Western Regional Research Publication

W2133
Benefits and Costs of Natural Resources Policies Affecting Public and Private Lands

Twenty-Second Interim Report
and
Proceedings from the Annual Meeting

September 2010

Annual Meeting held at:
Tanque Verde Ranch
Tucson, AZ
February 24-26, 2010

Compiled by:
Roger H. von Haefen
Department of Agricultural and Resource Economics
North Carolina State University
NCSU Box 8109
Raleigh, NC 27695-8109
roger_von_haefen@ncsu.edu

Table of Contents

Introduction

These proceedings contain selected research papers presented at the 2010 Annual Meeting of the W2133 Regional Project, "Benefits and Costs of Resource Policies Affecting Public and Private Lands," held in Tucson, AZ, February 24-26, 2010.

The annual convergence of W2133 scientists from academia and government took place at the lovely Tanque Verde Ranch. The Ranch was a fantastic gathering spot, attendance was at near-record levels, and the meeting provided an ideal venue for research collaboration, interaction, and exchange. W2133 also celebrated its 42nd anniversary of providing an invaluable outlet for leading research in environmental valuation, policy, and management.

The collection of papers included herein illustrate the breadth and depth of research conducted by project members and affiliates. W2133 members and affiliates continue to develop methodologically innovative and policy relevant research in the broad areas of recreation demand analysis, land use, ecosystem service valuation, benefits transfer, stated preference, and climate change. These areas support W2133 objectives and meet future information needs of federal, state, and local resource managers and policy makers.

The Annual Meeting ran smoothly thanks to the assistance of several W2133 members, affiliates, and supporters. The efforts of Klaus Moeltner, Brent Sohngen, Kim Rollins, Don Snyder, Fen Hunt and as always, John Loomis and Randy Rosenberger deserve special recognition.

I am proud to have served as President for the 2009/2010 project year and look forward to next year's meeting in Albuquerque!

Cheers,

Roger H. von Haefen
North Carolina State University

W2133 Past and Present Objectives

2007-2011 (W2133)

1. Natural Resource Management Under Uncertainty
    a. Economic Analysis of Agricultural Land, Open Space and Wildland-Urban Interface Issues
    b. Economic Analysis of Natural Hazards Issues (Fire, Invasive Species, Natural Events, Climate Change)
2. Advances in Valuation Methods
    a. Improving Validity and Efficiency in Benefit Transfers
    b. Improving Valuation Methods and Technology
3. Valuation of Ecosystem Services
    a. Valuing Changes in Recreational Access
    b. Valuing Changes in Ecosystem Services Flows
    c. Valuing Changes in Water Quality

2002-2006 (W1133)

1. Estimate the Economic Benefits of Ecosystem Management of Forest and Watersheds
2. Calculate the Benefits and Costs of Agro-Environmental Policies
3. Estimate the Economic Value of Changing Recreational Access for Motorized and Non-Motorized Recreation
4. Estimate the Economic Values of Agricultural Land Preservation and Open Space

1997-2001 (W133)

1. Valuing Ecosystem Management of Forests and Watersheds
2. Benefits and Costs of Agro-Environmental Policies
3. Valuing Changes in Recreational Access
4. Benefits Transfer for Groundwater Quality Programs

1992-1996 (W133)

1. Provide Site-Specific Use and Non-Use Values of Natural Resources for Public Policy Analysis
2. To Develop Protocols for Transferring Value Estimates to Unstudied Areas

1987-1991 (W133)

1. To Conceptually Integrate Market and Nonmarket Based Methods for Application to Land and Water Resource Base Services
2. To Develop Theoretically Correct Methodology for Considering Resource Quality in Economic Models and for Assessing the Marginal Value of Competing Resource Base Products
3. To Apply Market and Nonmarket Based Valuation Methods to Specific Resource Base Outputs

W2133 Past and Future Objectives (cont.)

1974-1986 (W133)

1. N/A

1967-1972 (WM-59)

1. N/A

Participating Institutions

Colorado State University
Cornell University
Iowa State University
Louisiana State University
Michigan State University
North Carolina State University
North Dakota State University
Ohio State University
Oregon State University
Penn State University
Texas A&M University
University of California Statewide Administration
University of California, Berkeley
University of California, Davis
University of Connecticut-Storrs
University of Delaware
University of Georgia
University of Illinois
University of Kentucky
University of Maine
University of Massachusetts
University of New Hampshire
University of Rhode Island
University of Wyoming
Utah State University
Washington State University
West Virginia University

2010 List of Attendees

Last Name	First Name	Affiliation	Email
Abbott	Josh	Arizona State	joshua.k.abbott@asu.edu
Abidoye	Babatunde	Iowa State	babidoye@iastate.edu
Bergstrom	John	Georgia	jberg@uga.edu
Bhattacharjee	Subhra	Iowa State	subhra@iastate.edu
Boyle	Kevin	Virginia Tech	kjboyle@vt.edu
Braden	John	Illinois	jbb@illinois.edu
Carson	Richard	UC San Diego	rcarson@ucsd.edu
Colby	Bonnie	Arizona	bcolby@email.arizona.edu
Drake-McLaughlin	Niko	Reed College	drakemcn@reed.edu
Duffield	John	Montana	john.duffield@mso.umt.edu
Dvarskas	Anthony	NOAA	anthony.dvarskas@noaa.gov
Evans	Keith	Iowa State	jaherrig@card.iastate.edu
Fletcher	Jerry	West Virginia	jfletch@wvu.edu
Hanemann	Michael	Berkeley	hanemann@berkeley.edu
Hansen	LeRoy	USDA-ERS	lhansen@ers.usda.gov
Herriges	Joe	Iowa State	jaherrig@iastate.edu
Hoehn	John	Michigan State	hoehn@msu.edu
Hoffmann	Sandy	RFF	shoffmann@rff.org
Hunt	Fen	NIFA / USDA	fhunt@nifa.usda.gov

Last Name	First Name	Affiliation	Email
Jakus	Paul	Utah State	paul.jakus@usu.edu
Kaplowitz	Michael	Michigan State	kaplowit@msu.edu
Katuwal	Hari	New Mexico	katuwalh@unm.edu
Klaiber	Allen	Penn State	aklaiber@psu.edu
Kobayashi	Mimako	Nevada-Reno	mkobayashi@cabnr.unr.edu
Kuminoff	Nick	Arizona State	kuminoff@asu.edu
Loomis	John	Colorado State	jloomis@lamar.colostate.edu
Lupi	Frank	Michigan State	lupi@msu.edu
Mansfield	Carol	RTI	carolm@rti.org
McLeod	Don	Wyoming	dmcLeod@uwyo.edu
Moeltner	Klaus	Nevada-Reno	moeltner@cabnr.unr.edu
Mueller	Julie	Northern Arizona	julie.mueller@nau.edu
Navrud	Stale	Norwegian University	stale.navrud@umb.no
Nepal	Naresh	New Mexico	nnepal@unm.edu
Netusil	Noelwah	Reed College	netusil@reed.edu
Poe	Greg	Cornell	glp2@cornell.edu
Ready	Rich	Penn State	rcr10@psu.edu
Rollins	Kim	Nevada-Reno	krollins@cabnr.unr.edu
Rosenberger	Randy	Oregon State	r.rosenberger@oregonstate.edu
Schnier	Kurt	Georgia State	kschnier@gsu.edu
Shaw	Douglass	Texas A&M	wdshaw@ag.tamu.edu

Last Name	First Name	Affiliation	Email
Shultz	Steven	Nebraska-Omaha	sshultz@mail.unomaha.edu
Siikamäki	Juha	RFF	juha@rff.org
Smith	Kerry	Arizona State	kerry.smith@asu.edu
Snyder	Don	Utah State	don.snyder@usu.edu
Sohngen	Brent	Ohio State	sohngen.1@osu.edu
Spiridonov	Georgi	Delaware	georgisp@udel.edu
Stefanova	Stela	NECA	sstefanova@neca.org
von Haefen	Roger	NC State	rhhaefen@ncsu.edu
Wandschneider	Phil	Washington State	pwandschneider@wsu.edu
Weber	Matt	US EPA - Oregon	weber.matthew@epa.gov

2010 Final Program

Notes:

- Paper presenters are in **boldfaced** below.
- All sessions will be held in the Saguaro Room.

Wednesday, February 24

5:30pm	**Opening Reception, Cottonwood Grove**
6:30pm	**Group Cookout, Cottonwood Grove**

Thursday, February 25

7:30am **Group Breakfast & Registration, Main Dining Room**

<u>Session 1</u> **<u>Recreation I</u>**

8:30am **Frank Lupi (Michigan State)** and Min Chen (Michigan State)
"When Site Characteristics in Recreation Demand Models are Endogeneously Supplied, Are Estimated Values Biased?"

8:50am **Babatunde Abidoye (Iowa State)** and Joseph Herriges (Iowa State)
"RUM Models Incorporating Nonlinear Income Effects"

9:10am **Juha Siikamäki (RFF)**
"Use of Time for Outdoor Recreation in the United States, 1965-2007"

9:30am **15-Minute Break**

<u>Session 2</u> **<u>Hedonics / Land Use I</u>**

9:45am **Steven Shultz (Nebraska-Omaha)** and Nick Schmitz (Minnesota-Mankato)
"Hedonic Estimates of Open-Space and Low Impact Development Sub-Division Designs to Evaluate the Feasibility of Stormwater Management and Flood Control Programs"

10:05am **Noelwah R. Netusil (Reed College)** and Niko Drake-McLaughlin (Reed College)
"Valuing Walkability and Vegetation in Portland, Oregon"

10:25am **Don McLeod (Wyoming)**, Graham McGaffin (Wyoming), Christopher Bastian (Wyoming), Catherine Keske (Colorado State) and Dana Hoag (Colorado State)
"Identifying Influential Factors for Colorado and Wyoming Landowners

Regarding Conservation Easement Acceptance"

10:45am **15-Minute Break**

Session 3 **Ecosystem Services I**

11:00am **John Bergstrom (Georgia)**, Alan Covich (Georgia), Rebecca Moore (Georgia), James Caudill (Fish and Wildlife Service) and Peter Grigelis (Fish and Wildlife Service)
"A Conceptual Framework and Plan for Valuing Ecosystem Goods and Services Provided by U.S. National Wildlife Refuges"

11:20am **LeRoy Hansen (ERS)**, Ronald Reynolds (Fish and Wildlife Service) and Charles Loesch (Fish and Wildlife Service)
"Coupling Economic and Ecosystems Models to Better Target Conservation Funds"

11:40am **John Hoehn (Michigan State)**, Michael Kaplowitz (Michigan State) and Frank Lupi (Michigan State)
"Valuing Ecosystem Services: Testing the Extent of the Market in Benefits Transfer"

12:00pm **Group Lunch, Main Dining Room**

Session 4 **Meta Analysis / Benefits Transfer**

1:30pm **Randy Rosenberger (Oregon State)** and Tom Stanley (Hendrix College)
"Publication Selection Bias in Empirical Estimates of Recreation Demand Own-Price Elasticity: A Meta-Analysis"

1:50pm **Stale Navrud (Norwegian University)** and Henrik Lindhjem (Norwegian Institute for Nature Research)
"Using Meta-Analysis for International Benefit Transfer of Forest Ecosystem Services"

2:10pm **John Braden (Illinois)**, Xia Feng (William and Mary) and DooHwan Won (Sugshen Women's University)
"Waste Sites and Property Values: A Meta-Analysis"

2:30pm **15-Minute Break**

Session 5 **Stated Preference**

2:45pm **Richard Carson (UC-San Diego)**, Brett Day (East Anglia), Ian Bateman (East

Anglia), Diane Dupont (Brock), Jordan J. Louviere (Technology-Sydney), Sanae Morimoto (Kobe), Riccardo Scarpa (Waikato) and Paul Wang (Technology-Sydney)
"Task Independence in Stated Preference Studies: A Test of Order Effect Explanations"

3:05pm **John Loomis (Colorado State)** and Catherine Keske (Colorado State)
"Did the Great Recession Reduce Visitor Spending and Willingness to Pay for Nature-Based Recreation? Evidence from 2006 and 2009"

3:25pm **Julie Mueller (Northern Arizona)** and John Loomis (Colorado State)
"Using Bayesian Estimation to Improve Efficiency in Willingness-to-Pay Estimation: An Example Using the Mexican Spotted Owl"

3:45pm **15-Minute Break**

<u>Session 6</u> <u>Recreation II</u>

4:00pm **Keith Evans (Iowa State)** and Joseph Herriges (Iowa State)
"Rounding in Recreation Demand Models: A Latent Class Count Model"

4:20pm **Georgi Spiridonov (Delaware)** and George Parsons (Delaware)
"The Effect of Choice Set Formation on Welfare Measures: An Application of Random Utility Models to Beach Recreation in the Mid-Atlantic Region"

4:40pm **Carol Mansfield (RTI), Roger von Haefen (NC State)**, Daniel Phaneuf (NC State) and George Van Houtven (RTI)
"Measuring Nutrient Reduction Benefits for Policy Analysis Using Linked Non-Market Valuation and Environmental Assessment Models"

5:00pm **Business Meeting w/ Comments from Fen Hunt and Donald Snyder**

5:45pm **Reception, Rincon Terrace**
6:45pm **Group Dinner, Main Dining Room**

Friday, February 26

7:30am **Group Breakfast, Main Dining Room**

<u>Session 7</u> <u>Stated Preference II</u>

8:30am **Sandy Hoffmann (RFF/Alberta)**, Allen Krupnick (RFF) and Vic Adamowicz (Alberta)
"Who Speaks for the Household: Differences in Spouses' Willingness to Pay and How These are Resolved in a Couple"

8:50am **Subhra Bhattacharjee (Iowa State)**, Joseph Herriges (Iowa State) and Catherine Kling (Iowa State)
"Capturing Preference Uncertainty Under Incomplete Scenarios Using Elicited Choice Probabilities"

9:10am **Kim Rollins (Nevada-Reno)** and Mimako Kobayashi (Nevada-Reno)
"Risk Preferences of Private Property Owners Facing Wildfire Risks in Nevada: Preliminary Results from the Pilot Survey Data"

9:30am **15-Minute Break**

Session 8 **Hedonics / Land Use II**

9:45am **Joshua Abbott (Arizona State)** and Allen Klaiber (Penn State)
"Assessing Tradeoffs in Land Use Service Flows Within Subdivisions at Multiple Spatial Scales"

10:05am **Nick Kuminoff (Arizona State)** and Jaren Pope (Virginia Tech)
"Hedonic Valuation, Land Value Capitalization and the Willingness to Pay for Public Goods"

10:25am **Allen Klaiber (Penn State)** and Kerry Smith (Arizona State)
"Quasi Experiments, Capitalization, and Estimating Tradeoffs for Changes in Spatially Delineated Amenities"

10:45am **15-Minute Break**

Session 9 **Ecosystem Services II**

11:00am **Douglass Shaw (Texas A&M)**, Therese Grijalva (Weber State) and Robert Berrens (New Mexico)
"Species Preservation versus Development: An Experimental Investigation under Uncertainty"

11:20am **Matt Weber (EPA)** and Joe Marlow (Sonoran Institute)
"Public Values Related to the Santa Cruz River in Southern Arizona"

11:40am **Brent Sohngen (Ohio State)**, Sujithkumar Surendran Nair (Ohio State), Kevin King (Ohio State), Norman Fausey (Ohio State), Jonathan Witter (Ohio State), Douglas Southgate (Ohio State)
"Integrated Watershed Economic Model for Non-Point Source Pollution Management in Upper Big Walnut Watershed, OH"

12:00pm **Group Lunch, Main Dining Room**

Session 10 **Stated Preference and Recreation**

1:30pm **Greg Poe (Cornell)**, Antonio Bento (Cornell), Ben Ho (Cornell) and John Taber (Cornell)
"Culpability and Willingness to Pay for Environmental Quality: A Contingent Valuation and Experimental Economics Study"

1:50pm **Paul Jakus (Utah State)** and Dale Blahna (USDA Forest Service)
"The Welfare Effects of Restricting Off-Highway Vehicle Access to Public Lands"

2:10pm **John Duffield (Montana)**, David Paterson (Montana) and Chris Neher (Montana)
"What is the Value of a Trip to a National Park? Searching for a Reference Methodology"

2:30pm **15-Minute Break**

Session 11 **Climate Change, State Preference, and Fisheries**

2:45pm **Rich Ready (Penn State)** and Jacqueline Yenerall (Penn State)
"Using a Choice Modeling Framework to Project Land Use Decisions"

3:05pm **Kevin Boyle (Virginia Tech)**, Darla Hatton-MacDonald (CSIRO), Mark Morrison (Charles Sturt) and John Rose (Sydney)
"Untangling Differences in Values from Internet and Mail Stated Preference Studies*"*

3:25pm **Kurt Schnier (Georgia State)**
"Heterogeneous Spatial Preferences and Mobility Effects in Fisheries: The Case of the Deadliest Catch"

3:45pm **15-Minute Break**

Session 12 **Stated Preference III**

4:00pm **Klaus Moeltner (Nevada-Reno)**, Mimako Kobayashi (Nevada-Reno) and Kimberly Rollins (Nevada-Reno)
"Latent Thresholds Analysis of Choice Data with Multiple Bids and Response Options"

4:20pm **Hari Katuwal (New Mexico)**, Alok Bohara (New Mexico), Jennifer Thacher (New Mexico) and James Price (New Mexico)
"Valuing Urban River Water Quality Improvements in Developing Cities: An Application of Choice Experiments"

4:40pm **Carol Mansfield (RTI) and Roger von Haefen (NC State)**
"Piping Plovers, Off-Road Vehicles and Beach Closures at Cape Hatteras National Seashore"

5:00pm **Adjourn**

Modeling Spatial Spillover Effects in Willingness to Pay Estimates from Dichotomous Choice Contingent Valuation Surveys: An Example Using the Mexican Spotted Owl

Julie Mueller
Assistant Professor
The W.A. Franke College of Business
Northern Arizona University
P.O. Box 15066
Flagstaff, AZ 86011
phone (918) 523-6612
julie.mueller@nau.edu

John Loomis
Professor
Dept. of Agricultural and Resource Economics
Colorado State University
Fort Collins, CO 80523-1172
fax (970) 491-2067
phone (970) 491-2485
jloomis@lamar.colostate.edu

Abstract

We present an application of a Bayesian spatial probit model estimating US residents' WTP to protect critical habitat for the endangered Mexican Spotted Owl from a Dichotomous-Choice Contingent Valuation (DC CV) survey. If respondents' propensities to vote "yes" on a WTP question is similar to those in nearby locations, spatial dependence exists within the data, and traditional probit models will result in biased estimated coefficients and thus biased WTP estimates. Few studies applying spatial probit models to estimate WTP exist, however, recent advances in Bayesian estimation through application of Markov Chain Monte Carlo simulations and Gibbs sampling allow tractable estimation of spatial probit models that explicitly model spatial dependence. We estimate WTP using a traditional non-spatial probit and a spatial probit. The spatial autoregressive parameter is statistically significant in the spatial probit, indicating that spatial spillover effects exist within our data. Values of WTP calculated from the spatial models are statistically different from the WTP from the non-spatial probit. Therefore, we conclude that failure to model spatial dependence with our CV data results in underestimation of WTP.

Introduction and Background

Mexican Spotted Owls are found in the Southwestern United States and Mexico. In the early nineties, it was proposed that without habitat protection the Mexican Spotted Owl would be extinct within 15 years. Therefore, the Mexican Spotted Owl was added to the list of Endangered Species in 1993. [1] Despite the large amounts of protected critical habitat, the Mexican Spotted Owl remains threatened today.[2] Four million of the designated acres of critical habitat for the Mexican Spotted Owl are in Arizona. The spotted owl requires old growth forests for its habitat, and the designation of forests as protected areas has sparked a controversial debate in the Southwest region of the US about the benefits and costs of endangered species habitat recovery. In previous studies, the benefits of habitat recovery for the Mexican Spotted Owl were obtained using Non-market Valuation. Non-market Valuation is a methodology for obtaining values for environmental goods and services not bought and sold in typical markets. Because no market "price" exists for preservation of Mexican Spotted Owl habitat, estimation techniques are employed to determine a value. The most commonly applied method of Non-market Valuation to obtain values for endangered species habitat is Contingent Valuation (CV). CV is a stated preference methodology of Non-market Valuation. Stated preference methodologies obtain values for environmental goods and services from survey data. Empirical methodologies are used to obtain average Willingness to Pay (WTP) estimates, or values for protecting critical habitat. Total benefits are calculated by summing average WTP across the relevant geographical region.

Previous studies on the Mexican Spotted Owl have found average WTP to protect habitat are approximately $45 per person per year (Loomis and Elkstrand, 1997). Because Spotted Owl habitats have value beyond the species preservation through recreation and use, it is reasonable

[1] http://ecos.fws.gov/speciesProfile/profile/speciesProfile.action?spcode=B074
[2] http://www.biologicaldiversity.org/species/birds/Mexican_spotted_owl/index.html

to believe that people living closer to the habitat may have a higher WTP for preservation. While distance to an environmental amenity is a common indicator of an individual's value, few studies have examined how WTP varies with distance from protected habitat. This study uses data already obtained from a Contingent Valuation Survey on the Mexican Spotted Owl, focusing the empirical analysis to consider how WTP varies with distance to habitat.

Many CV studies apply the dichotomous-choice elicitation format as recommended by Arrow et al (1993). Dichotomous-choice methodologies involve sampling a large number of respondents using a WTP question that is in a "voting" or "bid" format. Estimating WTP from a dichotomous choice survey traditionally involves the use of Maximum Likelihood estimation techniques. Application of other estimation procedures is uncommon, and to date, few studies apply alternative methods (Halloway, Shankar and Rahman, 2002).

Yet, it is reasonable to believe that WTP will be similar for respondents living in the same region, particularly when the non-market good used for valuation has both use and non-use values. If observations of the dependent variable are similar to those in nearby locations, spatial dependence exists within the data, and traditional probit models will result in biased estimated coefficients and therefore biased WTP estimates. Few studies applying spatial probit models exist and none have been applied to CVM of endangered species habitat. Recent advances in Bayesian estimation through application of Markov Chain Monte Carlo simulations allow tractable estimation of spatial probit models that explicitly allow for spatial dependence and alleviate the possibility of biased estimated coefficients. In this paper, we present an application of a Bayesian Spatial Probit model to investigate spatial spillover effects on WTP estimates.

Method

Bayesian estimation of a spatial probit involves repeated sampling using the Gibbs MCMC method. The spatial dependence in the probit model is represented as follows, where W is an $n \times n$ spatial weights matrix, ρ is the spatial autoregressive parameter, y is the observed value of the limited-dependent variable, y^* is the unobserved latent (net utility) dependent variable and X is a matrix of explanatory variables.

$$y = \begin{cases} 1 \; if \; y^* > 0 \\ 0 \; if \; y^* \leq 0 \end{cases}$$

$$y^* = (I_n - \rho W)^{-1} X\beta + \varepsilon$$

$$\varepsilon \sim N(0, I_n)$$

If ρ is not statistically significant, the spatial model collapses to the standard binary probit model. We estimate the general model and relax the strict independence assumption used in traditional probit models by allowing changes in one explanatory variable for one observation to impact the values of other observations within a neighboring distance as defined by the spatial weights matrix, *W*. Intuitively, if the amount of endangered species habitat protected is reduced for an individual observation, this will likely result in an increased distance to habitat for that household *and* neighboring households, resulting in a marginal impact that goes beyond what is represented in a simple estimated coefficient. LeSage and Pace (2009) label the differing spatial impacts direct, indirect and total. In a traditional probit, marginal impacts are measured by

$$\partial E[y|x_r]/\partial x_r' = \varphi(\bar{x}_r \beta_r)\beta_r, \qquad \textbf{(1)}$$

where x_r is the rth explanatory variable, $\bar{x}_r$ is its mean, β_r is a non-spatial probit estimate, and $\varphi(\cdot)$ is the standard normal density. With a spatial probit,

$$\partial E[y|x_r]/\partial x_r' = \varphi(S^{-1} I_n \bar{x}_r \beta_r) \odot S^{-1} I_n \beta_r, \qquad \textbf{(2)}$$

where $S = (I_n - \rho W)$. In the spatial probit, the expected value of the dependent variable due to a change in x_r is now a function of the product of two matrices instead of two scalar parameters. The *direct* impact of changing x_r is represented by the main diagonal elements of **(2)**, and the *total* impact of changing x_r is the average of the row sums of **(2)**. Note that the *direct* impact is a function of ρ and W and is therefore different than the traditional estimated coefficient. The *indirect* or spatial spillover effect is the *total* impact minus the *direct* impact. We obtain WTP using estimated coefficients from a spatial probit, and we also obtain WTP taking into account the total possible impacts across space for the three explanatory variables.

We test the following hypotheses:

1. H_O: ρ=0

   H_A: $\rho \neq 0$
2. Ho: $WTP_{\text{Non-Spatial}} = WTP_{\text{Spatial Using Estimated Coefficients}}$

   H_A: $WTP_{\text{Non-Spatial}} \neq WTP_{\text{Spatial Using Estimated Coefficients}}$
3. Ho: $WTP_{\text{Non-Spatial}} = WTP_{\text{Spatial Using Total Impacts}}$

   H_A: $WTP_{\text{Non-Spatial}} \neq WTP_{\text{Spatial Using Total Impacts}}$

Data

The data are from a survey of US residents for WTP to preserve habitat for the Mexican Spotted Owl. See Loomis (2000) for a detailed description of the data. In addition to the typical questions for a contingent valuation survey, information was obtained about the distance from the respondents' residence to the nearest Mexican Spotted Owl habitat. WTP is proposed to be a function of the bid amount, distance from the nearest habitat and the importance the respondent places on jobs and environmental protection.

Results

Both traditional ML and Bayesian spatial probit models are estimated. The results are presented in Table 1. The spatial autoregressive parameter shows the Bayesian equivalence of statistical significance in the spatial probit, thus we reject the first null hypothesis in favor of the spatial model.[i] The statistically significant ρ indicates that the estimated coefficients in the non-spatial probit are biased, and may lead to incorrect estimates of WTP.

To test whether the WTP estimates are statistically different, we use the Krinsky-Robb (1986) procedure to estimate 9,000 draws for WTP from the non-spatial probit, and we use the post-estimation draws for estimated coefficients to find WTP from the non-spatial models.[ii] We find statistical evidence to reject our second null hypothesis of equality of WTP in the non-spatial and spatial models with both types of spatial calculations of WTP at the 95% level of confidence. Tables 2 and 3 show the results from the hypothesis tests.

It is noteworthy that the WTP per household using the estimated coefficients from the spatial model is 5% higher than the non-spatial model. However, when we account for the *total* spatial impacts of the three independent variables to calculate WTP, we find that WTP using the total impacts is higher than WTP using simply estimated coefficients.

Conclusions and Implications for Further Research

We find that WTP obtained from a traditional probit model is less than WTP from a spatial probit. In addition, we find WTP in a spatial probit to be even greater when spatial spillover effects are incorporated, indicating that failure to model the spatial dependence in our data leads to underestimates of WTP.

Figures and Tables

Table 1: Probit Estimation Results for Spatial and Non-spatial Models with Estimates of WTP

	Non-spatial		**Spatial**				
Variable	*Coefficient*	*p-value*	*Coefficient*	*p-value*	*Direct Impacts*	*Indirect Impacts*	*Total Impacts*
Constant	0.3005	0.3006	0.3344	0.1266			
Bid Amount	-0.0053	<0.0001	-0.0054	<0.0001	-0.0015	-0.0004	-0.0019
Distance	-0.0002	0.0298	-0.0002	0.0128	-0.0001	0.0000	-0.0001
Pro-job	-0.2065	<0.0001	-0.2056	<0.0001	-0.0578	-0.0136	-0.0714
Protect	0.1279	<0.0001	0.1292	<0.0001	0.0363	0.0086	0.0449
Rho			0.2002	0.0049			
WTP per household	**$51.16**		**$54.85**				**$59.81**

Figure 1: WTP Draws from Estimated Models

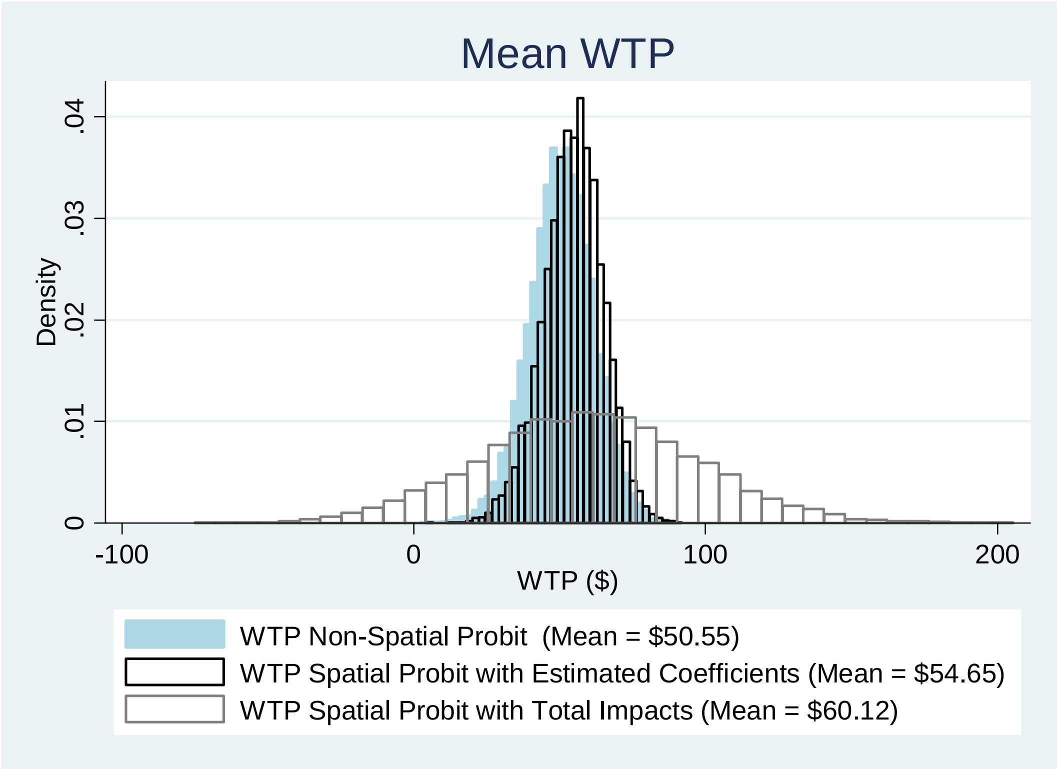


Table 2: Hypothesis Test Results for Equality of Means for Non-Spatial WTP and WTP from Spatial Model Using Estimated Coefficients

Two-sample t test with unequal variances

Variable	Obs	Mean	Std. Err.	Std. Dev.	[95% Conf.	Interval]
meanwtp	9000	50.55991	.1167873	11.07942	50.33098	50.78884
wtpest~f	9000	54.65041	.1084862	10.2919	54.43775	54.86306
combined	18000	52.60516	.081143	10.88648	52.44611	52.76421
diff		-4.090499	.1594006		-4.402939	-3.778058

```
    diff = mean(meanwtp) - mean(wtpestcoeff)                      t = -25.6618
Ho: diff = 0                     Satterthwaite's degrees of freedom =    17901

    Ha: diff < 0                 Ha: diff != 0                 Ha: diff > 0
 Pr(T < t) = 0.0000         Pr(|T| > |t|) = 0.0000          Pr(T > t) = 1.0000
```

Table 3: Hypothesis Test Results for Equality of Means for Non-Spatial WTP and WTP from Spatial Model Using Total Impacts

```
. ttest meanwtp == wtptotalimpact, unpaired unequal

Two-sample t test with unequal variances
------------------------------------------------------------------------------
Variable |     Obs        Mean    Std. Err.   Std. Dev.   [95% Conf. Interval]
---------+--------------------------------------------------------------------
 meanwtp |    9000    50.55991    .1167873    11.07942    50.33098    50.78884
wtptot~t |    9000    60.12353    .3929917    37.28247    59.35318    60.89388
---------+--------------------------------------------------------------------
combined |   18000    55.34172    .2080589    27.91403     54.9339    55.74953
---------+--------------------------------------------------------------------
    diff |           -9.563623    .4099778               -10.36726   -8.759989
------------------------------------------------------------------------------
    diff = mean(meanwtp) - mean(wtptotalimpact)                   t = -23.3272
Ho: diff = 0                     Satterthwaite's degrees of freedom =  10576.2

    Ha: diff < 0                 Ha: diff != 0                 Ha: diff > 0
 Pr(T < t) = 0.0000         Pr(|T| > |t|) = 0.0000          Pr(T > t) = 1.0000
```

References

Arrow et al. “Report of the NOAA Panel on Contingent Valuation.” *Federal Register* 58(10) 4602-14.

Krinsky, I. & Robb, A. L. (1986) On approximating the statistical properties of elasticities, *Review of Economics and Statistics*, 68, pp. 715–719.

Holloway, Garth, Shankar, Bhavani and Rahman, Sanzidur. (2002) Bayesian spatial probit estimation: a primer and an application to HYV rice adoption. *Agricultural Economics* 27 pp. 383-402.

Li et. Al. (2009) “Public support for reducing US reliance on fossil fuels: Investigating household willingness-to-pay for energy research and development. *Ecological Economics,* 68, pp. 731–742.

LeSage, J. and R.K. Pace. Introduction to Spatial Econometrics. 2009. CRC Press.

Loomis, John and Elkstrand, Earl. “Economic Benefits of Critical Habitat for the Mexican Spotted Owl: A Scope Test Using a Multiple-Bounded Contingent Valuation Survey.” *Journal of Agriculture and Resource Economics* 22(2) 356-366.

Loomis, John B. and White, Douglas S. “Economic Benefits of Rare and Endangered Species: Summary and Meta-Analysis.” *Ecological Economics* 18 (1996): 197-126.

Loomis, John B. “Vertically Summing Public Good Demand Curves: An Empirical Comparison of Economic Versus Political Jurisdiction.” (2000). *Land Economics* 78 (2): 312-321.

Smith, Tony E. and LeSage, James P. (2004). “A Bayesian Probit Model with Spatial Dependencies.” Spatial and Spatiotemporal Econometrics. Advances in Econometrics Volume 18, pp. 127-160.

Yoo, Seung-Hoon. (2004) A Note on a Bayesian Approach to a Dichotomous Choice Environmental Model, *Journal of Applied Statistics*, Vol. 31, No. 10, pp.1203–1209.

---

[i] The “p-values” are calculated using the method described in Gelman et al *****

[ii] Jeanty, P. Wilner. 2007. "wtpcikr: Constructing Krinsky and Robb Confidence Interval for Mean and Median Willingness to Pay (WTP) Using Stata."North American Stata Users' Group Meetings 2007, 8.

Publication Selection Bias in Empirical Estimates of Recreation Demand Own-Price Elasticity: A Meta-Analysis

Randall S. Rosenberger
Department of Forest Ecosystems & Society
Oregon State University
Corvallis, OR

T. D. Stanley
Department of Economics & Business
Hendrix College
Conway, AR

Acknowledgments:
This research was supported by funds from the U.S. EPA STAR Grant #RD-832-421-01 to Oregon State University and the USDA Forest Service, Rocky Mountain Research Station. Although the research described in this article has been funded wholly or in part by the United States Environmental Protection Agency through grant/cooperative agreement number RD-832-421-01 to Oregon State University, it has not been subjected to the Agency's required peer and

policy review and therefore does not necessarily reflect the views of the Agency and no official endorsement should be inferred.

