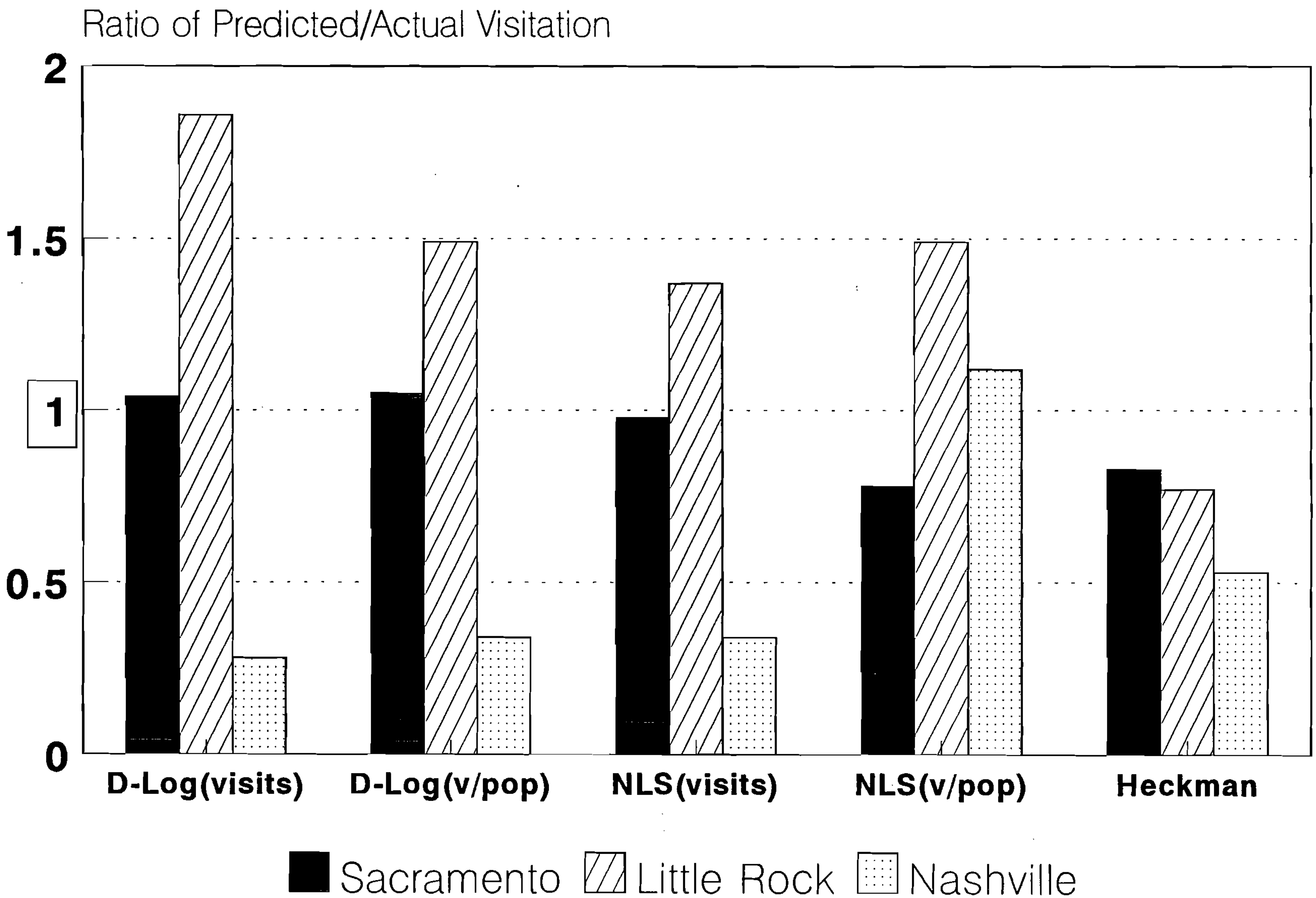
Table 3. **Comparison of NLS Estimates of Average Consumer Surplus Per Visit1 ($1980 Dollars)**

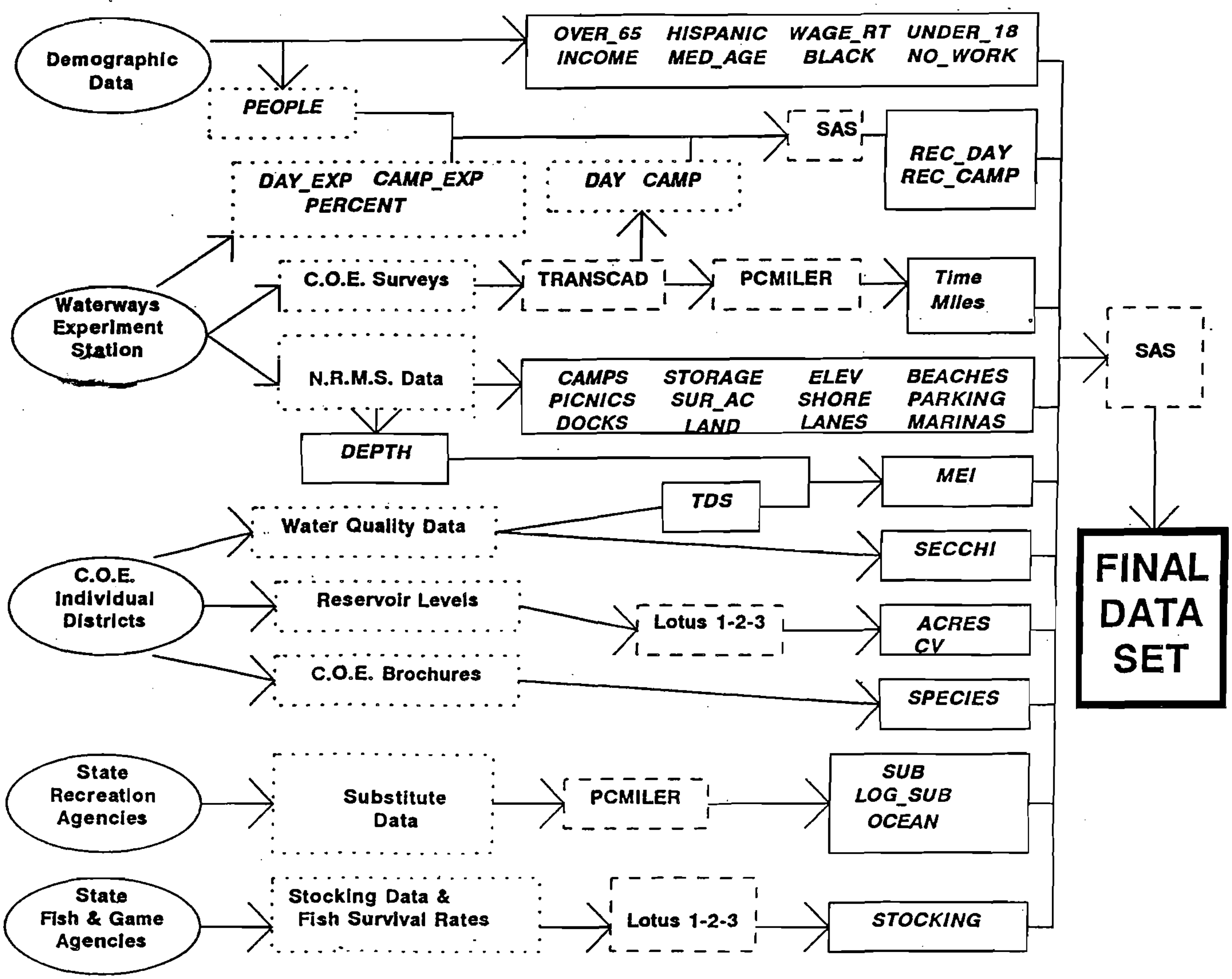
DISTRICT	OWN COEFFICIENTS	TRANSFERRED COEFFICIENTS	
	SACRAMENTO	LITTLE ROCK	NASHVILLE
Success	$3.07	$8.61	$8.34
Englebright	$2.61	$11.57	$12.08
Kaweah	$2.07	$7.05	$6.82
Eastman	$3.61	$10.07	$10.04
Hensley	$2.48	$9.21	$9.50
Mendicino	$2.11	$12.14	$11.31
New Hogan	$3.56	$11.23	$11.45
Black Butte	$2.67	$10.20	$10.08
Pine Flat	$2.32	$7.36	$7.31
Isabella	$5.70	$12.22	$11.77

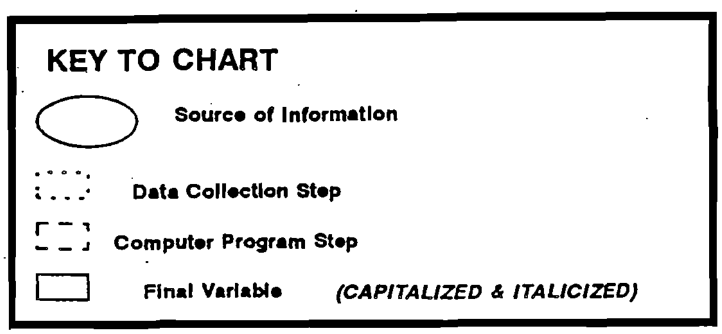
	LITTLE ROCK	SACRAMENTO	NASHVILLE
Blue Mountain	$11.41	$3.38	$10.92
Nimrod	$9.80	$2.56	$9.29
Norrork	$7.75	$1.35	$5.83
Beaver	$7.05	$1.42	$6.58
Millwood	$9.03	$1.95	$8.55
Dardanelle	$8.41	$1.83	$8.17
Table Rock	$7.89	$1.38	$6.73
Blue Shoals	$7.94	$1.40	$6.12

	NASHVILLE	SACRAMENTO	LITTLE ROCK
Laurel River	$7.96	$1.30	$8.16
Cheatham	$8.46	$1.82	$8.56
Cordell-Hull	$8.22	$1.29	$8.56
J.Percy Priest	$8.85	$2.46	$9.07
Center-Hill	$8.47	$1.64	$9.02
Dale Hollow	$12.96	$1.46	$10.57
Barkley	$8.18	$1.44	$8.84
Cumberland	$8.25	$1.38	$8.90

Model Specification Performance
Predicted/Actual Visitation




Demographic Data
OVER_65 HISPANIC WAGE_RT UNDER_18
INCOME MED_AGE BLACK NO_WORK
PEOPLE
SAS
REC_DAY
REC_CAMP
DAY_EXP CAMP_EXP
PERCENT
DAY CAMP
Waterways Experiment Station
C.O.E. Surveys
TRANSCAD
PCMILER
Time Miles
N.R.M.S. Data
CAMPS STORAGE ELEV BEACHES
PICNICS SUR_AC SHORE PARKING
DOCKS LAND LANES MARINAS
SAS
DEPTH
MEI
TDS
Water Quality Data
SECCHI
FINAL DATA SET
C.O.E. Individual Districts
Reservoir Levels
Lotus 1-2-3
ACRES
CV
C.O.E. Brochures
SPECIES
State Recreation Agencies
Substitute Data
PCMILER
SUB
LOG_SUB
OCEAN
State Fish & Game Agencies
Stocking Data & Fish Survival Rates
Lotus 1-2-3
STOCKING


KEY TO CHART
Source of Information
Data Collection Step
Computer Program Step
Final Variable (CAPITALIZED & ITALICIZED)

Benefits Transfer in a Random Utility Model of Recreation

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Benefits Transfer in a Random Utility Model of Recreation

Introduction

Time and other resource constraints often call for benefit-cost analyses of environmental policies that rely on models from existing studies. When such analyses are done the benefit or cost assessment is said to be "transferred" from an existing study to a policy site. For example, we may estimate the benefits of cleaning rivers in the state of Maine using the model from a study that estimated the benefits of cleaning rivers in Pennsylvania.

Little economic analysis has been done to judge the viability of such transfers. This is unfortunate given their widespread use. In this paper we present the results of an experiment designed to help judge the viability of transferring a Random Utility Model (RUM) of recreation. Our experiment follows the design of Atherton and Ben-Akiva (1976). For applications of the Random Utility Model (RUM) to recreation decisions see Bockstael, McConnell, and Strand (1989), Caulkins, Bishop, and Bouwes (1986), Hanemann (1978), Kaoru and Smith (1990), Milon (1988), Morey, Shaw, and Rowe (1990), and Parsons and Kealy (1992). For a discussion of issues concerning benefits transfer see the collection of papers appearing in *Water Resource Research* 28(3), 1992, and more recently Loomis, Roach, Ward, and Ready (1993).

We analyze a RUM of lake recreation in the state of Wisconsin. The model is based on the survey results of 603 individuals who visited lakes in the state. We divide the data set into two groups: (1) respondents living in Milwaukee County (n=117) and (2) respondents not living in Milwaukee County (n=486). Then, we transfer a model estimated with the non-Milwaukee sample (hereafter called the State sample) to the Milwaukee sample. Our (hypothetical) purpose throughout is to estimate the benefits of improving water quality for the 117 Milwaukee residents. Since we have the same information for individuals in this sample as we have on the State sample, we have a means of judging the viability of transfer.

The Milwaukee and non-Milwaukee populations are quite different. Milwaukee is urban. Most of the rest of the state is rural or smaller towns. The Milwau-kee sample has a lower average income and a lower average age. Residents of Milwaukee are close to Lake Michigan, while residents from outside the state are typically much further away. Lake Michigan is not included in our analysis and is an important 'unobserved' substitute site affecting the two populations. The lakes near Milwaukee are also dirtier on average than elsewhere in the state. These differences, we believe, challenge the viability of a transfer from the state to the Milwaukee sample.

Our experiment is in four steps:

First, we estimate separate RUMs of the same specification using the State sample and the Milwaukee sample and formally test the stability (or transfer-ability) of the model.

Second, we transfer benefits from the State model to our Milwaukee sample assuming we have no behavioral information on the Milwaukee residents. (By "behavioral information" we mean knowing where individuals made trips during the year.) We simply estimate benefits over the Milwaukee sample using the parameters estimated in the State model. In this sense our experiment is really a "model transfer". We compare these transfer estimates to benefits estimated for the Milwaukee sample with the Milwaukee model. We loosely treat these latter estimates as the 'true' benefits.

Third, we transfer benefits assuming we have behavioral information on a fraction of the Milwaukee sample. We use the behavioral information in three ways: (1) to estimate a new RUM using only the small Milwaukee sample and then use that model to estimate benefits (really no transfer at all); (2) pooling the small Milwaukee and State sample into a single data set to estimate a new RUM and then use that model to estimate benefits; and (3) to estimate a RUM using the small Milwaukee sample, using that model to update the existing State model, and then using the updated model to estimate the benefits. The third approach is our Bayesian transfer. Again, we compare the benefits estimated in these models with the 'true' benefits estimated in the full Milwaukee model.

Fourth, we compare Goodness-of-Fit measures and the predictive power of all of the models developed above. These are used as final measures of performance.

We begin with a discussion of the model and data used. The presentation here is short because the model and data are well documented in Parsons and Kealy (1992). We then turn to a discussion of the experiment and results, and close with conclusions.

The Data and Model

The Data

We use data from a 1978 random phone survey of Wisconsin residents, the "Statewide Water Quality Survey", and two supplementary sources, one on lakes and their characteristics and the other on travel distances and times. The survey was conducted by the Wisconsin Survey Research Laboratory and funded by the U.S. Environmental Protection Agency. The survey was done in the Fall and questioned people about their trips to Wisconsin lakes during the preceding 12 months. Lakes less than 100 acres large, Lake Michigan and Lake Superior were excluded. The remaining set included 1133 lakes.

Nearly 1000 individuals over 18 years old were interviewed. All were asked a list of questions pertaining to their socioeconomic status -- age, income, ownership of property on a lake, hometown, and so on. In addition, each person was asked to identify his or her primary use of Wisconsin lakes. The primary use categories are: boating, fishing, swimming, viewing (including picnicking and hiking), or no use.

Approximately 60% of the people surveyed made at least one visit to a Wisconsin lake during the year. These persons were asked to identify and estimate the number of trips taken to each of the lakes they visited. No person was questioned about more than six lakes. We only consider day trips in our analysis.

The lake characteristic data set is from the Water Resources Center at the University of Wisconsin, Uttormark and Wall (1976) and the Wisconsin Department of Natural Resources. It includes information for all 1133 lakes on acreage, depth, water quality, and measures of access such as presence of boat ramps.

We constructed a matrix of road distances and travel times between each interviewed person's hometown and the set of 1133 lakes using the software, HYWAYS/BYWAYS. This software computes road distances and travel times between more than 500 towns in Wisconsin. The travel time measure in HYWAYS/BYWAYS accounts for different average speeds over different routes -- travel on interstates is faster than travel on county roads, open road travel faster than city traffic, and so on. Travel time is not converted from a distance measure. The location of each lake was assumed to be the nearest town recognized by the software. We then estimated the travel cost of a trip to all 1133 lakes for each person.

The Model

We estimate a linear Random Utility Model of recreation site choice. Recreation includes boating, fishing, swimming, and viewing. In our model, individuals make a site (lake) choice for a recreation trip based on characteristics of the lakes. These characteristics include the following:

Cost of Reaching the Lake
Acreage of the Lake
Depth of Lake
Boat Ramp at the Lake (yes/no)
Inlet at the Lake (yes/no)
Commercial Facilities at the Lake (yes/no)
Located in a Remote Place (yes/no).
Located in a Northern County (yes/no)
High Dissolved Oxygen Level at the Lake (yes/no)
Low Dissolved Oxygen Level at the Lake (yes/no)
High Level of Water Clarity at the Lake (yes/no)

These characteristics are arguments in our Random Utility Model. We do not present the theory of RUMs applied to recreation decisions here. Our treatment is standard and is nicely outlined by Bockstael, McConnell, and Strand (1991). Also, our earlier paper (Parsons and Kealy (1992)) has a formal presentation of a version of the model used here.

Precise variable definitions for the arguments are given in Table 1. Our actual model specification is shown in Table 2. You will notice that we use some interaction terms to allow boat ramp, inlets, and depth to matter only to boaters and anglers and other interaction terms to allow water clarity to matter only to swimmers and viewers. We also interact our cost term with an income dummy to allow for some effect of diminishing marginal utility of income.

Because the number of lakes in an individual's opportunity set is potentially quite large in our data set (in the thousands), we estimate the RUM using a random draw of lakes. Each person's opportunity set is represented by the lake actually visited plus 11 more lakes randomly drawn from all lakes within 180 miles of an individual's hometown (we assume this is a maximum day's drive). See Parsons and Kealy (1992) for details on the draw procedure.

Many individuals in our sample make more than one recreation trip during the season. For each individual we assume that each trip decision is independent of previous and upcoming trip decisions. For example, in our State sample we have 486 individuals making 6,869 trips. We treat these as 6,869 independent trips in the Random Utility Model. This is admittedly a poor assumption but one that is often made.

Each lake in the state falls into one of three dissolved oxygen groups: highDO (DONO=0 and DOYES=1), mediumDO (DONO=0 and DOYES=0), and lowDO (DONO=1 and DOYES=0. The DONO and DOYES variables are defined in Table 1. HighDO is the best and lowDO is the worst. We estimate the benefits of hypothetically moving all lowDO lakes to the mediumDO level. About 16% of the lakes in the state are lowDO. About 28% of the lakes within 180 miles of Milwaukee are lowDO. We use standard measures of welfare change in RUMs to calculate per choice occasion benefits (again, see Bockstael, McConnell, and Strand). Although we estimate the model with 12 lakes in each individual's opportunity set, we estimate benefits using all lakes within 180 miles of each individual's home.

Tests of Transferability

In this section we estimate separate models using the State sample (# of individuals = 486, # of trips = 6,869) and the Milwaukee sample (# of individuals = 117. # of trips = 1,215) to test the stability (or transferability) of the model. The models are presented in Table 2 and for the most part are reasonable. Only NORTH, CF and DOYES in the Milwaukee model have unexpected signs. Otherwise, in both models individuals appear to prefer lakes that are close, large, and clean. Individuals that use lakes primarily for boating and fishing prefer deeper lakes with boat ramps and inlets. There is evidence of a preference for lakes located in the northern part of the state (a more natural setting than the south) in the State sample but not in the Milwaukee sample. Similarly, the presence of commercial facilities seems to be a plus for individuals in the State sample and a drawback for individuals in the Milwaukee sample.

Our first test is of the null hypothesis that the set of coefficients for the State model are the same as the set of coefficients for the Milwaukee model over the Milwaukee sample. We take the State model coefficient estimates and compute the log-likelihood over the Milwaukee sample. Then, we take the Milwaukee model coefficient estimates and compute the log-likelihood over the same sample. Using these values we compute a standard log-likelihood ratio that is known to have a $\chi^2(12)$ distribution. This is our test statistic for model transferability. For our models this ratio is 768 (-2*(-1407.3 + 1791.1)). The probability of exceeding this ratio is less than one, so we strongly reject the null hypothesis that the sets of coefficients are the same.

Our next text of transferability is a comparison of the individual coeffi-cients used in the two models. In Table 2 we present the t-statistics testing the hypothesis that the individual State and Milwaukee coefficients are equal. These are shown in the third column of the table. Only PRICE*INCDUM, LNACRES, INLET*BF, and DONO (the key water quality variable in our transfer) are stable, i.e., not statistically different across models at a 95% confidence level.

Although these results caution against transferring the State model to the Milwaukee sample, we notice that the degree of difference between many of the coefficients in the model is not large. This is especially true for the critical coefficients to be used in our benefits assessment -- PRICE, PRICE*INCDUM, and DONO. Hence, the State model may still provide a reasonable approximation for estimating water quality benefits. Next, we transfer the State model to the Milwaukee sample to judge this approximation.

Benefits Transfer without Behavioral Information

We consider two cases in this section. In both cases we assume we have no information on the lakes visited or the number of trips taken by anyone in Milwaukee. In the first case we also assume that we know nothing about the characteristics of lakes within 180 miles of Milwaukee or the characteristics of individuals living in Milwaukee. That is, we have no data with which we can simulate our State model.

In this case, we estimate the per choice occasion benefits of water quality improvements per person over the State sample, calculate the sample mean, and use that mean as an estimate of the mean benefits for Milwaukee residents. Implicitly, we assume that the residents of Milwaukee have the same preferences and incomes and face the same opportunities (lakes) as the State residents. The estimate, presented in Table 3 is $0.44 per person (in 1978 dollars). We refer to this case as the Simple Transfer. This approach is similar to using unit day values from existing studies to estimate the value of a recreation day at a policy site. No policy site information is used to adjust or amend an existing per person point estimate.

In our second case we assume we have the information necessary to simulate the State model over the Milwaukee sample. We simply calculate the benefits for the residents in the Milwaukee sample using the parameters of the State model. Here, we implicitly assume that the structure of preferences of the Milwaukee residents is the same as the State residents. But now, we account for the different opportunities (lakes) they face and the different incomes they have. The estimated mean benefits per person is $0.65 and is also reported in Table 3. We refer to this transfer as the State Model Transfer.

The State Model Transfer results are substantially (48%) larger than the Simple Transfer. Preference structure is being held constant across these transfers; we use the Random Utility Model estimated for State residents in both cases. The increase here is due to the differences in recreation opportunities. The Milwaukee sample has larger benefits because there are more dirty lakes nearby (lakes that fall in the lowDO category).

The income distribution in Milwaukee works to lower benefits in the State Model Transfer against the Simple Transfer but seems to have little effect relative to the 'more dirty lakes' impact. To understand the effect of income, first recognize that the RUM benefit assessment has the following form:

$$\frac{\Delta(\textit{Expected Utility})}{\beta_y} \tag{1}$$

where β_y is the marginal utility of income. In our models, β_y is the negative of the coefficient on the PRICE term for individuals with incomes above $12,500 and is the negative of the sum of the coefficients on PRICE and PRICE*INCDUM for individuals with incomes below $12,500. For high income individuals $\beta_y = .22$ and for low income individuals $\beta_y = .28$. (Our model has evidence of diminishing marginal utility of income). By equation (1) lower income individuals receive lower benefits from the water quality improvement. In the Milwaukee sample 60% of the individuals have incomes less than $12,500, while only 54% of the State sample falls in the lower group. This should work to lower benefits in the State Model Transfer against the Simple Transfer. The effect seems to be small.

Our "true" measure of mean choice occasion benefits for the Milwaukee sample is $0.55. This is estimated using the Milwaukee model over the Milwaukee sample. Hence, we understate benefits in the Simple Transfer (by 20%) and overstate benefits in the State Model Transfer (by 18%).

Comparing the State Model Transfer with the True Model, we hold opportun-ities constant (same set of lakes), so differences in benefits are due to difference in preference structure. The per choice occasion benefits in the "True" Model fall below the State Model Transfer largely because the coefficient estimate on DONO is lower in the Milwaukee model (-.79) than the State model
(-1.0). This implies that the residents of Milwaukee care less about water quality improvements than the residents of the State. The lower coefficient on PRICE in the Milwaukee Model on the other hand, works to raise benefits in the "True" Model versus the State Model Transfer. This effect of higher marginal utility of income is apparently less than the effect of lower concern for water quality.

Comparing the Simple Transfer to the True Model, preference structure and opportunities differ. The Milwaukee residents care less about water quality improvements but have more dirty lakes nearby. The latter effect dominates so Milwaukee residents have larger benefits. But, this raises an interesting point. Suppose the Milwaukee model coefficient on DONO was even lower (in absolute value) -- Milwaukee residents truly care even less about water quality relative to State residents. If so, the true benefits would move closer to the Simple Transfer estimate even though we are making preference structures diverge more. The effect of difference in preferences is being used to offset the effect of differences in opportunities. Understanding the degree of these types of offsetting effects, or even if effects are simply offsetting, is critical in assessing a transfer. Under some circumstances the Simple Transfer may out-perform the State Model Transfer simply due to offsetting impacts.

In our judgement 18 to 20% deviations from what we accept as truth makes for a reasonably accurate transfer. Next, we turn to how we might incorporate behavioral information on the Milwaukee residents in our hypothetical benefits transfer.

Benefits Transfer with Behavioral Information

Now we assume that we have behavioral information for a fraction of the Milwaukee sample. We know how many trips and which lakes were visited by some of the individuals in the sample. We assume alternately that we have information on 13, 28, and 55 randomly chosen individuals from the Milwaukee sample. In each case we use the behavioral information to estimate three models:

(1) RUM with the Milwaukee data only.

(2) RUM with the combined Milwaukee and State data.

(3) RUM by updating the State model with a new Milwaukee model.

In approach (3) we follow conventional Bayesian Statistics. We treat the parameters estimated in our State model as our "prior information". The parameters estimated in our Milwaukee model are treated as "sample information". We use the sample information to "update" our prior information. Our updated parameter estimates are computed as follows.

$$\underline{\hat{\beta}} = \left[V(\underline{b}_N)^{-1} + V(\underline{b}_M)^{-1}\right]^{-1} \left[V(\underline{b}_N)^{-1}\underline{b}_N + V(\underline{b}_M)^{-1}\, \underline{b}_M\right] \qquad (2)$$

where

$\underline{\hat{\beta}}$ is the (kx1) Bayesian estimator,

$\underline{b}_N$ is a (kx1) parameter estimates from the State model,

$\underline{b}_M$ is a (kx1) parameter estimates from the Milwaukee model,

$V(\underline{b}_N)$ is a (kxk) variance/covariance matrix, and

$V(\underline{b}_M)$ is a (kxk) variance/covariance matrix.

Our updated parameter estimates, $\underline{\hat{\beta}}$, are a weighted average of the State and Milwaukee parameter estimates -- weighted by the inverse their variance/covariance matrices. The weighting has intuitive appeal. If the variance on our prior $\underline{b}_N$ is small (perhaps due to a large sample) and the variance on our sample $\underline{b}_M$ is large, then $\underline{b}_N$ is given more weight and dominates our Bayesian estimate of $\underline{\hat{\beta}}$. If the relative sizes of the variances is reversed, greater weight is given to the sample parameter $\underline{b}_M$. The intuition really applies by individual coefficient within the parameter vectors -- some may be dominated by the prior, others by the sample, and still others given an equal weighting.

Atherton and Ben-Akiva note a few properties of the Bayesian update estimates that are worth repeating. First, subjective information can easily be entered via the variance/covariance matrix. If an analyst has a well supported argument for giving a prior parameter estimate little weight, then the prior variance can be increased thereby reducing the weight it is given. Second, the updating can be done with very small samples at the policy site. Alone, these surveys may be of little use but when used to update a widely accepted study with a much larger sample they may be very useful in an updating approach. Third, the prior parameter vector and the sample parameter vector need not be the same. Either can be a subset of the other. Parameters that overlap are updated, others are not.

The benefit estimates are presented in Table 3 as cases 3 through 5 in the bottom half of the table. For each of our experiments we present the estimates (always a sample average over the 117 Milwaukee residents) using a Milwaukee only regression, using a pooled Milwaukee and State regression, and using our Bayesian Transfer. To save space we have not presented the Pooled and Bayesian parameter estimates.

In all three cases (n=13, n=28, and n=55), the Milwaukee Only Models do worse than the Pooled and Bayesian Transfers. Using the sample data to update the existing State Model gives a model that more accurately estimates benefits for the Milwaukee sample. With the exception of the n=55 case the Pooled and Bayesian Transfer also outperform the State Model Transfer. The Pooled Transfer is a slight favorite. In the n=13 and n=55 case the benefit transfer error is only +4%. Using small sample data to update existing models seems to payoff.

In the following section we offer a few tests to judge how well each of our transfer models predicts behavior over the Milwaukee sample. As we know from the previous section close transfer estimates need not coincide with accurate models of behavior.

Testing the Predictive Power of the Transfer Models

Following Atherton and Ben-Akiva, in this section we evaluate the performance of our transfer models over the Milwaukee sample using two measures: Goodness-of-Fit to the Milwaukee data and predicting ability.

Our goodness-of-fit measure is the standard:

$$\rho^2 = 1-\{\mathcal{L}(\hat{\underline{\beta}})/\mathcal{L}(\underline{o})\} \qquad (3)$$

where

$\mathcal{L}(\hat{\underline{\beta}}_i)$ = the log-likelihood of the transfer model i over the Milwaukee sample, and

$\mathcal{L}(\underline{o})$ = the log-likelihood of the Milwaukee sample for $\underline{\beta} = \underline{o}$.

This approach is convenient because it provides a scalar measure of performance across models. The results are shown in Table 4.

For the cases with n=13 and n=28, the Pooled Transfer and Bayes Transfer outperform the Milwaukee only regressions by a considerable margin. Even the State Model Transfer outperforms these Milwaukee only regressions. Between the Pooled, Bayesian, and State Model Transfer, the Pooled Transfer does best. In the case with n=55, our findings change. Here the Milwaukee only regression performs best (.462). The Pooled Transfer (.445) still does better than the Bayesian Transfer (.436), and the performance of the Model Simulation (.404) now falls far behind the updating models.

Next, we consider the relative predictive power our transfer models. We selected four lakes in the State (Pewaukee, Okauchee, Fox, and Wazeecha) and used each model to predict the total number of trips made to these lakes by the Milwaukee sample. We compare these to the actual number of trips made by Milwaukee residents. The results are reported in Table 5.

The models all underestimate trips to Pewaukee Lake (the most frequently visited lake by individuals in our sample). The Pooled Transfer is a slight favorite over the other models. For Okauchee Lake all models overestimate the number of trips. Here, the Bayesian Transfer is a clear winner. For Fox Lake all models seriously understate benefits and there is little variation in their predictive performance. Finally, for the distant Wazeecha Lake, all models accurately predicted zero trips.

Generally, the results are mixed in this test. The Pooled, Bayesian, and State Model all do better than Milwaukee only models, but among these three transfer models it is difficult to select a winner.

Conclusions

Although we find that the State and Milwaukee models are significantly different in formal statistical tests, the State Model transferred to the Milwaukee sample estimates the benefits of water quality improvements with a fair degree of accuracy (within 18% of our "true" model). While difference in the structure of the models is significant statistically, the differences are not large. Even the Simple Transfer which accounts for no information on the Milwaukee sample does reasonably well (within 20% of the "true" model). However, we showed that offsetting effects in this case mask how well we are actually modelling behavior.

We do not intend to paint a general rosy picture for benefits transfer. Clearly, the degree of error found here will not be the same for any transfer. Indeed, many features of our study model and policy site are uncharacteristic of actual transfer exercises: the study (State) and policy (Milwaukee) site are in the same state, the study and policy site share some of the same lakes, the data analyzed are from the same survey, and model simulated at the policy site is exactly the same as the study site. Those are unusually favorable circumstances for a transfer and must be considered in weighing the generality of our findings. (Of course, it is exactly some of these features that allowed us to perform a test.)

We also found that updating the State models with behavioral information from small Milwaukee samples can be used to improve the performance of the benefits transfer. Pooled and Bayesian transfers help; the Pooled models have perhaps a slight edge. Researchers should perhaps consider small data gathering efforts at policy sites, specifically for the purpose of updating accepted models over a resource similar to that at the policy site. (Choose a model, then consciously gather data at the policy site for updating.)

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Table 1

Variable Definitions for Lake Characteristics
in the Random Utility Model

Variable Name[1]	Description
PRICE[2] (40,20)	Opportunity Cost of Time + Travel Cost: {(1/3)*(Annual Income/2080) * Travel Time to and from Lake} + {.10 * (Distance to and from Lake)}. Note: There are 2080 working days in the year; .10 is the cost per mile (in dollars) of operating a car in 1978.
LNACRES (5.7,.95)	Logarithm of acreage of the lake.
LNMXD (3.1,.73)	Log of maximum depth of the lake.
REMOTE (.07,.25)	REMOTE = 1 if lake is in a remote location and 0 if not. Note: Remote means that the lake can only be reached by navigable water or is located in a public wilderness area without a road or defined trail within 200 feet.
BR (.45,.50)	BR = 1 if a boat ramp is present at lake and 0 if not.
INLET (.67,.47)	INLET = 1 if lake has an inlet and 0 if not.
CF (.65,.48)	CF = 1 if commercial facilities are present and 0 if not. Note: Commercial facilities can be restaurants, bait shops, hotels, or boat services.
NORTH (.75,.43)	NORTH = 1 if lake is located in a northern county and 0 if not. Note: Northern counties include: Douglas, Bayfield, Ashland, Iron, Vilas, Forest, Florence, Burnett, Washburn, Sawyer, Price, Oneida, Marinette, Polk, Barron, Rusk, Lincoln, Langlade, and Oconto.
DOYES	DOYES = 1 if dissolved oxygen in hypolimnion is greater (.07,.25) than 5 ppm virtually all the time and 0 if not.