Publication Selection Bias in Empirical Estimates of Recreation Demand Own-Price Elasticity: A Meta-Analysis

Abstract

A meta-regression analysis of own-price elasticity of recreation demand estimates in the U.S. shows significant publication selection bias based on simple and multivariate FAT-PET tests. However, these tests also reveal that there is a genuine empirical elasticity measure. While the raw average from the data shows elasticity to be unitary (-0.997), this estimate is one-fold to six-fold too elastic, respectively, when compared to the multivariate PEESE estimate accounting for heterogeneity (-0.893) and the simple PEESE estimate (-0.158). These results are based on nearly 600 estimates of own-price elasticity drawn from the recreation demand literature. One previous MRA was conducted on own-price elasticity estimates (Smith and Kaoru, 1990). We estimate a similar OLS regression model and find substantial consistency between our model and Smith and Kaoru's model according to sign and significance of moderator variables (i.e., determinants of elasticities). However, when a multivariate FAT-PET-MRA, which captures variations in elasticity estimated due to their standard errors and weights the data according to these standard errors, most of the moderator variables change in sign or significance. Nonetheless, we confirm Smith and Kaoru's model and general conclusions that researcher modeling decisions and assumptions, along with theoretical expectations, do matter. This is exhibited in the high degree of heterogeneity in the recreation demand literature.

JEL Classifications: C21; C51; Q26; Q51; R22

Keywords: Meta-analysis; Price Elasticity; Publication selection bias; Recreation Demand

Introduction

Recreation demand models have been empirically estimated for over a half-century using an indirect method proposed by Harold Hotelling in 1947. Collectively, there have been over 329 recreation demand studies[1] providing over 2,700 empirical estimates of the access value to recreation resources from 1958 to 2006 (Rosenberger and Stanley 2007). Only one other study evaluated estimates of own-price elasticity of recreation demand. Smith and Kaoru (1990) conducted a meta-regression analysis (MRA) of recreation own-price elasticity estimates, including approximately 77 studies providing 185 own price elasticity estimates from 1970 to 1986. They did not formally test for publication selection bias in their data. This paper tests for publication selection bias in elasticity estimates from the recreation demand literature.

Own-price elasticity measures the sensitivity of demand to changes in prices. Price elasticity is typically defined as the percentage change in quantity (e.g., recreation trips) resulting from a one-percentage change in price (e.g., travel costs). While price elasticities are unitless measures of demand's responsiveness to price changes, they are a function of an estimated price coefficient ($\delta q/\delta p$) and the ratio of prices and quantities (p/q) typically evaluated at their mean values. If a double-log model is estimated, it can easily be shown that the price coefficient is the elasticity.

Previous meta-regression analyses have been conducted on elasticity measures, including private good brands/markets (Tellis 1988), money demand (Knell and Stix 2005), residential water demand (Espey, Espey and Shaw 1997; Dalhuisen et al. 2003), gasoline demand (Espey 1997, 1998), and cigarette demand (Gallett and List 2003). Dalhuisen et al. (2003) included a

[1] This estimate includes only those studies that reported access values for recreation resources. Not included in this total are studies that estimated demand functions without providing consumer surplus estimates and studies providing estimates of marginal values or values per choice occasion.

dummy variable identifying unpublished studies and found a significant difference between elasticity measures in published and unpublished studies, ceteris paribus. Gallett and List (2003) included a dummy variable identifying the top 36 journals, finding a significant difference in elasticity estimates for the top journals, ceteris paribus. Stanley (2005a) evaluated the residential water elasticity data using the funnel asymmetry and precision effect tests (FAT-PET), uncovering significant publication bias as a function of the standard error of the price elasticity measures; price elasticities of water demand are exaggerated four-fold through publication selection bias. However, Knell and Stix (2005) also apply the FAT-PET test on elasticities of money demand and found small and insignificant publication selection bias.

Publication Selection Bias Tests

Publication selection bias results from a literature of reported estimates that are not an unbiased sample of the actual empirical evidence.[2] Researchers and reviewers often have a preference for statistically significant results or for results that conform to prior theoretical expectations, or both. Publication selection has long been recognized as an important problem in economics (e.g. Card and Krueger 1995; DeLong and Lang 1992; Feige 1975; Leamer 1983; Leamer and Leonard 1983; Lovell 1983; Roberts and Stanley 2005; Rosenberger and Johnston 2009; Tullock 1959, to cite but a few). The tendency to report only statistically significant results is greatly bolstered when there is also professional consensus regarding the existence and direction of an effect—such as the ‘Law’ of demand. When primary survey data are used to estimate the price coefficient of a demand relation, the first estimated coefficient produced will

[2] Some regard ‘publication selection bias’ to be a misnomer, because publication need not be involved. Researchers will learn that there is a preference for statistically significant findings and will tend to selectively report these in any report, published or not. ‘Selection biases’ or ‘reporting biases’ are more descriptive terms for the phenomena discussed in this paper.

not necessarily be the one that is reported. Rather, analysts will wish to be sure that the estimated demand relation is 'valid.' Validity will require, at a minimum, that the price coefficient be negative and in many cases that it be statistically significant as well. Thus, the sample of reported estimates may not be random, and, if not, any summary of estimates will be biased. "Publication bias (aka 'file-drawer problem') is a form of sample selection bias that arises if primary studies with statistically weak, insignificant, or unusual results tend not to be submitted for publication or are less likely to be published" (Nelson and Kennedy 2009, p. 347).

Wide application of MRA in economics suggests that publication biases are often as large as or larger than the underlying parameter being estimated (Doucouliagos and Stanley 2009; Hoehn 2006; Krassoi Peach and Stanley 2009; Stanley 2005a, 2008). For example, the negative sign of own-price elasticity is often required to validate the researcher's estimated demand relation. Should a positive coefficient be produced, researchers feel obligated to re-specify the demand relation, find a different econometric estimation technique, identify and omit outliers, or somehow expand the dataset. As a result of such publication selection, reported price elasticities of water demand are exaggerated by a factor of nearly four (Dalhuisen et al. 2003; Stanley 2005a). Needless to say, the water board of a drought-stricken area will be greatly disappointed to find that a doubling of residential water rates reduces usage by a mere 10% and not the expected 40%. Because the 'Law' of demand is so widely accepted, demand studies will ironically exhibit the greatest publication bias.

Over the past decade, meta-analysis has become routinely employed to identify and correct publication selection in economics research (Ashenfelter et al. 1999; Card and Krueger 1995; Coric and Pugh 2008; Doucouliagos 2005; Doucouliagos and Stanley 2009; Egger et al.

1997; Gemmill et al. 2007; Görg and Strobl 2001; Knell and Stix 2005; Krassoi Peach and Stanley 2009; Longhi, Nijkamp and Poot 2005; Mookerjee 2006; Roberts and Stanley 2005; Rose and Stanley 2005; Stanley 2005a, b, 2008). However, in environmental economics, meta-analysis has been widely applied but with limited focus on publication selection and other potential biases (Hoehn 2006; Nelson and Kennedy 2009; Rosenberger and Johnston 2009). Previous MRAs in environmental economics have treated publication selection bias as arising from the source of an estimate; a form of systematic heterogeneity among the metadata (Smith and Huang 1993, 1995; Rosenberger and Stanley 2006). Typically a dummy variable identifying the publication type is added as an independent variable in the MRA (Rosenberger and Stanley 2006) or a sample selection model is estimated as a form of model specification test (Smith and Huang 1993, 1995). However, more robust tests of publication selection bias are available.

In economics, it has become standard practice to include the standard errors (or their inverse, precision) in a MRA to identify and correct for publication selection bias

$$effect_i = \beta_0 + \alpha_0 SE_i + \sum \beta_k \mathbf{Z}_{ki} + \varepsilon_i \qquad (1)$$

(Card and Krueger 1995; Doucouliagos 2005; Doucouliagos and Stanley 2009; Egger et al. 1997; Gemmill et al. 2007; Rose and Stanley 2005; Stanley 2005a, 2008). Where ε_i is a random error, $\mathbf{Z_i}$ is a matrix of moderator variables that reflect key dimensions in the variation of the 'true' empirical effect (heterogeneity) or identify large-sample biases that arise from model misspecification, and SE_i are the reported standard errors of the estimated effects.

Simulations have shown that meta-regression model (1) provides a valid test for publication bias (H_1:$\alpha_0 \neq 0$), called 'funnel-asymmetry test' (FAT), and a powerful test for genuine empirical effect beyond publication selection (H_1:$\beta_0 \neq 0$), called a 'precision-effect test' or PET) (Stanley 2008). The reason why this approach works is that the standard error serves as a proxy for the amount of selection required to achieve statistical significance. Studies that have large standard errors are at a disadvantage in finding statistically significant effect sizes. Effect sizes need to be proportionally larger than their standard errors, because statistical significance is typically determined by a calculated t-value where the standard error is in the denominator. Such imprecise estimates will likely require further re-estimation, model specification, and/or data adjustments to become statistically significant. Thus, we expect to see greater publication selection in estimates with larger SE, ceteris paribus. This correlation between reported effects and their standard errors has been observed in dozens of different areas of economics research (Doucouliagos and Stanley 2008).

However, Eq (1) likely contains substantial heteroskedasticity because SE is an estimate of the standard error of the elasticity measure that varies from observation to observation. Eq (1) therefore can be estimated using weighted least squares (WLS) by dividing through by SE:

$$t_i = {effect_i}/{SE_i} = \alpha_0 + \beta_0 \, 1/{SE_i} + \sum \beta_k \, \mathbf{Z}_{ki}/{SE_i} + v_i \qquad (2)$$

A simplified version of Eq (2) has been used as a test for publication selection bias:

$$t_i = \alpha_0 + \beta_0 \, 1/{SE_i} + v_i \qquad (3)$$

(Egger et al. 1997; Sutton et al. 2000). The null hypothesis of no publication selection bias (H_0: $\alpha_0 = 0$) is the test for publication selection bias. This method is related to funnel graphs and therefore is called a ‘funnel-asymmetry test’ (FAT) (Stanley 2005a). A funnel graph plots precision (1/SE) against the elasticity estimate. Figure 1 shows a funnel graph of union-productivity partial correlations where FAT tests show little sign of publication selection bias (Stanley 2005a). Compare Figure 1 with Figures 2 and 3 that show asymmetric distributions for elasticity measures of efficiency wage and residential water demand, respectively. In these latter two cases, the null hypothesis of no publication selection bias is rejected.

The meta-regression estimate of β_0 in Eq (3) is shown to serve as a test for a genuine empirical effect corrected for publication bias (Stanley 2008). Given 1/SE is a measure of the precision of the empirical effect, the test (H_0: $\beta_0 = 0$) is called the ‘precision effect test’ (PET), where the null hypothesis is no genuine empirical effect. Combining these two tests, Eq (3) is called a FAT-PET-MRA.

FAT (H_0: $\alpha_0 = 0$) has low power as a publication selection bias test and PET (H_0: $\beta_0 = 0$) shows a downward bias in β_0 (Stanley and Doucouliagos 2007; Stanley 2008). However, in the presence of publication selection bias, the observed effect and its standard error have a nonlinear relationship. This nonlinearity with respect to SE forms the basis for estimating an empirical effect corrected for publication selection bias, or precision-effect estimate with standard error (PEESE). A simple power series is used to estimate the nonlinear relationship. Beginning with the simplest form:

$$effect_i = \beta_0 + \alpha_0 SE_i^2 + \varepsilon_i \tag{4}$$

Note, the square of SE (i.e., the variance of each estimated elasticity) is included. A WLS version of Eq (4) to control for heteroskedasticity is derived by dividing through by SE:

$$t_i = \alpha_0 SE_i + \beta_0 \, {}^{1}\!/_{SE_i} + v_i \tag{5}$$

Note that there is no intercept and a second independent variable (SE) is included as compared with Eq (3). In Eq (5), $\hat{\beta}_0$ is the estimate of the effect (elasticity) corrected for publication selection or the precision-effect estimate with standard error (PEESE). Stanley and Doucouliagos (2007) provide simulations that show PEESE greatly reduces the potential bias of publication selection.

Determinants of Elasticity

Several factors are known to affect elasticity estimates, including presence of substitutes, income effect, necessity of the good, time dimensions of price changes and scope of the affected resource. These factors give rise to variation in elasticity estimates. For example, a demand model that evaluates price changes for a particular campground with substitutes will estimate a more elastic demand than a model that evaluates the demand for camping in general, where substitution across multiple sites holds demand fairly constant at the activity level with price changes at a particular site. In addition to these expected variations due to theoretical considerations, researcher decisions and assumptions regarding experimental design, and treatment and analysis of data may affect elasticity estimates (Smith and Kaoru 1990). In previous MRAs of price elasticities (Tellis 1988; Espey, Espey and Shaw 1997), determinants have been classified as demand model specification factors, environmental characteristics factors, data characteristics factors, and estimation method factors.

Demand model specification factors include measures of model structure, specification (omitted variables), functional form, and type of travel cost method. Environmental characteristics include measures of activity type, geographic region, presence of developed facilities at the recreation site, and land management agency. Dummy variables identifying the resource type are included, such as lake, river, ocean, etc, as well as differentiating warmwater and coldwater resources. Data characteristics include measures of survey mode, scope of model, types of visitors, sample design, and types of trips. Estimation methods include measures of estimator types such as ordinary least squares (OLS), Poisson and Negative Binomial, corrections for endogenous stratification, ML-truncation, and censored models.

Data

Empirical estimates of own-price elasticity of recreation demand were derived from the published literature as part of a larger project (Rosenberger and Stanley 2007). Empirical recreation demand studies were identified through previous bibliographies, electronic database searches, and formal requests sent to graduate programs and listservers. Each document was screened for inclusion in the database using the following criteria—(1) written documentation must be available; (2) estimate of use value must be provided; (3) use values must be for outdoor recreation related activities; (4) these use value estimates must be measures of access value (all-or-nothing, not marginal values); and (5) studies must evaluate recreation resources in Canada or the United States. Therefore, the selection criteria were not directly targeting demand functions and elasticity measures; however, the database does cover the majority of recreation demand studies.

The database currently contains 329 documents that jointly provide 2,705 estimates of recreation use values. The studies were documented from 1958 to 2006 based on data collected from 1956 to 2004. Own-price elasticity measures are only derived from travel cost studies, including individual and zonal, and were either directly coded from estimates provided in the documents, or were calculated when enough information was provided to do so. The price elasticity database contains 610 estimates from 119 documents from 1960 to 2006.

Table 1 provides variable definitions and descriptive statistics. Own-price elasticity of recreation demand (P_ELAST) is the dependent variable in all subsequent analyses. ELAST_SE is the standard error of the elasticity estimate. The independent variables account for potential factors that affect the variation in price elasticity estimates. Model specification variables include the presence and number of site characteristic variables in the demand model (SITEVR and NSITEVAR, respectively); the presence of substitute site price (SUBPRICE) and whether the value of time was included in the travel cost variable (TIMECOST). Functional form is captured by a linear-linear (LINLIN) and log-linear (LOGLIN) forms, with double log and linear-log the omitted category. A dummy variable also identifies whether outliers were removed from the data prior to model estimation (OUTLIER).

Environmental characteristics factors include several activity types (the omitted category include all other recreation activities that individually have low sample sizes) and geographic region (NEAST and SOUTH, with other regions omitted due to correlations with other variables). These factors also identify sites with developed facilities (DEVREC) and sites located on national forests (USFS) and state parks (STPARK) (omitted categories include other public agencies and private lands). Resource types are identified, including LAKE, BAY (or estuary), OCEAN and RIVER, with land being the omitted category. Water temperature was also coded as warmwater (WARMWAT) and coldwater (COLDWAT).

Data characteristics include MAIL surveys (all other modes are omitted due to correlation with other factors) and single site models (SSITE). Visitor type includes resident visitors

(RESIDENT) with non-resident and mixed visitors as omitted. ONSITE identifies studies that derived their sample on-site (other sampling designs such as user list and general population are omitted). Models that only include single destination trips (SINGDEST) or primary purpose trips (PRIMARY) are also identified, as well as models based on day trips only (DAYTRIP).

Estimation methods include OLS, Poisson/Negative Binomial count data models (POISNB), and estimators that corrected for truncation (TRUNC), censoring (CENSOR), and endogenous stratification (ENDOGST). Other independent variables include a TREND variable and whether the elasticity measure was calculated (ELASTC), not directly reported in the primary documents.

Results

Figure 4 plots the funnel graph for elasticity estimates against their precision (1/SE). The plot is asymmetric with more precise estimates corresponding to inelastic measures. The raw average elasticity is unitary elasticity (-0.997), while the median elasticity is inelastic (-0.567). Table 2 reports the simple FAT-PET-MRA and PEESE tests without moderator variables. The FAT test null hypothesis (H_0: $\alpha_0 = 0$) is rejected, signaling publication selection bias. The PET test null hypothesis (H_0: $\beta_0 = 0$) is also rejected, meaning there is a genuine empirical estimate of elasticity. The PEESE estimate of empirical elasticity ($\hat{\beta}_0$) is significant and -0.158. These simple FAT-PET and PEESE tests ignore heterogeneity captured by the determinants of elasticity measures.

Nelson and Kennedy (2009) note that MRAs should account for heteroskedasticity, dependence and heterogeneity of metadata. Heteroskedasticity is captured through the use of standard error weights in the models. Hausman tests for dependency among the data emerging as intrastudy correlation among observations derived from the same study reject the classical regression in favor of a fixed or random effects panel model (Rosenberger and Loomis 2000). Further, Lagrange Multiplier tests favor a random effects specification that captures intrastudy dependence in the error term. However, when the standard error weights are used, the WLS specification is preferred. Therefore, Table 3 reports the fully specified multivariate FAT-PET and PEESE models.

Four estimated models are provided in Table 3, including an OLS model with White's heteroskedastic consistent coefficient standard errors (Model A), an OLS unweighted FAT-PET-MRA (Model B), a WLS FAT-PET-MRA with standard errors of elasticity measures as weights (Model C), and a WLS PEESE-MRA with standard errors of elasticity measures as weights (Model D). Our primary focus will be on Models C and D; however, Models A and B are provided for general comparisons.

Model C performs best with an adjusted-R^2 of 0.78 as compared with Model A (0.54) and Model B (0.67). Including the FAT-PET measure of publication selection bias improves model performance, as well as weighting the data by the SE of elasticity measures. Of the 41 moderator variables, over half (21 out of 41) change in sign or significance when accounting for FAT-PET and weighting the data, signaling substantial heteroskedasticity among the data related to varying standard errors of elasticity measures (Figure 4). The weighted FAT-PET-MRA, when accounting for heterogeneity among the data still rejects the FAT null hypothesis (H_0: $\alpha_0 = 0$) of no publication selection bias and rejects the PET null hypothesis (H_0: $\beta_0 = 0$) of no genuine empirical effect, although the magnitude of these coefficients differ from the simple FAT-PET in Table 2.

The estimated coefficients for the moderator variables are interpreted based on the direction of the effect—a positive sign means more inelastic (i.e., decreases elasticity) while a negative sign means more elastic (i.e., increases elasticity). Interpretations of elasticity determinants or moderator effects are restricted to Model C. Seven out of eight demand model characteristics factors are statistically significant, with five having a positive effect (more inelastic) and two having a negative effect (more elastic). Including site characteristic measures (SITEVAR) in the demand model increases the elasticity measure, while increases in the number of site characteristic variables (NSITEVAR) in the demand model specification decreases the elasticity measure (each additional site characteristic variable decreases elasticity by 0.186). A linear-linear (LINLIN) functional form provides more inelastic elasticities than other functional forms, as does including a price of substitute sites (SUBPRICE) and the value of time in the travel cost measure (TIMECOST). Individual travel cost models (TCMIND) likewise provide more inelastic elasticities than zonal travel cost models. Removal of outlier observations from the data (OUTLIER) increases the elasticity, where these outliers may either be uncharacteristically large prices or number of trips.

Eleven out of 19 environmental characteristics factors are statistically significant, with the majority leading to more elastic elasticities. Camping (CAMP) and motorized boating (MBOAT) provide more elastic elasticities whereas fishing (FISH) and general recreation (GENREC) studies provide more inelastic measures. Studies conducted in the northeastern U.S. (NEAST) estimated more price responsive demands. Sites with developed recreation facilities (DEVREC) showed less price responsive demands. National forest studies (USFS) showed more elastic demands. Studies of lake (LAKE) and bay/estuary (BAY) resources, in addition to water temperature (warmwater (WARMWAT) and coldwater (COLDWAT)), had more elastic demands.

Overall, data characteristics factors did not influence elasticity measures with three out of seven being statistically significant. These statistically significant factors were all negative, meaning resident visitors only studies (RESIDENT), studies drawing samples onsite (ONSITE), and studies for primary purpose users (PRIMARY) resulted in more elastic demands.

Estimation method factors were mostly significant in determining elasticity measures (three out of five). All statistically significant factors led to more inelastic elasticities, including OLS models (OLS), censored models (CENSOR), and models correcting for endogenous stratification (ENDOGST). There is a general trend in more elastic elasticity estimates over time (TREND), with an increase in elasticity of -0.009 per year. Those studies that did not report elasticities but provided enough information for them to be calculated were from more inelastic demand models (ELASTC).

The fully specified multivariate PEESE-MRA has an adjusted-R^2 of 0.70. It is generally consistent in sign and significance of most moderator variables. However, the primary interest in this model is the precision effect estimate with standard errors (PEESE) for the true elasticity estimate. The PEESE estimate of empirical elasticity ($\hat{\beta}_0$) from Model D is statistically significant and is -0.893.

Conclusions

The recreation demand literature shows substantial publication bias in estimates of own-price elasticity based on the simple FAT-PET tests, but does demonstrate that there is a genuine empirical effect. However, based on a simple PEESE test, the precision effect estimate with standard errors shows the standard error-corrected empirical elasticity is -0.158—recreation demand is not price responsive (i.e., inelastic). Compared with this PEESE estimate, the raw average elasticity measure (-0.997) is six-fold more elastic while the raw median elasticity measure (-0.567) is four-fold too elastic. This means that management decisions or policies based on central tendency measures based on the raw data will exaggerate the price responsiveness of recreation demand. For example, pricing decisions based on these raw measures will underestimate potential revenue from price increases, or will overestimate demand responses to changes in prices. Accounting for the variation in the standard error of elasticity measures is important. The standard error weighted average is -0.172, a much more moderate bias of one-fold higher elasticity.

Even after accounting for the substantial heterogeneity of the recreation demand literature through determinants of elasticities, there is still substantial publication selection bias and a genuine empirical effect present in this literature based on the multivariate FAT-PET-MRA. However, conclusions about the magnitude of publication selection bias when accounting for heterogeneity of the data is much more modest. The PEESE-MRA estimate of elasticity (-0.893) is close to the raw average elasticity measure (-0.997), about a one-fold exaggeration similar to the difference in the simple PEESE estimate and the standard error weighted average.

Smith and Kaoru (1990) estimated a meta-regression analysis of own-price elasticities of recreation demand for the early literature. Their MRA is consistent with Model A (OLS with heteroskedastic consistent coefficient standard errors). The demand model, environment and data characteristics factors were the same in sign and significance for those factors in both models. However, compared with Model C (FAT-PET-MRA with standard errors as weights), most of the factors changed in sign or significance. For example, the inclusion of substitute site price in the demand model specification was estimated to be significant and negative in Smith and Kaoru's (1990) model, as it was in Model A in this study. However, when the FAT measure (1/SE) is included and the data are weighted by the standard error of the elasticity measures, the sign switches to positive and significant (Model C).

Smith and Kaoru's (1990) general implications still hold, but with different directional effects of moderator variables. They conclude that "modeling assumptions do matter" (p.271). With over half of the moderator variables being related to the magnitude of the elasticity estimated, researcher decisions and assumptions continue to affect this literature beyond what is theoretically expected.

Table 1. Data Description (N = 610)

Variable	Definition	Mean	Std Dev	Min	Max
P_ELAST	Own price elasticity of demand	-0.997	1.040	-5.981	-0.006
ELAST_SE [a]	Std error of elasticity	0.208	0.269	0.003	3.161
SITEVAR	1 = Site characteristics variables in demand model	0.238	0.426	0	1
NSITEVAR[b]	# of site characteristics variables in demand model	0.387	0.919	0	5
LINLIN	1 = Linear-linear demand model functional form	0.251	0.434	0	1
LOGLIN	1 = Log-linear demand model functional form	0.359	0.480	0	1
SUBPRICE	1 = Price of substitute site included in demand model	0.500	0.500	0	1
TIMECOST	1 = Cost of time included in travel cost variable	0.608	0.488	0	1
TCMIND	1 = Individual travel cost model	0.597	0.491	0	1
OUTLIER	1 = Outlier observations removed from data	0.311	0.463	0	1
BIKE	1 = Bicycling	0.034	0.182	0	1
CAMP	1 = Camping	0.046	0.209	0	1
FISH	1 = Fishing	0.323	0.468	0	1
NMBOAT	1 = Non-motorized boating	0.043	0.202	0	1
HIKE	1 = Hiking	0.070	0.256	0	1
HUNT	1 = Hunting	0.090	0.287	0	1
MBOAT	1 = Motorized boating	0.067	0.250	0	1
GENREC	1 = Generalized recreation	0.144	0.352	0	1
NEAST	1 = Northeast region	0.105	0.307	0	1
SOUTH	1 = Southern region	0.236	0.425	0	1
DEVREC	1 = Developed recreation facilities available on-site	0.516	0.500	0	1
USFS	1 = National forest land	0.139	0.346	0	1
STPARK	1 = State park	0.136	0.343	0	1
LAKE	1 = Lake resource	0.306	0.461	0	1
BAY	1 = Estuary or bay resource	0.090	0.287	0	1
OCEAN	1 = Ocean resource	0.044	0.206	0	1
RIVER	1 = River or stream resource	0.148	0.355	0	1
WARMWAT	1 = Warm water resource (lake, river, etc.)	0.128	0.334	0	1
COLDWAT	1 = Cold water resource (lake, river, etc.)	0.090	0.287	0	1
MAIL	1 = Mail survey mode	0.397	0.490	0	1
SSITE	1 = Single site evaluated	0.695	0.461	0	1
RESIDENT	1 = Resident visitors only	0.439	0.497	0	1
ONSITE	1 = Sample drawn on site	0.441	0.497	0	1
SINGDEST	1 = Single destination trips only modeled	0.454	0.498	0	1
PRIMARY	1 = Primary purpose visitors only modeled	0.416	0.493	0	1
DAYTRIP	1 = Day trips only modeled	0.479	0.500	0	1
OLS	1 = Ordinary least squares estimator	0.693	0.461	0	1
POISNB	1 = Poisson/Negative Binomial estimator	0.184	0.387	0	1
TRUNC	1 = Observations truncated in demand model	0.380	0.486	0	1
ENDOGST	1 = Demand model corrected for endogenous stratification	0.134	0.341	0	1
CENSOR	1 = Censored demand model	0.115	0.319	0	1
TREND	Trend (1 = 1960, 2 = 1961,…, 44 = 2003)	25.448	9.355	1	44
ELASTC	1 = Elasticity measure calculated by researcher	0.415	0.493	0	1

[a]N = 558
[b]N = 594

Table 2. Publication Bias Tests (n=558)

Coefficient	**FAT-PET**		**PEESE**
	No Weights	**Weights[a]**	**Weights[a]**
β_0	-0.552*** (0.047)	-0.048*** (0.012)	-0.158*** (0.012)
α_0	-2.151*** (0.137)	-5.946*** (0.295)	----
ELAST_SE (α_0)	----	----	-7.273*** (0.933)
Adj-R^2	0.30	0.42	0.10
F	246***	407***	61***

Dependent variable = P_ELAST.
Coefficient standard errors in parentheses.
*** = p-value ≤ 0.01
** = p-value ≤ 0.05
* = p-value ≤ 0.10
[a]Weights = 1/ELAST_SE

Table 3. FAT-PET and PEESE Meta-Regression Analysis Models.

Variable	**Model A** OLS White's[a]	**Model B** FAT-PET unweighted	**Model C** FAT-PET weighted[b]	**Model D** PEESE weighted[b]
β_0	-1.601*** (0.248)	-1.399*** (0.195)	-0.742*** (0.078)	-0.893*** (0.089)
α_0	----	-1.326*** (0.111)	-4.067*** (0.276)	----
ELAST_SE (α_0)	----	----	----	-3.925*** (0.584)
SITEVAR	-0.370** (0.175)	-0.274* (0.145)	-0.300*** (0.066)	-0.396*** (0.075)
NSITEVAR	0.335*** (0.066)	0.267*** (0.053)	0.186*** (0.025)	0.239*** (0.028)
LINLIN	0.289** (0.114)	0.365*** (0.103)	0.414*** (0.047)	0.385*** (0.054)
LOGLIN	-0.140 (0.127)	-0.078 (0.115)	0.088 (0.060)	0.064 (0.068)
SUBPRICE	-0.289*** (0.087)	-0.154** (0.069)	0.101*** (0.035)	0.063 (0.040)
TIMECOST	0.054 (0.076)	0.103 (0.068)	0.078*** (0.029)	0.142*** (0.033)
TCMIND	0.699*** (0.117)	0.669*** (0.096)	0.526*** (0.045)	0.578*** (0.051)
OUTLIER	-0.176 (0.108)	-0.060 (0.089)	-0.193*** (0.035)	-0.220*** (0.040)
BIKE	0.248 (0.218)	0.301 (0.249)	0.143 (0.092)	0.216** (0.105)
CAMP	-0.344 (0.264)	-0.310 (0.200)	-0.342*** (0.080)	-0.225** (0.091)
FISH	0.383* (0.219)	0.329** (0.164)	0.289*** (0.067)	0.317*** (0.077)
NMBOAT	-0.072 (0.337)	-0.082 (0.219)	-0.091 (0.082)	-0.054 (0.094)
HIKE	-0.199 (0.206)	-0.387** (0.172)	-0.052 (0.071)	-0.036 (0.081)
HUNT	0.116 (0.163)	0.090 (0.150)	0.012 (0.061)	0.012 (0.070)
MBOAT	-0.948*** (0.235)	-0.900*** (0.221)	-0.616*** (0.120)	-0.750*** (0.137)
GENREC	0.070 (0.181)	0.156 (0.152)	0.182*** (0.063)	0.262*** (0.072)
NEAST	0.404** (0.170)	0.013 (0.165)	-0.156*** (0.053)	-0.240*** (0.060)
SOUTH	0.317*** (0.114)	0.343*** (0.090)	-0.059 (0.037)	-0.028 (0.043)
DEVREC	0.124 (0.096)	-0.015 (0.096)	0.312*** (0.042)	0.268*** (0.048)
USFS	-0.291* (0.176)	-0.121 (0.132)	-0.179*** (0.058)	-0.324*** (0.065)
STPARK	0.476*** (0.125)	0.456*** (0.121)	0.034 (0.052)	0.069 (0.060)

Variable	**Model A** OLS White's[a]	**Model B** FAT-PET unweighted	**Model C** FAT-PET weighted[b]	**Model D** PEESE weighted[b]
LAKE	-0.469*** (0.146)	-0.537*** (0.131)	-0.086* (0.044)	-0.150*** (0.050)
BAY	0.002 (0.168)	-0.190 (0.147)	-0.111** (0.052)	-0.172*** (0.060)
OCEAN	-0.257 (0.246)	-0.245 (0.212)	0.058 (0.069)	-0.007 (0.080)
RIVER	-0.291 (0.184)	-0.580*** (0.152)	0.053 (0.041)	0.006 (0.047)
WARMWAT	-0.544** (0.214)	-0.317** (0.154)	-0.179** (0.076)	-0.208** (0.087)
COLDWAT	-0.186 (0.256)	0.001 (0.172)	-0.137** (0.067)	-0.092 (0.076)
MAIL	0.399*** (0.123)	0.306*** (0.106)	-0.014 (0.040)	-0.072 (0.045)
SSITE	0.229** (0.116)	0.245** (0.101)	0.008 (0.045)	-0.035 (0.051)
RESIDENT	-0.340*** (0.116)	-0.198** (0.090)	-0.101** (0.046)	-0.065 (0.053)
ONSITE	-0.209* (0.118)	-0.289*** (0.082)	-0.158*** (0.039)	-0.108** (0.044)
SINGDEST	-0.034 (0.206)	-0.030 (0.138)	0.013 (0.062)	0.052 (0.071)
PRIMARY	-0.574*** (0.204)	-0.355*** (0.134)	-0.223*** (0.055)	-0.207*** (0.063)
DAYTRIP	0.372*** (0.120)	0.173** (0.088)	0.041 (0.044)	0.031 (0.050)
OLS	0.705*** (0.136)	0.682*** (0.103)	0.234*** (0.062)	0.249*** (0.071)
POISNB	0.510** (0.210)	0.452** (0.178)	0.061 (0.094)	0.178* (0.108)
TRUNC	0.515*** (0.148)	0.499*** (0.097)	-0.011 (0.044)	0.056 (0.050)
ENDOGST	-0.086 (0.172)	-0.032 (0.137)	0.150** (0.066)	-0.003 (0.074)
CENSOR	-0.146 (0.206)	-0.214 (0.139)	0.308*** (0.077)	0.335*** (0.088)
TREND	-0.022*** (0.007)	-0.019*** (0.007)	-0.009*** (0.003)	-0.016*** (0.003)
ELASTC	0.371*** (0.119)	0.299*** (0.100)	0.126*** (0.036)	0.236*** (0.040)
Adj-R^2	0.54	0.67	0.78	0.70
F	18***	27***	45***	32***
N	594	542	542	542

Dependent variable = P_ELAST.
Coefficient standard errors in parentheses.
*** = p-value ≤ 0.01
** = p-value ≤ 0.05
* = p-value ≤ 0.10
[a]White's robust heteroskedasticity corrected covariance matrix

[b]Weights = 1/ELAST_SE

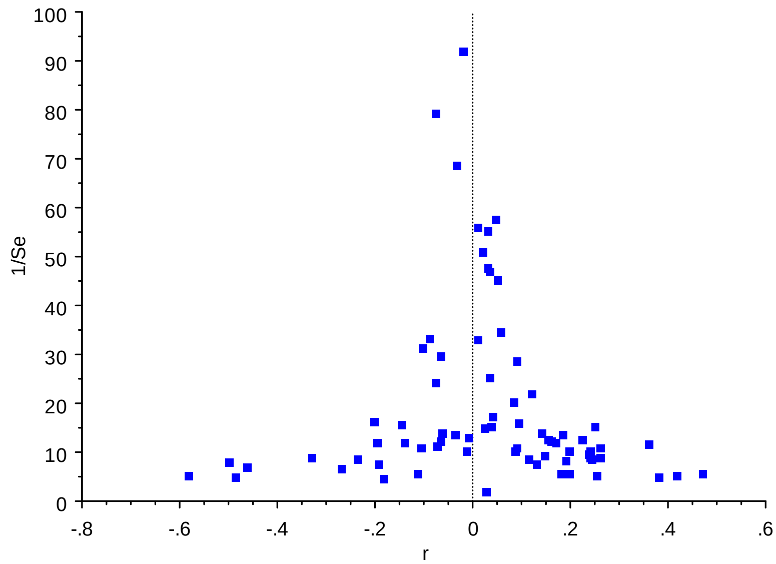


Figure 1: Funnel Graph of Union-Productivity Partial Correlations (r) (Source: Doucouliagos and Laroche (2003)).

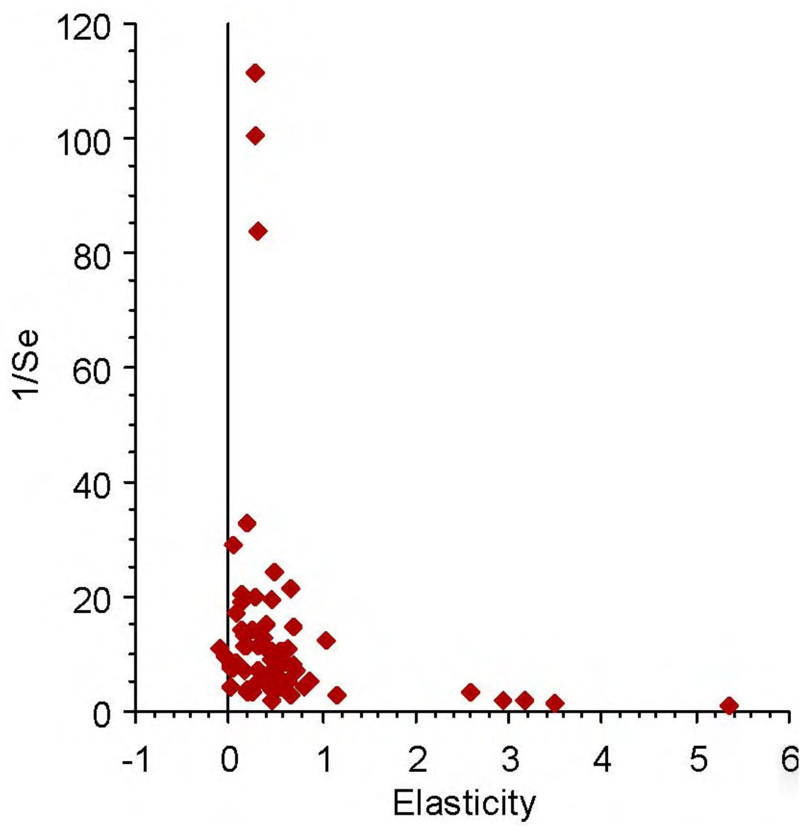


Figure 2: Funnel Graph of Efficiency Wage Elasticities (Source: Stanley and Doucouliagos (2007)).