Table 1 (continued)

DONO	DONO = 1 if the entire hypolimnion is void of oxygen at critical times and 0 if not.	(.16,.37)
INCDUM (.55,.50)	INCDUM = 1 if household income $\leq$ \$12,500 and 0 if not.	
FB (.36,.48)	FB = 1 if the individual uses Wisconsin lakes primarily for fishing or primarily for boating and 0 if not.	
SV (.24,.43)	SV = 1 if the individual uses Wisconsin lakes primarily for swimming or primarily for viewing and 0 if not.	

Notes:

1 Mean values and standard deviations are given in parentheses beneath the variable name: (mean, standard deviation). The means and standard deviations for LNACRES, LNMXD, REMOTE, BR, INLET, CF, NORTH, DOYES and DONO are computed for the set of 1133 lakes. The means and standard deviations for INCDUM, FB, and SV are computed over the individuals in the sample. The mean and standard deviation on PRICE is computed for the data set used to estimate the Random Utility Model.

2 Annual income was missing for approximately 15% of the sample. For these individuals we predicted income using a linear regression. The following regression was estimated using the portion of the sample that report their income:

$$\underset{(7)}{\text{INCOME}} = \underset{(9)}{-138} + \underset{(9)}{7.6*\text{AGE}} - \underset{(8)}{.08*\text{AGE}^2} + 8.3*\text{EDUCATION}$$

$$+ \underset{(3)}{110*\text{d1}} + \underset{(2)}{52*\text{d2}} + \underset{(3)}{126*\text{d4}} + \underset{(10)}{54*\text{MARRIED}}$$

where INCOME and AGE are defined above; EDUCATION is years of schooling, d1 = 1 if individual is a lawyer and 0 if not, d2 = 1 if individual is an engineer and 0 if not, and d3 = individual is a physician and 0 if not; MARRIED is 1 if individual is married. (t-statistics are in parentheses below coefficients). Annual income was then predicted using this equation for those not reporting income.

Table 2

Maximum Likelihood Estimates of State and Milwaukee
Random Utility Models

VARIABLES:	STATE	MILWAUKEE	T-STATISTIC FOR DIFFERENCE IN COEFFICIENT
PRICE	-.22 (40)	-.17 (18)	4.5
PRICE*INCDUM	-.06 (7.0)	-.07 (4.5)	0.8
LNACRES	.64 (34)	.60 (19)	0.9
NORTH	.65 (6.7)	-4.6 (4.2)	4.8
CF	1.0 (15)	-1.0 (10)	16.7
REMOTE	-1.2 (8.8)	-.52 (3.2)	3.3
LNMXD*FB	.59 (13)	.35 (3.7)	2.3
BR*FB	.14 (1.9)	.75 (4.7)	3.5
INLET*FB	.93 (8.0)	.92 (3.3)	0.0
DONO	-1.0 (12)	-.79 (7.3)	1.6
DOYES	.71 (6.6)	-4.3 (2.1)	2.5
CLEAR*SV	1.3 (10)	1.9 (8.9)	2.7

	STATE	MILWAUKEE
Log-Likelihood	-4337.2	-1407.3
Log-Likelihood (β=0)	-17034	-3005
Number of Individuals	486	117
Number of Trips	6869	1215

T-statistics are given in parentheses beside the coefficient estimates. Variables are defined in Table 1.

Table 3

Mean Values of Transferred Benefit Estimates
(1978 Dollars)

	Per Choice Occasion Benefits	Deviations from the "True" Model
"TRUE" MILWAUKEE MODEL	**$ .55**	
TRANSFER MODELS:		
Cases with no Behavioral Information for Milwaukee:		
1. Simple Transfer	.44	(-20%)
2. State Model Transfer	.65	(+18%)
Cases with Behavioral Information for Milwaukee:		
3. Behavioral Data Available for 13 Individuals		
A. Milwaukee Only	.47	(-15%)
B. Pooled Transfer	.57	(+ 4%)
C. Bayesian Transfer	.63	(+15%)
4. Behavioral Data Available for 28 Individuals		
A. Milwaukee Only	1.08	(+96%)
B. Pooled Transfer	.66	(+20%)
C. Bayesian Transfer	.65	(+18%)
5. Behavioral Data Available for 55 Individuals		
A. Milwaukee Only	.31	(-56%)
B. Pooled Transfer	.57	(+ 4%)
C. Bayesian Transfer	.62	(+13%)

Note: The total number of trips taken by individuals in the n=13 case is 275. In the n=28 case the total number of trips is 228; in the n=55 case it is 653.

Table 4

Goodness-of-Fit (ρ^2) Measures for
Models Fit to Milwaukee Data

	Goodness-of-Fit (ρ^2)
"TRUE" MILWAUKEE MODEL	**.532**
TRANSFER MODELS:	
Cases with no Behavioral Information for Milwaukee:	
1. Simple Transfer	N/A
2. State Model Transfer	.404
Cases with Behavioral Information for Milwaukee:	
3. Behavioral Data Available for 13 Individuals	
A. Milwaukee Data Only	.309
B. Pooled Transfer	.418
C. Bayesian Transfer	.413
4. Behavioral Data Available for 28 Individuals	
A. Milwaukee Data Only	.229
B. Pooled Transfer	.408
C. Bayesian Transfer	.405
5. Behavioral Data Available for 55 Individuals	
A. Milwaukee Data Only	.462
B. Pooled Transfer	.445
C. Bayesian Transfer	.436

Table 5. Success of Competing Models in Predicting Sites Visited by Individuals in the Milwaukee Sample				
	Lake Name			
	Pewaukee	Okauchee	Fox	Wazeecha
Actual number of trips made by individuals in Milwaukee sample	243	54	56	0
State Model	138.8	68.9	.7	0
Milw 13	45.8	404.4	.9	0
Pooled 13	181.1	105.2	1.0	0
Bayes 13	123.3	59.1	.7	0
Milw 28	173.5	206.8	.7	0
Pooled 28	157.11	83.4	.8	0
Bayes 28	131.1	68.5	.7	0
Milw 55	109.1	238.9	1.4	0
Pooled 55	167.0	107.3	1.2	0
Bayes 55	113.6	59.1	.9	0
True Model	93.5	67.6	5.8	0

RPA Assessment of Outdoor Recreation: Current Applications and Future Directions

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RPA Assessment of Outdoor Recreation: Current Applications and Future Directions

Introduction

The objectives of this paper are to provide a review of outdoor recreation demand and supply analyses conducted for previous RPA Assessments, examine current applications of the recreation demand and supply analyses conducted for the 1989 RPA Assessment, and present a framework for conducting recreation demand and supply analyses in future RPA Assessments. In Section II, recreation demand and supply analyses conducted for the 1975, 1980, and 1989 RPA Assessments are reviewed briefly. Current applications of the 1989 RPA Assessment recreation demand and supply assessment results are reviewed in Section III. A general conceptual framework for addressing some of the issues and problems related to the application of the Assessment results to resource policy and management is discussed in Section IV. This discussion leads to some general implications, discussed in Section V, for recreation demand and supply analyses conducted for future RPA Assessments.

Review of Outdoor Recreation Demand and Supply Analyses Conducted for Previous RPA Assessments

In 1974, the U.S. Congress passed the Forest and Rangeland Renewable Resources Planning Act (RPA). This legislation requires the Secretary of Agriculture to conduct an assessment of the demand and supply situation for the nation's forest and rangeland resources every 10 years. The RPA was extensively amended in 1976 by the National Forest Management Act (NFMA). In addition to specifying detailed procedures and guidelines for National Forest management, the NFMA amendments officially linked the RPA process and products to National Forest management (Shands, 1981). Additional minor amendments were added to the RPA by the Food, Agriculture, Conservation, and Trade Act of 1990. These amendments direct the Secretary of Agriculture to assess the effects of global climate change on forest and rangeland resources as part of the RPA assessment process.

To date, the Secretary of Agriculture has designated the U.S.D.A. Forest Service as the lead agency for conducting the RPA Assessment. The first RPA Assessment was published in 1975, a year after the passage of the 1974 Act. The second Assessment was published five years later in 1980. The third Assessment (the 1989 Assessment) was completed in 1989 and published in 1990 - commencing the 10 year interval for publishing future Assessments. The three Assessments published to date are briefly reviewed next. It is important to note that in the previous RPA Assessments, the terms "demand" and "supply" are used both in a technical sense and a broad sense. In the technical sense, the terms refer to economic demand and economic supply. In the broad sense, "demand" refers to overall participation or consumption, and "supply" refers to the overall availability of recreational opportunities. In this paper, the terms "demand" and "supply" are also used in these technical and broad senses.

1975 Assessment

__Demand Assessment__. In the 1975 Assessment, outdoor recreation demand was assessed using a two-step approach. In the first step, a participation function was estimated for various recreational activities. The participation functions were used to estimate the proportion of the U.S. population that participated in each activity. In the second step, the quantity of recreation demanded per participant was calculated using estimated participant demand functions. The total quantity of activity k demanded was estimated by multiplying total population by the probability of participation in activity k by the per capita quantity of activity k demanded by participants (Adams, Lewis, and Drake, 1973; Cicchetti, Seneca, and Davidson 1969; Kalter and Gosse 1969).

The participation functions estimated in the first step of the recreation demand assessment were specified as:

(1) $Y=f(INC,EDUC,RES,CENRG,AGE,RACE,SEX,MARS,FAMSZ,PHYS,WORK,VAC)$

where, Y = variable indicating whether individual i participated in activity k (1=participant; 0=nonparticipant), INC = individual family income, EDUC = individual education level, RES = individual residence (urban or rural), CENRG = U.S. Bureau of Census region where individual resides, AGE = individual age, RACE = individual race, SEX = individual sex, MARS = individual marital status, FAMSZ = size of individual's family, PHYS = variable indicating whether or not the individual was physically challenged, WORK = number of days worked per week by the individual, VAC = number of vacation days taken per year by the individual.

Participation equations corresponding to (1) were estimated for outdoor recreational activities using data from the 1972 National Recreation Survey. Separate participation equations were estimated for vacations, trips, and outings. Vacations were defined as "the most recent overnight journey taken during the summer quarter of 1972". Trips were defined as "other overnight excursions". Outings were defined recreation occasions which occurred "within one day" (Adams, Lewis, and Drake, 1973).

The participant demand functions estimated for the second step of the demand assessment process were specified as:

(2) $$Q = f(PRC,INC,AGE,RACE,SEX,CENRG)$$

where Q = average number of activity k days demanded by participant i[1], PRC = average price or cost of an activity day, and all other variables are as defined for Equation (1). Equation (2) was estimated using data from the 1972 National Recreation Survey (Adams, Lewis, and Drake, 1973).

Equations (1) and (2) were used to predict the number of recreation activity days Americans would demand through 1978. The prediction procedure first involved projecting future changes in the independent variables in (1) and (2). These projections were then substituted into (1) and (2) in order to estimate future participation rates and quantity of days demanded per participant. Estimated participation rates and estimated quantity demanded per participant were combined with projections of future population to estimate the total number of activity days demanded in the future (Adams, Lewis, and Drake 1973). For reporting in the 1975 Assessment document, demand projections for each activity were converted to indices with 1975 as the base year (U.S.D.A. Forest Service 1977, page 56).

Supply Assessment. The supply analysis for the 1975 Assessment was more limited than the demand analysis. A number of secondary sources were used to develop estimates of the current (1974-75) levels of recreational resources and facilities which support the recreational activities considered in the demand analysis. The total number of public and private developed campsites, for example, was estimated from published campground directories (U.S.D.A. 1977, pp. 57-74).

Demand/Supply Comparison. The 1975 Assessment did not provide a separate quantitative or qualitative comparison of future demand and supply trends. The 1975 Assessment did contain a brief, qualitative assessment of opportunities for increasing the supply of recreation resources and facilities. In certain cases, this qualitative supply assessment was combined with the quantitative demand projections to provide general observations and insights on the future demand and supply situation for outdoor recreation (U.S.D.A. 1977, pp. 57-74).

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[1]An activity day was defined as "one person participating in an activity for any part of one calendar day" (Adams, Lewis, and Drake, 1973).

1980 Assessment

Demand Assessment. The first step in the demand analysis for the 1980 Assessment was to estimate participation functions for recreation activities which were similar to the participation functions specified for the 1975 Assessment. These participation functions were specified as:

(3) Y=f(PRIC,INC,EDUC,RES,AGE,RACE,SEX,WORK,VAC,PARKS,FACIL)

where, PRIC = proxy for the price or cost of participation, PARKS = the number of parks within a close proximity, FACIL = available recreation resources and facilities, and all other variables are as specified for Equation (1). Data for estimating the participation functions were obtained from several sources including the 1977 National Outdoor Recreation Survey, the 1972 Bureau of Outdoor Recreation Survey of Public Recreation Facilities, and the 1973 National Association of Conservation Districts Private Sector Recreation Inventory (Hof 1979; Hof and Kaiser 1983).

In the second step of the demand analysis, projections of future values for the right-hand-side variables were substituted into Equation (3) to estimate expected participation rates (defined as the percent of the U.S. population which was expected to participate in each activity). The expected number of future participants was then estimated by multiplying the projected participation rate by the projected population. Participant projections for each activity were converted to indices with 1977 as the base year (U.S.D.A. 1980, pp. 100-101).

Supply Assessment. The supply assessment in the 1980 Assessment was limited to estimating and reporting the current levels of resources and facilities which support certain recreation activities. No attempt was made to estimate long-run projections of supplies of recreation resources and facilities. Various secondary sources were used to estimate the current quantity of recreation resources and facilities. As compared to the 1975 Assessment, the 1980 Assessment contained expanded information on the quantity of privately-owned recreation resources and facilities which were available to the public (U.S.D.A. 1980, pp. 105-143).

Demand/Supply Comparisons. As in the 1975 Assessment, the 1980 Assessment provided a qualitative assessment of the opportunities for increasing the supplies (quantities) of recreational resources and facilities in the future. This qualitative assessment of future recreation resource and facility availability ("supply") was combined with the quantitative assessment of future participation ("demand") to provide qualitative comparisons of future recreation demand and supply trends. As compared to the 1975 Assessment, the comparisons of recreation demand and supply made in the 1980 Assessment were more extensive and focused. However, these comparisons were still very broad and general (U.S.D.A. 1980, pp. 119-143).

1989 Assessment

<u>**Demand Assessment**</u>. In the 1975 and 1980 Assessment, recreation demand was modeled at the individual level. In the 1989 Assessment, recreation demand was modeled at an aggregate or community level. The aggregate demand functions estimated for the 1989 Assessment were specified as:

$$\text{DTRIPS} = f(\text{PRICE},\text{POPC},\text{INCC},\text{AGEC},\text{FARMC},\text{SUITC},\text{SUBSC}) \tag{4}$$

where, DTRIPS = total quantity of activity k trips demanded by community i, PRICE = price or cost of an activity k trip, POPC = total community i population, INCC = percent of community i population with income greater than or equal to $30,000 per year, AGEC = percent of community i population age 18 to 32 years old, FARMC = percent of community i population living on farms, SUITC = average suitability of sites available to community i for activity k, and SUBSC = an index of substitute recreational opportunities available to community i. The primary sources of data for estimating these equations was the Public Area Recreation Visitors Study (PARVS) conducted between 1985 and 1989, and U.S. Census data (Cordell and Bergstrom, 1991; Bergstrom and Cordell, 1991).

The estimated demand functions were used to estimate projections of "maximum preferred demand" for various outdoor recreational activities. Maximum preferred demand was defined as the amount of recreational trips Americans would desire to consume in the future if the price or cost of trips remained the same as in 1987. For reporting purposes, maximum preferred demand projections for each activity were converted to indices with 1987 as the base year (Cordell, Bergstrom, Hartmann, and English 1990, page 44; U.S.D.A. 1989, pp.27-29).

<u>**Supply Assessment**</u>. In the 1989 Assessment, recreation supply was conceptualized as having two components: a physical component and a human component. The physical component encompasses the recreation resources and facilities which support recreational activities. The human component encompasses what people do with these

resources and facilities. More specifically, the human component refers to the process by which people combine recreation resources and facilities with their own equipment, time, talents, and skills to "produce" outdoor recreational trips. The theoretical basis for this trip "production" process is household production theory (Cordell and Bergstrom 1991).

In order to assess the physical component of recreation supply, an inventory of recreation resources and facilities (e.g., swimming pools, beaches) available to communities was conducted using various secondary sources. The quantities of available recreation resources and facilities were then weighted by population and distance to calculate the "effective amounts" of recreation resources and facilities which were available to communities (Cordell, Bergstrom, Hartmann, and English 1990, pp. 49-54; U.S.D.A. 1989, pp. 25-27).

The quantities of recreational trips communities were expected to produce and consume using the effective amounts of recreation resources and facilities were calculated by first estimating aggregate consumption functions of the form:

$$(5) \qquad CTRIPS = f(POPC,INCC,AGEC,FARMC,SUITC,SUBSC,ROC)$$

where, CTRIPS = total quantity of activity k trips consumed by community i, ROC = effective amounts of recreational resources and facilities available to community i for activity k, and all other variables are as defined for Equation (4). The primary source of data for estimating Equation (5) was the Public Area Recreation Visitors Study (PARVS) and U.S. Census data (Cordell and Bergstrom, 1991).

After estimating (5) for various outdoor recreational activities, future changes in the right-hand-side variables, including the effective amounts of recreation resources and facilities, were projected. These projections were then substituted into the estimated consumption functions to calculate the amount of recreational trips communities were expected to produce and consume in the future. Following the household production theory framework, the projections of expected consumption of recreation trips were interpreted in the 1989 Assessment as the future "expected supply" of recreational trips. For reporting purposes, the projections of expected supply were converted to indices with 1987 as the base year (Cordell, Bergstrom, Hartmann, and English 1990, page 59; U.S.D.A. 1989, page 30).

Demand/Supply Comparisons. The 1989 Assessment contained formal, quantitative comparisons of recreation demand and supply. The demand assessment conducted for the 1989 Assessment generated projections of the amount of recreational trips Americans would prefer to take in the future given changes in population size and characteristics, but holding the price or cost of trips constant.[2] The supply assessment generated projections of the amount of recreational trips Americans would be able to take in the future given changes in population size and characteristics, as well as changes in the effective amounts of recreation resources and facilities which may cause the price or cost or trips to increase (or decrease).[3]

In the 1989 Assessment, the recreation demand and supply projections were compared to calculate expected "gaps" between the future demand and supply of recreational trips for each activity. These "gaps" represented the difference between the quantity of trips Americans would prefer or desire to take in the future (maximum preferred demand) and the quantity they would actually be able to take in the future (expected supply). Expected gaps for each activity were reported on a percentage basis (Cordell, Bergstrom, Hartmann, and English 1990, page 67).

Overview of Current Applications of the 1989 Assessment Results

How are the Assessment results used? What issues and problems have arisen in applications of the results to forest policy and management? How can the usefulness of the Assessment results, in general, be increased for resource policy and management? These and other questions were addressed by conducting an informal survey of individuals in various positions who, in some form or fashion, make use of the Assessment results.

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[2]The assumption of constant trip price or cost implies that the effective amounts of recreational resources and facilities must increase at rates sufficiently high to keep the price or cost of participating in outdoor recreational activities constant. Thus, maximum preferred demand can also be interpreted as "unconstrained consumption" of trips with respect to recreation resources and facilities.

[3]The "expected supply" of recreational trips takes into account the fact that effective amounts of recreation resources and facilities available to communities are expected to change in the future. Changes in the effective amounts of these resources and facilities, in turn, are expected to change the price or cost of recreational trips. Expected supply can therefore also be interpreted as "constrained consumption" of trips with respect to available recreation resources and facilities.

During the time period from January 1992 to April 1993, individuals in U.S. Forest Service offices and other organizations listed in Appendix I were interviewed by person or by phone. This list does not represent a scientifically selected random sample of Assessment users. Rather, the sample was selected with the advice of U.S. Forest Service personnel to represent a reasonable cross-section of potential users of the Assessment results.

Interviewees were asked to: 1) describe how they use the Assessment results, 2) identify particular issues and problems they have encountered in their applications of the results, 3) suggest new analyses and information which would be useful to include in future Assessments. Compiled responses obtained in the interviews are summarized in the rest of this section. Because of the limited sample of potential Assessment users interviewed, the compiled responses should be interpreted with caution and not generalized too broadly.

Because of institutional arrangements established by the RPA, the most extensive user of the Assessment results is the U.S.D.A. Forest Service. As mentioned previously, the Secretary of Agriculture has assigned the Forest Service to conduct the Assessment. Thus, many individuals within the agency are familiar with the Assessment and its results. The RPA also directs the Forest Service to use the results of the Assessment to help guide its various programs including National Forest Systems, Research, State and Private Forestry, and International Forestry.

The process for integrating the RPA resource assessments into the Forest Service programs mentioned above is illustrated in Figure 1. In addition to requiring the Secretary of Agriculture to conduct the Assessment, the RPA legislation requires that a national-level "Program" be developed for the Forest Service. The RPA Program is the Forest Service's "strategic plan" for forest policy and management.

The framers of the RPA envisioned certain institutional linkages between the RPA Assessment, the RPA Program, regional-level data collection and planning, and forest-level data collection and planning. As shown on the left side of Figure 1, the intention of the RPA is for data collected at the forest-level to be aggregated at a regional-level, and then eventually aggregated to a national-level. One of the intended uses of these data is to provide part of the data base for conducting the national-level Assessment.[4]

The right side of Figure 1 illustrates the intentions of the RPA with respect to the application of the Assessment results. The results of the national-level Assessment provide a basis for the development of the national-

[4]The national-level Assessment is conducted using data from many sources within and outside of the U.S. Forest Service.

level Program. The national-level Program is a strategic plan that provides long-term policy and management guidance for National Forest Systems, Research, State and Private Forestry, and International forestry.

How have the linkages illustrated in Figure 1 operated in practice with respect to the 1989 Assessment? At the national-level, interviewee responses suggest that the 1989 Assessment results were used in the development of the 1990 Program, but not in a direct, analytical manner. Rather, it appears that certain key findings of the 1989 Assessment were used to guide development of the 1990 Program in a more indirect, qualitative manner. For example, the 1989 Assessment results suggest a substantial increase in the future for recreational trips of shorter duration taken to sites located closer to home (e.g., dayhiking trips, picnicking trips). Particular National Forests which are located within a relatively short driving distance of major urban areas provide a potential major source of opportunities for these type of recreational activities (for example, recreation trips of one day or less). This observation influenced the development of a greater emphasis in the 1990 Program on the provision of recreational opportunities on National Forests.

The 1989 Assessment also provided projections of future demand/supply relationships for specific recreational activities. These results suggest for which activities "shortages" of recreational opportunities are likely to develop in the future. It appears from interviewee responses that these demand/supply projections were also used in an indirect, qualitative manner to guide development of the 1990 Program. For example, the demand/supply projections suggest that shortages of recreational opportunities may develop in the future for a broad variety of activities. This observation contributed to a focus in the 1990 Program on providing a greater diversity of recreational opportunities on National Forests.

As illustrated by Figure 1, one of the intents of the RPA is to provide guidance for regional-level and forest-level planning and management. Interviewee responses suggest that the national-level recreation goals and objectives stated in the Program have contributed, at least in some cases, to a greater emphasis in regional-level and forest-level planning on the provision of diverse recreational opportunities. Some interviewees stated the Program was especially helpful for discerning the specific priorities held by the upper-level U.S. Forest Service administration for recreation management on National Forests. Overall, however, interviewee responses suggest that the linkages between the national recreation management strategy outlined in the Program and regional-level and forest-level planning and management are rather loose.

Interviewee responses suggest that outside of the information embodied in the RPA Program, the national-level Assessment results are not widely used in regional-level and forest-level planning and management. At least one region sampled, however, used the Assessment recreation demand/supply projections in the development of its regional planning guide. In addition, many regional-level and forest-level personnel are familiar with the Assessment results and utilize the results for other general purposes. For example, a number of interviewees reported that they use the Assessment results as background information (e.g., "benchmark" information) for preparation of papers or presentations dealing with National Forest management directed at professional or lay audiences. Some interviewees also reported that although they do not directly use the national-level Assessment results in forest planning and management, the results provide them with insight on the recreation demand/supply analyses which are needed in order to generate information which would be directly applicable to the development of regional and forest-level management and planning.

Many of the policy decisions which dictate forest policy and management occur within the Washington, D.C. "beltway" in the White House, Congress, and the U.S. Forest Service Washington Office (WO). For example, these three entities interact with each other to make final decisions on the U.S. Forest Service annual budget. The size and distribution of this annual budget, of course, can have major implications on resource policy and management.

Interviewee responses suggest that the 1989 Assessment results have had an important influence on the development of resource policy in Washington, D.C. - particularly Congressional policy. For example, as mentioned previously, one of key findings of the 1989 Assessment is a growing demand for most forms of outdoor recreation at sites located close-to-home. This finding was used by the Forest Service WO to support requests to Congress for adequate funding of Forest Service recreation programs.

Interviewee responses suggest that the Assessment results also influenced Congressional resource policy on a more indirect level. Members of Congress or their staff routinely request information from the Forest Service WO or the Congressional Research Service concerning recreational use of public lands. Because the Assessment contains recreation use and trend information not readily available elsewhere, the Assessment results are very often used to help meet such information requests. The Assessment information provided to Congress in this manner influences policy decisions in ways which may not be directly traceable.

Many policy decisions which affect forest policy and management are made internally within the Forest Service. The Forest Service has an extensive staff of policy analysts and researchers located in Washington and at various locations across the country (e.g., research stations located near universities). These policy analysts and researchers provide information which facilitates policy decisions made by upper-level Forest Service administrators.

Interviewee responses suggest that the Assessment results are widely used by Forest Service policy analysts and researchers as they perform this information support function. Interviewee responses also suggest that upper-level Forest Service administrators may sometimes directly use the Assessment results to facilitate internal policy decisions.

The Congressional Research Service, as already alluded to above, regularly uses the 1989 Assessment as a reference document for responding to requests from Members of Congress and their staffs related to the use of National Forests for outdoor recreation. Other government agencies and private resource management interest groups appear to use the Assessment as a general reference document, but on a limited basis. The use of the Assessment results by individual private citizens and businesses appears to be negligible.

One use of the Assessment results outside of the Forest Service which may be significant is the application of the results and associated analyses to support the research programs of college and university faculty and staff. The Assessment results, for example, may be used as supporting material for journal articles and other publications. These journal articles and other publications, in turn, may be used by the Forest Service and other agencies to facilitate policy, planning, and management decisions. The extent to which the Assessment results support and enhance general recreation research programs, both inside and outside of the Forest Service, is difficult to assess.

Issues and Problems Related to the Application of the 1989 Assessment Results

Interviewees indicated a number of issues and problems related to the application of the 1989 Assessment results. One problem consistently mentioned by interviewees is the lack of regional demand/supply projections. Many interviewees stated that regional demand/supply projections would greatly enhance the usefulness of the Assessment for regional-level and forest-level policy and planning. This information gap has been at least partially filled by the 1993 Assessment Update which includes limited regional demand/supply projections.

Another problem consistently mentioned by interviewees is the lack of information on the effects of quality changes on recreation demand/supply relationships. For example, the 1989 Assessment results do not assess how changes in congestion will affect the demand for certain recreational activities. The potential effects on recreation demand/supply relationships of changes in the condition of natural resources (e.g., water quality) and constructed facilities (e.g., campsites, bathrooms, trails, etc.) at recreation sites are also not considered in the 1989 Assessment.

Issues and problems related to units of measure were also mentioned by many of the interviewees. One specific issue is the lack of consistency in the recreation quantity measures across the three Assessments published to date. The 1975 Assessment measured recreation quantity in terms of activity days. The 1980 Assessment measured recreation quantity in terms of participation. The 1989 Assessment measured recreation quantity in terms of trips. The use of these different units of measure makes it difficult to compare and reconcile demand/supply projections reported in the 1989 Assessment with demand/supply projection reported in the 1975 and 1980 Assessments.

Interviewee responses suggest that the lack of consistency in units of measure across the published Assessments is particularly a problem with respect to application of the Assessment results to national-level policy decisions.

Interview responses also suggest that differences in units of measure for recreation quantity hinder the application of the Assessment results to regional-level and forest-level planning and management. At the regional-level and forest-level, recreation quantity in often measured in terms of recreation visitor days (RVDs). Because the 1989 Assessment measures recreation quantity in terms of trips, the demand/supply projections reported in the 1989 Assessment cannot be directly applied to a regional-level or forest-level analysis which is using RVDs (or some other unit besides trips) as the unit of measure for recreation quantity.

A number of interviewees also voiced concern about the implications of <u>unique local considerations</u> for the applicability of the Assessment results to forest planning. Special preferences for certain types of recreation by the local population surrounding a National Forest may suggest needed adjustments in national and regional demand projections to account for these special local preferences. Special local supply conditions (e.g., availability of substitutes) may have major implications with respect to the applicability of national and regional demand/supply projections to a management issue facing a particular National Forest. For example, the available supply of certain recreational opportunities on a National Forest may be restricted by limit systems which strictly regulate recreational access (e.g., big-game hunting limits, white-water rafting limits).

Some interviewees were also concerned that local <u>political concerns</u> may hinder effective application of the Assessment results to regional-level and forest-level planning and management. For example, the Assessment results may suggest that a particular region or forest should provide more (or less) opportunities for certain type of recreational activities. However, for any number of potential reasons, local political forces may challenge planning or management decisions which are based on the Assessment results.

Several interviewees also noted that the application of the Assessment results at the forest-level is difficult because the implications of the demand/supply projections for <u>management of resources and facilities</u> on a particular forest may not be clear. For example, the 1989 Assessment predicts that a considerable "shortage" of opportunities for day hiking may develop in the future. This result may suggest the need to develop new day hiking trails in a particular National Forest. Planners and managers, however, would still need to determine exactly how many miles of new trails to develop and where to locate these trails (e.g., Should the new trails be located in a more remote or more developed area of the Forest?). The Assessment, as currently written, provides little guidance on these type of practical "on-the-ground" management decisions.

Several interviewees suggested widespread application of the Assessment results is hindered by the limited amount of "ground-truthing" presented. "Ground-truthing" analysis would involve comparing Assessment demand/supply projections to actual observations of changes in demand and supply in order to assess how well the projections explain the "real-world". The question here is one of credibility and believability of the Assessment results. For example, are projections of increased demand for a certain activity, say primitive camping, consistent with actual trends in the issuance of backcountry camping permits? The more consistent Assessment results are with actual demand and supply behavior and actions observed by resource planners and managers "on-the-ground", the more likely are the Assessment results to be directly incorporated into resource planning and management.

A more institutional problem consistently given by interviewees as a reason why the Assessment results are not used more extensively in regional-level and forest-level planning and management is the timing of the RPA Assessment. The 10 year interval period for the RPA Assessment is not always in sync with the 10 year interval period for the development of individual forest management plans. Thus, in many cases the Assessment results may not be published and distributed in time to be incorporated into the development of individual forest management plans.

With respect to presentation, most interviewees felt that the level of detail and description provided by the current set of Assessment publications adequately document the Assessment results. Some suggestions for improving the presentation of the Assessment results were provided by several respondents. Presentation of key demand/supply results in a "statistical abstract" or "encyclopedia" type publication was suggested as a means of enhancing the accessibility and applicability of the Assessment results. It was also suggested that key demand/supply statistics be made available on computer discs. Another suggestion was to continue publishing a document (as was done in 1990) which describes the linkages between the Assessment, the RPA Program, and regional-level and forest-level planning and management.

New Analyses and Information

Interviewees provided a number of suggestions for new analyses and information which may be useful to include in future Assessments. As already discussed above, interviewees would like to see more quality effect analyses and more regional analyses. Desired quality effect analyses include analyses of the effects of site quality (including the condition of natural resources such as water and the "physical plant") on recreation demand, and more analyses of the demand/supply relationships for different "settings" (e.g., remote setting, developed setting).

Desired regional analyses include regional user characteristic profiles, regional expenditure profiles, and regional demand/supply projections. Interest in analyses related to different regions and settings extends to examination of urban recreation demand/supply relationships vs. rural demand/supply relationships. An important distinction between urban and rural recreation is that a considerable amount of urban recreation (e.g., recreation at local neighborhood parks) may involve negligible or zero trip expenditures. Thus, methods of measuring economic demand, benefits, and impacts (e.g., travel cost method, input-output models) which rely on observations of travel expenditures may not adequately assess the contribution and importance of urban recreation to society.

Interviewees consistently mentioned the need for new analyses and information on the economic effects of outdoor recreation on regional and local economies. The need for more analyses and information on historical trends in recreation demand and supply was also consistently mentioned. Previous Assessments have focused on <u>future trends</u> in recreation demand and supply. Many interviewees commented that it would be very informative to also have more information on <u>past trends</u> in recreation demand and supply.

Several interviewees mentioned the need for more analyses and information on the implications of increasing "customer diversity" for recreation and wilderness management. For example, what are the long-term implications of the wave of "new immigrants" on recreation demand and supply? What other key social and demographic changes should public and private organizations focus on in order to prepare for future changes in recreation demand and supply?

New analyses and information focusing on "ecosystem management" was mentioned by several interviewees as a need for the Assessment. The U.S. Forest Service is placing increasing emphasis on managing National Forests on a more holistic, sustainable basis. This emphasis may have major implications for the application of recreation demand and supply analysis results to resource policy and management.

A related concern mentioned by some interviewees was the need for more and better information which would facilitate "tradeoff analysis". For example, managers may be concerned with assessing the tradeoff between developing a new mountain bike trail and the potential loss of prime wildlife habitat. In an economic sense, data on the opportunity costs of particular management actions (such as developing a new mountain bike trail) are needed to perform tradeoff analysis.

Several interviewees recommended that ex post validation studies be conducted to verify the accuracy of the Assessment demand/supply projections. The 1989 Assessment provides projections of the number of outdoor recreation trips Americans are expected to take in the years 2000, 2010, 2020, 2030, and 2040. An ex post validation study of these projections would have the following steps. In the year 2000, for example, a study could be conducted to estimate the actual number of recreation trips taken by Americans. These estimates could be compared to the projections for the year 2000 reported in the 1989 Assessment. Any differences in the estimates could then be analyzed to gain insight into the accuracy of the 1989 Assessment projections. Similar validation studies could be conducted in future years.