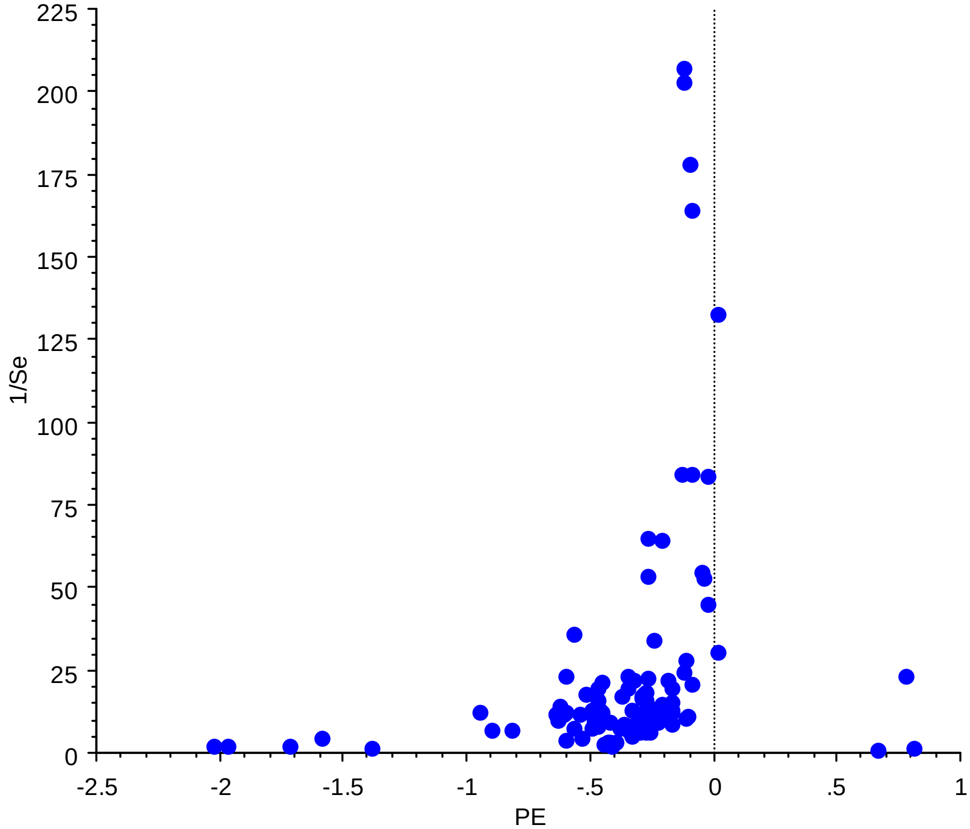


Figure 3: Funnel Graph of Price Elasticities (PE) for Water Demand (Source: Stanley (2005a)).

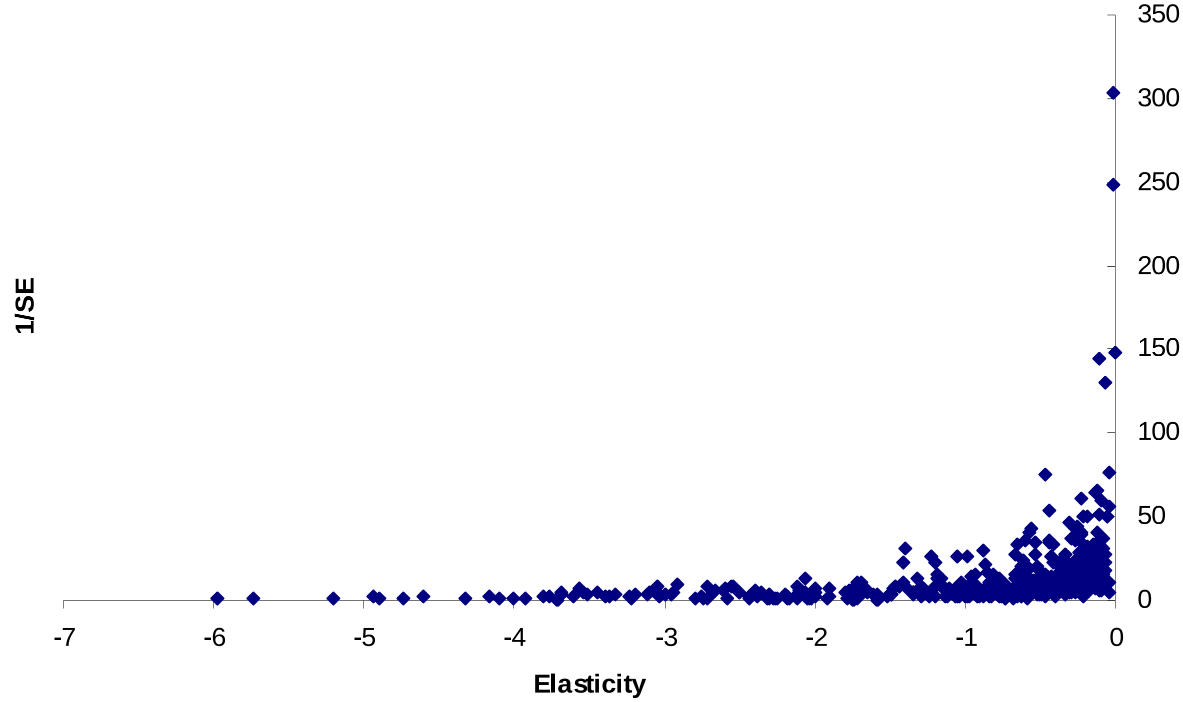


Figure 4: Funnel Graph of Recreation Demand Own Price Elasticities.

References

Ashenfelter O, Harmon C, Oosterbeek, H (1999) A review of estimates of the schooling/earnings relationship, with tests for publication bias. Labour Econ 6(4):453-470.

Card D, Krueger AB (1995) Time-series minimum-wage studies: a meta-analysis. Am Econ Rev 85(2):238-243.

Coric B, Pugh G (2008) The effects of exchange rate variability on international trade: a meta-regression analysis. Appl Econ (in press). DOI 10.1080/00036840801964500.

Dalhuisen J, Florax RJGM, deGroot HLF, Nijkamp P (2003) Price and income elasticities of residential water demand: a meta-analysis. Land Econ 79(2):292-308.

De Long JB, Lang K (1992) Are all economic hypotheses false? J Polit Econ 100(6):1257-1272.

Doucouliagos C (2005) Publication bias in the economic freedom and economic growth literature. J Econ Surv 19(3):367-388.

Doucouliagos C, Laroche P (2003) What do unions do to productivity: a meta-analysis. Industrial Relations 42(4):650-691.

Doucouliagos C, Stanley TD (2008). Theory competition and selectivity. Deakin Working Paper 2008-06.

Doucouliagos C, Stanley TD (2009) Publication selection bias in minimum-wage research? A meta-regression analysis. British J Industrial Relations (in press).

Egger M, Davey Smith G, Schneider M, Minder C (1997) Bias in meta-analysis detected by a simple, graphical test. British Med J 315(7109):629-634.

Espey, M (1997) Explaining the variation in elasticity estimates of gasoline demand in the United States: A meta-analysis. Energy Journal 17:49-60.

Espey, M (1998) Gasoline demand revisited: An international meta-analysis of elasticities. Energy Economics 20:273-295.

Espey, M, Espey J and Shaw WD (1997) Price elasticity of residential demand for water: A meta-analysis. Water Resources Research 33(6):1369-1374.

Feige EL (1975) The consequence of journal editorial policies and a suggestion for revision. J Polit Econ 83(6):1291-1295.

Gallett CA, List JA (2003) Cigarette demand: A meta-analysis of elasticities. Health Economics 12(10):821-835.

Gemmill MC, Costa-Font J, McGuire A (2007) In search of a corrected prescription drug elasticity estimate: a meta-regression approach. Health Econ 16(6):627-643.

Görg H, Strobl E (2001) Multinational companies and productivity: a meta-analysis. Econ J 111(475):723-739.

Hoehn JP (2006) Methods to address selection effects in the meta regression and transfer of ecosystem values. Ecol Econ 60(2):389-398.

Hotelling H (1947) Letter to National Park Service, in: An economic study of the monetary evaluation of recreation in the National Parks. Washington, DC: U.S. Department of the Interior, National Park Service and Recreational Planning Division.

Knell M, Stix H (2005) The income elasticity of money demand: a meta-analysis of empirical results. J Econ Surv 19(3):513-533.

Krassoi Peach E, Stanley TD (2009) Efficiency wages, productivity and simultaneity: a meta-regression analysis. J Labor Res (in press).

Leamer EE (1983) Let’s take the con out of econometrics. Am Econ Rev 73(1):31-43.

Leamer EE, Leonard H (1983) Reporting the fragility of regression estimates. Rev Econ Stat 65(2):306-317.
Longhi S, Nijkamp P, Poot J (2005) A meta-analytic assessment of the effect of immigration on wages. J Econ Surv 19(3):451-477.
Lovell MC (1983) Data mining. Rev Econ Stat 65(1):1-12.
Mookerjee R (2006) A meta-analysis of the export growth hypothesis. Econ Letters 91(3):395-401.
Nelson JP, and Kennedy PE (2009) The use (and abuse) of meta-analysis in environmental and natural resource economics: an assessment. Environ Resour Econ 42(3):345-377.
Roberts CJ, Stanley TD (eds) (2005) Meta-regression analysis: issues of publication bias in economics. Blackwell, Oxford.
Rose AK, Stanley TD (2005) A meta-analysis of the effect of common currencies on international trade. J Econ Surv 19(3):347-365.
Rosenberger RS, Johnston RJ (2009) Selection effects in meta-analysis and benefit transfer: avoiding unintended consequences. Land Econ 85(3):410-428.
Rosenberger RS, Loomis JB (2000) Panel stratification in meta-analysis of economic studies: an investigation of its effects in the recreation valuation literature. J Agric Appl Econ 32(2):459-470.
Rosenberger RS, Stanley TD (2006) Measurement, generalization, and publication: sources of error in benefit transfers and their management. Ecol Econ 60(2):372-378.
Rosenberger RS, Stanley TD (2007) Publication effects in the recreation use value literature: a preliminary investigation. Paper presented at the American Agricultural Economics Association Annual Meeting, Portland, 29 July-1 August 2007. http://purl.umn.edu/9883.
Smith VK, Huang JC (1993) Hedonic models and air pollution: Twenty-five years and counting. Environ Resour Econ 3(4):381-394.
Smith VK, Huang JC (1995) Can markets value air quality? A meta-analysis of hedonic property value models. J Polit Economy 103(1):209-227.
Smith VK, Kaoru Y (1990) What have we learned since Hotelling's letter: A meta-analysis. Economics Letters 32:267-272.
Stanley TD (2005a) Beyond publication bias. J Econ Surv 19(3):309-345.
Stanley TD (2005b) Integrating the empirical tests of the natural rate hypothesis: a meta-regression analysis. Kyklos 58(4):587-610.
Stanley TD (2008) Meta-regression methods for detecting and estimating empirical effect in the presence of publication selection. Oxford Bull Econ Stat 70(1):103-127.
Stanley TD, Doucouliagos H (2007) Identifying and correcting publication selection bias in the efficiency-wage literature: Heckman meta-regression. School working paper – Economic Series 2007, SWP 2007/11. Victoria, Australia: Deakin University.
Sutton AJ, Abrams KR, Jones DR et al (2000) Methods for meta-analysis in medical research. John Wiley and Sons, Chichester.
Tellis GJ (1988) The price elasticity of selective demand: A meta-analysis of econometric models of sales. Journal of Marketing Research XXV:331-341.
Tullock G (1959) Publication decisions and tests of significance – a comment. J Am Stat Assoc 54(287):593.

Did the Great Recession Reduce Visitor Spending and Willingness to Pay for Nature-Based Recreation? Evidence from 2006 and 2009

Dr. John Loomis, Professor
Dept. of Agricultural and Resource Economics
Colorado State University
Fort Collins, CO 80523-1172
John.Loomis@colostate.edu

Dr. Catherine Keske, Assistant Professor
Dept. of Soil and Crop Sciences
Colorado State University
Fort Collins, CO 80523-1170
Catherine.Keske@colostate.edu

July 4, 2010

Acknowledgements: Partial funding for this study came from the Colorado Agricultural Experiment Station project W2133. The Colorado Fourterneer's Initiative provided support for the data collection.

Did the Great Recession Reduce Visitor Spending and Willingness to Pay for Nature-Based Recreation? Evidence from 2006 and 2009

Abstract

Outdoor recreation is a relatively large industry that can diversify public land based economies that have traditionally relied upon resource extraction. But what happens to nature-based recreation visitor spending and benefits during times of national economic recession? To address this question, we replicate a 2006 high mountain recreation study in the same region, three years later during the 2009 recession. Results indicate that nature-based public lands recreation in this area did not experience reductions in total visitor expenditures or total number of visits during the recession. These results imply that nature-based recreation may represent an economically stable industry in public land mountain communities. Total benefits to the visitors themselves are also fairly stable, and there is not a statistically significant decrease in consumer surplus in 2009 compared to 2006.

(JEL Q26)

Keywords: Hiking, consumer surplus, contingent valuation, Colorado, spending, tourism

Introduction

The U.S. economy has undergone considerable change from 2006 to 2009. During 2006-2007, the unemployment rate was under 5%; however, in fall 2009, the national unemployment rate topped 10% for the first time since 1983 (Bureau of Labor Statistics, 2009). After reaching its peak of 14,164 in October 2007, the Dow Jones Industrial Average tumbled to below 6,600 in March 2009. In addition to declines in consumer confidence, the U.S. GDP declined four consecutive quarters during 2008-2009, marking the longest U.S. recession in 60 years (Bureau of Economic Analysis, 2009). The grey literature has dubbed this recession the “Great Recession,” attempting to draw parallels with the Great Depression of the 1930’s (Isidore, 2009).

Many sectors of the U.S. economy, such as the $300 billion domestic automobile industry (McAlinden, et al.), have been hit hard by the recession. However, there has been little attention paid to an industry with sales similar to the U.S. domestic automobile manufacturing industry. This is the active outdoor recreation industry with sales of $290 billion (Outdoor Industry Foundation, 2006). This industry is an aggregate of outdoor recreation equipment sales and recreation trip expenditures for camping, fishing, hunting, snow sports, hiking, water-based recreation and wildlife viewing. Hiker and backpacker spending is one of the largest categories of the active outdoor recreation industry. Trail-based recreation accounts for $83 billion, or nearly 30% of the industry (Outdoor Industry Foundation, 2006). Trail-based recreation spending also supports over 700,000 jobs (Outdoor Industry Foundation, 2006), a figure similar to direct employment in automobile manufacturing (McAlinden, et al.) The majority of the recreation spending is trip related spending (e.g., gasoline, hotels, food), rather than purely purchases of durable equipment. High expenditures and consumer surplus have been documented for nature-based recreation when the economy is at its peak (Outdoor Industry Foundation, 2006; Keske and Loomis, 2007), but have consumer expenditures and willingness to pay for nature-based recreation fallen during the Great Recession?

This study focuses on changes in visitation, expenditures and consumer surplus for nature-based, high mountain recreation during times of macroeconomic change. In this paper, we replicate a 2006 survey design and sampling frame to investigate how visitor use and visitor trip spending changed between 2006 and 2009 for trail-based recreation in Colorado. Hiking and backpacking are popular activities nationwide, and results from the Colorado study may be transferrable to other regions offering these activities. More than one-third of the U.S. population over the age of 16 participates in hiking or backpacking, representing over 50 million participants (Cordell, et al., 1999). Greater than one billion days are spent hiking or backpacking in the United States (Cordell, et al., 1999). Others project that this is a trend that will continue into the future. For example, Bowker et al. (1999) forecasts that the number of days of hiking is expected to increase by about 5% a decade between the years 2000 and 2050, for a cumulative increase of 25% over this time period.

If recreation expenditures are unchanged across periods of economic prosperity and decline, then recreation has potential to be a stabilizing economic sector in rural economies. Mountain economies, including our study area, frequently experience competing economic development interests from energy and mineral extraction industries (Loomis, 2002), which are known for economic volatility (Davis and Tilton, 2005). Extraction of energy/minerals is a notable driver of the Colorado economy. One report estimates the 2007 economic contribution of the energy and mineral industry to the Colorado economy to be as high as $11 billion (approximately 5%) of the state's gross state product (Corporation for a Skilled Workforce, 2009). However, like other extraction economies, the western side of Colorado has been quite hard hit by the economic recession, due in part to the drop in energy and mineral exploration in the region that encompasses our study area (Bureau of Land Management, 2007).

The economic boom-bust cycles associated with extraction are driven by fluctuations in prices and spending leakages that result from high rates of commodity exportation. When an economic structure is heavily export-based, other sectors within the economy may be under-developed, creating an over-reliance on one industry. This is commonly known as the "Dutch Disease" (Davis, 1995). The exportation of energy and minerals, combined with the often temporary workforce, frequently does not generate sustainable regional spending multipliers. The combination of commodity price volatility and an undiversified, extraction-based economy can result in a deep economic retraction. Economic diversification is a key component of achieving economic stability in these resource-based economies (Davis, 1995; Davis and Tilton, 2005; Iimi, 2007). One avenue toward diversification in resource based economies is to promote natural resource-based recreational use by outsiders. Of course tourism is also export driven, so one issue of interest in this paper is whether the tourism sector is able to withstand periods of significant recession. If so, then promotion of recreation industry in natural resource based economies may serve to diversify and thus stabilize rural economies that have been traditionally reliant on extraction. In this paper we test whether visitor recreation expenditures withstood the "Great Recession" and thus may be used as a strategy to diversify mountain economies.

Sales to visitors are only part of the economic picture. The economic efficiency benefits of outdoor recreation include how much more visitors themselves would pay in excess of their travel costs (i.e., their consumer surplus). If visitor income falls during the recession, then it is certainly possible that net willingness to pay (WTP) for recreation opportunities might fall, if the activity is a normal good. Further, net WTP might be influenced by visitors feeling “poorer” due to their wealth losses in the stock and housing markets. To investigate this, we implement a contingent valuation model to test whether willingness to pay changes from 2006 to 2009 in the same Colorado study area. Knowing whether or not benefit estimates are susceptible to macroeconomic fluctuations may be useful for long term federal and state agency public land management planning, including management of recreational areas that have time horizons of 10-15 years. Further, when conducting benefit transfers it is often necessary to combine prior benefit estimates over one or more decades. Thus, it would be useful to know whether these benefit estimates are not affected by economic cycles.

The paper proceeds as follows. First, we formalize the economic indicators to be compared. Next, we introduce the methods used to estimate those indicators during the 2006 boom year and the Great Recession in summer 2009. We then describe the data, statistical results, and draw conclusions.

Testing for Differences in Visitor Use, Visitor Expenditures and WTP

Sales and Visitor Expenditure Hypothesis Tests

Visitor spending for public land and nature-based recreation can be grouped into several categories. Transportation (e.g., gasoline, airfare, rental cars) and traveler accommodations (e.g., hotels, bed-breakfast) are two of the largest sectors. Other important direct sales to tourists include food establishments (e.g., restaurants), and retail sales.

We compare hiker expenditures in Colorado for each of the five spending categories, to determine whether there has been a statistically significant change in the sum of visitor expenditures between summer 2006 and summer 2009. The general form of our hypothesis test for each expend category is:

(1) Ho: $Expend_{i2006} = Expend_{i2009}$ vs Ha: $Expend_{i2006} \neq Expend_{i2009}$

Where $Expend_i$ is visitor expenditures of type i, where i= 1,…5.

This hypothesis will be tested using a t-test of difference in means for the two different sample time periods for each spending category.

Willingness to Pay (WTP)

The net WTP or consumer surplus associated with outdoor recreation on public lands in Colorado is more difficult to estimate than visitor expenditures. Given the relatively free access to public lands (Loomis, 2002), there is likely a benefit to visitors from the opportunity to hike on public lands in the Rocky Mountains in excess of their transportation costs and other trip related costs.

In order to measure net WTP, we utilize the contingent valuation method or CVM (Loomis and Walsh, 1997). In particular, we estimate WTP using a dichotomous choice CVM model. This WTP question format asks whether the visitor would pay a specific increase in trip cost, the magnitude of which is varied across the sample). This model is deemed more market-like and analogous to the price taking behavior familiar to consumers than asking an open-ended question of what the maximum amount a visitor would pay (Loomis and Walsh, 1997).

The utility theoretic foundations of the dichotomous choice model have been well developed (see Hanemann, 1984); and will only be summarized here. We assume that an individual's utility is a function of a recreation experience at site R and the consumption of all other goods (represented by income I). The utility function may be represented as:

(1) $U = f(R, I)$

Utility from visiting a recreation site also depends on an individual's personal preferences which are known only to that individual, so a portion of the utility function is not observable to the researcher. Therefore, some components of each individual's utility function are treated as stochastic, resulting in an indirect utility function and a random term, as follows:

(2) $U = f(R, I) = v(R, I) + e$

where “e” represents an error term.

With the dichotomous-choice WTP question format, survey respondents are asked whether or not they would still take their most recent trip to the recreation site if travel costs were $Bid higher. The respondent is predicted to answer “YES”, if utility from the recreation experience, along with the associated reduction of $*BID* in income, is greater than the individual's original utility level without taking the trip. The "YES" respondent would take the trip (R = 1) at the higher travel cost (I-$Bid), and the "NO" respondent would choose not to take the trip (R = 0). Therefore, the probability of a "YES" response is represented as follows:

(3) $P(YES|\$Bid) = P[v(R=1, I-\$Bid) + e_1 > v(R=0, I) + e_2]$

where e_1, and e_2 are error terms with means of zero (Hanemann, 1984).

In the random utility framework, a visitor is predicted to respond "Yes", if the gain in the deterministic part of the utility function (the indirect utility difference) is larger than the difference in the stochastic part ($e_1 - e_2$). If the difference of the errors ($e_1 - e_2$) is logistically distributed, this gives rise to the parametric logit model. The stylized version of the model estimated is:

(4) $\text{Log}[(\text{Prob YES})/(1-\text{Prob YES})] = \beta o - \beta_1(\$Bid) + \beta_2 X_2 \ldots + \beta n(Xn) + \varepsilon$

where \$Bid is the increase in trip cost the visitor is asked to pay, X's are other independent explanatory variables, and ε is the error term.

WTP Hypotheses Tests

We test for differences in visitor benefits between 2006 and 2009 by using two hypothesis tests. The first involves a statistical test on the equality of coefficients in the logit willingness to pay model. The second test is whether mean WTP has changed between the two time periods.

The statistical test is for differences in specific logit coefficients between the two time periods. In particular, we pool data for the two time periods and include an intercept shift variable, defined as "2006Dum", for 2006 responses. We also create an interaction term using this dummy variable with the other independent variable (Travel Distance) to allow the slope of this coefficient to be different in 2006 than in 2009. This second variable is defined as, "2006Dum*TravelDistance". Thus the empirical model is:

(5) $\text{Log}[(\text{Prob YES})/(1\text{-Prob YES})] = \beta o - \beta_1(\$\text{Bid}) + \beta_2(\text{2006Dum}) + \beta_3(\text{Travel Distance})$

$+\beta_4\,(\text{2006Dum*TravelDistance}) + \varepsilon$

where "2006Dum" =1 if the WTP responses are from 2006, 0 if from 2009.

We included the variable, "Travel Distance," defined as the distance visitors traveled from their home to the site. Previous studies found this variable to be a statistically significant explanatory variable for WTP for Fourteener recreation in 2006 (Keske and Loomis, 2008).

The hypothesis test evaluates whether the coefficients on the dummy variable and the dummy variable*Travel Distance interaction variable, respectively, are statistically significant:

(6a) Ho: β_2=0 vs Ha: $\beta_2 \neq 0$

(6b) Ho: β_4=0 vs Ha: $\beta_4 \neq 0$

The second test used compares the differences in mean WTP between the years 2006 and 2009. The formula for mean WTP is given in Hanemann (1989) and adapted here for each of the two time periods as:

(7) Mean WTP_{2006} =

$[\ln(1+\text{Exp}(\beta_o+\beta_2(\text{2006Dum})+\beta_3\,(\text{MeanTravelDistance}_i)+\beta_4(\text{2006Dum*MeanTravelDistance}_i))]/|\beta_1|$

Where i is 2006 in Equation (7)

(8) Mean $\text{WTP}_{2009} = [\ln(1+\text{Exp}(\beta_o+\beta_3\,(\text{MeanTravelDistance}_i))]/|\beta_1|$

Where i is 2009 in Equation (8).

The hypothesis to be evaluated is whether mean WTP per person per trip is statistically different in 2009 from 2006. Specifically:

(10) Ho: $WTP_{2006} = WTP_{2009}$ vs Ha: $WTP_{2006} \neq WTP_{2009}$

This will be tested by whether the confidence intervals on the two estimates of mean WTP overlap (Creel and Loomis, 1991). Confidence intervals are calculated for the mean WTP (Equation (9)) using the variance-covariance matrix and a procedure adapted to dichotomous choice CVM by Park, Loomis, and Creel (1991).

Data

Our case study area is Quandary Peak, a recreation area that is southwest of Denver, Colorado, and approximately ten miles directly south of the resort town of Breckenridge. Surveys were distributed over three days, on two separate non-holiday weekends during August and September 2006. The mail back survey booklet was designed along the lines of Dillman's Tailored Design Method (Dillman, 2000). The 2006 mail back surveys were distributed by two volunteers trained on survey distribution procedures. Hikers were approached at trailheads and in parking lots at the conclusion of their recreation activity. There were no refusals to take the survey in 2006. After providing the visitors with the survey and a postage paid return envelope, names and addresses were also collected so that a second survey could be mailed to non-respondents. Of the 199 mail back surveys handed out, 129 surveys were returned, for a response rate of 65%.

The survey included separate sections, described as follows:

Information regarding the specific trip: Seven questions regarding trip purpose and recreational activities.

Trip expenditures: Five questions addressing trip expenditures on the trip in Colorado. Respondents were asked to report the amount that they and members of their parties (e.g., family, companions) spent in each category. To put expenditures on a per visitor basis, these expenditures were divided by the number of people in the group. Asking for expenditures from the entire party and then dividing by group size is the preferred approach to avoid overestimating per person expenditures (Stynes and White, 2006).

Dichotomous Choice Contingent Valuation Question. The WTP question was:

> *As you know, some of the costs of travel such as gasoline, campgrounds, and hotels often increase. If the **total cost** of this most recent trip to the recreation area where you were contacted had been $BID **higher**, would you have made this trip to **this** Fourteener? Circle one: YES NO*

The $BID amount had values ranging from $2 to $950. Fourteener refers to the 14,000 foot peak that is often the attraction for many of the hikers visiting this area.

The 2009 data collection process, including trailhead location and survey distribution procedures, mirrored the 2006 data collection process. In 2009, two individuals were trained in the distribution of surveys: a graduate student, and one of the same volunteers who was instrumental in the distribution of the surveys in the 2006 study. As with the 2006 study, visitors were provided with the mail back survey and a postage paid return envelope. Three weeks later, replacement surveys were mailed to non-respondents. A total of 345 surveys were distributed over five weekend days during July and August, 2009. A total of 248 surveys were returned for a response rate of 72%.

2006 and 2009 Visitor Use Estimates Data

Obtaining accurate visitor use estimates for visitation to public lands has been a longstanding challenge (Loomis, 2000). Until the National Visitor Use Monitoring program (NVUM, see English *et al*, 2002), the USDA Forest Service had very inaccurate estimates of overall visitor use. With the advent of NVUM, the agency now has accurate estimates at the National Forest level, but not at specific sites within the National Forest as it is not within the project scope for

NVUM to go to that level of detail. Thus, we turned to alternative sources of data to estimate visitor use in 2006 and 2009.

The majority of the USDA Forest Service Fourteener visitor use data has been collected by the Colorado Fourteeners Initiative (CFI), a non-profit group that receives project direction and grants from the USDA Forest Service, Rocky Mountain Region. CFI is not viewed as a traditional activism organization, but rather, it is regarded as a non-profit group that assists the USDA Forest Service directly with implementing its Fourteener management plans. Visitor use data gathered by the CFI is mainly the result of a "Peak Stewarding Program", where volunteers and staff members approach visitors, primarily from the parking lot or from the summit.

The USDA Forest Service typically adopts CFI data as a measurement of its visitor use, as the CFI stewardship program provides the most accurate information on visitation use available to the USDA Forest Service. Longitudinal CFI data indicate that visitor use did not decline between 2006 and 2009. Data reveal that, if anything, visitor use increased from 2006 to 2009. In 2006, CFI Peak Steward results recorded 121 contacts over 2 non-holiday weekend days, (for an average of 60.5 climbers observed per day). Expanding and projecting this data over 32 non-holiday weekend days from June to September (optimal Fourteener climbing months, due to weather), the estimated weekend use data were roughly 1,936 visitors. In 2009, CFI Peak Stewards reported contact with 500 recreators over 6 days, for an average of 83.3 climbers observed per day, or 2,666 visitors over 32 non-holiday weekends. These observations show an increase in visitors in 2009, compared to 2006.

Survey contact rates from our study also reveals numbers that are consistent with Peak Steward data. In 2006, we distributed 199 mailback surveys over 3 weekend days, for an average of 66.3 per day (Keske and Loomis, 2008). In 2009, surveys were handed out at a similar rate (345 surveys handed out over 5 weekend days, for an average of 69 surveys per day). Thus our data confirms that visitor use did not decline during the times of economic recession. If anything, visits to Fourteeners may have increased, possibly as a result of a tendency for people to visit their home state, rather than to undertake more expensive travel out of state (e.g., Alaska) or internationally (Canadian Rockies or the European Alps).

Results

Prior to presenting the expenditure analysis, we wish to note that monetary expenditures in 2009 were converted to 2006 dollars using the Consumer Price Index (CPI).

Expenditure Hypothesis Test Results

Table 1 presents results from the statistical tests for differences between visitor expenditures in 2006 and 2009. In each of the six comparisons, there is no statistical difference between visitor expenditures in 2006 and 2009 at the 5% level of significance. The only difference that may be of marginal significance is in gasoline purchases, which is significantly different at the 10% level. However, some of the difference in gasoline purchases may be a result of fewer miles driven in 2009, as the price of gasoline increased by a $0.05/gallon according to the American Automobile Association. As can be seen in the last row of Table 1, our conclusions about each category are consistent with the lack of statistical difference in total visitor spending across all categories.

Thus, in terms of our hypothesis tests, we fail to reject the null hypothesis of no difference in visitor expenditures in key tourism sectors. Based on analysis of reported expenditures, hikers in our sample spent similar amounts in 2006 and 2009. When coupled with our discussion above that estimated visitor use did not decrease between 2006 and 2009, this suggests that communities and businesses that rely on nature-based tourism may not have been hard hit by the Great Recession.

WTP Test Hypothesis Results

Table 2 presents the results of the logit model, which pools visitor WTP responses for 2006 and 2009. As expected, the key price coefficient, the $Bid Amount, is negative and statistically significant. This serves as a validity check, indicating respondents took the dollar amount they were asked to pay seriously; the higher the dollar amount respondents were asked to pay, the lower the probability they would pay. The pooled data model has an intercept dummy variable for 2006, as well as the dummy interacted with the Travel Distance variable. In terms of our first hypothesis test, we find that the coefficient on the 2006 intercept dummy is not significant (p=.5258). The interaction of 2006 dummy*Travel Distance coefficient is also not statistically significant (p=.8983). Therefore, we fail to reject the null hypothesis that there is no difference in the 2006 and 2009 coefficients.

Using the coefficients from Table 2, and equations (7) and (8) mean WTP is calculated for year 2006 and 2009, respectively. Table 3 presents the mean WTP estimates obtained from the 2006 and the 2009 data, and the associated 90% confidence intervals. The mean WTP per person per trip in 2009 is \$139 which is 9% below the WTP per person in 2006 (\$152). However, as shown in Table 3, the 90% confidence intervals in 2006 overlap the mean WTP in 2009 and vice versa. This indicates there is no statistical difference between the WTP per person per trip in 2006 and

2009. Thus, we fail to reject the null hypothesis of no difference in mean WTP per visitor between the two time periods. Since our estimate of visitation in 2006 and 2009 showed no decrease, it appears there was no statistically significant change in total benefits between 2006 and 2009.

Perhaps one explanation for these results of no statistical difference in visitor spending and willingness to pay is that hikers visiting these Colorado mountains did not experience a large reduction in income during this recession in 2009 as compared to 2006. Our data indicates that average household income from the 2006 study was $108,733, while in 2009 it fell to $102,968 in 2006 dollars. While this is a 5.3% drop in income, the t-test yields a t-statistic of .21, with associated p-value of .42, indicating no statistical difference in household income in real terms between the two time periods.

However, income is only one measure of economic prosperity. We might have expected the large drop in wealth via the fall of the stock market and housing values to cause some retraction in spending and respondents' willingness to pay higher trip costs. These drops in the stock market and housing values would especially be of concern to higher income individuals, as they typically have substantial holdings in the stock market. Thus they might have felt a psychological impact on their wealth, which may have reduced their willingness to pay for recreation in 2009. However, this phenomenon did not manifest itself in nature-based recreation at this location of Colorado.

Conclusions

A comparison of visitor spending data collected from hikers in Colorado using the same survey questions in 2006 and 2009 demonstrates only one statistically significant reduction in expenditures in 2009—a reduction in gasoline expenditures. The reduction in gasoline expenditures may be a reflection of the 50 mile average reduction in distance travelled in 2009 relative to 2006. This drop in mileage appears to explain most of the drop gasoline spending, from $61 in 2006 to $42 in 2009, because there was only a nickel per gallon difference in gasoline between the two years. However, this is the only decrease in spending that is statistically significant at conventional levels, in this case, at 10%.

Other categories of visitor spending showed little or no change from before the recession. When adjusted for the modest amount of inflation during these years, there was a very slight increase in visitor spending for trip related equipment ($25 in 2006 vs. $28 in 2009), retail supplies ($13 in 2006 vs. $16 in 2009), and restaurant meals ($78 in 2006 vs. $80 in 2009), none of which were statistically significant. The greatest change in expenditures was for hotels, which showed an average ***increase*** from $81 in 2006 to $120 in 2009, but this was not significantly different at the 10% level. Further there was no statistically significant (p=0.44) change in overall total visitor spending summed across all expenditure categories. Thus, we conclude that visitor spending on nature-based tourism remained remarkably stable during this time period. We also compared two independent indicators of visitor use of this peak, and these indicators suggest that visitor use has not decreased between 2006 and 2009, and if anything, visitor use may have increased. The combined effect of no change in neither visitation nor expenditures per visit leads us to conclude that, at least in Colorado, nature-based tourism such as high mountain recreation appears to be fairly recession proof. From this finding, we may conclude that rural and public-lands based communities that have tried to diversify their economies from sole reliance on commodity extraction to include nature-based recreation appear to have made a smart move.