The need for additional information on supply functions for the provision of recreational opportunities was noted by several interviewees. Such supply functions would provide a statistical relationship between management inputs and recreation opportunity output. A particular challenge faced in the development of recreation supply functions is the proper specification of inputs and outputs.

Interviewees suggested more analyses and information may also be useful in the following areas: breakdown of demand/supply projections by Federal government agency, private land recreation demand/supply relationships, backlogged work and capital investment, effects of national environmental quality on recreation demand/supply trends, passive use of recreational resource and wilderness areas (e.g., existence values), and the use of private lands for recreation.

Conceptual Framework for the Outdoor Recreation Component of the RPA Assessment

Many suggestions provided by RPA Assessment users for improving the usefulness of the recreation demand and supply information reported in the Assessment were outlined in the previous section. Some suggestions are within the scope of how the Assessment is currently conducted, some probably are not. Because the Assessment cannot meet all information needs for resource policy and management, thought and discussion should be devoted to developing an overall conceptual framework (or strategy), for maximizing the usefulness of the Assessment results given constraints related to time, budgets, and the legislative intent of the Assessment.

According to the original RPA legislation, one of the primary intents of the Assessment is to provide a broad overview of recreation (and other resource) demand and supply trends in the United States. The three Assessments conducted so far appear to have met this intent reasonably well. Because of its national scope and emphasis, there are built-in data collection, data analysis, and administrative constraints that place sometimes major limits on the ability of the Assessment to answer certain, highly specific or specialized recreation demand and supply questions. Because of the legislative scope of the Assessment and practical constraints imposed on conducting the Assessment, there is a need to prioritize the various recreation demand and supply issues which future Assessments will attempt to address. Considering the issues which seemed to be of greatest concern to the greatest number of Assessment users, the following issues are suggested for priority status in future Assessment efforts: 1) national recreation demand and supply trends (historical and future); 2) regional demand and supply trends (historical and future); 3) effects of quality changes on recreation demand and supply trends; 4) flexibility between units of measure for recreation quantity; 5) effects of demographic/socioeconomic changes on recreation demand and supply trends; and 6) net economic value and regional economic impact.

Geographic Scope of the Assessment

The first two priority issues mentioned above relate to the geographic scope of the Assessment recreation demand and supply analyses. Much of the Assessment demand and supply analyses are focused at the national level. The usefulness of the Assessment results for forest policy and management, however, would be facilitated by providing more regional and subregional demand and supply analyses.

As illustrated in Figure 2, national level demand and supply function for outdoor recreation can be conceptualized as being derived from regional level demand and supply functions. These regional demand and supply functions, in turn, can be conceptualized as being derived from subregional demand and supply functions. Thus, in theory, it is possible to disaggregate national level demand and supply functions to regional level demand and supply functions.[5] Further disaggregation from the regional level to the subregional level would result in subregional demand and supply functions. As illustrated in Figure 2, it is also possible, in theory, to aggregate from subregional demand and supply functions to regional demand and supply functions, and from regional demand and supply functions to national demand and supply functions.

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[5]For the 1993 Assessment Update, regional level recreation demand functions were derived from the national level recreation demand functions estimated for the 1989 Assessment.

In order to disaggregate national level demand and supply functions to the regional level, the particular regions of interest must first be specified. For example, while disaggregating demand and supply functions by the administratively defined Forest Service regions may suit many Forest Service needs, such a breakdown may not be very useful to other Assessment users (e.g., National Park Service, U.S. Army Corps of Engineers). Other potential regional breakdowns include regions defined primarily by terrain (e.g., Great Plains region, Mississippi Delta region), various geographic/political breakdowns (e.g., Southeastern states, Midwestern states), or ecosystems.

Disaggregation from the regional level to the subregional level is more problematic conceptually. The primary problem is specifying regions and subregions such that there is a logical and consistent connection between the regional and subregional demand and supply functions. For example, for the 1993 Assessment Update, regional recreation demand functions were estimated for administratively defined Forest Service regions. In accordance with the Assessment objectives, these regional demand functions incorporated the demand for recreation on all private and public lands, not just Forest Service lands.

Suppose one was interested in disaggregating the regional demand functions for a particular region to derive recreation demand functions for each National Forest located in that region. It would be difficult, conceptually and empirically, to separate out the demand for recreation on a particular National Forest, from the demand for recreation on all types of private and public lands in a region. An alternative would be to estimate recreation demand functions for each Forest in a region separately. These Forest-level demand functions, however, would not aggregate to generate the regional demand functions reported, for example, in the 1993 Assessment Update (because, again, these regional demand functions represent recreation use on all public and private lands, not just National Forests).

The relationships between recreation demand and supply trends over time determine recreation consumption trends over time (Cordell and Bergstrom, 1991). Two sets of consumption trend lines are shown in Figure 3. On the right hand side of Figure 3, the lines labeled "UC" represents an "unconstrained consumption" trend lines. Unconstrained consumption is the amount of activity trips Americans would desire to take ("consume") in the future if the availability of recreational opportunities was sufficient to keep the cost of an activity trip constant.

The lines labeled "CC" on the right hand side of Figure 3 represent "constrained consumption" trend lines. Constrained consumption is the amount of activity trips Americans would take ("consume") in the future given actual constraints in the availability of recreational opportunities which may cause the price of an activity trip to increase. The price or cost of an activity trip would increase, for example, if the demand for recreational opportunities is increasing faster than the supply of opportunities. That is, recreational opportunities are becoming more scarce (Cordell and Bergstrom, 1991).

As mentioned in a previous section of this report, the difference between "unconstrained consumption" and "constrained consumption" represents a "gap" between the amount of trips Americans would like to take given constant trips costs, and the amount that they actually can take given supply constraints which may cause trip costs to increase. A larger "gap" represents a greater scarcity of recreational opportunities. Projected "gaps" are therefore likely to be of concern from a forest policy and management perspective. As illustrated in Figure 3, "gaps" can occur at the national, regional, and subregional levels.

Because of the legislative intent of the Assessment and inherent modeling and data limitations related to aggregation and disaggregation, the geographic scope of the Assessment is likely to remain primarily national. However, it does appear conceptually and empirically feasible, and highly desirable from a forest policy and management perspective, to place a priority in future Assessment on improving and expanding recreation demand and supply analyses at least at the regional level. Conceptual and empirical problems (primarily the lack of data) are likely to continue to limit the extent to which the Assessment can generate recreation demand and supply analyses at the subregional level.

There is a strong desire, at least on the part of some planners and managers, for improved and expanded recreation demand and supply information at the Forest level. Perhaps the most likely and useful manner to produce such information is for regional and forest level analysts to take the lead in this effort in cooperation with persons involved in conducting the national RPA Assessment. A coordinated effort would facilitate collection of data needed for both the Forest level analyses and the national and regional Assessment analysis. A coordinated effort would also help ensure that national, regional, and subregional (e.g., Forest level) demand and supply analyses are all being conducted in a theoretically and empirically consistent manner with the least amount of redundancy.

Quality Considerations

Closely related to the geographic scope of the Assessment, is the issue of the effects of quality changes on recreation demand and supply. The quality of recreation experiences, for example, is likely to vary across regions. This situation is illustrated in Figure 4. In Region A, the quality of recreational opportunities may by high, resulting in a high demand for activity trips as shown by D(H). The supply of high quality recreational opportunities, however, may by relatively low as shown by S(H). In Region B, the quality of recreational opportunities may be about medium, resulting in a lower demand for activity trips as shown by D(M). The supply of medium quality recreational opportunities in Region B, however, may be greater resulting in a relatively greater supply of activity trips as shown by S(M). In Region C, the quality of recreational opportunities may be low, resulting in a low demand for activity trips. The supply of low quality recreational opportunities in Region C, however, may be high resulting in relatively high supply of activity trips.

Because of the effects of quality differences on regional recreation demand and supply, different "gaps" for a recreational activity may occur across regions as illustrated in Figure 5.
For example, Region A may be characterized by an increasing demand for "high quality" recreational opportunities (e.g., groomed hiking trails) and a constant or decreasing supply of such opportunities. In this case, a large "gap" between unconstrained and constrained consumption would occur. Region C may be characterized by increasing demand for lower quality recreational opportunities (e.g., primitive hiking trails) and an increasing supply of such opportunities. In this case, a small "gap" between unconstrained and constrained consumption would occur. The consumption trend lines shown for Region B in Figure 5 depict a medium "gap" case.

As Figure 5 illustrates, quality differences may cause considerable differences in recreational opportunity "gaps" across regions. These regional quality and gap differences are "lost" when data are aggregated to estimate national consumption trends and gaps. Thus, if the assessment of regional quality effects is an important issue for the RPA Assessment, more research should be devoted to expanding and improving methods for incorporating quality effects into estimated regional demand, supply, and consumption functions.

Quality differences may also effect recreation demand and supply relationships at the subregional level. For example, suppose Region A is composed of four subregions as illustrated in Figure 6. Subregion 1 and 2 are characterized by high demand for high quality recreational opportunities but low supply of such opportunities. Subregion 4 is characterized by a low demand for low quality recreational opportunities and a relatively high supply of such opportunities. The demand and supply functions for Subregion 3 depict a medium or average situation.

The subregional demand and supply relationships shown in Figure 6 determine subregional consumption trends over time and the projected "gaps" between unconstrained and constrained consumption at the subregional (e.g., forest) level over time. Because of differences in demand and supply relationships at the subregional level, "gaps" across subregions may be quite different as illustrated in Figure 7. These subregional "gap" differences, however, are "lost" when consumption trend lines are aggregated and reported at the regional level.

Information on the effects of quality changes on recreational opportunity "gaps" at the subregional level would facilitate planning and managing for expected shortages of particular recreational opportunities. Thus, it would be desirable to devote more effort towards the task of developing methods for better analyzing quality effects at the subregional level. Given RPA budget and time constraints, accomplishment of this task most likely would require a cooperative, team effort between analysts at the subregional (e.g., forest) level, regional level, and national level.

Another major quality issue at the national, regional, and subregional levels is the effects of quality changes over time on recreation demand and supply. For example, one question of interest for forest policy and management is: "If public recreational facilities (e.g., developed campgrounds, picnic areas, boat ramps, etc.) deteriorate over time because of lack of funding for repair and maintenance, what will be the effect on recreation consumption or participation?" Addressing questions such as these requires that more effort be devoted to measuring and modeling the effects of quality changes on recreation demand and supply over time, as well as geographic space.

Recreation Quantity Measures

There are various units of measure for recreation quantity, including trips, activity days, RVDs, and participants. Which unit of measure is preferred? The answer to this question depends upon the forest policy or management problem or issue under consideration. For some problems or issues, a measure of total trips may be preferred. For other problems or issues, a measure of total participants may be preferred. For still other problems and issues, activity days or RVDs may be needed.

One way to generate recreation demand and supply information for different quantity measures is to conduct a separate demand/supply analysis for each unit of measure. Because of budget and time constraints, however, conducting a series of independent analyses for each unit of measure is not likely to be within the scope of the RPA Assessment. A more feasible course of action is to develop a conversion system which could be used to derive one unit of measure from another using a conversion factor. Such a system would allow for analytical "cross-walking" between different units of measure.

In order to develop a standard conversion system for recreation quantity measure, one unit of measure needs to be selected as the "base". The unit of measure which is argued to be the most consistent with the economic theory of demand and supply is trips or visits (Bockstael and McConnell, 1981; McConnell, 1975; Ward and Loomis, 1986). It is therefore suggested that trips serve as the "base" unit of measure for recreation quantity for the RPA Assessment. If trips were adopted as the base measure of recreation quantity, data collection and analysis efforts for the Assessment would focus on direct estimation of recreation trip demand and supply relationships. Conceptually, once recreation demand and supply relationships have been estimated for recreation trips, conversion factors can be applied to indirectly estimate recreation quantity for other units of measure. For example, in Figure 8, the demand and supply functions in the first column represent functions estimated from primary data on recreation trip demand and supply.[6] Using appropriate conversion factors, activity day or RVD recreation quantity measures can be derived from recreation trip quantity measures.

In Figure 9, the consumption trend lines in the second column represent unconstrained and constrained consumption trend lines for trips estimated from primary data. Once these functions have been estimated, appropriate conversion factors can be applied to indirectly derive consumption measures for activity days or RVDs. As illustrated in Figure 9, appropriate conversion factors could also be used to indirectly derive consumption measures for the number of activity participants.

Assessment of recreation demand and supply relationships for various types of "settings" or landscapes may also be of interest for resource policy and management. As illustrated in Figure 10, it is theoretically possible to develop conversion factors for deriving measures of trips to a particular setting (e.g., trips to wilderness areas for any activity) from measures of activity trips (e.g., trips for backpacking, horseback riding, hunting, fishing, etc.). Additional conversion factors could then be used to derive setting day and setting RVD measures from setting trip measures.

The consumption trend lines in the second column of Figure 11 represent consumption trend lines for activity trips estimated from primary data. Appropriate conversion factors can be used, as illustrated by Figure 11, to derive consumption projections for setting trips, setting days, or setting RVDs. Appropriate conversion factors can also be used to derive consumption projections for the number of participants expected to recreate at various settings (e.g., number of people expected to visit wilderness areas).

Effects of Demographic/Socioeconomic Factors

Another priority issue for the Assessment is expanding and improving analyses of the effects of demographic and socioeconomic changes on the demand for recreation. The American population is experiencing several mega-changes (large, population-wide changes) which may be of particular interest. One mega-change is the increasing mean age of the American population caused by the aging of the baby-boom generation. Another mega-change, which has been in process for decades, is the decreasing proportion of Americans who currently

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[6]An example of this type of primary data would be data collected in on-site interviews with recreation visitors on the number of annual trips taken to the site for various activities.

reside in rural areas, or have some sort of rural background. A more recent, ongoing mega-change is the increasing proportion of the American population whose cultural roots are primarily non-European.

Mega-changes and even more localized, micro-changes in American population characteristics, may have significant effects on preferences and demand for outdoor recreation. There is a need to develop conceptual models which carefully explain the expected relationships between changes in population characteristics and changes in the demand for various types of recreational experiences. These conceptual models can then be used to specify empirical functions (or equations) which would provide a means for estimating the expected change in recreation demand or consumption with a change in demographic/socioeconomic variables.

Net Economic Value and Regional Economic Impact

Of considerable interest and importance for resource policy and management is information on the net economic value and regional economic impact of outdoor recreation in the United States. Here, the net economic value of recreation refers to the net benefits of recreation measured in terms of consumer's surplus. Net economic value (consumer's surplus) is the appropriate measure of economic benefits for national economic development or economic efficiency analysis. Regional economic impact refers here to the effects of recreational expenditures on a regional economy measured, for example, in terms of gross output, employment, and income. Regional economic impact is the appropriate measure of economic benefits for regional economic development or distributional analysis (Stoll, Loomis, and Bergstrom, 1987).

In the past, estimates of the net economic value of recreation have been generated in a process associated with the RPA Program which was largely independent of the RPA Assessment demand and supply analysis. Information on the regional economic impact of recreation exists primarily as a result of individual, case-by-case analyses which are separate from both the RPA Assessment and Program efforts. In order to increase the efficiency and cost-effectiveness of data collection and analysis efforts, consideration should be given in the future to expanding the RPA Assessment process to include estimates of the net economic value and regional economic impact of outdoor recreation. It would be most feasible to generate these estimates for the "current situation" (e.g., situation in the base year of the Assessment). It may also be possible, though more difficult, to generate projections of the future net economic value and regional economic impact of outdoor recreation under alternative future scenarios.

One of the primary purposes of the RPA Assessment is to estimate recreation demand, supply, and consumption trends. In the process of estimating these trends, recreation demand functions must be estimated. These recreation demand functions, in turn, can be used to generate estimates of the net economic value of recreation (e.g., see Bergstrom and Cordell, 1991). Furthermore, if national, regional, and subregional demand functions were estimated as part of the RPA Assessment, these demand functions could be used to generate estimates of the net economic value of recreation at the national, regional, and subregional levels. This use of estimated demand functions would contribute to the development and application of "benefits transfer" techniques.[7]

In order to estimate the economic effects on a region of recreational visits, two primary pieces of data are needed: 1) an estimate of recreation expenditures per visit, and 2) an estimate of total visits. If regional and subregional demand and consumption functions are estimated as part of the Assessment process, these functions would provide a means for estimating total regional or subregional visits. The Assessment could also be expanded to collect and report estimates of recreational visit expenditure profiles for different regions and subregions.

Given estimates of expenditures per visit and total visits, total recreational expenditures in a region or subregion can be estimated. The economic effects of these expenditures on a regional economy can be estimated using a regional input-output model such as the U.S. Forest Service IMPLAN model. Estimates of regional economic effects provide a means for estimating regional multipliers which summarize the economic impact of recreational expenditures on a regional economy. Regional multipliers could be estimated for different regions and subregions and reported in the Assessment documents. These multipliers, combined with information on total recreational expenditures, would provide a convenient and flexible means for gaining insight into the economic impact of outdoor recreation in different regions and subregions.

Implications

Since the passage of the Renewable Resources Planning Act (RPA) in 1974, three RPA Assessments have been conducted. The most recent Assessment was completed in 1989. During 1992-93, an informal sample of RPA Assessment users were interviewed and asked to describe their applications of the recreation demand and supply analyses reported in the 1989 Assessment. Interviewees were also asked to critique these results and suggest needed improvements and extensions. The results of this informal survey have a number of implications for the RPA Assessment.

One general implication of the informal survey is that there appears to be fairly widespread use of the Assessment recreation demand and supply results inside and outside of the U.S.D.A. Forest Service. The Assessment results appear to be most frequently used in a broad, qualitative manner. For example, interviewees

[7]For a good introduction and discussion of current research on "benefits transfer" techniques, see the special section on "benefits transfer" in the March 1992 issue of Water Resources Research.

indicated that they use the Assessment as a source of "benchmark" data on recreation demand and supply trends for a variety of policy and management purposes. This use of the Assessment results is consistent with the primary objective of the RPA Assessment which is to provide overall, general guidance for resource policy and management.

Interviewees offered many suggestions for increasing the applicability of the Assessment results to resource policy and management. Because of various factors which constrain the scope of the Assessment, however, there is a need to prioritize which issues future Assessment efforts will attempt to address. Based on interviewee responses, it is suggested that the recreation demand and supply section of future Assessments focus on the following issues: national demand and supply trends, regional demand and supply trends, effects of quality changes on demand and supply trends, flexibility between units of measure for recreation quantity (e.g., development of conversion factors), effects of demographic/socioeconomic changes on demand and supply trends, and net economic value and regional economic impact.

Another issue of concern for future RPA Assessments is the new interest in "ecosystems management" of public lands. A move towards "ecosystem management" does not mean that future Assessments need to follow economic analysis approaches which are radically different from approaches used in previous Assessments which are based primarily on neoclassical economic theory. However, "ecosystems management" may require that economic data presented in the Assessment be applied to resource policy and management decisions in a much different manner as compared to past applications. For example, "ecosystem management" may require changes to be made in the various ways that the RPA Assessment results are applied to the RPA Program.

Interviewees also expressed a desire for more information on subregional (e.g., forest-level) recreation demand and supply trends. Although subregional recreation demand and supply information is very important for subregional policy and management, generating such information for all subregions in the nation is probably outside the scope of the Assessment. An alternative is for subregional analysts to take the lead on conducting subregional recreation demand and supply analyses with the technical support and assistance of the Assessment analysts.

A flow of data and analysis which incorporates more assessment work at the subregional and regional levels is illustrated in Figure 12, which is a modification of Figure 1. The solid horizontal arrows at the bottom of Figure 12 suggests that it may be desirable for subregional and regional analysts to generate more of their own recreation demand and supply information directly. Such efforts should be consistent and coordinated with the Assessment analysis and results (as indicated by the vertical dotted arrows) as well as the parallel RPA Program process shown on the right hand side of Figure 12.

In conclusion, the RPA Assessment appears to provide very useful information to many "clients" who would have difficulty obtaining similar information from other sources. There are many issues which future Assessments could, and perhaps should, address in the future. There is a need to set a research agenda for the Assessment which incorporates and coordinates the efforts of researchers, analysts, and decision-makers involved in policy and management at the national, regional, and subregional levels.

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Appendix I

I. U.S. Forest Service Washington Office

A. National Forest System

1. Recreation, Cultural, and Wilderness Management Staff
2. Land Management Planning Staff
3. Wildlife and Fisheries Staff

B. Programs and Legislation

1. Deputy Chief's Staff
2. Resource Program and Assessment Staff

C. National Forest Research

1. Deputy Chief's Staff
2. Forest Inventory, Economics, and Recreation Research Staff

D. State and Private Forestry

II. U.S. Forest Service Field Offices

A. National Forest System

1. Regional Office - Region 1
    a. Land and Financial Planning Staff
    b. Wilderness, Recreation, and Cultural Resources Staff
    c. Wildlife and Fisheries Staff

2. Regional Office - Region 2
    a. Recreation, Wilderness, Cultural Affairs, and Landscape Management Staff
    b. Planning and Program Budget Staff

3. Regional Office - Region 6
    a. Planning Staff

4. Idaho Panhandle National Forest
    a. Planning Staff
    b. Recreation Staff

5. Rout National Forest
    a. Planning Staff
    b. Recreation Staff

6. Mt. Baker-Snoqualmie National Forest
    a. Planning Staff

7. Kisatchie National Forest
    a. Forest Supervisor's Office
    b. Planning, Evaluation, and Recreation Staff

B. National Forest Research

1. North Central Forest Experiment Station
2. Southeastern Forest Experiment Station
3. Rocky Mountain Forest Experiment Station

III. Other Government Agencies

A. Congressional Research Service
B. U.S. Environmental Protection Agency
C. U.S. Army Corps of Engineers
D. U.S. Department of Commerce, NOAA
E. National Park Service
F. U.S.D.A., Economic Research Service
G. U.S.D.A., Soil Conservation Service

IV. Private Individuals and Organizations

A. National Wildlife Federation
B. Wildlife Society
C. American Forestry Association
D. Wilderness Society
E. William Shands (private consultant)
F. Resources for the Future
G. Doug Tims (private recreation tourguide and outfitter)

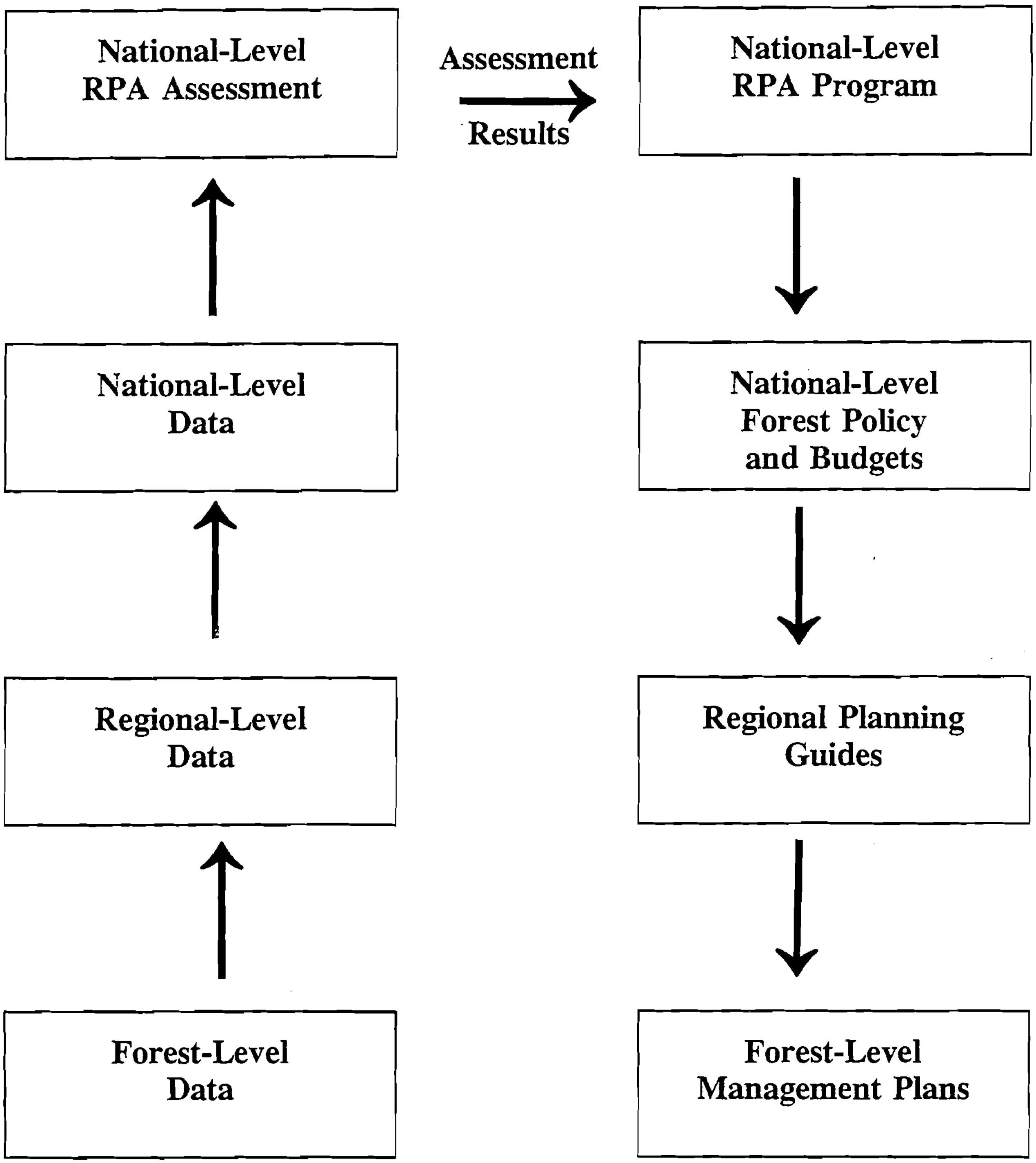

Figure 1. Flow of Data and Analysis Results as Envisioned by the RPA Legislation

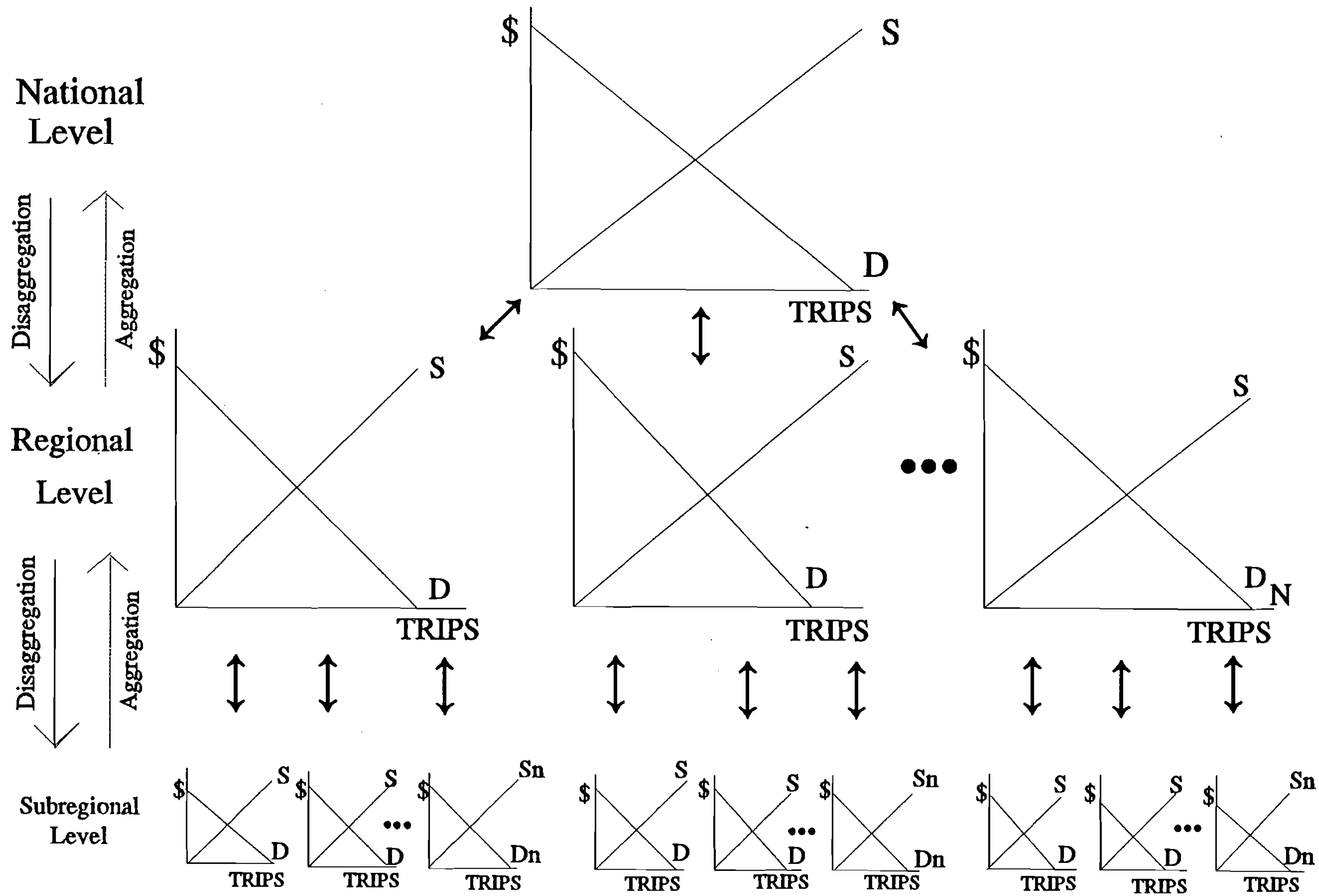

Figure 2. National, Regional and Subregional Recreation Demand and Supply Functions: General Relationships

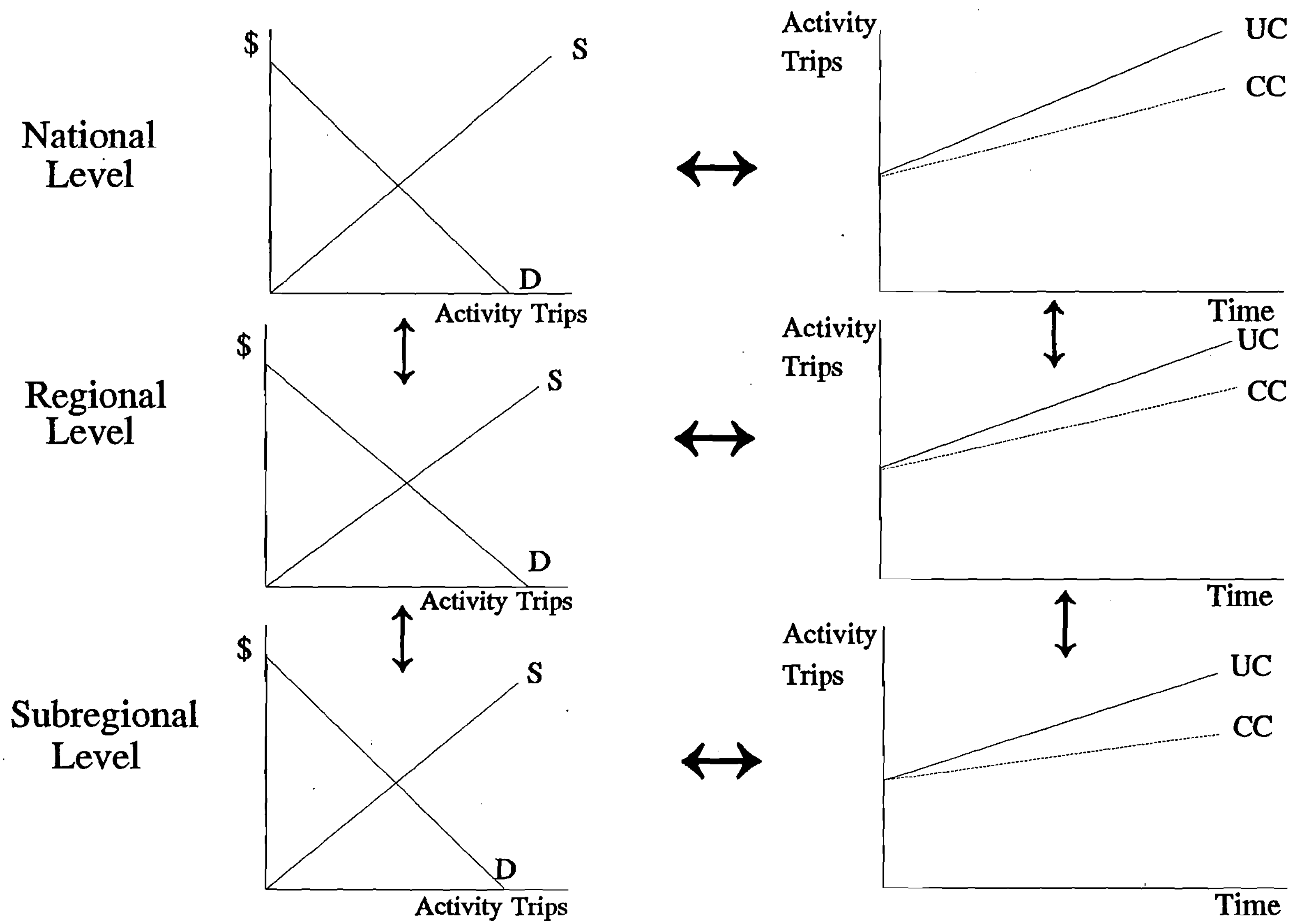

Figure 3. National, Regional, and Subregional Consumption Trend Lines and "Gaps"