The benefits to the visitors themselves, as measured by net willingness to pay, showed an 18% decrease from $162 per person per trip in 2006 to $139 in 2009. However, this difference was not statistically significant, even at the 10% level. Thus, despite a 250% increase in the unemployment rate and a 50% drop in the stock market, WTP changed by just 9%. Thus, for public lands management agencies who are required to develop long term (10-15 year) plans, it may not be unreasonable for them to presume that recreation benefits over such a long time period are fairly stable. That is, while there will likely be economic downturns and booms during a 15 year planning horizon, the economic efficiency benefits to common public lands visitors such as hikers, will not change significantly during that time period. This stability also bodes well for benefit transfer, as many of the original empirical studies have often been done at different points in the business cycle.

Of course there are limitations to any study, and ours is no exception. It would be beneficial to have such studies before and during the recession for other public lands based recreation to see if this same pattern is observed. Unfortunately, longitudinal data is rare in recreation studies. While hiking is one of the most popular public lands based recreation activities, it would be desirable to have data on other recreation activities such as water-based recreation as well. These limitations point to important avenues for future research.

TABLE 1

Comparison of 2006 and 2009 Per Trip Hiker Expenditures in Colorado ($2006)

Category	**2006 Mean**	**2009 Mean**	**T-Statistic (P-value)**
Miles Driven	264	214	1.12 (.267)
Gasoline Purchases	$61.04	$42.00	1.69 (.092)
Retail Supplies	$13.24	$15.85	-.363 (.717)
Equipment Purchases	$25.14	$28.28	-.441 (.659)
Hotel	$81.62	$129.40	-1.29 (.196)
Food in Restaurants	$78.32	$80.48	-.401 (.689)
Total Expenditures	$246.11	$271.17	-.760 (.447)
Est. Total Seasonal Use*	1936-2126	2208-2665	NA
Est. Total Expenditures*	$476,469- $522,147	$543,411- $665,031	NA

* Range of visitor use estimates calculated from our survey and that of Colorado Fourteener's Initiative for 32 non-holiday weekend days.

TABLE 2

Logit WTP Model Results

Constant	0.861***
(T-statistic)	(4.280)
$ Bid Amount	-0.00579***
	(-8.021)
Travel Distance	0.0023***
	(4.090)
2006 Dummy	0.2182
	(.634)
(2006 Dummy* Travel Distance)	-0.000144
	(-.1278)
McFadden R-squared	.301
Log likelihood	-168.098
LR statistic	144.841
Probability (LR statistic)	0.000
N	348

*** statistical significance at 1% confidence level

TABLE 3

Mean WTP Per Person Per Trip and 90% Confidence Intervals

	Mean WTP	**90% Lower CI**	**90% Upper CI**
2006 data	$152	$123	$190
2009 data	$139	$119	$167

References

Bowker, J.M., D. English and K. Cordell. 1999. Projections of Outdoor Recreation Participation to 2050. In Cordell, K. "Outdoor Recreation in American Life" Sagamore Publishing, Champaign, IL.

Bureau of Economic Analysis. 2009. National Income Accounts. November 9, 2009. http://www.bea.gov/national/index.htm#gdp. Last Accessed November 9, 2009.

Bureau of Labor Statistics. 2009. Employment Situation Summary. November 6, 2009. http://www.bls.gov/news.release/empsit.nr0.htm. Last Accessed November 9, 2009.

Bureau of Land Management. 2007. Draft Oil Shale and Tar Sands Resource Management Plan Amendments to Address Land Use Allocations in Colorado, Utah, and Wyoming and Programmatic Environmental Impact Statement. Volumes I, II, and III. December 2007. DES 07-60.

Cordell, K., B. McDonald, R.J. Teasley, J. Bergstrom, J. Martin, J. Bason and V. Leeworthy. 1999. Outdoor Recreation Participation Trends. In Cordell, K. "Outdoor Recreation in American Life" Sagamore Publishing, Champaign, IL.

Corporation for a Skilled Workforce. 2009. Industry Guidebook: Energy Extraction. Produced for the WEIC Task Force of the Colorado Workforce Development Council by Corporation for a Skilled Workforce. February 2009.

Creel, M. and J. Loomis. 1991. Confidence Intervals for Welfare Measures with Application to a Problem of Truncated Counts. *Review of Economics and Statistics*, Volume 73(2):

Davis, G.A. 1995. Learning to Love the Dutch Disease: Evidence from the Mineral Economies. *World Development* , 23(10): 1765-1779.

Davis, G., and J. Tilton, 2005, The Resource Curse. *Natural Resources Forum*, Aug 29: 233–242.

Dillman, D. 2000. Mail and Internet Surveys: The Tailored Design Method. John Wiley and Sons, New York, NY.

English, D., Kocis, K., Zarnoch, S., and Arnold, J.R. (2002), 'Forest Service national visitor use monitoring process', USDA Forest Service, General Technical Report SRS-57, Southern Research Station, Ashville, NC.

Hanemann, M. 1984. "Welfare Evaluations in Contingent Valuation Experiments with Discrete Responses." *American Journal of Agricultural Economics*. 66(3): 332-341.

Hanemann, M. 1989. "Welfare Evaluations in Contingent Valuation Experiments with Discrete Response Data: Reply." *American Journal of Agricultural Economics*. 71(4): 1057-1061.

Iimi, A. 2007. Escaping from the Resource Curse: Evidence from Botswana and the Rest of the World. International Monetary Fund Staff Papers 54: 663-699.

Isidore, C. 2009. The Great Recession. CNN Money. March 25, 2009.

Keske, C. and J. Loomis. 2008. Regional Economic Contribution and Net Economic Values of Opening Access to Three Colorado Fourteeners. 2008. *Tourism Economics* 14(2): 249-262.

Keske, C.M., and J.B. Loomis. 2007. "High Economic Values from High Peaks of the West". *Western Economics Forum*. 4(1): 34-41.

Loomis, J. and R. Walsh. 1997. Recreation Economic Decisions: Comparing Benefits and Costs. Venture Publishing Inc., State College, PA.

Loomis, J. (2000), 'Counting on recreation use data: a call for long-term monitoring', *Journal of Leisure Research*, 32(1): 93–96.

Loomis, J.B. 2002. Integrated Public Lands Management, Second Edition. Columbia University Press.

McAlinden, S., K. Hill, B. Swiecki. Economic Contribution of the Automotive Industry to the U.S. Economy—An Updated. Economics and Business Group, Center for Automotive Research. Ann Arbor, Michigan.

Outdoor Industry Foundation. 2006. The Active Outdoor Recreation Economy. Available at: http://www.outdoorindustry.org/images/researchfiles/RecEconomypublic.pdf?26

Park, T., J. Loomis, M. Creel. 1991. Confidence intervals for evaluating benefit estimates from dichotomous choice contingent valuation studies. *Land Economics* 67: 64–73.

Stynes, D. and D. White. 2006. Reflections on Measuring Recreation and Travel Spending. *Journal of Travel Research* 45: 8-16.

U.S. Census. Annual Demographic Survey. http://pubdb3.census.gov/macro/032006/hhinc/new06_000.htm. Accessed November 21, 2009.

Hedonic Equilibria, Land Value Capitalization, and the Willingness to Pay for Public Goods[†]

Nicolai V. Kuminoff
Department of Economics
Arizona State University

Jaren C. Pope
Department of Economics
Brigham Young University

This version: May, 2010

First version: December, 2008

[†] We thank Kelly Bishop, Richard Carson, Tom Downes, Roger von Haefen, Matt Kahn, Mike Keane, Glenn MacDonald, Gilbert Metcalf, Alvin Murphy, Ray Palmquist, Jonah Rockoff, Ed Schlee, V. Kerry Smith, Jeff Zabel, seminar participants at Arizona State University, Brigham Young University, Tufts University, University of Tennessee, Utah State University, Virginia Tech, Washington University in St. Louis, and conference participants at AEA and W2133 for valuable comments and suggestions on earlier drafts of this paper. We gratefully acknowledge support from the Lincoln Institute of Land Policy and the National Science Foundation.

Abstract

Models of household location choice provide a theoretical foundation for measuring the willingness to pay for public goods. The difficulty is identification. Empirical work was traditionally believed to suffer from widespread identification problems. Recent studies have revived this literature by demonstrating that quasi-experiments can provide credible estimates for the rates at which shocks to public goods are capitalized into land values. In this paper, we develop a unified framework that relates land value capitalization to the underlying concept of market equilibrium on which welfare measurement is based. The foundation for our analysis is Rosen's description of a differentiated product market with heterogeneous buyers and sellers. First we define the restrictions on preferences and technology that support a welfare interpretation for the rate at which an exogenous shock is capitalized into equilibrium prices. Then we translate those restrictions into testable conditions on micro data sets and on the design of quasi-experiments. Finally, we use the new framework to analyze the differences between: (i) hedonic estimates of the willingness to pay for improvements to public school quality from boundary discontinuity regressions in ten markets and (ii) capitalization rates for changes in test scores that occurred over the first four years of the federal No Child Left Behind program. We find that hedonic measures of the average resident's willingness to pay for improved school quality are four times as large as capitalization based measures.

Keywords: capitalization, hedonic, identification, welfare, school quality.

JEL Codes: C31, D12, L11.

I. Introduction

How can we measure the public's willingness to pay for a public good? This problem has intrigued economists for decades.[1] Recently, Chay and Greenstone (2005) proposed a novel solution—use quasi-experiments to identify the rates at which shocks to public goods are capitalized into land values. The appeal of combining a credible identification strategy with a welfare interpretation of the capitalization effect has led to a resurgence of interest in using markets for private property to assess the benefits of public programs.[2] Despite the growing importance of this methodology, the assumptions that enable us to translate capitalization effects into welfare measures have not been closely examined.

This paper uses the concept of hedonic equilibrium to investigate what land value capitalization reveals about the willingness to pay for public goods. We identify problems with using capitalization to measure willingness to pay, and we propose solutions to those problems. Our conceptual model builds on Rosen's (1974) description of the market for a differentiated product. We consider a market for housing where: (i) a house conveys a bundle of public and private goods; (ii) heterogeneous buyers and sellers make trades to maximize profits and utility; and (iii) equilibrium is described by a hedonic price function. Our point of departure from Rosen is to describe how the price function adjusts following an unexpected shock to a public good influencing the market equilibrium. Depending on the severity of the shock, adjustment may involve a movement along the hedonic price function or a change in its shape. We express the rate of change in equilibrium prices (i.e. the capitalization rate) in terms of the reduced form parameters

[1] Past proposals have included the median voter model (Bergstrom and Goodman 1973, Rubinfeld, Shapiro, and Roberts 1987) the conventional land value capitalization model (Lind 1973, Starrett 1981), the hedonic model of housing market equilibrium (Scotchmer 1985, 1986, Bartik 1987), and equilibrium sorting models of neighborhood choice (Epple and Sieg 1999, Bayer, Ferreira, and McMillan 2007).

[2] For examples see the recent quasi-experimental capitalization studies by Davis (2004), Chay and Greenstone (2005), Greenstone and Gallagher (2008), Linden and Rockoff (2008), Pope (2008), Bin, Landry, and Meyer (2009), Horsch and Lewis (2009), and Cellini, Ferreira, and Rothstein (2010).

of the price function which, in turn, depend on market primitives (preferences, income, and technology). This functional relationship reveals that, in general, capitalization rates do not identify the willingness to pay for public goods.

The scope for divergence between capitalization and welfare depends on the size of the shock and the duration of the study period. As both approach zero, the capitalization rate approaches the marginal willingness to pay (MWTP). In the limit, our model provides a conceptual foundation for Chay and Greenstone's (2005) estimator. As the size of the shock grows, so does the wedge between capitalization and welfare. The identification problem is intuitive. In a hedonic demand system, such as Epple (1987), a non-marginal shock to *any* attribute of a differentiated product will change the MWTP for *every* attribute. All of these changes are condensed into the same capitalization rate. To isolate the willingness to pay for a single attribute, more information is needed.

One way to provide the extra information is to place restrictions on the primitives of Rosen's model. Consider a non-marginal shock to a public good that is capitalized over an interval when the supply of housing is less than perfectly elastic. We prove three restrictions are both necessary and sufficient to interpret the capitalization rate as an exact measure of MWTP. First, preferences, income, and technology must be fixed over the duration of the study period. Second, utility must be separable in the public good and its demand curve must be perfectly elastic over the range of the shock. Third, the second derivative of the hedonic price function with respect to the public good must be zero. If any one of these restrictions is violated, capitalization rates may understate or overstate MWTP.

Restrictions on the primitives of Rosen's model have testable implications for the evolution of the hedonic price function. Using a linear-in-parameters specification for the price function, we derive conditions on the data under which capitalization rates will identify the average consumer's MWTP. One can identify MWTP in the pre-shock equilibrium if the hedonic gradient is constant over the duration of the study period. If this condition does not hold, one can still identify MWTP in the post-shock equilibrium if the shock (or an instrument for the shock) is orthogonal to all other variables. A key point is that randomization of an instrument can provide the extra information needed to identify post-shock MWTP in lieu of restrictions on market primitives.

In the second half of the paper, we apply our framework to the problem of measuring the willingness to pay for improving the quality of public schooling. We have assembled a unique set of micro data for the analysis. The data describe a quarter of a million individual homes that sold in the cities and suburbs of Fairfax VA, Portland OR, Detroit MI, Los Angeles CA, and Philadelphia PA during the 2003 and 2007 school years.[3] Each observation includes the sale price of a home, its structural features, the demographic composition of its neighborhood, the local public goods available to its residents, and most importantly, measures of academic performance at the public schools to which children living in that home would have been assigned.

Most schools reported significant increases in their students' math and reading proficiency between 2003 and 2007, with the largest improvements reported by the lowest quality schools. These trends are consistent with the new incentives that school administrators faced after the No Child Left Behind Act (NCLB) took effect in 2003 (Dee and Jacob 2009, Neal and Schanzenbach forthcoming). NCLB required each state to implement a test-based accountability system for its public schools. Schools that repeatedly failed to meet targets for math and reading

[3] After an exhaustive search over potential study regions, we concluded that Fairfax, Portland, Detroit, Los Angeles, and Philadelphia were the only metro areas with public school assignment laws, consistent reporting of test scores, and micro data on recent property sales that would allow us to develop hedonic boundary discontinuity designs at the standards set by Black (1999) and Bayer, Ferreira, and McMillan (2007).

proficiency would face a schedule of sanctions. NCLB also required every school to publicly report the share of its students who achieve proficiency in each subject. We use these data to compare the willingness to pay for improved school quality with the capitalization of publicly reported changes in math and reading proficiency.

Our measures of willingness to pay are derived by estimating hedonic price functions in each of the ten (school year, metro area) pairings. The price functions are identified by boundary discontinuity designs that exploit the discreteness in each area's laws for assigning children to schools. Table 1 compares our main findings to the results from previous boundary discontinuity studies of Boston (Black 1999) and San Francisco (Bayer, Ferreira, and McMillan 2007). Hedonic estimates for the elasticity of property values with respect to test scores are remarkably similar across metro areas (column 1). In 2003 our estimates range from 0.12% in Fairfax to 0.27% in Philadelphia. Converting these estimates into constant year 2000 dollars reveals the average resident would be willing to pay between $422 (Detroit) and $743 (Philadelphia) for a 1% increase in test scores (column 3).

When we repeat the boundary discontinuity analysis for 2007 we find significant changes in hedonic gradients. These changes drive a wedge between our hedonic measures of willingness to pay in 2003 and estimates based on the capitalization of changes in test scores between 2003 and 2007. Columns 3 and 4 illustrate that the two sets of estimates differ by more than 100% for the average resident in Fairfax, Portland, Detroit, and Philadelphia. Furthermore, correlation between changes in test scores and other variables drives a wedge between capitalization and willingness to pay in 2007. Aggregating our hedonic results over all five study areas reveals that the average resident's willingness to pay for a 1% increase in scores increased from $536 in 2003 to $688 in 2007. These figures are four times as large as our capitalization-based measures. We conclude that researchers must be cautious in using capitalization as the sole basis for evaluating the benefits of public programs.

Overall, our findings add to three distinct literatures. First, we define the connection between land value capitalization (Lind 1973, Starrett 1981) and hedonic equilibria (Scotchmer 1985, 1986, Bartik 1987) in the revealed preference literature on using private market outcomes to predict the willingness to pay for public goods. Second, we establish a conceptual framework for interpreting evidence on capitalization from the new quasi-experimental literature on policy evaluation (Davis 2004, Chay and Greenstone 2005, Greenstone and Gallagher 2008, Linden and Rockoff 2008, Cellini, Ferreira, and Rothstein, 2010). Finally, our boundary discontinuity results extend the literature on valuing school quality by providing the first consistent evidence on variation in willingness to pay across time and space (Oates 1969, Kain and Quigley 1975, Black 1999, Figlio and Lucas 2004, Bayer, Ferreira, and McMillan 2007).

The rest of the paper proceeds as follows. Section II briefly reviews the ideas behind hedonic and capitalization based approaches to valuing public goods. Section III presents our conceptual model and defines conditions under which capitalization rates identify willingness to pay. We translate those conditions into testable econometric restrictions in section IV. Section V describes our data, section VI reports regression results, and section VII analyzes implications for measuring willingness to pay. Finally, section VIII concludes by summarizing the problems with capitalization-based benefit measurement and the potential solutions.

II. Hedonic and Capitalization Models for Valuing Public Goods

In his seminal 1956 paper, Tiebout hypothesized that freely mobile households will reveal their preferences for public goods through the location choices they make. His reasoning influenced the development of two revealed preference techniques: the capitalization model and the hedonic property value model. Hundreds of applications of these methods over the past 40 years have contributed much of what we currently know about the willingness to pay for public goods.

Capitalization studies use data before and after a market shock to measure its effect on property values.[4] The power of this technique is the ability to simultaneously measure a change in asset values and demonstrate that the change was caused by some event. Capitalization models are routinely used by expert witnesses in litigation over private property externalities (Simons 2006). They are also used to measure the market value of risk and uncertainty (Brookshire et al. 1985). A limitation of the technique is that it lacks a precise welfare interpretation. Lind (1973) and Starrett (1981) demonstrated that, under the type of sorting behavior Tiebout envisioned, market capitalization of a large shock may understate or overstate the change in household welfare.[5]

In contrast, the hedonic property value model based on Rosen (1974) offers a theoretically consistent approach to welfare measurement. The difficulty is identification. Scotchmer (1985, 1986) proved that data from a single market are only sufficient to identify marginal values. To identify a demand curve, one must collect multi-market data on the characteristics of households and their houses, plus instrumental variables for endogenous characteristics (Bartik 1987, Epple 1987). Unfortunately, barriers to obtaining these data have stymied demand estimation.[6] The vast majority of empirical studies only aspire to recover marginal values.[7]

---

[4] The idea for using panel data to measure how changes in quality characteristics influence housing prices dates back at least to Bailey, Muth, and Nourse (1963). Economic applications begin with Palmquist (1982).

[5] While Lind (1973) does not develop a formal utility theoretic framework, he proves that any welfare interpretation of capitalization requires there be zero consumer surplus. This effectively rules out preference-based sorting by heterogeneous agents, as Starrett (1981) later demonstrated.

[6] An alternative strategy to identify demand is to provide some information about the structure of consumer preferences. This information may consist of a parametric representation for the utility function (Epple and Sieg 1999, Bajari and Benkard 2005), separability restrictions on preferences (Ekeland, Heckman and Nesheim 2004), an assumption that the populations residing in different cities share a common distribution of unobserved tastes (Bartik

Even the seemingly modest task of estimating marginal values is now believed to be plagued by omitted variable bias. Chay and Greenstone (1998, 2005) characterized the problem and proposed a solution. They replaced the conventional hedonic estimator with an instrumental variables strategy that isolates how property values are affected by unexpected shocks to public goods. Their microeconometric model bridged the capitalization and hedonic literatures. It integrated a quasi-experimental version of the identification strategy from the capitalization literature with the welfare interpretation of Rosen's hedonic model.

To illustrate the basic idea, let the price of housing be expressed as $p = p(g, h, \xi)$, where g is the public good of interest, h measures all other public goods and housing characteristics observed by the analyst, and ξ represents unobserved variables. It is standard practice to specify a linear-in-parameters price function such as

$$p_1 = g_1\theta_1 + h_1\eta_1 + \varepsilon(\xi_1), \tag{1}$$

where the subscripts indicate the time period. Assuming the specification is correct, the first order conditions from Rosen (1974) allow us to interpret θ_1 as the marginal willingness to pay (MWTP) for the public good in period 1. However, θ_1 is not identified if ξ_1 is correlated with g_1 or h_1.

Now suppose *p*, *g*, and *h* are also measured after an unexpected shock. First-differencing the data produces a new estimator,

$$\Delta p = \Delta g\phi + \Delta h\gamma + \Delta\varepsilon, \tag{2}$$

1987), or an assumption that migration decisions do not arise from changes in individual tastes (Bishop and Timmins 2008).

[7] That said, the *number* of studies that aspire to recover marginal values is also vast. To give a rough sense of scale, there are more than 1600 citations of Rosen (1974) in the Social Science Citation Index and approximately 4000 reported by Google Scholar. Property value applications are one of (if not *the*) most frequent application. See Palmquist (2005) for a review of the literature.

where $\Delta d = d_2 - d_1$ for $d = [p, g, h, \varepsilon]$. If the bias from omitted variables is purged by differencing the data, (2) provides an unbiased estimator for ϕ. Alternatively, if one suspects that $E[\Delta\varepsilon \mid \Delta g, \Delta h] \neq 0$, instrumental variables may be used to develop a consistent estimator.

Interpreted literally, ϕ is the average rate of change in property values associated with the shock to *g*. Chay and Greenstone observe that this capitalization rate will equal MWTP if the gradient of the price function in (1) is constant over time (i.e. $\theta_1 = \theta_2$ and $\eta_1 = \eta_2$ implies $\phi = \theta_1 = \theta_2$ and $\gamma = \eta_1 = \eta_2$). Recent studies have used this result to estimate the willingness to pay for changes in cancer risk (Davis 2004), air quality (Chay and Greenstone, 2005), hazardous waste (Greenstone and Gallagher 2008), crime (Linden and Rockoff 2008, Pope 2008), open space (Bin, Landry, and Meyer 2009), invasive species (Horsch and Lewis 2009), low income housing credits (Baum-Snow and Marion 2009), and investment in education (Cellini, Ferreira, and Rothstein 2010). In all of these studies, the validity of welfare measures rests on the maintained assumption that the gradient of the hedonic price function is constant over the duration of the study. The assumption has been made for periods between 10 and 20 years, for areas ranging from a single county to the contiguous United States.

Because the price function is an equilibrium outcome generated by interactions between all of the buyers and sellers in a market, assumptions about its evolution implicitly restrict preferences and technology.

III. Hedonic Equilibria and the Capitalization of Market Shocks

This section considers the evolution of the hedonic gradient. After introducing the primitives of the hedonic model and characterizing market equilibrium, we define restrictions on preferences and technology that are sufficient to assure the gradient will be time-constant. A proof is followed by brief discussion.

A. Demand, Supply, and Market Equilibrium

Price-taking households are assumed to be free to choose a home with any combination of housing characteristics (e.g. bedrooms, bathrooms, sqft) in the neighborhood that provides their desired levels of amenities (e.g. school quality, air quality, racial composition). The utility maximization problem is

$$\max_{g,X,b} U(g, X, b; \alpha) \quad subject\ to \quad y = b + P(g, X; \Theta), \tag{3}$$

where $X = [h, \xi]$. A household chooses housing characteristics, amenities, and the numeraire composite commodity (b) to maximize its utility, given its preferences (α), income (y), and the after-tax price of housing, $P(g, X; \Theta)$, which is expressed as a general parametric function of g, X, and a parameter vector, Θ. The first order conditions are

$$\frac{\partial P(g, X; \Theta)}{\partial g} = \frac{\partial U/\partial g}{\partial U/\partial b} \equiv D(g; X, \alpha, y), \tag{4a}$$

$$\frac{\partial P(g, X; \Theta)}{\partial X} = \frac{\partial U/\partial X}{\partial U/\partial b} \equiv R(X; g, \alpha, y). \tag{4b}$$

The first equality in (4a) implies that each household will choose a neighborhood that provides a quantity of g at which their marginal willingness-to-pay for an additional unit exactly equals its marginal implicit price. Assuming the marginal utility of income is constant, the second equality observes that as g varies the marginal rate of substitution defines the inverse demand curve, conditional on X. Equation (4b) states analogous first order conditions for X.

Producers in this market may include developers, contractors, and individuals selling their homes. Let $C(g, M, X; \beta)$ denote a producer's cost function, where M is the number of type-(g,X) homes they sell and β is a vector of parameters describing the producer.[8] Variation in β captures differences in costs faced by different producers. Following Rosen (1974), we treat each producer as a price taker who is free to vary the number of units they sell as well as a subset of the characteristics of each unit. For notational convenience, g is assumed to be exogenously determined.[9] In this case, the profit maximization problem is

$$\max_{X,M} \ \pi = M \cdot P(g, X; \Theta) - C(g, M, X; \beta), \tag{5}$$

with the corresponding first order conditions

$$P(g, X; \Theta) = \frac{\partial C(g, M, X; \beta)}{\partial M}, \qquad \frac{\partial P(g, X; \Theta)}{\partial X} = \left(\frac{1}{M}\right)\frac{\partial C(g, M, X; \beta)}{\partial X}. \tag{6}$$

Producers choose M to set the offer price of the marginal home equal to its production costs, and they choose X to set the marginal per unit cost of each attribute equal to its implicit price.

Equilibrium occurs when the first order conditions in (4) and (6) are simultaneously satisfied for all households and producers. This system of differential equations implicitly defines the equilibrium hedonic price function that clears the market (Rosen 1974). It will be useful to rewrite the price function to acknowledge its dependence on model primitives,

$$P(g, X; \Theta) \equiv P[g, X(g, \mathrm{A}, \mathrm{B}); \Theta(g, \mathrm{A}, \mathrm{B})]. \tag{7}$$

Equilibrium levels of X are determined by all of the exogenous variables: g the public good of interest, $\mathrm{A} : F(y, \alpha) \sim \mathrm{A}$, a vector of parameters that describes the joint distribution of household income and preferences, and $\mathrm{B} : V(\beta) \sim \mathrm{B}$, a parameter vector describing the distribution of

[8] For a developer or contractor, the cost function will reflect the physical, labor, and regulatory costs of building a home. For a homeowner, the cost function will reflect their psychological attachment to the home as well as the cost of renovation.
[9] The results of this section are not altered by allowing firms to choose g or by restricting their ability to choose X. The key restriction needed to relate our model to the new empirical capitalization literature is that g may be influenced by forces that are exogenous to the model.

producer characteristics.[10] Naturally, the reduced form parameters describing the shape of the price function are also functions of the exogenous variables.

B. Necessary Conditions to Interpret the Capitalization Rate as a Measure of MWTP

Now consider two different hedonic equilibria, observed before and after an unexpected shock to g. The change in the value of a house j depends on the difference in the pre and post-shock price functions,

$$P\left[g_{2j}, X_{2j}(g_2, \mathrm{A}_2, \mathrm{B}_2); \Theta(g_2, \mathrm{A}_2, \mathrm{B}_2)\right] - P\left[g_{1j}, X_{1j}(g_1, \mathrm{A}_1, \mathrm{B}_1); \Theta(g_1, \mathrm{A}_1, \mathrm{B}_1)\right], \qquad (8)$$

where the 1 and 2 subscripts denote pre and post-shock equilibria. To isolate the capitalization rate, we condition on X and divide the change in property value by the change in g,

$$\phi_j = \frac{P\left[g_{2j}; \Theta(g_2, \mathrm{A}_2, \mathrm{B}_2) \middle| X_{2j} = \overline{X}\right] - P\left[g_{1j}; \Theta(g_1, \mathrm{A}_1, \mathrm{B}_1) \middle| X_{1j} = \overline{X}\right]}{g_{2j} - g_{1j}}. \qquad (9)$$

This difference quotient provides a general expression for the capitalization parameter estimated in the literature.[11]

Because ϕ_j depends on two (potentially different) price functions, it is not the measure of MWTP from Rosen (1974). To interpret ϕ_j as the MWTP, we must restrict preferences and technology to assure that the capitalization rate will equal the partial derivative of the pre-shock and/or post-shock price functions. Severity of the restriction depends on the size of the shock. If the change in g is small, we need only restrict $\mathrm{A}_1 = \mathrm{A}_2$ and $\mathrm{B}_1 = \mathrm{B}_2$. Under this condition, the difference quotient in (9) approaches the partial derivative in (4a) as $g_{2j} - g_{1j}$ approaches

[10] M drops out of the expression for X in (7) because it is a function of model primitives.
[11] P and g are typically measured in levels or logs.

zero.[12] In the limit, pre-shock MWTP equals post-shock MWTP which equals the capitalization rate. This is intuitive. An infinitesimal change in one hedonic characteristic will not alter the shape of the price function; equilibrium prices simply increase by MWTP.

In the case of a nonmarginal shock, three restrictions are needed to establish a welfare interpretation for the capitalization rate. We state this formally as

ASSUMPTION 1.

a. $\mathrm{A}_1 = \mathrm{A}_2$ and $\mathrm{B}_1 = \mathrm{B}_2$.

b. $\partial\Theta/\partial g = 0$.

c. $\partial P(g, X; \Theta)/\partial g = f(X, \Theta)$.

Condition *a* restricts preferences, income, and technology to be constant over the duration of the study.[13] It follows that supply and demand curves for each characteristic are also fixed. The last two conditions restrict the shapes of those curves. Condition *b* states that changes in *g* have no effect on the shape of the price function. If supply curves are less than perfectly elastic, for example, condition *b* is satisfied if demand is perfectly elastic. Condition *c* further restricts supply and demand so that the marginal price function for *g* does not depend on *g*. If all three conditions are satisfied, it is straightforward to show that the capitalization rate in (9) must equal the MWTP in (4a).

THEOREM 1. If assumption 1 holds for a shock to *g*, then the capitalization rate, ϕ, equals the pre-shock MWTP, which equals the post-shock MWTP.

Proof. Consider any home, *j*, with characteristics $X_j = \overline{X}$ for which g_j changes from

[12] Proof of this statement follows immediately from the definition of a derivative.
[13] This condition can be relaxed as long as other restrictions are added to guarantee that changes in income, preferences, and technology have no effect on the shape of the equilibrium price function. More precisely, Θ must be invariant to any changes in the elements of A and B.

g_{j1} to g_{j2}. Since $\mathrm{A}_1 = \mathrm{A}_2$, $\mathrm{B}_1 = \mathrm{B}_2$, and $\partial\Theta/\partial g = 0$, we know that $\Theta_1 = \Theta_2$. Combining this result with the assumption that $\partial P(g, X; \Theta)/\partial g = f(X, \Theta)$ implies $f(\overline{X}, \Theta_1) = f(\overline{X}, \Theta_2)$. It follows from the Mean Value Theorem that $\phi_j = f(\overline{X}, \Theta_1) = f(\overline{X}, \Theta_2)$. The second term measures pre-shock MWTP and the third term measures post-shock MWTP, as defined by the first-order conditions from Rosen (1974). QED.

The model proposed by Chay and Greenstone (2005) provides an example. Their linear price function (1) is consistent with condition *c*, and their assumption that $\Theta_1 = \Theta_2$ implies conditions *a* and *b* are satisfied. Using these restrictions, it is a simple algebraic exercise to demonstrate that (9) returns the MWTP for *g*. Alternatively, for models that violate assumption 1 the Mean Value Theorem implies

$$\phi_j \neq \partial P(g_{1j}, X_{1j}; \Theta_1)/\partial g \neq \partial P(g_{2j}, X_{2j}; \Theta_2)/\partial g .$$

In this case, the direction and magnitude of the bias from misinterpreting the capitalization rate as a welfare measure will depend on the correlations in the data and the shapes of supply and demand curves.[14]

C. Discussion

We have established that the capitalization rate approaches the partial derivative of the price function as $\mathrm{A}_2 \to \mathrm{A}_1$, $\mathrm{B}_2 \to \mathrm{B}_1$, and $\Delta g \to 0$. Based on this limiting result, we would expect the capitalization rate to provide a good approximation to average MWTP for small shocks that can be tracked over short periods. However, recent studies have focused on large shocks and/or

[14] This dependence is easily demonstrated using a closed form expression for the equilibrium price function such as Tinbergen's (1959) linear-quadratic-normal model. Section IV demonstrates the role of correlations in the data using the standard empirical specification for the hedonic price function.

periods of a decade or more. For example, Chay and Greenstone (2005) measure the capitalization of large air quality improvements during the 1970s. Davis (2004) tracks the capitalization of a six-fold increase in pediatric leukemia risk. Greenstone and Gallagher (2008) estimate capitalization rates for the cleanup of hazardous waste sites over the first 20 years of the federal "Superfund" program (1980 to 2000).[15] We use these studies as examples for two reasons. First because they develop ingenious identification strategies to provide what are perhaps the most credible estimates for public good capitalization rates. Second because their exploitation of large shocks and/or long intervals means the ability to interpret their estimates as measures of average MWTP rests on the validity of assumption 1.

Consider what assumption 1 implies. At a single point in time, condition *c* requires the distribution of marginal prices for g to be degenerate once we condition on X. This is a special case of a linear marginal price function, which Ekeland, Heckman, and Nesheim (2004) prove is a nongeneric property of hedonic equilibrium. Even if we invoke the degeneracy restriction with the idea that it represents a linear approximation to the true price function, conditions *a* and *b* impose deeper restrictions on preferences and technology.

Recall that the hedonic gradient provides a mapping to the distribution of marginal values in the consumer population (4) and marginal costs in the producer population (6). The only theoretically-grounded restriction on this mapping that supports $\partial\Theta/\partial g = 0$ is that either the demand for g or its supply is perfectly elastic.[16] Utility must also be separable in g and X. Oth-

---

[15] Unlike the first two examples, the scope of the Superfund shock was small in the sense that only 1% of census tracts contained sites that were cleaned. The key assumptions that enable Greenstone and Gallagher to use the capitalization rate for benefit-cost analysis are that: (i) the MWTP for cleanup does not depend on the degree of contamination, and (ii) the hedonic gradient was invariant to all changes in public goods, housing characteristics, income, preferences, and construction costs that occurred in the United States over their 20-year study period.

[16] All else constant, a positive shock to g will decrease MWTP (changing Θ) if demand is downward sloping. It is possible to offset the change in Θ by a concomitant shock to preferences. While this type of mathematical restriction presents a technical possibility, it has no economic content and, in our opinion, does not merit serious consid-

erwise, a shock to g could change the implicit prices of the elements of X. If g is the crime rate, for example, we must be willing to assume that changes in crime do not affect the willingness to pay for home security systems, fences, or proximity to city parks. These restrictions on own and cross-price elasticities are not limited to the public good of interest. They also apply to all of the elements of X that are subject to exogenous shocks. A change in the relative price of *any* hedonic characteristic violates $\partial\Theta/\partial g = 0$ and drives a wedge between MWTP and the capitalization rate for *any other* characteristic. Finally, even if the demand for every characteristic is perfectly elastic, we must still restrict their relative prices to be unaffected by changes in wealth, preferences, and construction costs that may occur during the study period.

If assumption 1 is violated, the gradient of the price function may change between the pre and post-shock observation periods. The bias associated with interpreting the capitalization rate as a measure of MWTP will depend on: (i) the shape of the price function; (ii) magnitudes of changes in the reduced-form parameters; and (iii) correlations in the data. Given a parametric representation for the price function, the capitalization bias can be expressed in terms of $\Theta_1, \Theta_2, g_1, g_2, X_1,$ and X_2. We derive this relationship in the next section and use it to define testable restrictions on the data that neutralize the capitalization bias.

IV. Sufficient Conditions for Capitalization Based Welfare Measurement

Empirical studies almost always specify the price function to be linear in parameters.[17] We follow this convention and abstract from econometric complications such as measurement error and approximation error in the choice of functional form. These abstractions allow us to focus

---

eration.