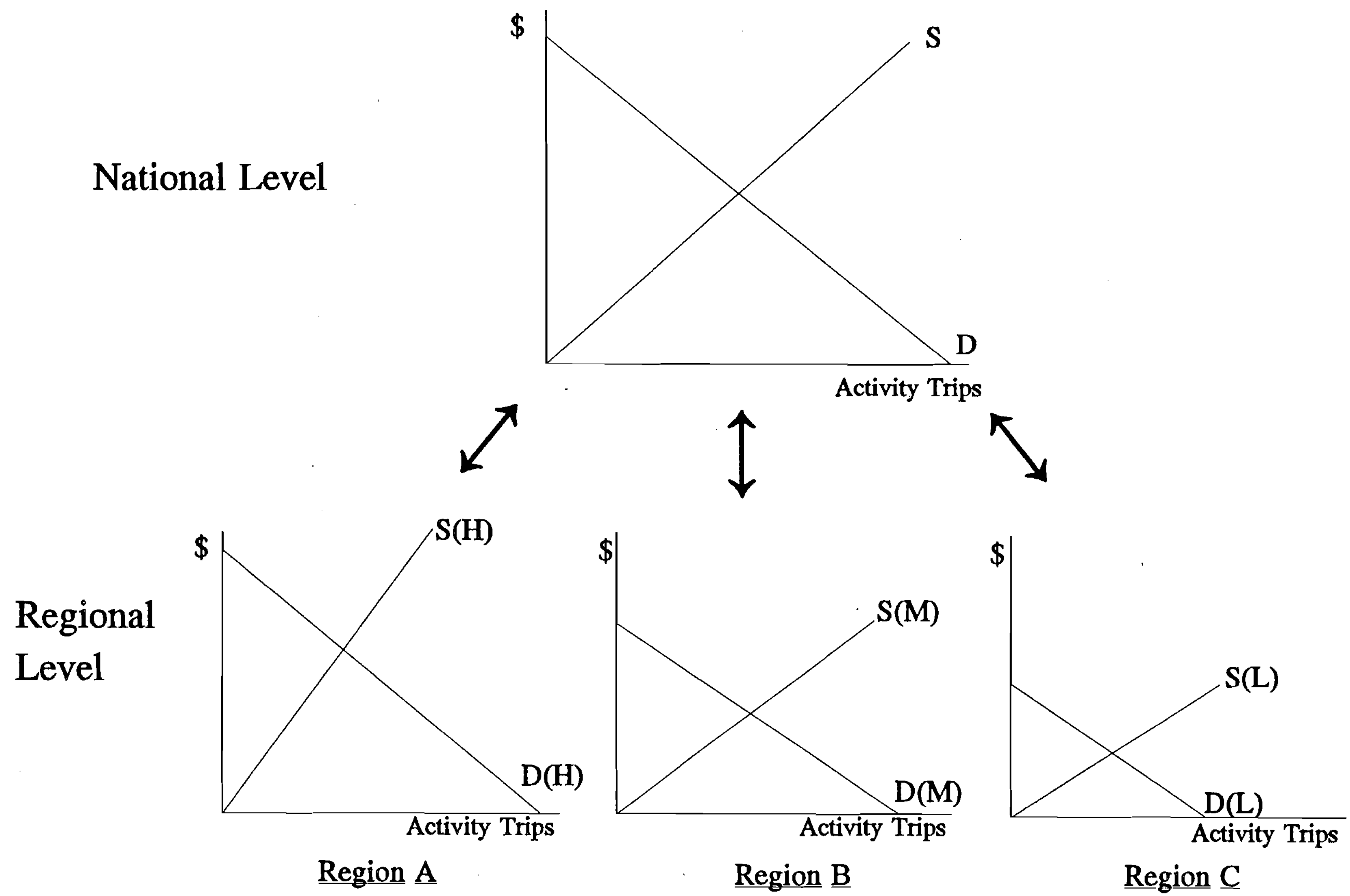

Figure 4. Illustration of Effects of Regional Quality Differences on Demand and Supply Functions

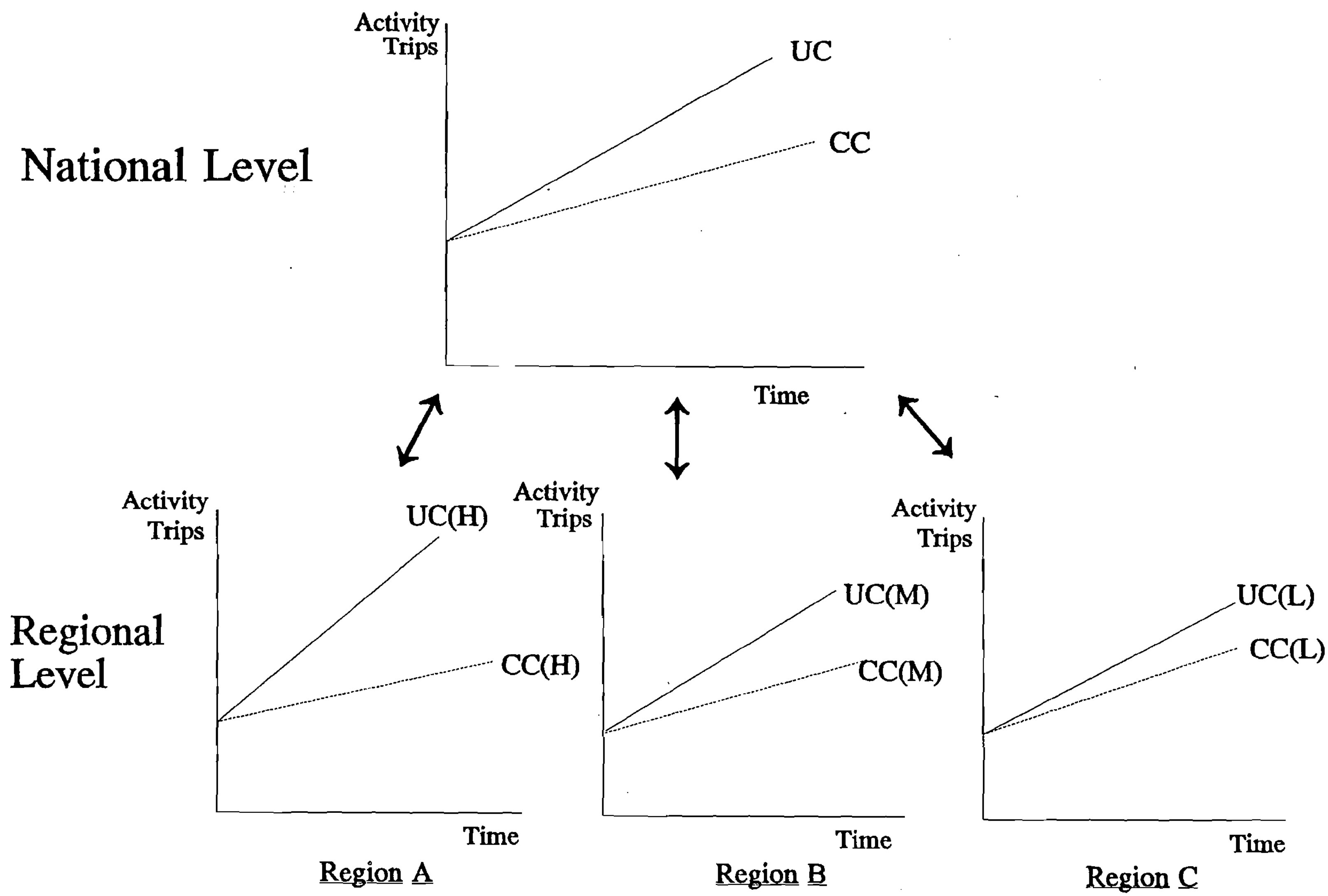

Figure 5. Illustration of Effects of Regional Quality Differences on Consumption Trend Lines and "Gaps"

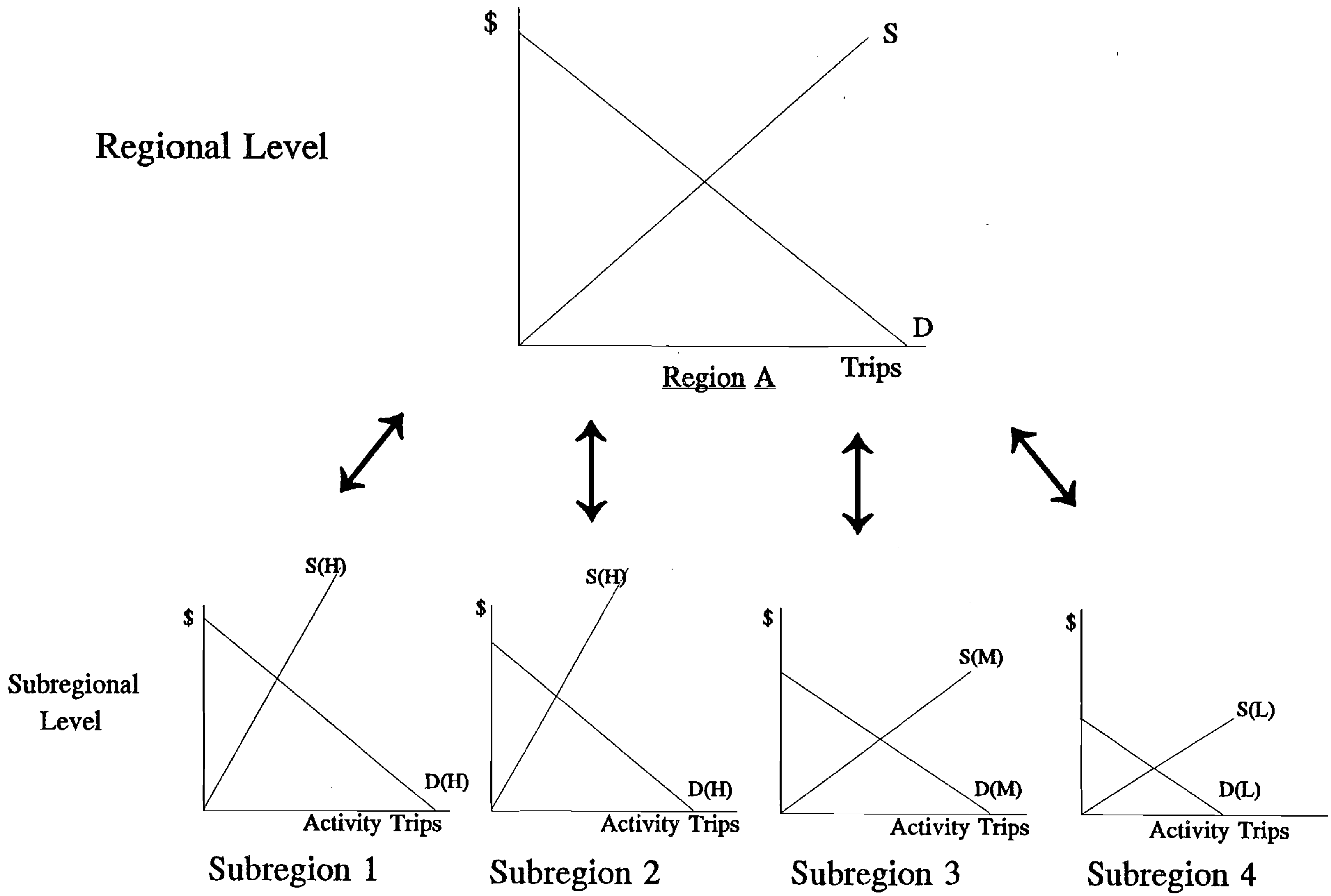

Figure 6. Illustration of Effects of Subregional Quality Differences on Demand and Supply Functions

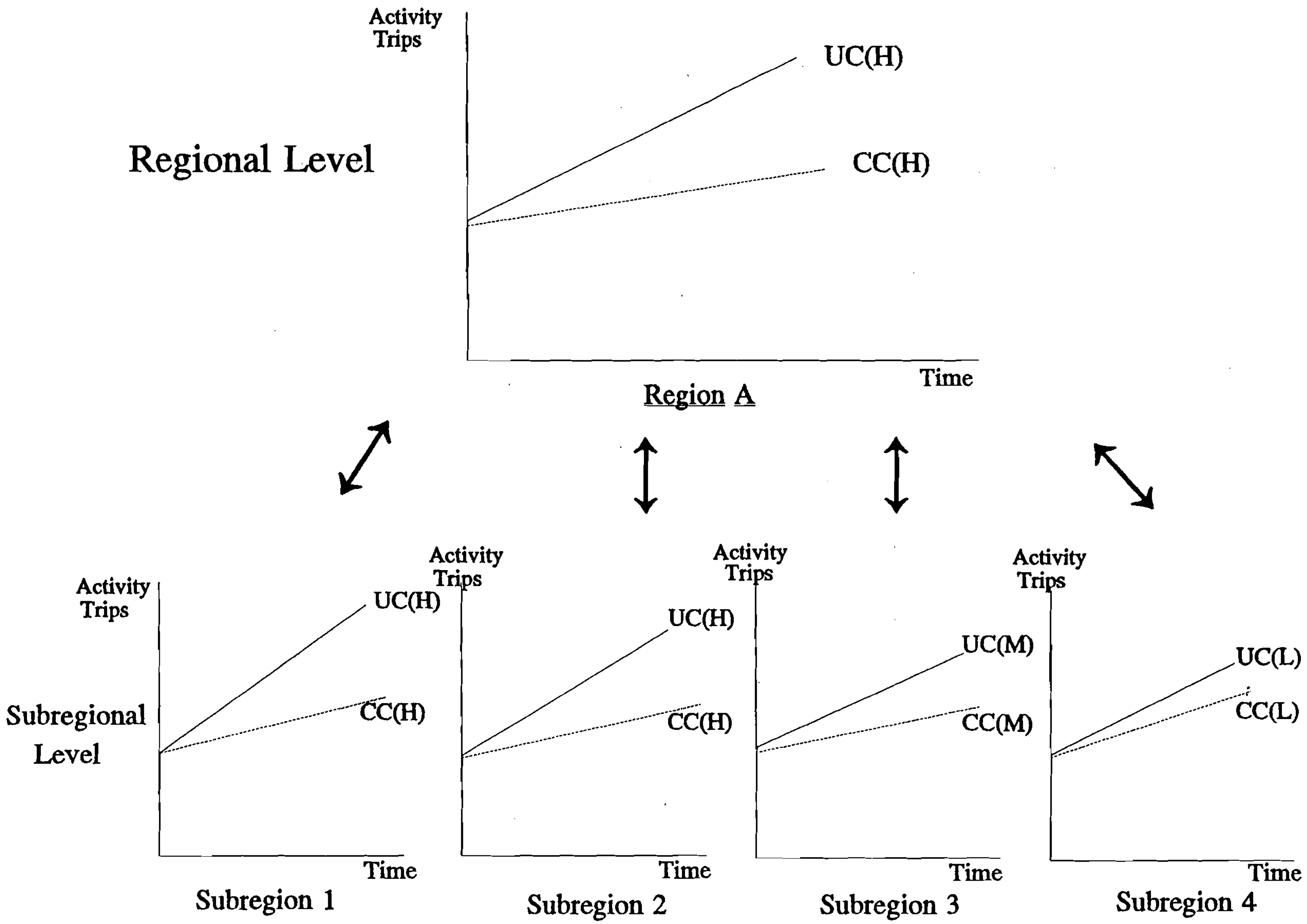

Figure 7. Illustration of Effects of Subregional Quality Differences on Consumption Trend Lines and "Gaps"

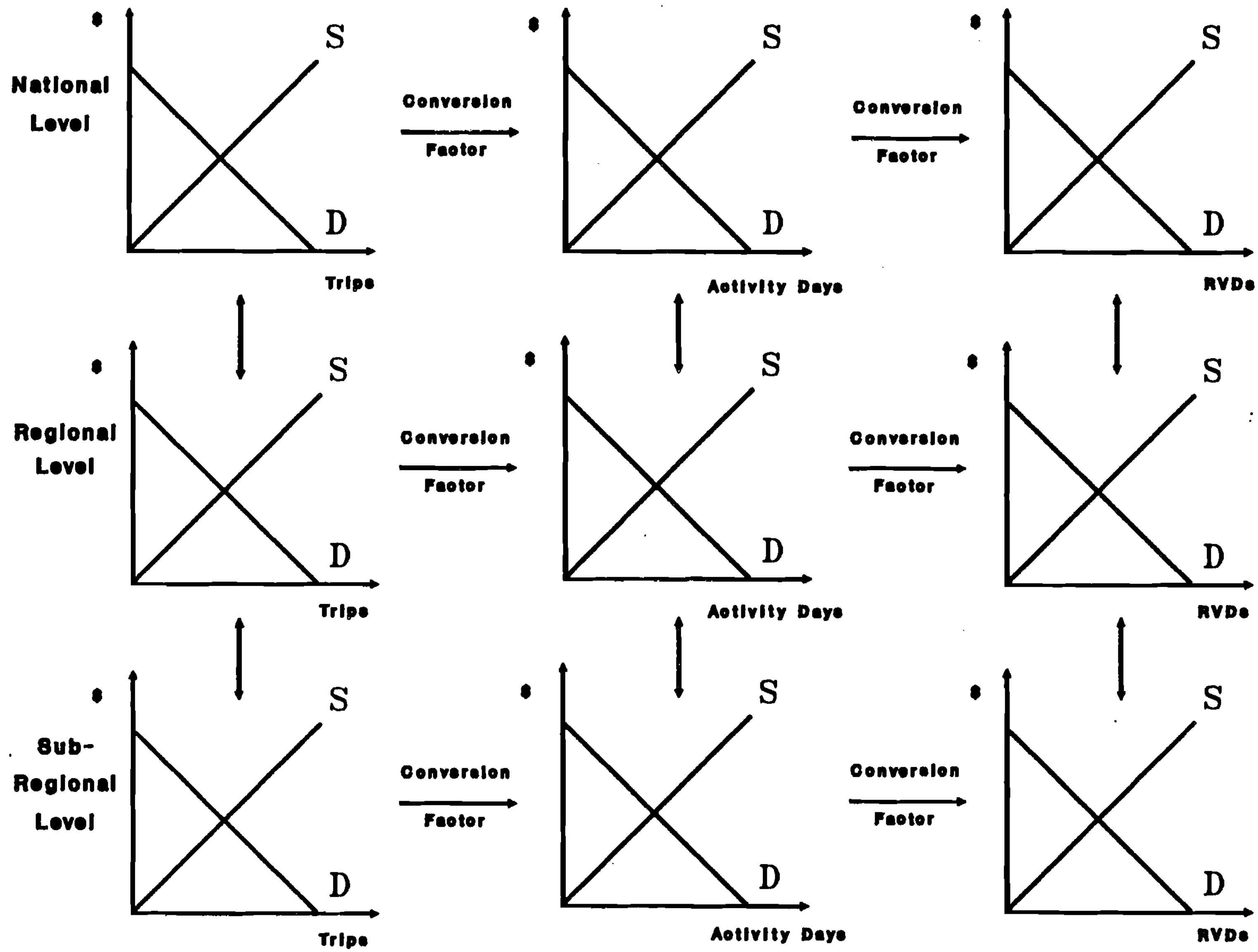

Figure 8. Illustration of Conversion from Activity Trips to Activity Days or Activity RVDs (Demand and Supply Analysis)