[17] The prevalence of the linearity assumption is partly due to Cropper, Deck, and McConnell (1988). Working with simulated data, they found that linear approximations tended to provide better predictions for MWTP than a more flexible Box-Cox quadratic specification in the presence of unobserved variables and errors in variables.

attention on the relationship between capitalization and welfare in the workhorse model of the empirical literature.[18]

We begin by repartitioning X into observed (h) and unobserved (ξ) components. Using this partition, the linear price functions that describe market equilibria before and after an unexpected shock to g are $p_1 = g_1\theta_1 + h_1\eta_1 + \varepsilon_1(\xi_1)$ and $p_2 = g_2\theta_2 + h_2\eta_2 + \varepsilon_2(\xi_2)$.[19] Parameter subscripts recognize that the shape of the function may have been altered by the shock to g and by concomitant changes in h, ξ, $F(y,\alpha)$, and $V(\beta)$.

Subtracting the old price function from the new one yields a general time-differenced model,

$$\Delta P = (g_2\theta_2 - g_1\theta_1) + (H_2\eta_2 - H_1\eta_1) + \Delta\varepsilon\,. \tag{10}$$

In the special case where $\theta_1 = \theta_2$ and $\eta_1 = \eta_2$, equation (10) reduces to the capitalization estimator from (2), $\Delta p = \Delta g\phi + \Delta h\gamma + \Delta\varepsilon$.

Applying the Frisch-Waugh Theorem, the relationship between the estimated capitalization rate ($\hat{\phi}$) and MWTP (θ_1, θ_2) can be expressed as:

$$\hat{\phi} = \theta_2 + \frac{r'g_1}{r'r}(\theta_2 - \theta_1) + \frac{r'h_1}{r'r}(\eta_2 - \eta_1) + \frac{r'\Delta\varepsilon}{r'r}\,, \tag{11}$$

where $r = \Delta g - \Delta h(\Delta h'\Delta h)^{-1}\Delta h'\Delta g$. The estimate for ϕ is a function of all the parameters of the true price functions that precede and follow the shock. Put differently, (11) reports what we can expect to learn about MWTP from estimating (2) when (10) is the true model.

The estimate for the capitalization rate is a function of ex-ante MWTP, ex-post MWTP,

[18] That said, one could repeat our analysis in this section under any set of assumptions about the shape of the price function and the sources of error. If the price function lacks a closed form solution, numerical methods could be used to solve for the equilibrium, as in Klaiber and Smith (2009) or Kuminoff and Jarrah (2010).

[19] The h_1 matrix of control variables may also include a vector of ones so that η includes an intercept.

and correlations between housing characteristics. The second term on the right of the equality in (11) is a "price effect" that arises from a change in the implicit price of g between the initial equilibrium and the new equilibrium. The third term is a "substitution effect" that arises from changes in the implicit prices of other housing characteristics that affect utility and, in some sense, serve as substitutes for g. The last term reflects the bias that arises from correlation between changes in observed and unobserved variables.

Without any restrictions on the data, $\hat{\phi}$ may fall outside the range of values for MWTP defined by θ_1 and θ_2. Consider a quality improvement that decreases MWTP but has no effect on the control variables or their marginal implicit prices: $\Delta h = \eta_2 - \eta_1 = \Delta\varepsilon = 0$. In this case, (11) implies that $\hat{\phi} < \theta_2 < \theta_1$ if $\Delta g' g_1 > 0$. Alternatively, $\theta_2 < \theta_1 < \hat{\phi}$ if $\Delta g' \Delta g < -\Delta g' g_1$. It is clear that additional restrictions are needed to give the estimated capitalization rate a welfare interpretation.

Two sets of restrictions are sufficient for the capitalization model to provide an unbiased estimate of MWTP. The first set follows directly from assumption 1. If assumption 1 is satisfied, the hedonic gradient must be time-constant. Adding the usual orthogonality restriction on the error term gives us

SUFFICIENT CONDITION 1. $\theta_1 = \theta_2$, $\eta_1 = \eta_2$, and $\Delta g, \Delta h \perp \Delta\varepsilon$. (12)

Under these restrictions, equation (11) reduces to $\hat{\phi} = \theta_1 = \theta_2$. In this case the capitalization model (2) provides an unbiased estimate of ex ante MWTP which equals ex post MWTP. If estimation of single-period price functions is possible, time-constancy of the hedonic gradient can be tested.

The second set of restrictions relaxes the need for the gradient to be constant over time by adding orthogonality restrictions on the data that exploit the linearity of the model. More precisely, it can be seen from (11) that $\hat{\phi} = \theta_2$ if the following restrictions hold

SUFFICIENT CONDITION 2. $$g_1, h_1, \Delta h \perp \Delta g \quad \text{and} \quad \Delta g, \Delta h \perp \Delta\varepsilon . \qquad (13)$$

In words: if the shock to g is orthogonal to its initial level, and to the initial levels of the control variables, and to changes in those variables, then the capitalization rate provides an unbiased estimate of MWTP in the post-shock equilibrium, even if the gradient changes between the two observation periods. The data may still contain capitalization bias. If $\theta_1 \neq \theta_2$ or $\eta_1 \neq \eta_2$ the price change for any given home may lie above or below the resident's ex post MWTP. However the positive and negative differentials for individual homes cancel out of the first-differenced estimate for ϕ due to the linearity of the price function and the orthogonality of the shock.[20]

If instruments are available, sufficient conditions 1 and 2 can be relaxed. Some authors have sought to develop instruments for Δg out of concern for the potential correlation between changes in observed and unobserved variables. Notably, Chay and Greenstone (2005) and Greenstone and Gallagher (2008) use discontinuities in the structure of public policies to break potential correlation between Δg and $\Delta\varepsilon$. Equally important is the fact that these "policy discontinuity" instruments offer the potential to identify subsets of "treated" and "untreated" homes that are similar in many other respects. With this in mind, let z denote a set of valid instruments for Δg. The instrumental variables analog to the capitalization bias function in (11) simply replaces Δg with $\Delta\hat{g} = z(z'z)^{-1} z'\Delta g$. Likewise, (12) and (13) are replaced with (14) and (15).

[20] Linearity and orthogonality are both necessary. In the context of assumption 1, we have strengthened condition *c* such that $\partial P(g, X; \Theta)/\partial g = f(\Theta)$. Under the original condition *c*, where the marginal price function may be nonlinear in *X*, orthogonality restrictions on the data may not be sufficient to identify θ_2.

SUFFICIENT CONDITION 1.a. $\theta_1 = \theta_2$, $\eta_1 = \eta_2$, and $z, \Delta h \perp \Delta\varepsilon$. (14)

SUFFICIENT CONDITION 2.a. $g_1, h_1, \Delta h \perp z$ and $z, \Delta h \perp \Delta\varepsilon$. (15)

Assuming valid instruments are available, it is straightforward to test whether they induce sufficient randomization to satisfy the orthogonality condition in the first part of (15). Also notice that z must not contain Δh. Adding the elements of Δh as control variables in a first-stage regression would violate the first orthogonality condition.

There is an important caveat to the randomization strategy justified by conditions 2 and 2.a. If the shape of the price function changes over the duration of the study, an accurate estimate of θ_2 may be of limited use for policy evaluation. Consider an extreme case where a large positive shock to the public good drives the MWTP to zero. The shock may have dramatically increased consumer welfare, but knowing θ_2 does not allow us to distinguish this outcome from the alternative hypothesis that people do not care about the change that occurred. More generally, a welfare approximation based on $\theta_2 \times \Delta g$ will understate the benefits from a ceteris paribus improvement during the study period and overstate the costs of a decline.

In summary, the relationship between capitalization and MWTP depends on the evolution of the price function gradient. If the gradient is found to be time-constant, (12) or (14) can be invoked to interpret capitalization rates as measures of ex-ante MWTP. If the gradient changes but the data satisfy the orthogonality conditions in (13) or (15), capitalization rates can be interpreted as measures of ex-post MWTP. Together, (12)-(15) define sufficient conditions for developing consistent welfare measures based on quasi-experimental estimates for capitalization effects. These conditions are analogous to Chetty's (2009) "sufficient statistics" for quasi-experimental welfare measurement.

To assess the practical importance of conceptual differences between capitalization and willingness to pay requires tracking how the hedonic gradient evolves over time. The difficulty lies in identifying single-period price functions. Perhaps the most credible identification strategies to date are the boundary discontinuity designs used to measure the willingness to pay for improving the quality of public schooling (Black 1999, Bayer, McMillan, and Reuben 2007). Therefore we focus the remainder of our attention on using this strategy to compare the capitalization of changes in school quality with the willingness to pay for improvements.

V. Capitalization of School Quality Changes and the MWTP for Improvements

Understanding the willingness to pay for school quality is crucial for determining the benefits of undertaking a wide range of academic reforms. Hedonic property value models offer the most intuitively appealing method. Because a household's access to a public school is determined by whether or not the household lives within the attendance zone for that school, property value differentials should reflect what parents are willing to pay for their children to attend schools where students score higher on standardized tests. A large empirical literature evolved around this idea, beginning with Oates (1969).[21]

The 40-year history of the literature on valuing school quality is a microcosm for the broader literature on valuing public goods. Early studies used cross-section models with few or no controls for omitted variables. Then researchers noted a potential source of confounding—schools with higher test scores tended to be located in more exclusive neighborhoods. Subsequent studies sought to avert the potential bias by developing quasi-experimental identification strategies. This work began with Black (1999). She noticed that school quality shifts discretely

[21] Kain and Quigley (1975) is another early example. Recent applications include Black (1999), Bogart and Cromwell (2000), Downes and Zabel (2002), Gibbons and Machin (2003), Reback (2005), and Bayer, Ferreira, and McMillan (2007). Figlio and Lucas (2004) is an example of a capitalization-based study.

as one crosses an attendance zone boundary, but other neighborhood characteristics do not (e.g. crime rates, air quality, access to the city center). Therefore, the composite price effect of all the unobserved amenities that are common to homes on both sides of a boundary can be absorbed by a fixed effect for the “boundary zone”. By focusing on sales that occurred near a boundary and including fixed effects for each boundary zone, Black forced the identification to come from price differentials between structurally similar homes located on opposite sides of a boundary.

Bayer, Ferreira, and McMillan (2007) refined Black’s approach to control for correlation between preferences for schools and preferences for the demographic characteristics of one’s neighbors. The problem stems from sorting. If preferences for school quality are correlated with demographic characteristics, such as race or education, then similar “types” of households will tend to locate in the same attendance zones. This helps to explain why neighborhood demographics also tend to shift discretely as one crosses an attendance zone boundary. Since prospective homebuyers may care about the characteristics of their neighbors, one must control for changes in the demographic composition of the neighborhood in order to isolate the implicit value of academic performance.

We use the hedonic boundary discontinuity design developed by Black and refined by Bayer, Ferreira, and McMillan to identify single-period price functions in five metropolitan areas. Then we calculate the MWTP for school quality, test for time-constancy of the hedonic gradient, and compare our estimates for MWTP to capitalization rates for the changes in test scores that occurred during the first four years of the No Child Left Behind Act. The remainder of this section summarizes the Act, our data, and key features of the research design.

A. No Child Left Behind

President George W. Bush announced his "No Child Left Behind" framework for education reform three days after taking office, and within a year the NCLB act had been passed. NCLB was one of the most sweeping reforms in the recent history of public education in the United States. Since its enactment, states have been required to implement accountability systems that measure student performance in reading and math. Standardized testing is done in grades 3 through 8 and at least once during high school. State test scores are used to determine if each public school is making "Adequate Yearly Progress" (AYP) toward the goal of having 100% of its students attain state-specific standards for minimum competency in reading and mathematics by 2014. Schools that do not meet AYP face a series of repercussions.

Importantly, NCLB established a consistent set of metrics for comparing academic performance across schools and improved accessibility of the information. To obtain a ranking of schools in their area or to see specific test scores, parents need only visit one of several websites that collect the information and distribute it freely.[22] A low cost of obtaining information should strengthen the link between property values and the willingness to pay for higher academic performance.

While test scores have trended up since NCLB was enacted, its impact on the quality of education has been debated. Advocates argue that school quality will be improved by developing consistent metrics for tracking school performance, publicizing results, and sanctioning schools that fail to meet APY. Detractors argue that NCLB creates perverse incentives to "teach to the test", to lower state standards, to expel poorly performing students, or even to lie when reporting scores. Several authors have investigated these issues. Perhaps the most convincing analyses are those by Neal and Schanzenbach (forthcoming) and Dee and Jacob (2009). These

[22] See for example the popular websites: www.greatschools.org and www.schooldigger.com .

papers also provide excellent reviews of the literature. Neal and Schanzenbach show that the introduction of NCLB increased reading and math scores for students in the middle of the achievement distribution for fifth graders in the Chicago Public School system. Dee and Jacob (2009) employ a comparative interrupted time series design to identify the impact of NCLB on a panel of state test scores from the National Assessment of Education Progress (NAEP). The key feature of their research design is that changes in NAEP scores should be unaffected by the perverse incentives that critics of NCLB have emphasized. They found that NCLB did indeed cause large and broad gains in NAEP math achievement scores of 4th and 8th graders, especially in the bottom decile of the achievement distribution.[23] These results suggest that the upward trend in NCLB scores is consistent with alternative metrics for judging public school quality.

B. Ten Boundary Discontinuity Designs

There have been few applications of the boundary discontinuity methodology to study the willingness to pay for school quality, and the vintage of data used by Black and Bayer, Ferreira, and McMillan was early to mid 1990's. We significantly update and extend the literature by applying the methodology to 10 new markets: 5 geographic regions (Fairfax County VA, Portland OR, Philadelphia PA, Detroit MI, and Los Angeles CA) in 2 distinct periods (the 2003 and 2007 school years). After an exhaustive search over prospective study regions, these five areas were chosen because they each satisfied three key criteria: (i) a sufficient number of boundary zones to conduct the estimation;[24] (ii) a sufficient number of housing transactions available for estima-

---

[23] Mean increases in the National Assessment of Educational Progress math test scores were approximately 1-8 points from the start of NCLB to 2007 for 4th and 8th grade math scores.

[24] Boundary discontinuity analysis is extremely data-intensive because it discards housing transactions that occur beyond small distances from the school district boundaries.

tion; and (iii) NCLB test scores were reported for the 2003 and 2007 school years.[25] Together, the five regions also provide considerable geographic diversity.

Black (1999) and Bayer, Ferreira, and McMillan (2007) used elementary school attendance zones as the basis for identification. We use this same approach in Fairfax and Portland, where children are still assigned to elementary schools based on the attendance zones where their parents live. However, this type of school-specific assignment is no longer the norm. Since the mid-1990s, there has been an explosion of state and local regulations that mandate open enrollment at the school district level. In an open enrollment area, parents are free to send their children to any public school that lies within the school district. There is evidence that parents take advantage of these laws by sending their children to schools outside the elementary attendance zone where their home is located (Reback 2005, 2008). Philadelphia, Detroit, and Los Angeles all have open enrollment policies. For these areas, our identification strategy is based on the relationship between property values and average test scores on opposite sides of the school district boundary.

Implementing the boundary discontinuity design at the school district level requires taking a weighted average over the test scores in each district. This has the advantage of smoothing over idiosyncratic variability in annual school-specific scores. Yet, it also requires extra caution. Property tax rates often vary discretely across school districts, and district boundaries may be more likely than attendance zone boundaries to overlap with features of the landscape. Therefore, we are careful to control for property tax rates and to drop all district boundaries that overlap with discernable landscape features such as rivers and highways.

[25] States were not required to start reporting test scores until 2006 and so many states did not have test score data available early enough for the analysis.

C. Data and Summary Statistics

We collected detailed information on test scores, neighborhood demographics, and homes that were sold during the 2003 and 2007 school years. The 2003 school year is defined as October 1, 2003 through September 30, 2004, and the 2007 school year is defined as October 1, 2007 through September 30, 2008.[26] The test scores that we use are combined rates of math and reading proficiency reported by states under NCLB. Scores are reported at the school and school district levels. We matched each housing sale with lagged test scores for the relevant school or school district. Homes that sold during the 2003 school year were matched with math/reading proficiency scores from the 2002 school year, for example. We will simply refer to the lagged scores as the "2003 score" and "2007 score" from here on.[27] Unlike the NAEP data used by Dee and Jacob (2009), these scores are not directly comparable across states. We use the state-specific NCLB scores because they capture variation within metro areas and they contain the same information that is readily available to prospective homebuyers.

Table 2 reports the 2003 baseline NCLB test scores and 2007-2003 differences for the 10th, 50th, and 90th percentiles of schools within each study area. In Fairfax, for example, math/reading scores in the bottom 10th percentile of schools increased by an average of 11 points (or 14%) with a standard deviation of approximately 8 points. The corresponding changes for the other four metro areas are all positive and typically large. There are smaller gains (and even losses) at the middle and 90th percentiles. These statistics are consistent with Dee and Jacob's (2009) finding that NCLB had the biggest impact on schools that began the program with the

[26] These definitions for the school year were chosen because the NCLB test scores and school grades for the preceding school year are typically announced at the end of August or the beginning of September. Thus we want to allow time for our proxy for school quality—test scores—to matter in the home buying decisions.
[27] The school quality information was obtained from www.schooldatadirect.org. The combined measure of reading and math is an overall measure (calculated by Standard & Poor's) that provides an average of the proficiency rates achieved across all reading and math tests, weighted by the number of tests taken for each school or school district.

lowest scores.

The remaining components of the data were collected from various sources. Sale prices and structural characteristics of every home sold during the 2003 and 2007 school years were purchased from a commercial vendor that assembles the data from public records maintained in the county/counties that comprise each study region. Tax rates were calculated using tax assessment data also available from public records. All other neighborhood characteristics were collected at the Census block group level, using annual data from Geolytics.[28] These block group data were spatially merged to each home.

Table 3 provides summary statistics for the complete set of data from our Fairfax County, VA sample. Columns 1-2 report means and standard deviations of all the variables used in the hedonic and capitalization regressions. In 2003 the average home sold for approximately $567,000 but by 2007 the price had dropped slightly to $563,000. Over this same period, the average test score rose from 83.56 to 84.36.[29] This seemingly small change masks considerable heterogeneity across individual schools (table 2). The average home was 34 years old, with 4 bedrooms, 3 baths, and 2,100 square feet of living area on a 0.4 acre lot. It was located in a block group where 23% of the neighborhood was nonwhite, 24% was under 18 years of age, 85% of homes were owner occupied, 1% of homes were vacant, and 0.37 was the normalized measure of population density. The average ratio of assessed to taxed value called a "tax rate" in this area was 112.

Columns 3-5 summarize the sub sample used in the boundary discontinuity analysis.

---

[28] Geolytics combines demographic information from the decennial Census with postal records and actuarial tables of births and deaths to develop an annual series for neighborhood demographics of Census block groups.

[29] It should be noted that the mean for the 2003 score levels is slightly different than the 2003 score level reported in Table 2 and the corresponding appendix tables. This is because Table 3 scores are weighted by enrollment whereas Table 2 is weighted by housing transactions. In other words, the difference represents the fact that the spatial distribution of housing transactions is not the same as the spatial distribution of enrollments.

Column 3 reports means over sales of homes located within 0.2 miles of a boundary. While this cuts the sample in half, the characteristics of the average home are virtually the same as in the full sample (column 1). Column 4 reports the difference in mean characteristics of homes located on the "high score" and "low score" sides of a boundary, and column 5 reports T-statistics on the differences. Differences in test scores are large and statistically significant whereas differences in housing characteristics are mostly small and insignificant. Like Bayer, Ferreira, and McMillan (2007), we find significant differences in the racial composition of homeowners on the high and low-score sides of a boundary. This underscores the importance of controlling for demographic characteristics during the estimation.

Columns 6-7 report means and standard deviations for the average home in each Census block group. These are the data we use to estimate the capitalization rate for changes in test scores between 2003 and 2007.[30] Notice that aggregation does not substantially change the summary statistics relative to the micro data. Finally, columns 8-9 report correlations between the change in test scores and levels and changes in all other variables. The orthogonality condition in (13) is clearly violated.

The Fairfax county data illustrate several features that are common to the data sets for Portland, Philadelphia, Detroit, and Los Angeles. In particular: (i) variable means are very similar across the full micro, 0.2 mile micro, and block group samples in each metro area; (ii) test scores and racial composition both tend to change discretely across the boundary zones; (iii) changes in test scores are negatively correlated with the baseline level of test scores; and (iv) changes in test scores are generally correlated with levels and changes in other housing characte-

[30] There are insufficient repeated sales of individual homes to implement a micro data analysis as in Davis (2004). Relative to our block-group averages, other recent capitalization studies have used more aggregate data such as census tracts medians or county averages (e.g. Chay and Greenstone 2005, Greenstone and Gallagher 2008, Baum-Snow and Marion 2009).

ristics. Complete summary statistics for each metro area are reported in the appendix.

VI. Results

A. Single-Period Hedonic Regressions

Our hedonic estimates of the MWTP for school quality are based on the following specification for the price function:

$$\ln(P) = testscore\,\theta_{03} + D \cdot testscore\,\theta_{07} + h\eta_{03} + D \cdot h\eta_{07} + BFE_{03} + BFE_{07} + \varepsilon . \tag{16}$$

$testscore$ denotes the log of math and reading proficiencies for the year prior to the housing sale, D is an indicator for sales that occurred in the 2007 school year, h includes all structural housing characteristics, neighborhood demographic variables, and the tax rate, and BFE_{03}, BFE_{07} are boundary fixed effects in 2003 and 2007. The boundary regions are 0.2 mile areas that overlap adjacent school attendance zones (Fairfax, Portland) or adjacent school districts (Philadelphia, Detroit, Los Angeles).[31] Under the null hypothesis that the hedonic gradient is constant over the duration of the study, $\theta_{07} = \eta_{07} = 0$.

We begin by using the sample of homes that sold within 0.2 miles of a boundary to assess the bias from omitted variables. Panels A and B of table 4 report the OLS estimates of θ_{03} and θ_{07} from regressions with and without boundary fixed effects. Since test scores are measured in logs, their coefficients are elasticities. For example, the results in column 2 indicate that the prices of homes sold in Portland during 2003 were approximately 0.456% higher in school attendance zones where math and reading proficiency was 1% higher. This price elasticity is virtually the same when we compare school districts in Philadelphia in column 3. Notice that Philadelphia is also one of four metro areas to have a significant increase in the price elasticity over the

[31] We found similar results for boundary regions of 0.35 and 0.15 miles.

first four years of the NCLB program. It increased from 0.481 in 2003 to 0.710 in 2007 (i.e. 0.481 + 0.229). Overall, panel A provides tentative evidence that NCLB test scores matter for property values and that the functional relationship between them changed over the duration of our study.

The evidence in panel A is tentative because we have not controlled for possible correlation between school quality and unobserved amenities. Positive correlation seems likely to arise from the sorting mechanism that underlies hedonic equilibrium. The intuition for this mechanism begins with the observation that household income is a strong predictor of a child's academic performance.[32] With this in mind, consider the household's location choice problem. If homebuyers appreciate low crime rates, access to parks, and scenic views, they will bid up housing prices in the neighborhoods that provide those (and other) amenities. Wealthier parents who can afford to live in the higher-amenity neighborhoods will have children who tend to perform better on standardized tests. Therefore, the inability to control for crime, parks, and views will produce an upward bias on the OLS estimator for the test score coefficient. The boundary fixed effects address this problem by absorbing the average price effect of unobserved amenities in the regions between adjacent school districts or adjacent attendance zones, allowing us to isolate the property value effect of higher test scores.[33]

Panel B reports the regression results after adding boundary fixed effects. In each metro area the coefficient of variation increases and the test score coefficients decrease, consistent with

[32] Correlation between household income and academic performance reflects a web of interaction between several underlying factors. Income is correlated with parental education and ability which, in turn, may help to explain the quality of the early parenting environment. Income is also correlated with the education and ability of the parents' of the child's peers, and so on. While positive correlation between income and test scores is sufficient to develop intuition for the endogeneity problem in our model, understanding the underlying causal mechanisms is critical to the development of effective education policies. See Heckman (2008) for a summary of the evidence.

[33] For more background on this identification strategy see Black (1999) and Bayer, Ferreira, and McMillan (2007).

intuition.[34] A quick comparison between panels A and B confirms that omitted variables are a serious problem. They inflate most of the test score elasticities by more than 100%!

Raw test scores are not directly comparable across states because each state develops its own standardized tests. Nevertheless, since the state-specific scores represent different proxy measures of the same underlying variable—school quality—they can be compared in terms of a common proportionate change. The test score elasticities in columns 6-10 are remarkably similar across the five metro areas in 2003. They suggest a 1% increase in math and reading proficiency would increase property values by 0.12% to 0.27%. In comparison, Black's (1999) preferred specification indicates an increase of approximately 0.42% for Boston suburbs in 1993-1995 and the results from Bayer, Ferreira, and McMillan (2007) indicate an increase of approximately 0.12% for the San Francisco metro area in 1990.

In 2007 our range of point estimates for the test score elasticity is considerably wider: 0.04 to 0.57. The changes are large and significant for Fairfax, Portland, Detroit, and Los Angeles. Several factors may be contributing to the changes in elasticities between 2003 and 2007. These include: (i) changes in NCLB test scores; (ii) changes in wealth; (iii) the information shock created by the new format for reporting test scores under the NCLB program; (iv) changes in neighborhood demographics; (v) changes in other housing characteristics that serve as substitutes or complements for school quality; and (vi) changes in the stock of housing. Parsing out the relative importance of these effects would require estimating the demand curve for school quality. While demand estimation is beyond the scope of this study, we conjecture that changes in the hedonic gradient may provide the extra information needed to overcome past problems

[34] The impact on the test score coefficients of including the boundary fixed effects is quite similar (in percentage terms) to the results reported by Black (1999) and Bayer, Ferreira, and McMillan (2007). Coefficients on the control variables are generally consistent across metro areas with the usual signs and plausible magnitudes. Like Bayer, Ferreira, and McMillan we find that, more often than not, inclusion of the boundary fixed effects decreases the magnitudes of the coefficients on neighborhood demographics.

with identification. An explanation is saved for section 8. Until then, we continue to focus on the relationship between marginal effects in the capitalization and hedonic models.

B. Capitalization Regressions and Robustness Checks

Large changes in the hedonic test score coefficients provide the first signal that capitalization rates are unlikely to identify MWTP. A second indication is the fact that changes in other coefficients are large enough to reject the hypothesis of a time-constant gradient in each metro area (F-tests are reported in panel B). Since we lack a randomized instrument for the change in test scores, there is little hope for circumventing capitalization bias. Measures of correlation in tables 2 and 3 (and appendix tables 1-4) reveal that the orthogonality conditions in (13) are systematically violated. For example, the changes in NCLB test scores for schools in Fairfax are positively correlated with some neighborhood characteristics (e.g. percent nonwhite residents in 2003, percent renting in 2003, population density in 2003) and negatively correlated with others (e.g. NCLB score in 2003, tax rate, change in percent nonwhite). Thus, it comes as no surprise that the capitalization-based estimates for the test score elasticity in panel C of table 4 look very different from their hedonic counterparts in panel B.

The results in panel C were generated by OLS estimation of the first-differenced capitalization model using the full sample of block groups. Notice that Los Angeles is the only place where the capitalization rate (0.17%) lies within the range defined by the price function parameters from 2003 and 2007 (0.14% to 0.22%). In Fairfax, Portland, Philadelphia, and Detroit, our capitalization-based estimates for the test score elasticity are far below the lower bound of point estimates from the hedonic model. The capitalization rate is at least positive and marginally significant in Philadelphia. In Fairfax and Portland the downward bias is so large that capitalization rates would imply the willingness to pay for improved school quality is essentially zero. In Detroit the capitalization rate is negative and marginally significant. This could simply reflect approximation error in the linear form of the estimating equation, but the hedonic estimates in column 9 are quite reasonable by contrast.

We consider three alternative explanations for the large differences between our estimates for capitalization rates and hedonic parameters: sample selection, data aggregation, and unobserved shocks that may be correlated with the change in school quality. Table 5 reports the results from indirect tests of each hypothesis.

First consider the scope for sample selection bias. Houses located outside the 0.2 mile boundary zones are included in the capitalization model but excluded from the hedonic regressions. The excluded homes comprise a large share of total housing sales in each metro area, from 35% in Portland to 92% in Los Angeles. Differences between the capitalization and hedonic results could arise from differences in the distribution of properties located in the excluded and included areas. To test this possibility, we repeat estimation of the basic hedonic model (without boundary fixed effects) using all of the micro data that were used to construct the block group averages for the capitalization model. Results are reported in columns 1-5 of table 5. They essentially mirror the original hedonic estimates from columns 1-5 of table 4. Given the large sample sizes, it is remarkable that only two of the ten coefficients are statistically different (Fairfax and Detroit in 2003). From this we conclude that sample selection is unlikely to explain the differences between our baseline results from the hedonic and capitalization models.

A second possibility is that the capitalization results are driven by aggregation bias that arises from averaging the micro data over Census block groups. The issue is that the “average” home in a given block group need not correspond to any point on the hedonic price surface. It is difficult to predict the direction and magnitude of the resulting bias. Past studies that have used Census aggregates have assumed the bias is sufficiently small to ignore (Chay and Greenstone 2005, Greenstone and Gallagher 2008, Baum-Snow and Marion 2009). To test this assumption, we aggregate the micro data from panel A into block groups and repeat the estimation. Results are reported in panel B. Comparing the two panels reveals that aggregation does not affect the general pattern of results. The magnitudes of the coefficients do change a bit, but the differences are mostly insignificant.

Finally, our estimates for the capitalization rate could be confounded by omitted variables. If changes in unobserved amenities are negatively correlated with changes in school quality, the first-differenced estimator will be biased downward. To test this possibility we extend the boundary discontinuity identification strategy to a panel data setting. First, we drop all houses that do not fall within 0.2 miles of a boundary. Then we aggregate the micro data into "boundary neighborhoods" on either side of each boundary. Finally, we add fixed effects for each boundary and estimate the resulting first-differenced model,

$$\Delta \ln(P) = \Delta testscore\,\phi + \Delta h\gamma + \Delta BFE + \Delta\varepsilon\,. \qquad (17)$$

These "boundary difference fixed effects" will absorb the capitalization of changes in unobserved amenities in each boundary region. Results are reported in panel C. While standard errors on the elasticities are quite large due to the decrease in sample size and the inclusion of fixed effects, the point estimates are remarkably similar to our baseline results. The point estimates in columns 11-15 of table 5 all fall within the 95% confidence intervals on the corresponding estimates from columns 11-15 of table 4. Thus, omitted variables do not provide much help in explaining why our baseline estimates for the capitalization rate are so much lower than the elasticities from the hedonic model. We are left to conclude that the differences we observe are primarily due to changes in the gradient of the hedonic price function.

VII. Summary and Implications of Empirical Results

The results from our boundary discontinuity regressions demonstrate that hedonic gradients can change significantly over a short period of time. We are not the first to document this type of instability. Costa and Kahn (2003) found that the implicit price of living in a metropolitan area with a temperate climate doubled between 1970 and 1980, and then doubled again between 1980 and 1990. In an application that more closely resembles ours, Brookshire et al. (1985) found that a discrete shock to information about earthquake risk changed the hedonic gradient over a 6-year period. Other researchers have reported annual changes in the relative prices of structural housing characteristics (Meese and Wallace 1997, Murphy 2007). However, all of these studies are vulnerable to the usual concern about confounding from omitted variables. By using a sharp discontinuity to mitigate the omitted variable pitfall, we have provided the strongest evidence to date that hedonic gradients do change.

We also find that changes in the hedonic gradient matter for evaluating the benefits of public education. Table 6 provides a summary comparison between our hedonic and capitalization based estimates for the average resident's willingness to pay for a 1% increase in test scores. Each column reports the MWTP predicted by a specific econometric model, averaged over the samples from all five study regions. In columns 1-3 we do not control for omitted variables. The resulting predictions for MWTP are fairly robust to how we define a data point (house, Census block group) and how we define the extent of the market (full metro area, 0.2 mile boundary zone). However, these predictions are twice as large as the ones from the model with boundary fixed effects (column 4). This reinforces past evidence on the potential for omitted variables to confound the results from property value studies (Black 1999, Chay and Grenstone 2005, Bayer, Ferreira, and McMillan 2007).

The hedonic boundary discontinuity design in column 4 is our preferred specification. It controls for omitted variables; it controls for sorting across boundaries on the basis of race; and it provides a theoretically consistent prediction for MWTP at a single point in time. It implies the average household would have been willing to pay $536 for a 1% improvement in school quality during the first year of the NCLB program.[35] Over the next four years, there were several changes. Property values increased by 6% on average, test scores increased by 10%, and there were smaller changes in the demographic composition of neighborhoods. There was also steady media coverage of the NCLB program and changes in the broader economy that would have affected expectations about permanent income (e.g. rapid growth in stock market indices and personal income per capita). These changes were accompanied by changes in hedonic gradients which, in turn, increased our prediction for average MWTP to $688 for the 2007 school year.

Relative to our hedonic model, capitalization rates severely understate the willingness to pay for academic performance. Column 5 reports the average MWTP predicted by our first-differenced capitalization model ($134 in 2003, $152 in 2007).[36] These figures are about ¼ the size of estimates from our hedonic boundary discontinuity regressions! The difference between hedonic MWTP and capitalization rates only narrows slightly when we add controls for time-varying omitted variables to the capitalization regressions (column 6). Placing these results in the context of our conceptual framework suggests that researchers must be cautious in using

[35] This average reflects variation across metro areas, from a low of $422 in Detroit to a high of $743 in Philadelphia. Interestingly, the area with the highest average MWTP, Philadelphia, also ranked third among all U.S. cities in terms of the volume of Google searches on the phrase "No Child Left Behind" in 2004. The top two cities were Pittsburg and Washington D.C.

[36] These figures were calculated by combining results from columns 11-15 in table 4 with data on average property values and populations in tables 3 and A1-A4.

capitalization rates as the basis for evaluating the benefits of public programs.

VIII. Conclusions

The hedonic property value model and the land value capitalization model are typically viewed as separate frameworks. We have sought to connect them. By extending Rosen (1974) to describe how the equilibrium price function adjusts to changes in the supply of a public good, we were able to express market capitalization as a function of hedonic willingness to pay. This unified framework provides a welfare theoretic basis for interpreting evidence on capitalization rates for shocks to public goods.

Our conceptual model produced three insights into the relationship between capitalization and MWTP. First, the scope for divergence between the two concepts grows with the size of the shock and the length of the study period. As both dimensions approach zero, the capitalization rate approaches MWTP. Second, if we want to guarantee that ex-ante MWTP is recoverable from the capitalization of a non-marginal shock, we must add further assumptions about preferences and technology to Rosen's model. These new assumptions have a testable implication. They imply the hedonic gradient will be constant over time. Finally, if the hedonic gradient changes over time we can still recover ex-post MWTP as long as the price function is linear in parameters and the shock (or an instrument for the shock) is randomized.

In the application to school quality, the average difference between capitalization and MWTP was quite large. To recover MWTP we developed the most comprehensive set of estimates to date on the contribution of academic performance to residential property values, using hedonic boundary discontinuity designs to control for omitted variables. By analyzing five metro areas at two points in time, we were able to generate ten separate estimates for the elasticity of property values with respect to test scores. We found that these hedonic gradients changed over time. As a result, our estimates for MWTP were three to four times as large as capitalization rates for changes in test scores.

More generally, our framework can guide future research on valuing public goods by illustrating how to overcome problems with capitalization-based welfare measurement. The simplest solution is to avoid interpreting capitalization rates as measures of willingness to pay unless the data make it possible to track small shocks over brief intervals. If the goal is to recover ex-post MWTP, a second solution is to find an instrument that randomizes the intensity of the public good "treatment". The instrument must be orthogonal to baseline levels of every control variable and to changes in those variables. This is a tall order which no instrument is likely to satisfy completely. But some natural experiments and policy discontinuities may come close. The validity of candidate instruments can be judged from the coefficients on the "price effect" and "substitution effect" terms in our expression for the capitalization bias (equation 11). For example, if $\left|r'g_1/r'r\right| < 0.01$ the bias in estimated MWTP that arises from a change in the implicit price of g will be less than 1% of the change that occurred.

Finally, we conjecture that the same market forces that drive a wedge between capitalization and MWTP also have the potential to help us overcome the classic problem with identifying demand curves. The problem is that the equilibrium price function intersects each household's demand curve at exactly one point (Epple 1987; Bartik 1987). To identify the rest of the curve from market data, we must observe similar households making choices along a different price surface. Pooling data from different geographic markets, while possible, raises concerns with selection bias (Rubinfeld, Shapiro, and Roberts 1987). Our conceptual model suggests a different solution. We have demonstrated that large shocks to public goods can change the hedonic price surface in a single geographic market. Thus, it may be possible to identify demand curves from repeated cross-sections of households collected before and after a shock to the distribution of public goods supplied in a single metro area. We view this quasi-experimental approach to hedonic demand estimation as a promising direction for future research.

TABLE 1
SUMMARY OF HEDONIC AND CAPITALIZATION-BASED ESTIMATES OF THE WILLINGNESS TO PAY FOR A SMALL IMPROVEMENT IN PUBLIC SCHOOL QUALITY

	STUDY REGION FOR ESTIMATES	TEST SCORE ELASTICITY		MEAN WILLINGNESS TO PAY FOR 1% INCREASE IN TEST SCORES	
		hedonic (1)	capitalization (2)	hedonic (3)	capitalization (4)
PREVIOUS STUDIES					
Black (1999)	Boston, MA [1990]	0.42		917	
Bayer, Ferreira, McMillan (2007)	San Francisco, CA [1993-95]	0.12		372	
THIS STUDY	Fairfax, VA [2003]	0.12	-0.04	608	-194
	Portland, OR [2003]	0.20	0.01	447	16
	Philadelphia, PA [2003]	0.27	0.12	743	317
	Detroit, MI [2003]	0.21	-0.29	422	-587
	Los Angeles, CA [2003]	0.14	0.17	596	740
	All Five Regions [2003]			536	134
	All Five Regions [2007]			688	152

NOTE.—Children in Boston, San Francisco, Fairfax, and Portland were assigned to elementary schools based on the attendance zones where their parents lived. Children in Philadelphia, Detroit, and Los Angeles were assigned to school districts but free to choose between schools within a district. Each state develops its own standardized tests, which change over time. Assignment laws and test scores are discussed in section V. In cols. 1 and 3, the hedonic estimates are identified by boundary discontinuity designs that use fixed effects to control for omitted variables. In cols. 2 and 4, the capitalization estimates are identified by first-differenced regressions that control for changes in neighborhood demographics and purge omitted variables. All measures of willingness to pay are reported in constant year 2000 dollars.

TABLE 2
SUMMARY STATISTICS OF SCHOOL TEST SCORE DIFFERENCES

	FAIRFAX, VA		PORTLAND, OR		PHILADELPHIA, PA		DETROIT, MI		LOS ANGELES, CA	
	mean	sd	mean	sd	mean	sd	mean	sd	mean	sd
2002/2003 math-reading score	81.88	10.44	79.35	11.34	67.43	13.19	67.17	11.39	45.73	17.34
Changes in math-reading score										
10th decile	11.35	8.03	1.45	9.22	18.78	0.80	15.08	2.43	10.60	3.26
middle deciles	0.62	5.02	-4.02	6.61	10.45	4.85	11.15	2.78	9.24	2.01
90th decile	-0.44	2.79	-4.50	4.07	6.28	1.36	5.13	1.72	5.97	1.51
% change in 10th decile	13.87%		1.83%		27.85%		22.46%		23.18%	

NOTE.—Means and standard deviations for test scores are based on NCLB information aggregated and reported by www.schooldatadirect.org. The math reading score is an overall measure (calculated by Standard & Poor's) that provides an average of the proficiency rates achieved across all reading and math tests, weighted by the number of tests taken for each elementary school (Fairfax and Portland) or school district (Philly, Detroit and LA). Raw scores are not directly comparable across states because each state develops its own standardized tests.

TABLE 3

SUMMARY STATISTICS FOR HOUSING, NEIGHBORHOODS, AND TEST SCORES IN FAIRFAX, VA

	Full Sample (micro data: N = 10,255)		Sample: 0.20 Mile Boundary Zone (micro data: N = 5,843)			Full Sample (Census block group data: N = 438)			
	mean	standard deviation	mean	difference in means: high score side -low score side	T-statistic on difference in means	mean	standard deviation	correlation: Δscore & variable in 2003	correlation: Δscore & Δvariable
Fairfax County, VA	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Sale price									-0.02
2003 price	567,322	247,727	546,575	-3,036	-0.40	571,742	226,270	0.06	
2007 price	562,683	305,748	542,998	14,512	1.36	599,474	268,952		
Average math/reading test result									
2003 score	83.56	9.54	83.01	8.91	38.67	82.86	9.25	-0.49	
2007 score	84.36	8.25	83.90	5.11	24.81	83.92	8.17		
Housing characteristics:									
square feet (100's)	21.12	9.93	20.66	0.07	0.26	21.32	7.06	0.01	-0.02
bathrooms	3.24	1.08	3.21	0.00	-0.10	3.24	0.72	0.00	-0.05
age	34.07	15.82	34.13	0.89	2.13	35.21	12.63	0.04	-0.02
lot acres	0.38	0.43	0.35	0.00	-0.51	0.43	0.42	0.07	0.04
bedrooms	3.94	0.77	3.93	0.03	1.56	3.92	0.36	-0.03	-0.08
Neighborhood characteristics:									
% block group nonwhite	0.23	0.11	0.23	-0.02	-6.89	0.24	0.12	0.16	-0.12
% block group under 18	0.24	0.04	0.24	0.00	-0.82	0.23	0.03	-0.04	0.07
% block group owner occupied	0.85	0.15	0.84	0.00	0.08	0.81	0.18	-0.18	0.00
% block group vacant	0.01	0.02	0.01	0.00	-2.06	0.02	0.02	0.10	0.04
block group pop density	0.37	0.22	0.40	0.00	0.77	0.39	0.26	0.06	-0.11
tax rate	111.85	49.52	111.45	-0.30	-0.28	117.30	38.00	-0.08	

NOTE.—This table reports summary statistics for the key variables included in the analysis for Fairfax, VA. Cols. 1, 2, 3, 6 and 7 are simply the means and standard deviations for the 3 different samples of data. The boundary zone sample includes all houses located within 0.20 miles of the boundary of another school attendance zone. Col. 4 reports the difference in means between houses located on the "high" test score side of a boundary with the corresponding mean for the "low" test score homes on the opposite side of the boundary. Col. 5 provides a T-statistic on the difference in these means. Cols. 8 and 9 report correlations between the change in test scores and levels and changes in all other variables for the full sample of census block group data.

TABLE 4

TEST SCORE COEFFICIENTS FROM HEDONIC AND CAPITALIZATION REGRESSIONS

	FAIRFAX, VA	PORTLAND, OR	PHILADELPHIA, PA	DETROIT, MI	LOS ANGELES, CA
	A. Test Score Parameters from Hedonic Regressions (micro data from 0.2 mile sample without boundary fixed effects)				
	(1)	(2)	(3)	(4)	(5)
log (test score), 2003 coefficient	0.122 (0.027)	0.456 (0.020)	0.481 (0.045)	0.524 (0.036)	0.274 (0.012)
log (test score), 2007 differential	0.554 (0.056)	0.034 (0.032)	0.229 (0.067)	0.516 (0.086)	0.084 (0.023)
R^2	0.74	0.70	0.68	0.68	0.75
Number of observations	6,036	14,443	3,973	6,252	12,287
	B. Test Score Parameters from Hedonic Regressions (micro data from 0.2 mile sample with boundary fixed effects)				
	(6)	(7)	(8)	(9)	(10)
log (test score), 2003 coefficient	0.116 (0.040)	0.200 (0.028)	0.272 (0.071)	0.208 (0.047)	0.140 (0.015)
log (test score), 2007 differential	0.293 (0.081)	-0.165 (0.048)	-0.120 (0.101)	0.357 (0.126)	0.075 (0.028)
R^2	0.85	0.77	0.76	0.74	0.85
Number of observations	6,036	14,443	3,973	6,252	12,287
F-test on H_0: time-constant gradient	4.69	1.98	1.86	4.41	8.22
p-value on F-test	0.000	0.031	0.047	0.000	0.000
	C. Test Score Parameters from Capitalization Regressions (block group data from full sample)				
	(11)	(12)	(13)	(14)	(15)
change in log (test score)	-0.037 (0.073)	0.007 (0.096)	0.116 (0.068)	-0.289 (0.134)	0.174 (0.033)
R^2	0.53	0.45	0.29	0.21	0.18
Number of observations	438	754	1,199	1,477	6,975

NOTE.—All regressions included controls for property taxes, structural housing characteristics (square feet, number of bathrooms, age, lot size, number of bedrooms) and neighborhood characteristics measured at the block group level (population density, percent nonwhite, percent under 18, percent owner occupied, and percent vacant). In cols. 1 through 10, the dependent variable is the natural log of the sale price of the home. All control variables are interacted with a dummy for sales made during the 2007-2008 school year. In cols. 11 through 15 the dependent variable is the change in the natural log of the average sale price in the census block group. All regressions use Eicker-White standard errors.

TABLE 5
ROBUSTNESS CHECKS ON TEST SCORE COEFFICIENTS

	FAIRFAX, VA	PORTLAND, OR	PHILADELPHIA, PA	DETROIT, MI	LOS ANGELES, CA
	A. Test Score Parameters from Hedonic Regressions (micro data from full sample without boundary fixed effects)				
	(1)	(2)	(3)	(4)	(5)
log (test score), 2003 coefficient	0.227 (0.023)	0.540 (0.016)	0.546 (0.017)	0.751 (0.019)	0.260 (0.004)
log (test score), 2007 differential	0.550 (0.044)	0.024 (0.024)	0.396 (0.028)	0.565 (0.042)	0.041 (0.008)
R^2	0.75	0.69	0.72	0.68	0.70
Number of observations	10,662	25,294	29,327	32,485	146,783
	B. Test Score Parameters from Hedonic Regressions (block group data from full sample without boundary fixed effects)				
	(6)	(7)	(8)	(9)	(10)
log (test score), 2003 coefficient	0.148 (0.068)	0.388 (0.045)	0.229 (0.046)	0.813 (0.052)	0.321 (0.012)
log (test score), 2007 differential	0.475 (0.121)	0.086 (0.070)	0.367 (0.083)	0.717 (0.108)	0.082 (0.022)
R^2	0.84	0.82	0.78	0.77	0.73
Number of observations	889	1,553	2,647	3,333	14,727
	C. Test Score Parameters from Capitalization Regressions (block group data from 0.2 mile sample with boundary fixed effects)				
	(11)	(12)	(13)	(14)	(15)
change in log (test score)	0.008 (0.111)	-0.025 (0.091)	0.130 (0.180)	-0.445 (0.521)	0.231 (0.177)
R^2	0.83	0.82	0.91	0.87	0.83
Number of observations	422	603	176	213	251

NOTE.—All regressions included controls for property taxes, structural housing characteristics (square feet, number of bathrooms, age, lot size, number of bedrooms) and neighborhood characteristics measured at the block group level (population density, percent nonwhite, percent under 18, percent owner occupied, and percent vacant). In cols. 1 through 10, the dependent variable is the natural log of the sale price of the home. All control variables are interacted with a dummy for sales made during the 2007-2008 school year. In cols. 11 through 15 the dependent variable is the change in the natural log of the average sale price in the census block group. All regressions use Eicker-White standard errors.

TABLE 6
IMPACT OF IDENTIFICATION STRATEGY ON ESTIMATES FOR THE AVERAGE RESIDENT'S WILLINGNESS TO PAY FOR A 1% INCREASE IN TEST SCORES

	(1)	(2)	(3)	(4)	(5)	(6)
Estimates for willingness to pay:						
2003 school year	1,238	1,222	1,041	536	134	169
2007 school year	1,685	1,572	1,660	688	152	190
Identification strategy:						
Model	hedonic	hedonic	hedonic	hedonic	capitalization	capitalization
Sample	full	full	0.2 mile	0.2 mile	full	0.2 mile
Data point	block group	house	house	house	block group	block group
Sample size	23,149	244,551	42,991	42,991	10,843	1,665
Controls for omitted variables	none	none	none	boundary fixed effects	differencing	differencing + boundary fixed effects

NOTE.—All measures of willingness to pay are reported in constant year 2000 dollars. Each measure is averaged over the samples from our five study regions, using the elasticities reported in tables 4 and 5. For example, the estimates in col. 4 are based on the elasticities reported in cols. 6 through 10 of table 4.

REFERENCES

Bailey, Martin J., Richard F. Muth, and Hugh O. Nourse. 1963. "A Regression Method for Real Estate Price Index Construction." *Journal of the American Statistical Association*, 58(304): 933-42.

Bajari, Patrick and C. Lanier Benkard. 2005. "Demand Estimation with Heterogeneous Consumers and Unobserved Product Characteristics: A Hedonic Approach." *Journal of Political Economy*, 113(6): 1239-76.

Bartik, Timothy J. 1987. "The Estimation of Demand Parameters in Hedonic Price Models." *Journal of Political Economy* 95(1): 81-88.

Baum-Snow, Nathaniel, and Justin Marion. 2009. "The Effects of Low Income Housing Tax Credit Developments on Neighborhoods." *Journal of Public Economics* 93(5-6): 654-66.

Bayer, Patrick, Fernando Ferreira, and Robert McMillan. 2007. "A Unified Framework for Measuring Preferences for Schools and Neighborhoods." *Journal of Political Economy*, 115(4): 588-638.

Bergstrom, Theodore C., and Robert P. Goodman. 1973. "Private Demands for Public Goods." *American Economic Review* 63(3): 280-96.

Bin, Okmyung, Craig E. Landry, and Gregory E. Meyer. 2009. "Riparian Buffers and Hedonic Prices: A Quasi-Experimental Analysis of Residential Property Values in the Neuse River Basin." *American Journal of Agricultural Economics* 91(4): 1067-79.

Bishop, Kelly, and Christopher Timmins. 2008. "Simple, Consistent Estimation of the Marginal Willingness to Pay Function: Recovering Rosen's Second Stage without Instrumental Variables." *Mimeo*.

Black, Sandra E. 1999. "Do Better Schools Matter? Parental Valuation of Elementary Education." *Quarterly Journal of Economics*, 114(2): 577-99.

Bogart William T., and Brian A. Cromwell. 2000. "How much is a neighborhood school worth?" *Journal of Urban Economics* 47: 280-305.

Brookshire, David S., Mark A. Thayer, John Tschirhart, and William D. Schulze. 1985. "A Test of the Expected Utility Model: Evidence from Earthquake Risks." *Journal of Political Economy*, 93(2): 369-89.

Cellini, Stephanie Riegg, Fernando Ferreira, and Rothstein Jesse. 2010. "The Value of School Facility Investments: Evidence from a Dynamic Regression Discontinuity Design." *Quarterly Journal of Economics*, forthcoming.

Chay, Kenneth Y., and Michael Greenstone. 1998. "Does Air Quality Matter? Evidence from the Housing Market." *NBER Working Paper #6826*.

Chay, Kenneth Y. and Michael Greenstone. 2005. "Does Air Quality Matter? Evidence from the Housing Market." *Journal of Political Economy*, 113(2): 376-424.

Chetty, Raj. 2009. “Sufficient Statistics for Welfare Analysis: A Bridge Between Structural and Reduced-Form Methods,” *Annual Review of Economics* 1: 451-488.

Costa, Dora L and Matthew E. Kahn. 2003. "The Rising Price of Nonmarket Goods." *American Economic Review*, 93(2): 227-32.

Cropper, Maureen L., Leland B. Deck, and Kenenth E. McConnell. 1988. "On the Choice of Functional Form for Hedonic Price Functions." *Review of Economics and Statistics*, 70(4): 668-75.

Davis, Lucas. 2004. "The Effect of Health Risk on Housing Values: Evidence from a Cancer Cluster." *American Economic Review*, 94(5): 1693-704.

Dee, Thomas, and Brian Jacob. 2009. “The Impact of No Child Left Behind on Student Achievement.” *NBER Working Paper 15531*.

Downes, Thomas A., and Jeffrey E. Zabel. 2002. "The Impact of School Characteristics on House Prices: Chicago 1987-1991." *Journal of Urban Economics* 52: 1-25.

Ekeland, Ivar, James J. Heckman, and Lars Nesheim. 2004. "Identification and Estimation of Hedonic Models." *Journal of Political Economy*, 112(1): S60-S109.

Epple, Dennis. 1987. "Hedonic Prices and Implicit Markets: Estimating Demand and Supply Functions for Differentiated Products." *Journal of Political Economy*, 95(1): 59-80.

Epple, Dennis and Holger Sieg. 1999. "Estimating Equilibrium Models of Local Jurisdiction." *Journal of Political Economy*, 107(4): 645-81.

Figlio, David N., and Maurice E. Lucas. 2004. "What's in a Grade? School Report Cards and the Housing Market." *American Economic Review* 94(3): 591-604.

Gibbons, Steve., and Stephen Machin. 2003. "Valuing English Primary Schools." *Journal of Urban Economics* 53: 197-219.

Greenstone, Michael and Justin Gallagher. 2008. "Does Hazardous Waste Matter? Evidence from the Housing Market and the Superfund Program." *Quarterly Journal of Economics*, 123(3): 951-1003.

Heckman, James J. 2008. “Schools, Skills, and Synapses.” *Economic Inquiry* 46(3): 289-324.

Horsch, Eric J., and David J. Lewis. 2009. "The Effects of Aquatic Invasive Species on Property

Values: Evidence from a Quasi-Experiment." *Land Economics* 85(3): 391-409.

Kain, John F., and John M. Quigley. 1975. "Housing Markets and Racial Discrimination." New York: National Bureau of Economic Research.

Klaiber, Allen H., and V. Kerry Smith. 2009. "Evaluating Rubin's Causal Model for Measuring the Capitalization of Environmental Amenities". *NBER Working Paper 14957*.

Kuminoff, Nicolai V. and Abdul S. Jarrah. 2010. "A New Approach to Computing Hedonic Equilibria and Investigating the Properties of Locational Sorting Models." *Journal of Urban Economics*, 67(3): 322-335.

Linden, Leigh and Jonah E. Rockoff. 2008. "Estimates of the Impact of Crime Risk on Property Values from Megan's Laws." *American Economic Review*, 98(3): 1103-27.

Lind, Robert C. 1973. "Spatial Equilibrium, the Theory of Rents, and the Measurement of Benefits from Public Programs." *Quarterly Journal of Economics*, 87(2): 188-207.

Meese, Richard A. and Nancy E. Wallace. 1997. "The Construction of Residential Housing Price Indices: A Comparison of Repeat-Sales, Hedonic-Regression, and Hybrid Approaches." *Journal of Real Estate Economics and Finance.* 14: 51-73.

Murphy, Alvin. 2007. "A Dynamic Model of Housing Supply." *Mimeo*.

Neal, Derek and Diane W. Schanzenbach. forthcoming. "Left Behind By Design: Proficiency Counts and Test-Based Accountability." *The Review of Economics and Statistics*.

Oates, Wallace. 1969. "The Effects of Property Taxes and Local Public Spending on Property Values: An Empirical Study of Tax Capitalization and the Tiebout Hypothesis." *Journal of Political Economy*, 77: 957-971.

Palmquist, Raymond B. 1982. "Measuring Environmental Effects on Property Values without Hedonic Regressions." *Journal of Urban Economics*, 33(3): 333-47.

Palmquist, Raymond B. 2005. "Property Value Models," in *Handbook of Environmental Economics, Volume 2*. Karl-Göran Mäler and Jeffery Vincent eds. Amsterdam: North Holland Press.

Pope, Jaren C. 2008. "Fear of Crime and Housing Prices: Household Reactions to Sex Offender Registries." *Journal of Urban Economics*, 64(3): 601-14.

Reback, Randall. 2005. "House Prices and the Provision of Local Public Services: Capitalization under School Choice Programs." *Journal of Urban Economics* 57: 275-301.

Reback, Randall. 2008. "Demand (and supply) in an inter-district public school choice program." *Economics of Education Review* 27: 402-416.

Rosen, Sherwin. 1974. "Hedonic Prices and Implicit Markets: Product Differentiation in Pure Competition." *Journal of Political Economy*, 82(1): 34-55.

Rubinfeld, Daniel L., Perry Shapiro, and Judith Roberts. 1987. "Tiebout Bias and the Demand for Local Public Schooling." *Review of Economics and Statistics* 69(3): 426-37.

Scotchmer, Suzanne. 1985. "Hedonic Prices and Cost/Benefit Analysis." *Journal of Economic Theory* 37(1): 55-75.

Scotchmer, Suzanne. 1986. "The Short-Run and Long-Run Benefits of Environmental Improvement." *Journal of Public Economics*, 30(1): 61-81.

Simons, Robert. 2006. *When Bad Things Happen to Good Property*. Washington D.C.: Environmental Law Institute.

Starrett, David A. 1981. "Land Value Capitalization in Local Public Finance." *Journal of Political Economy*, 89(2): 306-27.

Tiebout, Charles. 1956. "A Pure Theory of Local Expenditures." *Journal of Political Economy*, 64(5): 416-24.

Tinbergen, Jan. 1959. "On the Theory of Income Distribution," in *Selected Papers of Jan Tinbergen*. L.H. Klaassen, L.M. Koych and H.J. Witteveen eds. Amsterdam: North Holland.

APPENDIX TABLE 1
SUMMARY STATISTICS FOR HOUSING, NEIGHBORHOODS, AND PUBLIC SCHOOLS IN PORTLAND, OR

	Full Sample (micro data: N = 25,294)		Sample: 0.20 Mile Boundary Zone (micro data: N = 16,539)			Full Sample (Census block group data: N = 754)			
	mean	standard deviation	mean	difference in means: high score side -low score side	T-statistic on difference in means	mean	standard deviation	correlation: Δscore & variable in 2003	correlation: Δscore & Δvariable
Portland Metro Area	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Sale price									0.00
2003 price	241,875	142,991	237,021	2,664	0.91	244,315	127,426	0.00	
2007 price	324,181	173,171	316,236	-4,500	-1.14	336,280	156,920		
Average math/reading test result									
2003 score	79.82	10.93	79.89	7.41	44.93	78.94	10.33	-0.28	
2007 score	76.00	10.83	75.96	4.79	28.77	75.24	10.69		
Housing characteristics:									
square feet (100's)	17.88	7.76	17.75	-0.06	-0.48	17.59	5.18	0.04	-0.06
bathrooms	2.22	0.93	2.22	0.04	2.41	2.09	0.59	0.01	-0.03
age	39.67	30.16	38.39	-0.56	-1.17	50.17	22.91	0.00	-0.01
lot acres	0.20	0.29	0.18	0.00	0.72	0.30	0.43	0.08	0.01
bedrooms	3.07	0.94	3.07	-0.01	-0.41	3.03	0.47	0.09	-0.12
Neighborhood characteristics:									
% block group nonwhite	0.17	0.10	0.17	-0.01	-3.64	0.16	0.11	-0.14	0.00
% block group under 18	0.23	0.04	0.24	0.00	-1.14	0.22	0.04	-0.03	-0.02
% block group owner occupied	0.66	0.19	0.66	0.00	1.60	0.61	0.22	0.03	-0.06
% block group vacant	0.05	0.03	0.05	0.00	-7.41	0.05	0.03	-0.05	0.00
block group pop density	0.53	0.29	0.55	-0.01	-1.35	0.56	0.34	-0.08	0.04
tax rate	54.62	8.02	54.74	-0.23	-1.86	54.70	9.01	0.13	

NOTE.—This table reports summary statistics for the key variables included in the analysis for Portland, OR. Cols. 1, 2, 3, 6 and 7 are simply the means and standard deviations for the 3 different samples of data. The boundary zone sample includes all houses located within 0.20 miles of the boundary of another school attendance zone. Col. 4 reports the difference in means between houses located on the "high" test score side of a boundary with the corresponding mean for the "low" test score homes on the opposite side of the boundary. Col. 5 provides a T-statistic on the difference in these means. Cols. 8 and 9 report correlations between the change in test scores and levels and changes in all other variables for the full sample of census block group data.

APPENDIX TABLE 2

SUMMARY STATISTICS FOR HOUSING, NEIGHBORHOODS, AND PUBLIC SCHOOLS IN PHILADELPHIA, PA

	Full Sample (micro data: N = 29,333)		Sample: 0.20 Mile Boundary Zone (micro data: N = 3,973)			Full Sample (Census block group data: N = 1,199)			
	mean	standard deviation	mean	difference in means: high score side -low score side	T-statistic on difference in means	mean	standard deviation	correlation: Δscore & variable in 2003	correlation: Δscore & Δvariable
Philadelphia Metro Area	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Sale price									-0.07
2003 price	295,845	188,924	285,243	32,498	4.73	273,104	148,829	-0.35	
2007 price	334,662	221,967	324,197	49,222	5.44	316,940	170,204		
Average math/reading test result									
2003 score	67.88	13.93	69.43	11.05	30.63	64.38	16.84	-0.74	
2007 score	78.61	10.90	79.51	7.20	25.67	75.99	13.23		
Housing characteristics:									
square feet (100's)	20.87	9.48	20.03	1.21	4.21	19.85	5.95	-0.27	0.00
bathrooms	2.37	1.00	2.28	0.09	3.02	2.15	0.73	-0.35	-0.02
age	42.03	27.85	46.32	3.50	4.23	49.54	21.16	0.03	0.00
lot acres	0.49	0.65	0.44	0.02	1.15	0.45	0.46	-0.15	-0.01
bedrooms	3.38	0.77	3.33	0.07	2.82	3.40	0.44	-0.04	-0.03
Neighborhood characteristics:									
% block group nonwhite	0.12	0.14	0.11	-0.01	-1.30	0.14	0.19	0.22	-0.20
% block group under 18	0.23	0.04	0.22	0.00	2.54	0.22	0.04	0.01	0.10
% block group owner occupied	0.78	0.18	0.79	0.02	3.13	0.74	0.21	-0.12	0.04
% block group vacant	0.03	0.03	0.03	0.00	-3.77	0.04	0.03	0.14	-0.02
block group pop density	0.34	0.39	0.36	-0.04	-3.83	0.46	0.53	0.28	-0.13
tax rate	29.05	14.28	28.38	2.74	6.55	25.47	14.65	-0.30	

NOTE.—This table reports summary statistics for the key variables included in the analysis for Philadelphia, PA. Cols. 1, 2, 3, 6 and 7 are simply the means and standard deviations for the 3 different samples of data. The boundary zone sample includes all houses located within 0.20 miles of the boundary of another school attendance zone. Col. 4 reports the difference in means between houses located on the "high" test score side of a boundary with the corresponding mean for the "low" test score homes on the opposite side of the boundary. Col. 5 provides a T-statistic on the difference in these means. Cols. 8 and 9 report correlations between the change in test scores and levels and changes in all other variables for the full sample of census block group data.

APPENDIX TABLE 3

SUMMARY STATISTICS FOR HOUSING, NEIGHBORHOODS, AND PUBLIC SCHOOLS IN DETROIT, MI

Detroit Metro Area	Full Sample (micro data: N =32,486) mean (1)	standard deviation (2)	Sample: 0.20 Mile Boundary Zone (micro data: N = 6,285) mean (3)	difference in means: high score side -low score side (4)	T-statistic on difference in means (5)	Full Sample (Census block group data: N = 1,477) mean (6)	standard deviation (7)	correlation: Δscore & variable in 2003 (8)	correlation: Δscore & Δvariable (9)
Sale price									0.14
2003 price	219,857	131,658	214,048	14,186	2.92	214,626	123,303	-0.38	
2007 price	166,801	131,839	157,640	11,017	2.44	169,829	104,428		
Average math/reading test result									
2003 score	68.76	12.39	67.91	7.77	27.21	68.01	12.18	-0.60	
2007 score	79.28	10.52	78.51	7.72	32.09	78.80	10.38		
Housing characteristics:									
square feet (100's)	16.57	7.79	16.01	0.66	3.27	16.47	5.93	-0.32	0.02
bathrooms	2.06	1.00	2.00	0.08	3.12	2.03	0.73	-0.38	0.08
age	46.06	23.24	46.78	0.19	0.36	46.87	18.00	0.26	-0.07
lot acres	0.36	0.52	0.30	-0.02	-2.06	0.39	0.46	-0.11	0.01
bedrooms	3.15	0.73	3.11	0.03	1.66	3.15	0.45	-0.22	0.00
Neighborhood characteristics:									
% block group nonwhite	0.13	0.18	0.12	-0.03	-7.53	0.14	0.20	0.04	-0.30
% block group under 18	0.23	0.04	0.23	0.01	8.67	0.23	0.04	0.04	0.16
% block group owner occupied	0.80	0.18	0.82	0.02	3.79	0.78	0.20	-0.15	-0.01
% block group vacant	0.04	0.03	0.03	0.00	0.80	0.04	0.04	0.12	0.02
block group pop density	0.40	0.28	0.46	0.01	2.00	0.40	0.28	0.25	-0.07
tax rate	27.09	11.25	25.90	-0.61	-2.42	27.70	9.73	-0.11	

NOTE.— This table reports summary statistics for the key variables included in the analysis for Detroit, MI. Cols. 1, 2, 3, 6 and 7 are simply the means and standard deviations for the 3 different samples of data. The boundary zone sample includes all houses located within 0.20 miles of the boundary of another school attendance zone. Col. 4 reports the difference in means between houses located on the "high" test score side of a boundary with the corresponding mean for the "low" test score homes on the opposite side of the boundary. Col. 5 provides a T-statistic on the difference in these means. Cols. 8 and 9 report correlations between the change in test scores and levels and changes in all other variables for the full sample of census block group data.

APPENDIX TABLE 4
SUMMARY STATISTICS FOR HOUSING, NEIGHBORHOODS, AND PUBLIC SCHOOLS IN LOS ANGELES, CA

Los Angeles Metro Area	Full Sample (micro data: N =146,788) mean (1)	standard deviation (2)	Sample: 0.20 Mile Boundary Zone (micro data: N = 12,287) mean (3)	difference in means: high score side -low score side (4)	T-statistic on difference in means (5)	Full Sample (Census block group data: N = 6,975) mean (6)	standard deviation (7)	correlation: Δscore & variable in 2003 (8)	correlation: Δscore & Δvariable (9)
Sale price									-0.04
2003 price	460,747	353,758	509,207	38,511	4.03	486,539	350,354	-0.15	
2007 price	486,752	463,063	563,566	55,621	3.72	551,685	458,579		
Average math/reading test result									
2003 score	39.75	13.75	41.86	13.92	49.94	39.11	14.14	-0.46	
2007 score	48.81	12.92	51.14	12.81	50.50	48.39	13.22		
Housing characteristics:									
square feet (100's)	17.06	7.67	17.11	0.84	5.81	16.16	5.63	-0.19	0.02
bathrooms	2.13	0.86	2.16	0.13	7.89	1.97	0.61	-0.20	0.02
age	43.06	22.98	44.87	-3.49	-8.98	53.11	19.18	0.21	0.00
lot acres	0.25	0.38	0.20	0.00	0.24	0.22	0.28	-0.13	0.01
bedrooms	3.18	0.87	3.21	0.07	4.66	3.07	0.54	-0.14	0.00
Neighborhood characteristics:									
% block group nonwhite	0.26	0.18	0.31	0.01	3.68	0.28	0.18	0.14	-0.25
% block group under 18	0.25	0.06	0.25	0.00	4.27	0.25	0.06	0.09	0.09
% block group owner occupied	0.68	0.21	0.70	0.01	3.99	0.60	0.25	-0.09	-0.01
% block group vacant	0.06	0.10	0.05	0.00	2.47	0.05	0.07	-0.06	0.00
block group pop density	0.72	0.60	0.82	-0.03	-2.74	0.96	0.75	0.17	0.05
tax rate	84.33	137.66	82.44	1.11	5.61	83.00	16.05	-0.04	

NOTE.— This table reports summary statistics for the key variables included in the analysis for Los Angeles, CA. Cols. 1, 2, 3, 6 and 7 are simply the means and standard deviations for the 3 different samples of data. The boundary zone sample includes all houses located within 0.20 miles of the boundary of another school attendance zone. Col. 4 reports the difference in means between houses located on the "high" test score side of a boundary with the corresponding mean for the "low" test score homes on the opposite side of the boundary. Col. 5 provides a T-statistic on the difference in these means. Cols. 8 and 9 report correlations between the change in test scores and levels and changes in all other variables for the full sample of census block group data.

Assessing Tradeoffs in Land Use Service Flows Within Subdivisions at Multiple Spatial Scales

Joshua K. Abbott
Arizona State University
School of Sustainability
PO Box 875502, Tempe, AZ 85287 USA
Email: Joshua.K.Abbott@asu.edu
Phone: 1-480-965-5528

H. Allen Klaiber
The Pennsylvania State University
Department of Agricultural Economics and Rural Sociology
208A Armsby Building
University Park, PA 16802 USA
Email address: aklaiber@psu.edu
Phone: 1-814-865-0469

Assessing Tradeoffs in Land Use Service Flows Within Subdivisions at Multiple Spatial Scales

Abstract

Evaluating the importance of different forms of open space to households requires an evaluation of the service flows provided by each type of open space. For many non-market goods, these flows occur over multiple spatial scales and require analysis that simultaneously accounts for capitalization at each scale. To meet this often overlooked need, we apply a newly developed extension to the Hausman-Taylor model that treats multiple housing transactions occurring in a spatial location as a panel. This methodology allows us to account for omitted variables within a subdivision while instrumenting for variables identified through differences between subdivisions. We measure capitalization of open space at three distinct spatial scales: adjacency, walkability, and subdivision wide. We find that the interactions between subdivision open space and private open space in the form of lot size change from complementarity at small scales to substitutability at large scales. These results confirm much of the intuition developed by ecologists and public planners on the likely service flows associated with open space and show how our approach to accounting for multiple spatial scales of capitalization in the evaluation of non-market goods could be beneficial to other areas of applied research.

Keywords: Open Space; Capitalization; Spatial Scale; Endogeneity; Hedonic; Hausman-Taylor

Assessing Tradeoffs in Land Use Service Flows Within Subdivisions at Multiple Spatial Scales

1. Introduction

The increased decentralization and fragmentation of urban development (i.e. sprawl) creates significant challenges for developers and planners as well as substantial social costs to society (Glaeser and Kahn 2004). City planners, with finite land supply and limited funds for the extension of infrastructure to new development, face incentives to increase the density of development within their communities. Similarly, residential developers must allocate land between housing and undeveloped open space to maximize their returns on investment – thus providing developers with some incentive to increase the density of development. Within a given single-family residential development this density can be accomplished in one of two ways: reduce the size of private lots or the extent of common open space. Both of these reductions come at a sacrifice, however, as both open space and private lots can provide a variety of services to subdivision residents such as recreational opportunities, aesthetic enjoyment, flood mitigation, and privacy and noise buffering. The question of the desired mix of these uses (whether from the perspective of the developer or the planner) therefore hinges upon the value accorded to the mix and spatial allocation of these uses by households, or, more precisely, the tradeoffs between the services associated with this mix and allocation.

A key challenge in assessing the tradeoffs between private lot size and open space is that each is likely to provide suites of ecosystem services that, while similar in some respects, are also distinct. For instance, while both private lots and public open space provide recreational opportunities, the nature of the services provided may differ substantially due to the size, configuration or landscaping of private lots relative to open space areas. Along with these qualitative differences in service flows (and partly because of them), the ecosystem services literature suggests that different service flows from a single amenity are likely to vary substantially in their spatial extent (Daily 1997; Bolund and Hunhammar 1999; Hein, van Koppen et al. 2006). Furthermore, these multi-scale flows are likely heavily shaped by the completeness of property rights to private lots versus subdivision open space and the extent to which the associated services take on some of the characteristics of public goods. This interaction of amenities with complex, institutionally mediated service flows creates the possibility that the contribution of changes in land use and their interaction will vary depending on the spatial scale at which their capitalization is assessed. These complexities create both challenges and opportunities for revealed preference methods of ecosystem service valuation. Challenges arise due to the possibility that failure to properly incorporate all the appropriate scales of capitalized service flows into model specifications may lead to under or over valuation of the amenity. However, this realization also creates the opportunity to utilize a priori knowledge to focus more closely on the capitalization of an amenity's associated service flows and their tradeoffs (interactions) with other service flows.