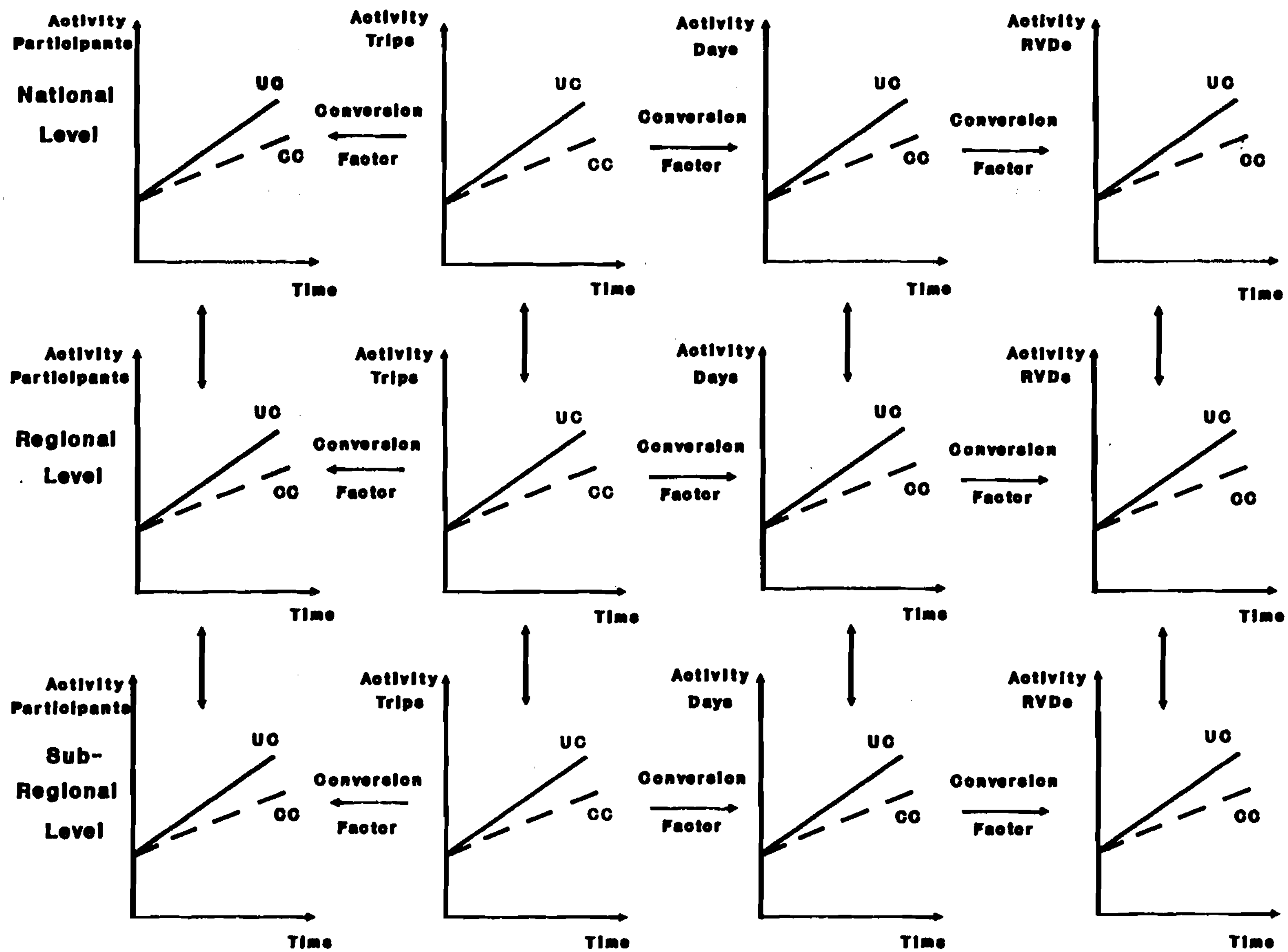

Figure 9. Illustration of Conversion from Activity Trips to Activity Days, Activity RVDs, or Activity Participants (Consumption and Gap Analysis)

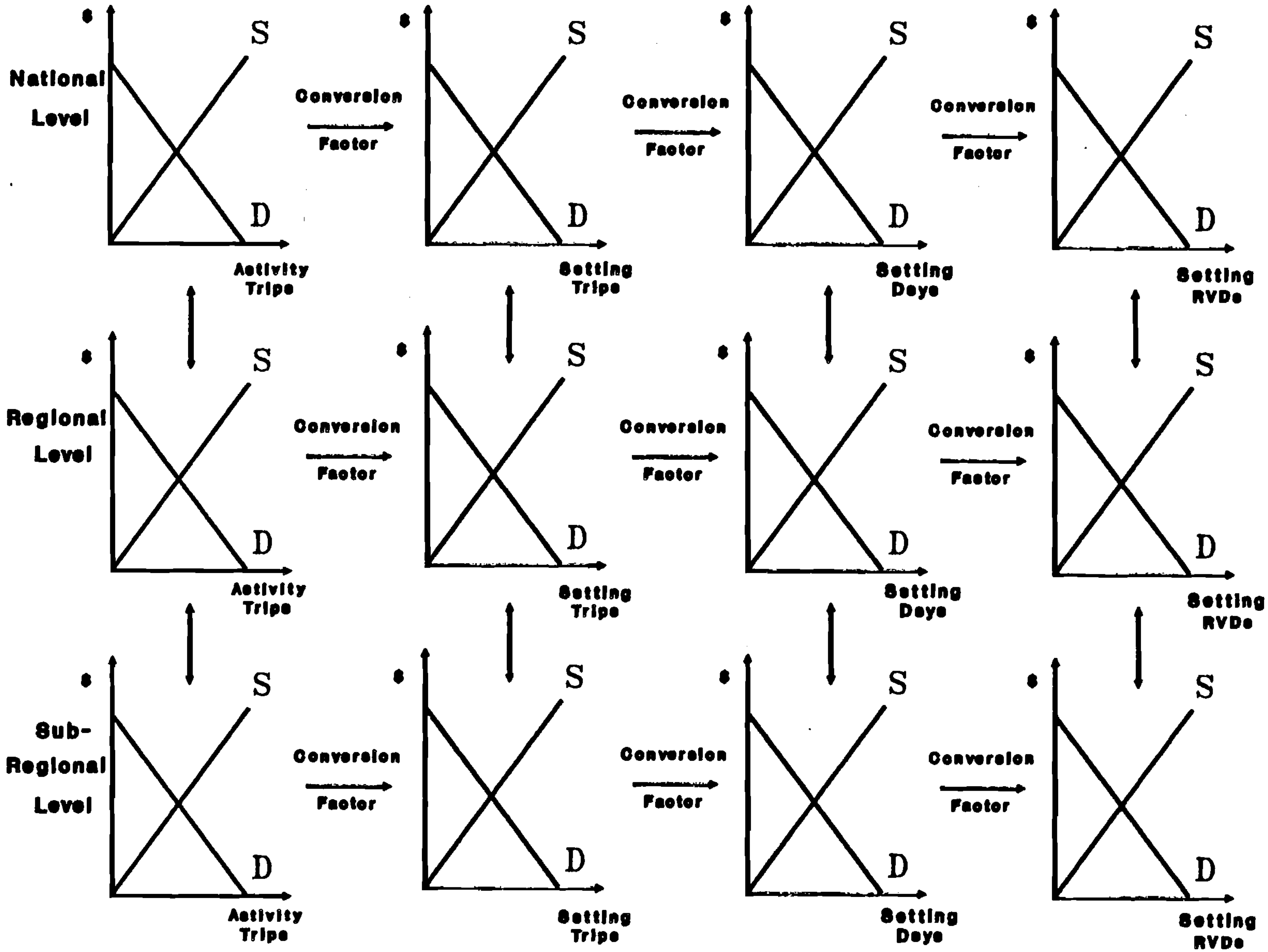

Figure 10. Illustration of Conversion from Activity Trips to Setting Trips, Setting Days, or Setting RVDs (Demand and Supply Analysis)

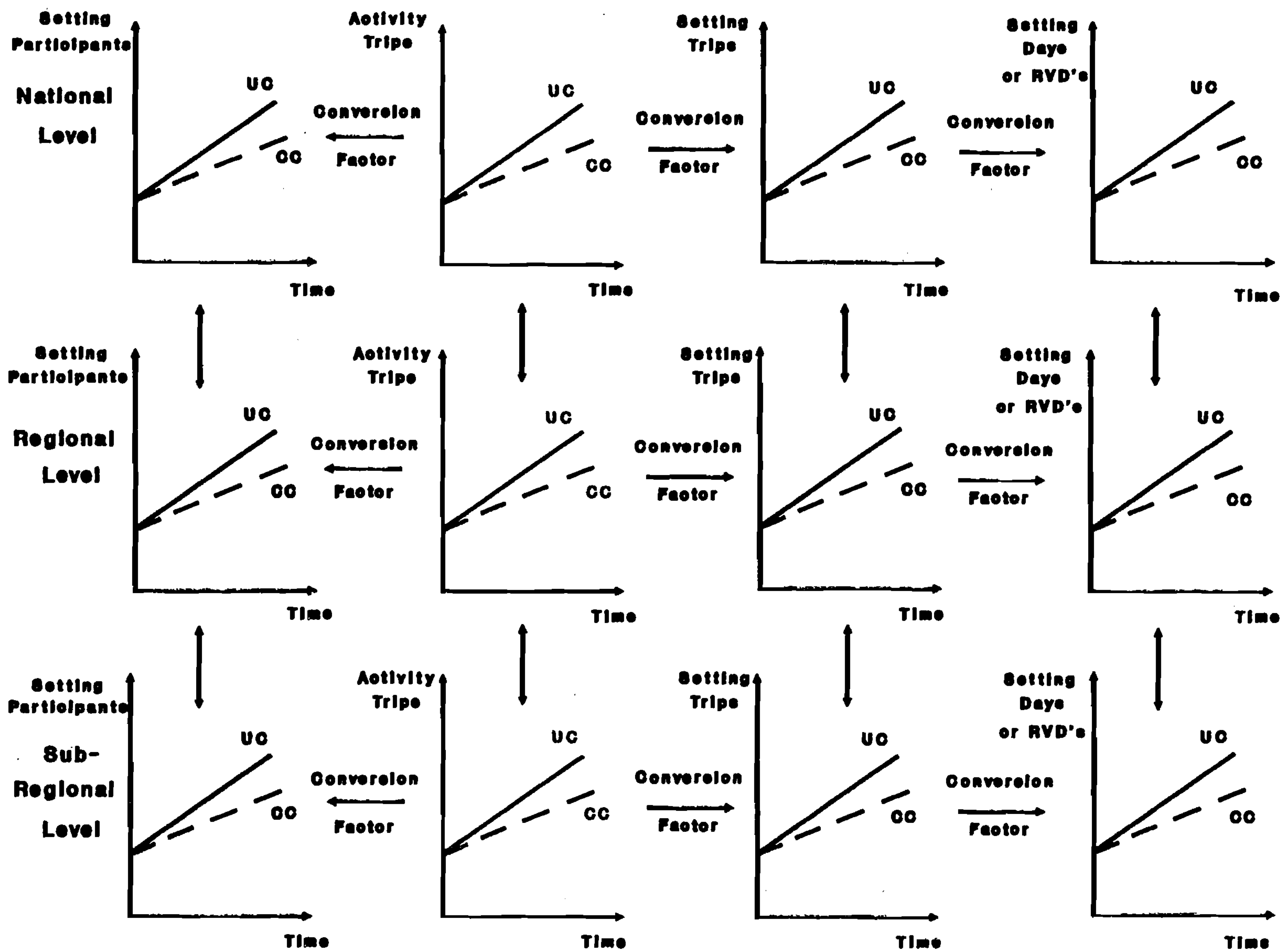

Figure 11. Illustration of Conversion from Activity Trips to Setting Trips, Setting Days, Setting RVDs, or Setting Participants (Consumption and Gap Analysis)

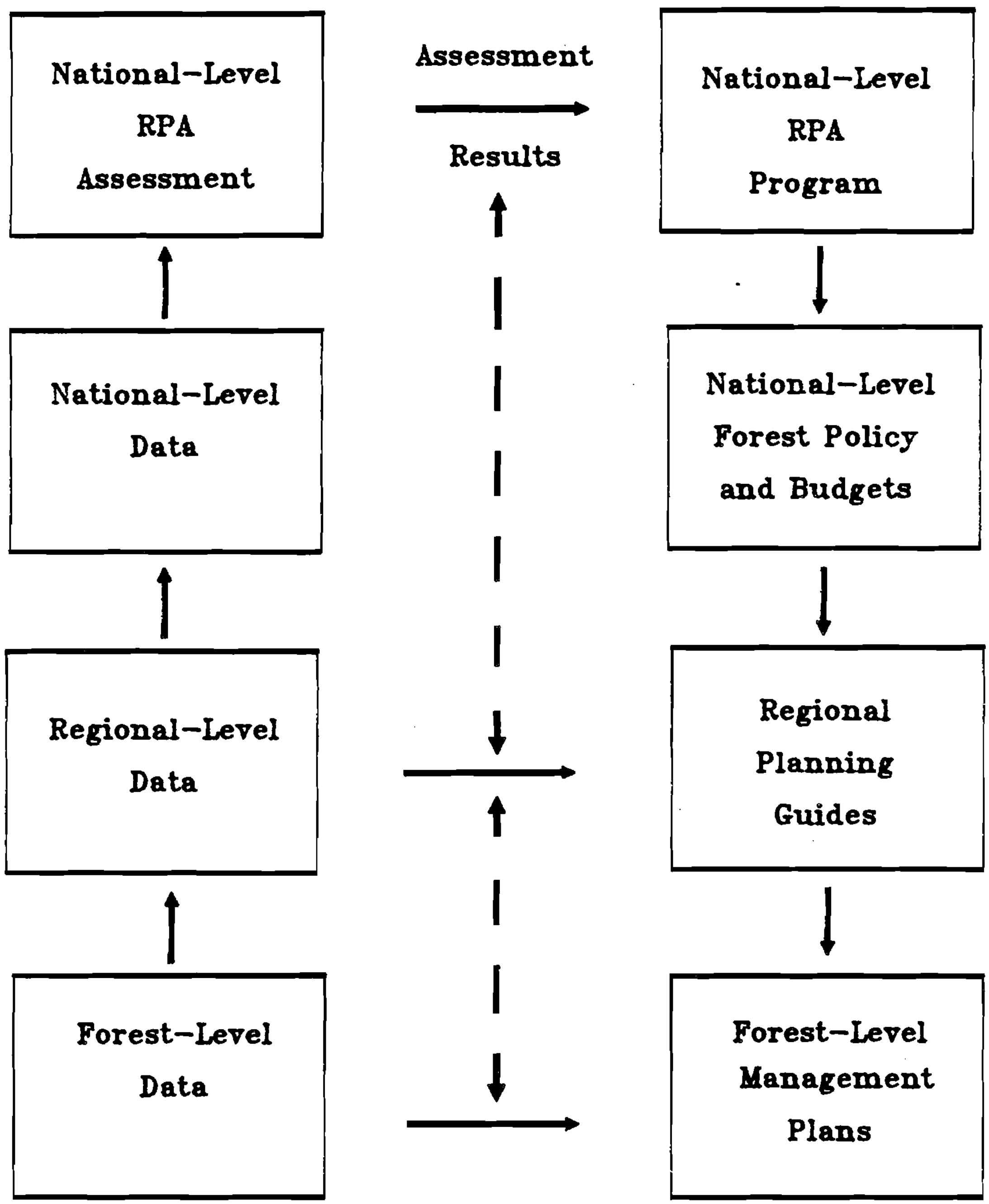

Figure 12. Modified Flow of Assessment Data and Analysis Results