To explore these challenges, we employ a hedonic regression model (Rosen 1974) to examine the capitalization of subdivision open space and its interactions with potentially substitutable or complementary land uses within (i.e. private lots) and outside (i.e. city or regional parks) the subdivision to single family residential property values in the Phoenix metropolitan area.[1] We explicitly consider the latent services that different land uses may provide to residents and utilize a priori information on the likely scales of these services to create model specifications attuned to the potential zones of capitalization for each land use. Specifically, we consider that subdivision open space yields services that may capitalize at three scales: first, at the scale of adjacency to the park; second, to progressively less "walkable" buffers; and, third, to the neighborhood (subdivision) as a whole so as to capture the overall provision of surrounding open space. Similarly, we consider that one's own lot size provides services that are exclusive to the homeowner and therefore capitalize to individual homes while the overall density of private space within the neighborhood may yield non-excludable benefits through its impact on density and its correlates. Through the selective use of interactions we are able to create a flexible model where the tradeoffs in capitalization from increases in one form of land use or another are allowed to vary across spatial zones – capturing the idea that land uses may substitute differently for one another depending on the service flows under consideration.

In estimating our model we deal with the potential for omitted neighborhood variable bias (Palmquist 1992; Irwin and Bockstael 2001) by utilizing a unique feature of our data that lets us identify the subdivision for each property. By treating our data as a panel of repeated sales within subdivisions we are able to utilize a recent extension of the Hausman-Taylor instrumental variables (IV) estimator (Hausman and Taylor 1981) to yield consistent estimates of open space capitalization at all scales (Abbott and Klaiber 2009). Our first finding is that both subdivision open space and lot size capitalize at a range of scales with subdivision open space affording considerable capitalization to adjacent properties. The effects of increased open space provision within “walkable” buffers is, by contrast, relatively small and is dwarfed in magnitude by the subdivision-wide capitalization of open space, suggesting open space provision offers valuable services to residents beyond recreational access. Secondly, we find that lot size, aside from offering benefits to the associated homeowner (as has been thoroughly demonstrated in the literature), also offers valuable spillovers at the scale of the subdivision. Finally, we find that the interactions between lot size and subdivision open space at different spatial scales shows that the tradeoffs between the two land uses vary from complementarity to substitutability depending on the scale of capitalization – suggesting that these differences occur due to the nature of the services capitalized for each land use at that particular scale. This suggests that a modeling and estimation approach that is attuned to multiple scales of capitalization can be enlightening compared to commonly used approaches and that failure to adopt such an approach may be problematic for both the consistency and interpretation of the estimated marginal effects from hedonic models.

2. The Literature

The literature on the valuation of various forms of “open space” using hedonic price models is extensive and varied in its focus.[2] Authors have examined a wide range of open space amenities such as wetlands (Mahan, Polasky et al. 2000), golf courses (Do and Grudnitski 1995), forested lands (Tyrvainen and Miettinen 2000; Thorsnes 2002), and greenbelts (Correll, Lillydahl et al. 1978; Lee and Linneman 1998). Some authors have focused upon temporal aspects of open space in terms of its protection from eventual development (Irwin and Bockstael 2001; Geoghegan 2002; Smith, Poulos et al. 2002; Klaiber and Phaneuf 2009). Others have focused upon specific qualitative aspects of parks (i.e. natural areas vs. urban parks) (Lutzenhiser and Netusil 2001). Finally, there has been an increasing interest in investigating spatial heterogeneity in the valuation of open space amenities through parametric (Geoghegan, Wainger et al. 1997; Anderson and West 2006) and locally weighted regression techniques (Cho, Poudyal et al. 2008; Cho, Clark et al. 2009).

There are a very small number of papers specifically focusing on the question of the valuation of open space within subdivisions and its interactions with private lots (Peiser and Schwann 1993; Kopits, McConnell et al. 2007; Towe 2009). Peiser and Schwann (1993) study transactions in a Dallas subdivision and find little evidence of a price premium for greenbelt alleyways behind houses. Towe (2009) examines Maryland transactions at the rural-urban fringe with a focus on determining whether the value of open space in "clustered" subdivisions varies based upon whether it is preserved for agricultural use or incorporated within the subdivision. While the specification controls for lot size and amount of subdivision open space, it does not include the interaction between the two, and he ultimately finds no significant capitalization effect for the quantity of open space. The closest in spirit to our study is that by Kopits, McConnell and Walls (2007). They utilize data from the urban-rural fringe of Washington, D.C. and utilize a hedonic model with block group fixed effects in which the acreage of open space in the subdivision is interacted with the lot size of the house. They find that open space acreage is a weak substitute for additional lot size. Similarly, they consider whether the capitalization of open space adjacency to private lots varies in the size of the lot.

While their work does shed light on the nature of tradeoffs between private lots and open space, there are a number of differences that distinguish our work. First, our data set is substantially different in that it reflects an urban/suburban environment with relatively dense development, small lots and small and scattered open space compared to the much more "sprawled" development in their study.[3] Second, while they do consider two scales of capitalization for open space (adjacency and at the subdivision scale), we add intermediate scales to our model that consider the quantity of open space available to a property within particular distance buffers – therefore accounting for differences of local provision. Third, we consider the possibility that private lots may have non-excludable effects on property values in a subdivision (perhaps due to an aversion to density and its correlates) and examine whether the magnitude of this effect depends on the quantity of open space in and immediately surrounding the subdivision. Fourth, we control for the presence of various forms of public (non-subdivision) open space in our estimates (including the possibility of interactions). Finally, our use of the Hausman-Taylor estimator with subdivision level random effects allows us to demonstrate how this seldom-used tool can parse between the within and between-neighborhood variation in covariates to consistently estimate the marginal effects of open space and lot size at all spatial scales in spite of the potential for omitted variables at the subdivision level (Abbott and Klaiber 2009).

3. Data

The Phoenix metro area provides an ideal laboratory to study subdivision-provided open space and its interactions with other housing features. The extreme heat in summer, monsoon rains in fall, and poor soil percolation all create the incentive (often formalized through zoning) for developers to leave undeveloped land in subdivisions for drainage and heat dissipation. In addition, the high density development and small lot sizes which distinguish Phoenix from many other urban areas make subdivision-provided open space a potential amenity to households seeking greater privacy and separation from neighbors. To explore these issues, we have assembled a detailed database of nearly 620,000 single family residential transactions occurring over the years 2000 through 2004 obtained from the Maricopa County assessor and merged with purchased sales data from Dataquick. Unique to our data, the Maricopa County assessor maintains a detailed inventory of over 20,000 subdivisions located throughout the Phoenix MSA and indicates the subdivision membership of each parcel. An important feature of the Phoenix housing market and many other "Sunbelt" markets is that many if not most of the homes within subdivisions (particularly those built in more recent years) are built by a single builder. This allows us to characterize observable and unobservable attributes of the housing stock and neighborhood amenities at an economically meaningful scale. The structuring of our data by subdivisions thus serves as the basis for our analysis of multiple spatial scales of capitalization for subdivision open space and the interactions of this form of open space with private open space in the form of lot size. We now briefly describe the key elements of our data in additional detail.

3.1. Transactions

To form our dataset of single family residential transactions, we combine data purchased from a private data vendor, Dataquick, with data from the Maricopa County assessor and restrict our attention to 2000 to 2004 sales containing a complete list of housing characteristics and a subdivision identifier. The 619,219 transactions in our filtered estimation sample are plotted in figure 1. As is evident from the figure, these data are widely distributed over virtually the entire metropolitan area . The key housing characteristics are square footage, lot size, number of bathrooms, number of stories, year built, and indicator variables for presence of a pool and garage. In addition to these structural characteristics, we also observe the sales price and sale date of each house as well as a unique identifier which allows us to link these data to GIS assessor parcel maps. Summary statistics for these data are shown in table 1.

To supplement our transactions data, we obtained information on the location of canals, railways, and schools as well as spatial data for Census 2000 block groups. We also calculate the distance from each house to the central business district in Phoenix. Combining these additional spatially defined data with our residential transactions data we are able to capture socio-demographic characteristics from the census as well as potential amenities and disamenitites from other land use types.

The most important spatial dimension of our data is an explicit link for each parcel to its subdivision. These subdivisions range in size from a single house to over 1,100 houses with an average size of 70 houses. Figure 2 shows the location of residential parcels with subdivision open space indicated by the darkest shading. After cleaning the data and limiting it to only subdivisions containing a minimum of 30 houses we are left with a sample of 7,598 subdivisions. These subdivisions will form the basis for the spatial panel in our Hausman-Taylor specification.

3.2. Open Space

Data on open space, including subdivision open space (SOS), was obtained through Arizona Parks and Recreation and the Maricopa County assessor. In the Phoenix area, SOS is typically landscaped and well maintained (often by the local homeowner's association) but has minimal facilities and typically no public parking. In addition, SOS is often small compared to public parks and is often integrated within the housing development; however, access is rarely effectively restricted, making the vast majority of this form of open space non-excludable (at least in principle). Figure 3 provides a graphical depiction of the four types of open space included in our study: subdivision open space, local parks, regional parks, and city parks. Due to the similarity in size and characteristics of "destination-oriented" parks, we combine city and regional parks into a single category of "large park". Several cleaning steps were required to isolate subdivision open space parcels, while no cleaning steps were necessary for the other forms of open space. Summary statistics for each type of open space are shown in table 1.

We identified subdivision open space using land use codes in the assessor parcel data. Unfortunately, these codes not only identified true subdivision open space, but also tended to delineate long slivers of common property only a foot or two wide or highly irregular shapes adjacent to local streets and sidewalks which are likely public easements. To identify and remove these aberrant parcels from consideration, we calculated the area and perimeter of each individual subdivision open space feature and developed cutoffs for minimum area and area/perimeter ratio. We fine tuned these measures by examining aerial photography of the identified open space parcels to minimize exclusion of "true" open space while eliminating the majority of the accidental characterization of areas as subdivision open space. Ultimately we settled on cutoff values of minimum area of 0.25 acres and an area/perimeter ratio of 40 or greater (in feet).[4] The resulting sample contains 3,956 individual subdivision open space parcels at an average size of 5.2 acres.

Using GIS software, we created an adjacency indicator for houses located next to each type of open space. For the "large park" category, we also calculated the distance to the nearest park from each house to capture the destination aspect of larger parks. For both local parks and subdivision open space, we calculated the total acreage of parkland within concentric ring buffers around each residential parcel. In particular, we used a 2,000 foot buffer to calculate total local park area and used 3 non-overlapping rings of 1,000, 2,000, and 3,000 feet for calculations of subdivision open space area. To form a subdivision specific measure of open space, we calculated the average subdivision open space area within 2,000 feet of houses in each subdivision.[5]

4. Model Specification and Estimation

Households (indexed by *k*) have heterogeneous preferences for the bundle of attributes associated with the purchase of a house. These can be characterized by the utility function:

$$\mathrm{U}^{k} = \mathrm{U}(\mathbf{X_i}, \mathbf{N_j}, \mathbf{OS_{ij}^{PUBLARGE}}, \mathbf{OS_{ij}^{PUBSMALL}}, \mathbf{OS_{ij}^{SUB}}, \mathbf{OS_{ij}^{LOT}}, \mathrm{g}, \boldsymbol{\alpha}^{k}), \tag{1}$$

where $\boldsymbol{X_i}$ is a vector of house specific (represented by subscript *i*) characteristics like square footage or number of bathrooms. $\boldsymbol{N_j}$ are "neighborhood" characteristics (indexed by *j*) that will be perceived as variable at the scale of a subdivision or a larger spatial unit but not at the resolution of individual houses. These can include aspects such as the school district, school attendance zone, air quality, aspects associated with a particular builder or developer (e.g. quality of construction or high-end finishes), or measurements of the provision of public goods for which local proximity are not important to an individual's preferences. The numeraire good is *g* and heterogeneity across households in preferences for home/neighborhood characteristics is captured by the parameter vector $\boldsymbol{\alpha}^{k}$.

$\mathbf{OS_{ij}^{PUBLARGE}}$, $\mathbf{OS_{ij}^{PUBSMALL}}$, $\mathbf{OS_{ij}^{SUB}}$, and $\mathbf{OS_{ij}^{LOT}}$ are vectors of varying dimension that contain measures of the provision and/or proximity of large public open space (i.e. "destination" parks), small public open space (local parks), privately provided (but collectively owned and managed) subdivision open space, and "private open space" (i.e. private lots) respectively.[6] The use of vectors for each qualitatively distinct form of open space allows different types of open space to impart utility to households in potentially distinct ways at different scales. Benefits of privacy or noise reduction from the presence of a greenbelt may accrue exclusively to contiguous properties while the recreational services provided by the same open space to houses located just across the street may be essentially identical to the adjacent property. Note as well that the vectors are subscripted by both house (*i*) and neighborhood (*j*) in order to allow for the possibility that consumers may perceive the flow of some classes of services from some forms of open space as essentially uniform within a neighborhood. This could be the case for many "destination" parks where proximity by car is essentially invariant at the neighborhood scale or may also apply to broader "landscape" effects of open space in its effect on neighborhood micro-climate or homeowners' perceptions of density and other aesthetic aspects of their subdivision. Finally, we have included private lots as a form of open space in our model in order to capture their potentially non-excludable services. For instance, additional private lot acreage to homes in a subdivision may generate some "public" benefits to the subdivision in a manner similar to explicitly public subdivision open space.

Given sufficient variability and continuity in the supply of housing characteristics and adequate preference for/against all the characteristics in the population, housing rents will be bid up or down according to these characteristics and will therefore capitalize into the value of the property. Given appropriate functional form and separability restrictions on preferences we can specify the following partially-linear, semi-log hedonic price function:

$$\ln P_i = \alpha + \boldsymbol{\beta}'\mathbf{X_i} + \boldsymbol{\gamma}'\mathbf{N_j} + f(\mathbf{OS_{ij}^{PUBLARGE}}, \mathbf{OS_{ij}^{PUBSMALL}}, \mathbf{OS_{ij}^{SUB}}, \mathbf{OS_{ij}^{LOT}}) + \varepsilon_i \,. \tag{2}$$

We choose the semi-log functional form given the interpretability of its marginal effects and the work of Cropper, Deck and McConnell (1988) suggesting that such simple functional forms tend to outperform more complex specifications in recovering marginal welfare effects when the hedonic price function is misspecified. Note that we have avoided choosing an explicit functional form for the effects of the open space variables at this stage in order to allow for potentially complex interplay in the hedonic price function between the different forms of open space across different spatial scales. The motivation for this flexibility is to allow for the possibility of interaction across individual open space measures to the extent that the value of the flow of services from a spatial amenity are enhanced by or substituted for by service flows from another amenity.

To implement this specification, we specify *f( )* as a linear function of measures of open space proximity and provision along with selected interactions. Table 1 lists each of these variables and their interactions. The key variables in our specification are those involving private lot size and subdivision open space. We include private lot variables at two spatial scales, the acreage of one's own lot (and acreage squared) and the average lot size within the subdivision. The rationale for lot size as a private good is well established empirically while mean lot size is included to capture non-excludable spillovers from the provision of private space within the neighborhood. We include variables for proximity and provision of subdivision open space at three primary scales. First, we include an indicator variable for whether a property is adjacent to subdivision open space in order to capture benefits that are excludable to properties connected to open space (i.e. privacy, noise absorption, view, etc.). Second, we utilize the aforementioned buffering strategy to consider how much open space is available to individual houses at 1000, 2000, and 3000 feet buffers. This buffering approach allows us to jointly examine both quantity and proximity of open space, and the buffers are designed to span the range of distances commonly discussed in the planning literature as "walkable" (Boone, Buckley et al. 2009). These variables can be viewed as capturing the value of an additional acre at a particular range for recreational and other highly proximity-dependent use. Third, we include the calculation of the average amount of subdivision open space within 2000 feet of homes within the subdivision in order to capture the contribution of public space within and around the subdivision to neighborhood character and other public/club goods.

In addition to these variables, we also consider a select number of interactions based upon hypotheses of how the services provided by the two land uses (and thus their spatial proxies) may enhance or substitute for one another. We include an interaction of subdivision open space adjacency with private lot size to test whether the private benefits from one's yard are enhanced by adjacency. We also include an interaction of adjacency with the size of the open space to see if the magnitude of "adjacency services" is influenced by the size of the space. We interact the provision of subdivision open space acreage within a 2000 foot buffer with the size of the house's private lot in order to examine whether the services from open space and own lots are substitutable, complementary or separable at this scale.[7] Finally, to consider the substitutability/complementarity of the two land uses at the subdivision scale, we interact the subdivision mean levels of lot size and subdivision open space with one another. While previous models have considered some of these scales and interactions (particularly those involving adjacency, see Kopits, et al. (2007)), ours is the first study to our knowledge to so extensively investigate multiple scales of capitalization and their interactions in a hedonic specification; it is also the first to consider that private lots and open space may provide service flows that capitalize at the neighborhood or subdivision level as well as to individual properties.

Estimation presents an array of challenges shared by most hedonic price models.[8] First, there is the possibility of endogeneity from the presence of unobserved factors that lower the value of land in residential development (i.e. high slopes) and therefore selectively increases the proportion of undeveloped land (open space) in areas of low residential value (Irwin and Bockstael 2001; Irwin 2002). This concern is likely moot for publically provided open space which is either predetermined with respect to developer decisions or allocated according to planning goals rather than market forces. It is likely to be a minor concern for subdivision open space as well given that much of this space is not undeveloped land but is actually graded and landscaped in a costly manner. Furthermore, zoning requirements for adequate storm water retention capacity play a strong role in the provision of this form of open space.

An issue of greater concern for our estimation is omitted variables bias. Of particular concern is the presence of omitted neighborhood variables that are possibly correlated with our open space metrics. For instance, developers working in areas with unusually high quality local parks with good pedestrian access may feel less compelled to provide subdivision open space. Areas with good schools may attract families with children and lead to provision of larger amounts of open space. Alternatively, developers that utilize high quality finishes in their construction may also provide greater (or more highly improved) open space. The end result of such unobserved neighborhood variation is a bias of indeterminate sign and magnitude. This problem is well recognized within the literature (Palmquist 1992) and has been dealt with primarily through the use of extensive proxy variables and spatial fixed effects to approximate neighborhood indicators and thus absorb any time-constant omitted variables so that the marginal effects in the regression are identified on the basis of within-neighborhood variation (Anderson and West 2006; Kopits, McConnell et al. 2007; Cho, Poudyal et al. 2008; Cho, Clark et al. 2009).

The question of the appropriate scale for these spatial fixed effects is a seldom discussed but important issue. In some cases, spatial fixed effects are included over well-defined areas of neighborhood heterogeneity such as cities or school districts and attendance zones (Black 1999; Cho, Clark et al. 2009). However, such coarse indicators leave the potential for substantial inter-neighborhood variation in omitted characteristics, encouraging the use of finer fixed effects (typically block groups) where data make them available (Anderson and West 2006). Unfortunately, the elimination of bias that comes with the use of increasingly fine fixed effects comes at the cost of non-identification or under-identification of the effects of variables that vary at or above the level of the fixed effect (non-identification) or exhibit little heterogeneity below the scale of the fixed effect (under-identification). If aspects of an amenity capitalize approximately evenly across space, then spatial fixed effects approaches run the risk of identifying a consistent but incomplete marginal effect of the amenity (Abbott and Klaiber 2009).[9] This suggests that the approach to omitted variables bias must be cognizant of the scale of both omitted variation and the scales of capitalization in the model.

Our first answer to this dilemma is to utilize our ability to identify the subdivision of each housing parcel to treat our data as repeated cross sections of sales of multiple houses within subdivisions over time. We can then utilize within-subdivision variation to consistently identify the effects of the open space variables that vary within subdivisions. This scale of fixed effect is ideal in that it matches naturally with an intuitively and economically sensible definition of a minimal neighborhood for our study area – lowering the chance of significant unobservable variation relative to somewhat more arbitrary and large-scale definitions of neighborhoods (i.e. block groups).[10] Nevertheless, the within-subdivision estimator comes at the cost of our ability to identify the subdivision level open space and lot size variables, yet common approaches like OLS or random effects estimates risk biased estimates for the open space variables at all scales.

Our answer to this dilemma is to adapt the Hausman-Taylor estimator (Hausman and Taylor 1981) to our spatial, repeated cross-section context. The formal development of this approach is presented elsewhere (Abbott and Klaiber 2009) so we heuristically develop it here. The Hausman-Taylor (HT) estimator is a special case of an instrumental variables random effects estimator (Cameron and Trivedi 2005). The within subdivision varying (henceforth "within-varying") variables are instrumented by their deviations from within subdivision means (i.e. they are estimated using the "within" or fixed effects estimator). Variables that vary at or beyond the scale of the spatial effects (henceforth "between-varying") are characterized as either exogenous or (potentially) endogenous. Exogenous between-varying variables are instrumented by themselves while the endogenous between-varying variables are instrumented using the subdivision means of any within-varying variables *in the model* that are presumed by the analyst to be exogenous.[11] There are several notable features of this estimator. First, the HT estimator achieves identification without external instruments by making dual use of a subset of the within-varying regressors by essentially treating their within and between subdivision variation as separate variables. This apparent "something for nothing" comes at the cost of assuming that a sufficient number of within-varying variables are uncorrelated with the unobserved neighborhood variation that are also sufficiently correlated with the endogenous between-varying variables to avoid the bias and inefficiency associated with weak instruments (Bound, Jaeger et al. 1995). While exogeneity may be difficult to establish a priori, it is possible to test for it empirically by employing a Wald test on an augmented regression – a robust analog of the Hausman test (Wooldridge 2002; Abbott and Klaiber 2009). This test works by effectively testing whether the random effects estimates for the coefficients on the within-varying instruments (inconsistent under violations of the null hypothesis that the variables are uncorrelated with neighborhood-scale omitted variables) are significantly different from the fixed effects estimates (which are consistent regardless). The HT estimator therefore provides an attractive alternative to common estimation techniques in that it makes explicit use of spatial variation within and between neighborhoods to provide consistent identification at all spatial scales while allowing for the falsification of its identifying assumptions.

5. Results

We are concerned that the subdivision scale measures of private lot acreage and SOS provision are correlated with unobserved neighborhood characteristics. In the HT framework, we must find two or more exogenous within-varying regressors to serve as instruments. Using the aforementioned modified Hausman test, we tested the null hypothesis of exogeneity for a number of candidate instruments and ultimately settled on the use of adjacency to subdivision open space and its two interactions. Correlation between these instruments and the endogenous variables are shown in table 2 along with p-value results from the modified Hausman test.[12] We fail to reject the null hypothesis of exogeneity of our instruments both individually and jointly and the correlations between our instruments and the endogenous variables are collectively fairly high – suggesting that our instruments have a strong empirical justification.[13]

Table 3 presents our estimates for the Hausman-Taylor model (where the standard errors are calculated using a nonparametric bootstrap to allow a less restrictive covariance structure than the traditional HT estimator). In order to establish the reasonableness of our estimates relative to the literature, we begin by discussing the non-open space characteristics in our specification before turning attention to the central results on public and private open space. Examining the structural housing characteristics identified using within subdivision variation, it is clear that our estimates are congruent with the literature. We find positive coefficients for square footage, lot size, bathrooms, year built (newer houses being preferred), presence of a garage, and presence of a pool. The negative coefficient on number of stories indicates a preference for single story dwellings (holding square footage and lot size constant), possibly reflecting the preferences of households to limit summer heat buildup in upper stories. In addition to the linear terms, we also estimate squared terms for square footage, lot size, and year built and find intuitive negative coefficients for each. The remaining within subdivision control attributes consist of adjacency measures to various forms of landscape features including schools, railways, and canals. Of these, only adjacency to railways is significant and suggests that railway adjacency reduces the value of a house by approximately 2.5 percent.

Focusing on characteristics which vary across subdivisions rather than over individual houses we find that census measures of block group minority racial composition show no significant capitalization effects relative to the Caucasian base group.[14] We find intuitive results that housing prices in areas with large numbers of children are lower than the omitted class of retired households. In addition, we find that subdivisions with households containing more working aged adults are associated with higher housing prices. We find a positive and concave gradient in distance to downtown Phoenix, perhaps reflecting the overwhelming of any positive effect of proximity to employment or urban amenities (perhaps limited in this highly polycentric city) by the negative correlates of urban life. We find no significant impact of having a school located within walkable distance of a subdivision, perhaps reflecting the offsetting of easy access to the school and its amenities with the associated congestion and noise from school-related traffic. Lastly, we seem to find that households prefer to distance themselves from large parks. While unexpected, a glance at figure 3 suggests that this effect may arise from the remote location of large parks (and in some cases, proximity of these parks to blighted neighborhoods) rather than an actual aversion to being near large parks. This discussion sounds a more general cautionary note about the interpretation of the subdivision-varying variables in our model. They are included as control variables to limit the scope of unobserved heterogeneity in our model. As such they are not instrumented for and should be interpreted with caution.[15]

Returning to our focus on open space, we first examine the most immediate spatial scale of adjacency. Adjacency effects are identified in the HT estimator using within subdivision variation so that potentially confounding unobserved quality differences across subdivisions are eliminated. We find positive and significant coefficients associated with adjacency to both local and large public parks but with substantially larger value accruing to properties adjacent to large public parks. This suggests that these larger public spaces provided greater services such as noise buffering, aesthetic views, recreational possibilities, or a feeling of “naturalness” relative to smaller, more dispersed local parks. Continuing this theme, we find that an interaction between subdivision open space adjacency and the size of the open space is positive so that the capitalization benefits of SOS adjacency are augmented by the size of the space itself. This is sensible since the flows of spatially attenuated services from open space adjacency (e.g. privacy and noise buffering) are likely enhanced by the size of the open space. We also find a positive percentage effect on the capitalization of adjacency from the size of the adjacent private lot (an addition of 0.1 acres to an adjacent lot increases the benefits from adjacency by 0.4% of the home's value). This is congruent with a situation where the services tied to adjacency are enhanced to the degree that one's own "private open space" facilitates their consumption through private outdoor activities – as is likely the case with a larger backyard. Together these findings suggest that both private and public open space within subdivisions are complementary in their service provision at these small spatial scales.

Our second spatial scale examines the capitalization of open space from close (non-adjacent) proximity to households. Not surprisingly, we find that capitalization of an additional acre of open space is substantially larger (approximately 50% larger) within the 1000 foot bound than when relocated at distances between 1000 to 2000 feet. We find no significant effect for SOS located between 2000 and 3000 feet from a house, suggesting benefits from household-level access to open space are quite localized in scale. Although statistically significant, the capitalization effect of open space proximity is rather small; an extra acre of SOS within 1000 feet adds a mere .05% to the value of the house (i.e. $100 for a $200,000 house).[16,17] Similarly, we found no significant effect for provision of local park acreage within a 2000 foot buffer. One possible (although untestable) explanation for both of these results may lie in the extreme desert heat that limits daytime recreational use of outdoor areas for a significant portion of the year. In such a setting, the typical homebuyer may place little premium on walkable access to open space. Finally, the interaction effect between SOS acreage and a house's private lot size is highly insignificant. This seems to indicate that the service flows emanating from SOS proximity and one's own private lot are not likely to be substitutable (as would be the case if the interaction was negative). Such separability may find its explanation in the size and character of private lots in the Phoenix metro. Many private lots are quite small, limiting their recreational use, and this use may be further curtailed in many areas by the utilization of low water use landscaping (xeriscaping) on private lots (Martin 2001). As SOS is typically far larger (and often more traditionally landscaped), the services provided by the two distinct land uses at these intermediate scales may be quite distinct.

The largest spatial scale we consider accounts for capitalization that occurs uniformly across subdivisions and is identified from differences between subdivisions. Both increases in the average amount of subdivision open space within (and given our buffering technique, around) the neighborhood and the average lot size in the subdivision result in increased capitalization. This indicates that some aspects of SOS capitalize broadly to homes within the subdivision, regardless of their differential proximity or adjacency to the space within the neighborhood. Such broad capitalization may be traceable to a range of "public" services from open space such as its contribution to feelings of reduced density, drainage, heat moderation and neighborhood character. The positive effect of mean subdivision lot size suggests that "private" lots yield some non-excludable services to households. Furthermore, the interaction of these two measures reveals that private and public space are substitutes at this large scale, with a significant, large and negative interaction coefficient. This finding suggests, at least for typical neighborhoods in our data, that some of the valued services provided by open space are exchangeable for those yielded by increased private space. This seems to lend further credence to the notion that the dominant value of subdivision open space (at the margin) at this scale derives from its public aspects rather than its use per se.

To provide quantitative context for these results, we report a series of conditional marginal willingness to pay (MWTP) calculations in table 4. MWTP for an extra acre of open space or private lot is reported for households located both adjacent and non-adjacent (but within 1000 feet) to subdivision open space and is also partitioned into the small scale (adjacency and proximity) and large scale (mean subdivision provision) valuations of the amenity to the household which must be added to get the complete MWTP.[18] As expected, we find a larger small scale MWTP for SOS acreage for adjacent properties due to the positive interaction of adjacency with SOS size.[19] Similarly, the marginal value of an extra private acre (allocated to a single lot) is substantially enhanced by SOS adjacency. The small scale MWTP *at the household level* for private acreage is three orders of magnitude larger than subdivision open space. This is reasonable given the very different services provided by the two forms of space, their differential excludability and their relative frequency of use. However, the MWTPs for these forms of space are not directly comparable for gross welfare calculations since additional open space will generate services to a number of households while the benefits of additional lot acreage at this small scale are exclusively private.[20]

Comparing the MWTP for SOS and lot size at the large (subdivision) scale, we find that the per household value of an additional acre of SOS is $1,138 compared to the MWTP of $2,918 for private space (where this subdivision-level change is translated into changes in space per household by dividing the acre over the mean number of 70 houses in a subdivision). Comparing the marginal household value for SOS between large scale and small scales reveals the importance of accounting for all scales of capitalization; the large scale capitalization comprises 70 to 90 percent of household MWTP depending on whether the house in question is adjacent to the new open space. By contrast, the large scale (public) effects of private open space, while substantial, are a small portion of household MWTP.

Until now we have only reported results for the Hausman-Taylor estimator. In order to demonstrate the importance of accounting for omitted variable bias both within and between neighborhoods and the utility of the HT estimator in this regard, we report results for the identical specification using the random (subdivision) effects (RE) estimator in table 5. While RE has the advantage of efficiency in the presence of unobservable subdivision-level heterogeneity, it is inconsistent if these variables are correlated with the variables of interest in the model. A glance at the RE and HT estimates reveals that they are quite close in most respects for within-varying variables. The RE estimator, being a “smart” weighted average of the within and between estimates, strongly favors the within estimates in our setting because of the long panels created by repeated sales within subdivisions; the bias in the RE estimates induced by the between estimator is therefore highly limited (Abbott and Klaiber 2009). The similarities end, however, when we examine between-varying characteristics. All three of the large scale open space estimates are attenuated by nearly the same proportions in the RE estimate relative to the HT estimator, so that subdivision-scale effects of increases in both forms of open space are understated by the RE estimator (as is the overall MWTP). This highlights the importance of accounting for the possibility of omitted variables bias at *all* relevant spatial scales when conducting revealed preference studies of spatial amenities.[21]

6. Conclusion

Ecologists have long emphasized the multiplicity of ecosystem services from single spatial amenities and their heterogeneous spatial transmission via ecosystem structures and processes. Economic theory – through its focus on private vs. public goods and how people sort themselves in housing markets according to perceptions of these goods – also has much to say about the spatial characterization of capitalization gradients from ecosystem services. Nonetheless, explicit characterizations of multiple scales of capitalization from a single, bundled amenity are extremely rare in hedonic applications, and attempts to ground these scales in the underlying service flows of the amenity are essentially unheard of.[22] Our paper addresses these issues by explicitly considering the scales of capitalization of private and public open space within subdivisions. Furthermore, we confront the effects of omitted neighborhood variables on our estimates at all scales by successfully adapting the Hausman-Taylor model to a new context.

Our finding that both private and public open space exhibit substantial capitalization at the scale of the neighborhood serves as a cautionary note to the increasingly common use of spatial fixed effects in hedonic modeling. Indiscriminate use of such estimators may be problematic when an amenity capitalizes in a broad fashion or (as is likely) the ultimate scale of capitalization is uncertain a priori. At the very least, analysts must be careful to balance the reduction of bias that comes with a finer neighborhood fixed effect with the risk of a (potentially more severe) bias from the absorption of the capitalized service flows into the fixed effects. We believe tools like the Hausman-Taylor estimator have substantial untapped potential to help analysts more wisely negotiate this tradeoff.

Our finding that the interaction between private and public open space within subdivisions ranges from complementarity at small spatial scales to substitutability at neighborhood-wide spatial scales highlights an important general finding when considering interactions between multiple land uses. It is the *services* from these land uses that substitute or complement one another, and the differential flow of these services across space implies that multiple land uses (the amenities themselves) may interact in their capitalization quite differently depending upon the scale of space under consideration. Failure to recognize this complex reality through specifications that mingle scales of capitalization arbitrarily or that favor one scale to the exclusion of others may yield rather arbitrary results with regard to the *gross* substitutability or complementarity of land uses.

Finally, our work helps to provide clarity on the capitalized value of competing land uses in a rapidly expanding urban environment. Aside from the usefulness of our estimates for determining the value of ecosystem services from residential land use, it also helps to clarify the micro-level incentives faced by developers in their land use allocation decisions within subdivisions and thereby can hopefully contribute to a deeper understanding of the micro-structure of urban and suburban development in Phoenix and beyond.

References

Abbott, J. K. and H. A. Klaiber (2009). An Embarrassment of Riches: Confronting Omitted Variable Bias and Multi-Scale Capitalization in Hedonic Price Models, Arizona State University.

Anderson, S. T. and S. E. West (2006). "Open space, residential property values, and spatial context." Regional Science and Urban Economics **36**(6): 773-789.

Black, S. E. (1999). "Do better schools matter? Parental valuation of elementary education." Quarterly Journal of Economics **114**(2): 577-599.

Bolund, P. and S. Hunhammar (1999). "Ecosystem services in urban areas." Ecological Economics **29**(2): 293-301.

Boone, C. G., G. L. Buckley, et al. (2009). "Parks and People: An Environmental Justice Inquiry in Baltimore, Maryland." Annals of the Association of American Geographers **99**(4): 767-787.

Bound, J., D. A. Jaeger, et al. (1995). "Problems with Instrumental Variables Estimation When the Correlation between the Instruments and the Endogenous Explanatory Variable Is Weak." Journal of the American Statistical Association **90**(430): 443-450.

Cameron, A. C. and P. K. Trivedi (2005). Microeconometrics : Methods and Applications. New York, NY, Cambridge University Press.

Chay, K. Y. and M. Greenstone (2005). "Does air quality matter? Evidence from the housing market." Journal of Political Economy **113**(2): 376-424.

Cho, S. H., C. D. Clark, et al. (2009). "Spatial and Temporal Variation in the Housing Market Values of Lot Size and Open Space." Land Economics **85**(1): 51-73.

Cho, S. H., N. C. Poudyal, et al. (2008). "Spatial analysis of the amenity value of green open space." Ecological Economics **66**(2-3): 403-416.

Correll, M. R., J. H. Lillydahl, et al. (1978). "Effects of Greenbelts on Residential Property-Values - Some Findings on Political-Economy of Open Space." Land Economics **54**(2): 207-217.

Cropper, M. L., L. B. Deck, et al. (1988). "On the Choice of Functional Form for Hedonic Price Functions." Review of Economics and Statistics **70**(4): 668-675.

Daily, G. C. (1997). Nature's services : societal dependence on natural ecosystems. Washington, D.C., Island Press.

Do, A. Q. and G. Grudnitski (1995). "Golf-Courses and Residential House Prices - an Empirical-Examination." Journal of Real Estate Finance and Economics **10**(3): 261-270.

Ewing, R., R. Pendall, et al. (2003). Measuring Sprawl and its Impact. Washington, D.C., Smart Growth America.

Geoghegan, J. (2002). "The value of open spaces in residential land use." Land Use Policy **19**(1): 91-98.

Geoghegan, J., L. A. Wainger, et al. (1997). "Spatial landscape indices in a hedonic framework: an ecological economics analysis using GIS." Ecological Economics **23**(3): 251-264.

Glaeser, E. and M. Kahn (2004). Sprawl and Urban Growth. Handbook of Regional and Urban Economics. V. Henderson and J. Thisse. Amsterdam, North Holland. **4:** 2481-2527.

Hausman, J. A. and W. E. Taylor (1981). "Panel Data and Unobservable Individual Effects." Econometrica **49**(6): 1377-1398.

Hein, L., K. van Koppen, et al. (2006). "Spatial scales, stakeholders and the valuation of ecosystem services." Ecological Economics **57**(2): 209-228.

Irwin, E. G. (2002). "The effects of open space on residential property values." Land Economics **78**(4): 465-480.

Irwin, E. G. and N. E. Bockstael (2001). "The problem of identifying land use spillovers: Measuring the effects of open space on residential property values." American Journal of Agricultural Economics **83**(3): 698-704.

Kiel, K. A. and K. T. Mcclain (1995). "House Prices during Siting Decision Stages - the Case of an Incinerator from Rumor through Operation." Journal of Environmental Economics and Management **28**(2): 241-255.

Klaiber, H. A. and D. J. Phaneuf. "Do Sorting and Heterogeneity Matter for Open Space Policy Analysis? An Empirical Comparison of Hedonic and Sorting Models." American Journal of Agricultural Economics 91, no. 5(2009): 1312-1318.

Kopits, E., V. McConnell, et al. (2007). "The trade-off between private lots and public open space in subdivisions at the urban-rural fringe." American Journal of Agricultural Economics **89**(5): 1191-1197.

Lee, C. M. and P. Linneman (1998). "Dynamics of the greenbelt amenity effect on the land market - The case of Seoul's greenbelt." Real Estate Economics **26**(1): 107-129.

Leggett, C. G. and N. E. Bockstael (2000). "Evidence of the effects of water quality on residential land prices." Journal of Environmental Economics and Management **39**(2): 121-144.

Lutzenhiser, M. and N. R. Netusil (2001). "The effect of open spaces on a home's sale price." Contemporary Economic Policy **19**(3): 291-298.

Mahan, B. L., S. Polasky, et al. (2000). "Valuing urban wetlands: A property price approach." Land Economics **76**(1): 100-113.

Martin, C. (2001). "Landscape water use in Phoenix, Arizona." Desert Plants **17**: 26-31.

McConnell, V. D. and M. A. Walls (2005). The Value of Open Space: Evidence from Studies of Nonmarket Benefits. Washington, D.C. , Resources for the Future.

Palmquist, R. B. (1992). "Valuing Localized Externalities." Journal of Urban Economics **31**(1): 59-68.

Palmquist, R. B. (2005). Property Value Models. Handbook of Environmental Economics. K.-G. Maler and J. R. Vincent, Elseiver. **2**.

Palmquist, R. B., F. M. Roka, et al. (1997). "Hog operations, environmental effects, and residential property values." Land Economics **73**(1): 114-124.

Peiser, R. B. and G. M. Schwann (1993). "The Private Value of Public Open Space within Subdivisions." Journal of Architectural and Planning Research **10**(2): 91-104.

Pope, J. C. (2008). "Buyer information and the hedonic: The impact of a seller disclosure on the implicit price for airport noise." Journal of Urban Economics **63**(2): 498-516.

Rosen, S. (1974). "Hedonic Prices and Implicit Markets - Product Differentiation in Pure Competition." Journal of Political Economy **82**(1): 34-55.

Smith, V. K., C. Poulos, et al. (2002). "Treating open space as an urban amenity." Resource and Energy Economics **24**(1-2): 107-129.

Thorsnes, P. (2002). "The value of a suburban forest preserve: Estimates from sales of vacant residential building lots." Land Economics **78**(3): 426-441.

Towe, C. (2009). "A valuation of subdivision open space by type." American Journal of Agricultural Economics **91**(5): 1319-1325.

Tyrvainen, L. and A. Miettinen (2000). "Property prices and urban forest amenities." Journal of Environmental Economics and Management **39**(2): 205-223.

Wooldridge, J. M. (2002). Econometric Analysis of Cross Section and Panel Data. Cambridge, Mass., MIT Press.

Table 1. Summary Statistics for Variables Used in Estimation

Variable	Obs	Mean	Std. Dev.	Min	Max	Variable	Obs	Mean	Std. Dev.	Min	Max
Price (ln)	619219	12.05413	0.5082354	9.740969	15.90708	Local park area (<2000 ft.)	619219	3.44556	9.278496	0	148.9016
Price	619219	207679.9	307895.7	17000	8097604	Lot size -x- Local park area	619219	0.626514	1.959936	0	195.4796
Subd. open parcel size	3956	5.22381	10.04932	0.478937	318.4651	Lot size -x- Subd. Open area (<2000 ft.)	619219	1.819031	4.323193	0	453.8388
Subdivision parcel count	13783	69.95284	81.27748	1	1115	Subd. open area (<1000 ft.)	619219	2.471076	5.69854	0	152.2393
Square footage	619219	18.90922	6.951536	6	60	Subd. open area (<2000 ft.)	619219	6.757736	13.38545	0	272.4614
Square footage^2	619219	405.8823	336.8476	36	3600	Subd. open area (<3000 ft.)	619219	9.87486	18.56362	0	342.6336
Lot acres	619219	0.1928654	0.135717	0.05	13.93744	Mean subd. open area	619219	0.085098	0.22284	0	42.22037
Lot acres^2	619219	0.0556161	0.3748973	0.0025	194.2523	Mean lot size	619219	0.192872	0.124815	0.051291	6.731887
# stories	619219	1.199298	0.3996708	1	3	Adjacent SOS -x- Lot size	619219	0.011031	0.060206	0	13.93744
# bathrooms	619219	2.617087	0.8038109	0.5	6	Adjacent SOS -x- Subd. open parcel size	619219	0.46223	3.083294	0	124.8617
Year built	619219	68.87268	15.02397	0	84	Adjacent subd. open	619219	0.051831	0.221687	0	1
Year built^2	619219	4969.165	1787.792	0	7056	School	619219	0.534509	0.498808	0	1
Garage	619219	0.9663915	0.1802193	0	1	Large Park distance	619219	19.071	13.43569	0.115201	58.05552
Pool	619219	0.3505157	0.4771319	0	1	CBD distance	619219	15.79318	6.032507	0.5202	31.91129
Year=2001	619219	0.1814318	0.3853758	0	1	CBD distance^2	619219	285.8222	193.883	0.270618	1018.355
Year=2002	619219	0.1901379	0.3924102	0	1	Pct. Hispanic	619219	0.187009	0.19398	0	0.977642
Year=2003	619219	0.2078796	0.4057905	0	1	Pct. Black	619219	0.02721	0.041759	0	0.817263
Year=2004	619219	0.2584546	0.4377855	0	1	Pct. Children	619219	0.282785	0.083929	0	0.589286
Adjacent local park	619219	0.0026146	0.0510662	0	1	Pct. 18 to 35	619219	0.238695	0.092992	0	1
Adjacent large park	619219	0.0023643	0.0485663	0	1	Pct. 35 to 55	619219	0.30168	0.077009	0	0.660194
Adjacent school	619219	0.0048658	0.0695855	0	1	Mean Lot Size -x- Mean subd. open area	619219	0.018997	0.096846	0	35.23113
Adjacent rail	619219	0.0020058	0.0447407	0	1						
Adjacent canal	619219	0.0102209	0.1005808	0	1						

Table 2. Correlations and Hausman Test of Exogeneity of Instruments

Hausman Test for Exogeneity	
Variable	p-value
Adjacent SOS -x- Lot size	0.3118
Adjacent SOS -x- Subd. open parcel size	0.3353
Adjacent subd. open	0.7493
Joint Test	0.5331

Correlation Matrix (endogenous variables in bold)

	Adjacent SOS -x- Lot size	Adjacent SOS -x- Subd. open area	Adjacent subd. open	**Mean subd. open area**	**Mean lot size**
Adjacent SOS -x- Lot size	1				
Adjacent SOS -x- Subd. open parcel size	0.5584	1			
Adjacent subd. open	0.7836	0.6412	1		
Mean subd. open area	0.361	0.4727	0.2553	1	
Mean lot size	0.1434	0.0406	0.029	0.0929	1

Table 3. Hausman-Taylor Estimates

Variable	Estimate	Std Err[a]	t-stat	Variable	Estimate	Std Err[a]	t-stat
Square footage	**0.03850**	0.00032	120.30	Local park area (<2000 ft.)	-0.00002	0.00011	-0.23
Square footage^2	**-0.00029**	0.00001	-41.88	Lot size -x- Local park area	0.00019	0.00058	0.32
Lot acres	**0.41215**	0.03711	11.11	Lot size -x- SOS area (<2000 ft.)	-0.00023	0.00036	-0.63
Lot acres^2	**-0.04230**	0.02152	-1.97	SOS area (<1000 ft.)	**0.00054**	0.00010	5.16
# stories	**-0.02892**	0.00112	-25.71	SOS area (≥1000ft & <2000 ft.)	**0.00022**	0.00009	2.53
# bathrooms	**0.02464**	0.00105	23.57	SOS area (≥2000ft and <3000 ft.)	0.00001	0.00004	0.33
Year built	**0.00245**	0.00058	4.21	Mean SOS area	**0.51764**	0.06008	8.62
Year built^2	-0.000004	0.00001	-0.73	Mean lot size	**1.04225**	0.36520	2.85
Garage	**0.04059**	0.00224	18.10	Adjacent SOS	**0.00852**	0.00463	1.84
Pool	**0.04453**	0.00063	71.05	Adjacent SOS -x- Lot size	**0.04467**	0.02212	2.02
Year=2001	**0.06100**	0.00076	79.87	Adjacent SOS -x- Subd. open parcel size	**0.00193**	0.00017	11.39
Year=2002	**0.10264**	0.00080	128.30	School	-0.00505	0.01694	-0.30
Year=2003	**0.16832**	0.00083	203.45	Large Park distance	**0.00091**	0.00050	1.83
Year=2004	**0.26444**	0.00083	318.93	CBD distance	**0.05225**	0.02923	1.79
Adjacent local park	**0.00756**	0.00348	2.17	CBD distance^2	**-0.00133**	0.00077	-1.74
Adjacent large park	**0.05988**	0.00699	8.56	Pct. Hispanic	0.09601	0.20113	0.48
Adjacent school	-0.00284	0.00410	-0.69	Pct. Black	0.21344	0.29192	0.73
Adjacent rail	**-0.02522**	0.01077	-2.34	Pct. Children	**-1.06640**	0.13297	-8.02
Adjacent canal	0.00168	0.00328	0.51	Pct. 18 to 35	0.42327	0.27513	1.54
Constant	**9.95597**	0.50032	19.90	Pct. 35 to 55	**1.67137**	0.34283	4.88
Number of Observations	619219			Mean Lot Size -x- Mean SOS area	**-0.69653**	0.19444	-3.58
Number of Subdivisions	7598						

[a]Robust standard errors calculated using 200 non-parametric boostraps

Table 4. Marginal Willingness to Pay for Additional SOS Acreage

	Open Space		Lot Size	
Spatial Area	Adjacent[a]	Non-Adjacent	Adjacent	Non-Adjacent
Small scale[b]	$513	$112	$91,484	$82,207
Large scale	$1,138		$2,918	

[a]Adjacency refers to whether a parcel is adjacent to subdivision open space

[b]Assume that for small scale the SOS is in the 1000 foot buffer

Table 5. Random Effects Estimates

Variable	Estimate	Std Err[a]	t-stat	Variable	Estimate	Std Err[a]	t-stat
Square footage	**0.03856**	0.00072	53.61	Local park area (<2000 ft.)	-0.00001	0.00015	-0.10
Square footage^2	**-0.00028**	0.00001	-19.29	Lot size -x- Local park area	0.00006	0.00064	0.09
Lot acres	**0.40296**	0.02363	17.05	Lot size -x- SOS area (<2000 ft.)	0.00017	0.00059	0.28
Lot acres^2	**-0.04488**	0.00825	-5.44	SOS area (<1000 ft.)	**0.00070**	0.00029	2.43
# stories	**-0.03361**	0.00279	-12.03	SOS area (≥1000ft & <2000 ft.)	**0.00034**	0.00020	1.70
# bathrooms	**0.02727**	0.00225	12.14	SOS area (≥2000ft and <3000 ft.)	**0.00017**	0.00008	1.99
Year built	0.00052	0.00096	0.54	Mean SOS area	**0.16993**	0.03038	5.59
Year built^2	**0.00002**	0.00001	2.33	Mean lot size	**0.29564**	0.03653	8.09
Garage	**0.04157**	0.00444	9.36	Adjacent SOS	0.00541	0.00675	0.80
Pool	**0.04515**	0.00098	46.17	Adjacent SOS -x- Lot size	**0.06431**	0.02413	2.66
Year=2001	**0.06076**	0.00253	24.00	Adjacent SOS -x- Subd. open parcel size	**0.00184**	0.00040	4.59
Year=2002	**0.10241**	0.00231	44.41	School	**-0.04945**	0.00574	-8.61
Year=2003	**0.16810**	0.00328	51.30	Large Park distance	**0.00205**	0.00021	9.84
Year=2004	**0.26415**	0.00293	90.07	CBD distance	**-0.02151**	0.00283	-7.61
Adjacent local park	0.00730	0.00560	1.30	CBD distance^2	**0.00054**	0.00009	6.10
Adjacent large park	**0.06350**	0.01290	4.92	Pct. Hispanic	**-0.48253**	0.02378	-20.29
Adjacent school	-0.00287	0.01090	-0.26	Pct. Black	**-0.52199**	0.05611	-9.30
Adjacent rail	-0.02407	0.02700	-0.89	Pct. Children	**-0.77788**	0.05339	-14.57
Adjacent canal	0.00217	0.00680	0.32	Pct. 18 to 35	**-0.22917**	0.04846	-4.73
Constant	**11.22154**	0.03906	287.30	Pct. 35 to 55	**0.83131**	0.05744	14.47
Number of Observations	619219			Mean Lot Size -x- Mean SOS area	**-0.20678**	0.04598	-4.50
Number of Subdivisions	7598						

[a]Robust standard errors

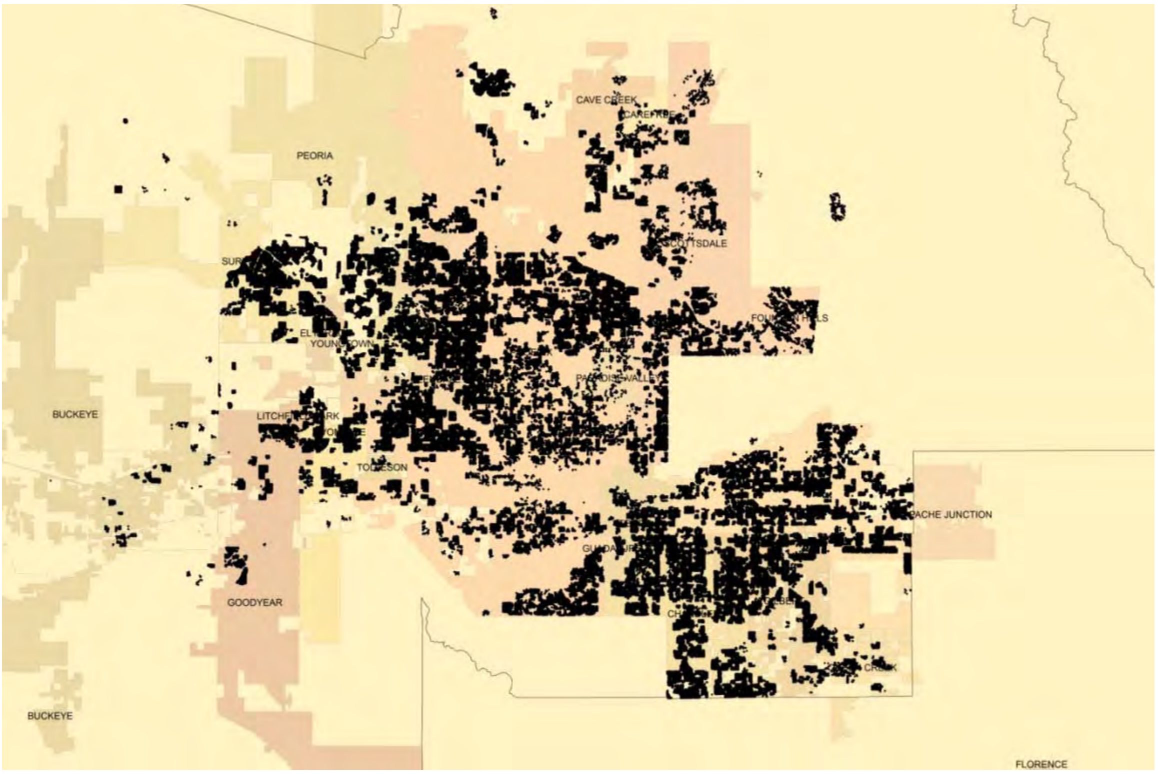


Figure 1. Housing transactions

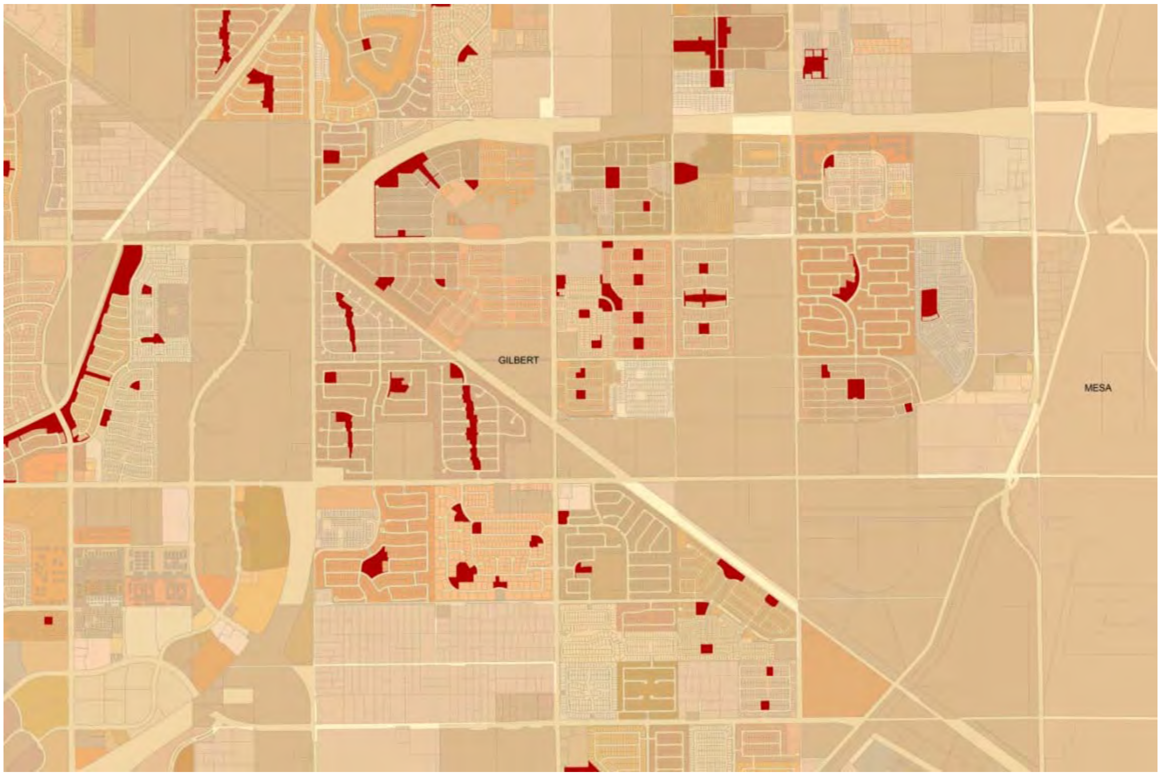


Figure 2. Location of subdivision open space within subdivisions

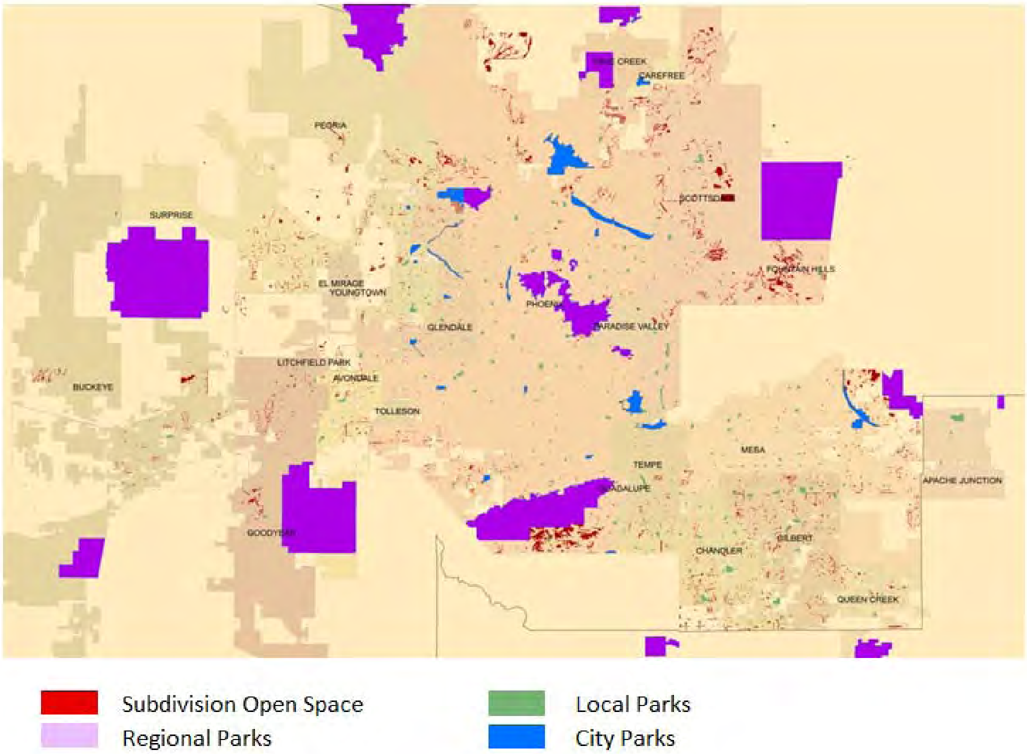


Figure 3. Spatial distribution of open space

Endnotes

[1] Hedonic regression techniques have been applied to a staggering array of environmental applications including water quality (Leggett and Bockstael 2000), airport noise (Pope 2008), air quality (Chay and Greenstone 2005), hazardous waste incinerators (Kiel and Mcclain 1995), Superfund sites (Cameron 2006; Thorsnes 2002), hog farming operations (Palmquist, Roka et al. 1997), and the presence and qualitative aspects of open space (Irwin 2002; Smith, Poulos and Kim 2002).

[2] We do not attempt a complete survey of this literature. See McConnell and Walls (2005) for a broad review of both the revealed and stated preference literatures.

[3] Despite its reputation to the contrary, residential development in the Phoenix area is actually relatively compact. One prominent study of 83 MSAs found that only 17 MSAs exhibited greater residential density than Phoenix (Ewing, Pendall et al. 2003).

[4] We later relaxed these measures substantially to include areas filtered out by our criteria and found no significant difference in our results. These results are available from the authors upon request.

[5] Our buffering strategy for the measurement of subdivision open space intentionally captures some open space parcels in nearby subdivisions that are nevertheless within a walkable distance to a particular household. As an alternative to this "non-exclusive" characterization of SOS, we recalculated subdivision rings and total open space based on an exclusive measure of subdivision open space that only includes the area of subdivision open space located within the same subdivision as the house. We found no significant differences between the two approaches and so we only report results from buffers which potentially overlap neighboring subdivisions. Given the apparent non-excludability of many services from subdivision open space (including

recreational use), we feel this is likely a more accurate representation of the extent of subdivision open space as perceived by households.

[6] This specification can be extended to capture qualitative aspects of open space as well such as the presence of playground equipment, the average slope, or the percentage of the park in grass versus other ground cover.

[7] The buffer in this case contains all open space up to 2000 feet (i.e. unlike the buffers previously described, it contains open space up to 1000 feet). The choice to interact a single buffer rather than all three was made for reasons of parsimony and to avoid collinearity in a highly specified model.

[8] For a current summary of the hedonic price literature see Palmquist (2005)

[9] While this dilemma is more apparent for very fine fixed effects, studies employing large scale fixed effects may not be immune. For instance, highly unpopular land uses (e.g. Superfund sites, prison facilities) may lend a widespread stigma to entire census tracts or even cities so that inclusion of fixed effects at these scales could “soak up” some of the capitalized effect of these land uses beyond their zone of immediate impact.

[10] Of course, the potential for bias remains if there are correlated home specific unobservable variables or time-varying neighborhood characteristics. Our method could be easily extended to allow for fixed effects for the interaction of year and subdivision to address the second concern.

[11] The order condition for identification requires that the number of exogenous within-varying variables be at least as large as the number of endogenous between-varying variables.

[12] The p-values in this table derive from Wald and z-tests based on a robust (sandwich) estimator of the covariance matrix rather than the more restrictive covariance structure imposed by the random effects error structure associated with a traditional Hausman test.

[13] The modified Hausman test rejected the exogeneity of the majority of candidate instruments – even those whose fixed effects and random effects estimates were virtually identical. That this same test failed to reject in the case of these three instruments and that the within and between estimates were essentially identical seems to lend weight to our case for instrument validity.

[14] Given the nature of racial segregation in Phoenix neighborhoods, it is possible that our controls for distance to large parks and the central business district are confounded with these racial/ethnic variables.

[15] The same critique can be levied at numerous hedonic studies employing neighborhood level variables without regard to their potential for correlation with the error term.

[16] One could argue that there is simply insufficient within-subdivision variation in these measures of SOS provision to permit identification. However, OLS and random effects estimates (which utilize between subdivision variation) achieved similarly small estimates. Furthermore, the provision of SOS within 1000 foot buffers does vary substantially within subdivisions so that its marginal effect in the HT model is precisely estimated, and yet the estimated value of SOS acreage is quite small.

[17] Our analysis is focused on the mean effect of adding SOS acreage in 1000 foot “stair steps” of varying walkability. It is possible, however, that finer buffers in the sub-1000 foot range could find larger effects of SOS proximity.

[18] For all calculations, we rely on the mean price of housing of $207,680 and use the means of variables where interaction terms or nonlinearities require the imputation of values into the MWTP calculations.

[19] These MWTP calculations do not value the marginal value of adjacency itself (i.e. moving from non-adjacent to adjacent status). Rather, they consider the marginal value of an acre of open space for an existing adjacent property.

[20] To compare MWTP for private and public goods, it is well known that we must sum the individual MWTPs for the public good.

[21] Our comparison of estimators is necessarily terse. Please consult Abbott and Klaiber (2009) for a much more exhaustive and rigorous comparison in a similar setting.

[22] Ideally, we would substitute measurements of all the expected service flows from an amenity associated with a house into the hedonic price function. It is our inability to clearly map between spatial location and these service flows that necessitates an approach based on spatial "proxies" for the flows themselves.

Valuing Walkability and Vegetation in Portland, Oregon

Niko Drake-McLaughlin and Noelwah R. Netusil
Reed College, Department of Economics
3203 SE Woodstock Blvd.
Portland, Oregon 97202-8199

Abstract

This study uses the hedonic price method to examine if vegetation on a property, and in the surrounding neighborhood, and proximity to urban amenities influence the sale price of single-family residential properties in a highly urbanized part of Portland, Oregon. We combine structural and location information for 30,786 single-family residential transactions with high-resolution land cover data and a walkability index developed by city planners. We estimate multiple models and find evidence of omitted variable bias in models that do not include both variables and their interactions. A one standard deviation increase in the walkability index, starting from the mean score, is estimated to increase a property's sale price from 1.47% to 6.89%.

JEL codes: Q51, Q24, R14

Keywords: hedonic price method, urban amenities, walkability, vegetation

Valuing Walkability and Vegetation in Portland, Oregon

I. Introduction

An urban neighborhood's desirability depends on many factors including the amount, type and distribution of vegetation and whether residents have easy access to parks, shopping, schools and public transit. These factors are also related to broader environmental concerns. The amount and placement of vegetation can reduce stormwater runoff, improve water quality and enhance wildlife habitat (Metro 2008), and walkable, mixed-use neighborhoods have been found to reduce traffic congestion, improve air quality and reduce greenhouse gas emissions (Bureau of Planning and Sustainability 2009).

Portland, Oregon is ranked as the most sustainable city in the United States (Haight 2009), but it faces several environmental challenges resulting from a high percentage of impervious surface area, a sewer system in older neighborhoods that combines untreated sewage and stormwater runoff, and declines in steelhead trout and Chinook salmon populations that resulted in their listing as "threatened" under the Endangered Species Act (Bureau of Environmental Services 1999).

Approximately 35% of the city of Portland is classified as impervious, which includes "hard" surfaces such as roads, rooftops and driveways. This percentage is a byproduct of Oregon's 19 land use planning goals, specifically Goal 14, which requires urban growth boundaries be "established and maintained by cities, counties and regional governments to provide land for urban development needs and to identify and separate urban and urbanizable land from rural land" (Oregon Department of Land Conservation and Development 2006). While the Portland metropolitan area's urban growth boundary has contained sprawl, the density of development has resulted in an amount of impervious surface area that exceeds the level past which water quality is found to degrade rapidly (Booth, Hartley, and Jackson 2002).

Oregon’s water quality index scores for the Lower Willamette Basin, which includes the Portland metropolitan area, range from good to very poor. Water quality at a monitoring site located in downtown Portland is “impacted by high concentrations of fecal coliform, total phosphates, nitrate and ammonia nitrogen, and biochemical oxygen demand with additional influence from high total solids” (Oregon Department of Environmental Quality). The poorest ratings occur during winter when “Portland’s combined sewer/stormwater system is under the most pressure and overflows are most likely to occur” (Oregon Department of Environmental Quality).

To reduce the volume of untreated discharges coming from Portland’s combined sewer system, the city of Portland implemented a series of programs to reduce the amount of stormwater entering the system and to increase the physical capacity of its treatment facilities. This “physical capital” approach is complemented by a “natural capital” approach, the Grey to Green Program, that aims to plant 33,000 yard trees and 50,000 street trees, add 43 acres of ecoroofs, construct 920 green street facilities, and purchase 419 acres of natural areas over a 5-year period (Bureau of Environmental Services 2010).

Portland is also exploring, as part of an update to its long-range development plan, the “20-minute neighborhood” concept which would lead to the redevelopment of neighborhoods to improve access to urban amenities such as shopping, schools, public transit and parks. This is expected to decrease residents’ transportation expenditures, reduce pollution, increase safety by having more people on the street, and encourage healthier lifestyles by promoting walking and biking. Economic benefits include a likely increase in housing values, reduction in infrastructure costs, ability to attract workers and new businesses, and an increase in tourism (Bureau of Planning and Sustainability 2009).

The research on walkability and the sale price of single-family residential properties finds mixed results about factors, such as proximity to bus stops and shopping, which contribute to walkability. The majority of studies find that vegetation increases the sale price of single-family residential properties in urban areas, but no research has included both walkability and vegetation or examined if a synergistic relationship exists between these variables. Because they are likely to be negatively correlated, studies that look at just one variable may produce biased estimated coefficients thereby leading to inaccurate policy recommendations.

Our paper is structured as follows. The following section reviews the literature on property values, walkability and vegetation. Section III provides background information about the study area and data used in our analysis. Models are presented in Section IV with results and key findings in Section V. The final section concludes and offers policy recommendations.

II. Literature Review

There is a rich literature investigating the effects of vegetation on the sale price of single-family residential properties in urban areas. Donovan and Butry (2010) estimate the effect of street trees on the sale price of single-family residential properties on the east side of Portland, Oregon. In addition to finding a statistically significant increase in sale price of $8,870 (3% of the median sales price) from the combined effect of street trees in front of a property, and canopy from street trees within 100 feet of a property, the authors find that street trees reduce a property's average time on market by 1.7 days.

Numerous studies examine the effect of urban forests on the sale price of residential properties. While studies find a positive effect from proximity to urban forests (Mansfield et al. 2005; Tyrvainen and Miettinen 2000), the evidence on forest views is mixed with studies finding a positive effect (Tyrvainen and Miettinen 2000), a negative effect (Paterson and Boyle 2002), or effects that vary based on tree type (Garrod and Willis 1992).

A modeling approach used by several authors includes the amount of the area surrounding a property classified as forested (Mansfield et al. 2005; Netusil, Chattopadhyay, and Kovacs 2010; Paterson and Boyle 2002; Payton 2008) or that have trees, and other kinds of vegetation, as land use categories (Acharya and Bennett 2001; Geoghegan, Wainger, and Bockstael 1997). Important findings from these studies include evidence of diminishing returns from tree canopy (Netusil, Chattopadhyay, and Kovacs 2010) and the superiority of models that incorporate spatial patterns compared to a more traditional approach that includes straight-line distance to certain land use/land cover types (Acharya and Bennett 2001).

While research in the Portland metropolitan area has examined if proximity to specific urban amenities such as open spaces (Lutzenhiser and Netusil 2001), wetlands (Mahan, Polasky, and Adams 2000), and public transit (Chen, Rufolo, and Dueker 1998) affect the sale price of single family residential properties, the literature on walkability's effect is limited to one study that evaluates the importance of neighborhood design on the west side of the Portland metropolitan area (Song and Knaap 2003). The authors find mixed results for variables that capture walkability—properties with easier access to commercial uses, measured as the percentage of land within ¼ mile of a property classified as commercial, are found to sell for a premium while properties close to bus stops sell for a discount.

The modeling approach that is closest to the one used in our study is Pivo and Fisher's (2010) analysis of how walkability, measured using a value generated by walkscore.com, affects the market value of office, retail, apartment and industrial properties. A 10-point increase in walkability, which is measured on a 100-point scale, is estimated to increase property values by 1 percent for apartments and 9 percent for office and retail properties. No statistically significant effect was found for industrial properties.

Pivo and Fisher (2010) note several limitations from using walkscore.com including its assignment of equal weights to urban amenities such as schools, parks, retail establishments, etc. that are located within buffers of up to 1 mile from a property. Barriers to walking, such as highways, rivers and steep slopes, and access to mass transit, are not taken into account in walkscore's algorithm, which uses a straight-line distance to amenities. The walkability index used in our analysis, which is described in detail below, overcomes these limitations.

III. Data and Study Area

Our data set includes 30,786 single-family residential transactions that occurred between January 1st, 2005 and December 31st, 2007 in a highly urbanized part of the city of Portland, Oregon. The data, which are from the Multnomah County Assessor's office, were evaluated to make sure that transactions occurred at arms length. Summary statistics, which are broken down by the five areas of Portland (North, Northeast, Northwest, Southwest and Southeast), are presented in Table 1. Sale price is in 2007 dollars after deflating using the CPI-U.

Table 1: Summary Statistics for House Sales by Area

Land cover information was derived from a 2007 land cover layer that classifies each 3x3 foot square in the study area as high structure vegetation (trees), low structure vegetation (grass, shrubs and small trees), impervious surface or open water (rivers, streams and lakes) (Metro Data Resource Center 2007). The proportion of each land cover type was computed for each property, within 200 feet of each property, between 200 feet and ¼ mile, and between ¼ mile and ½ mile of each property. Neighborhood data, such as distance to major arterial roads, distance to highways, slope and elevation were derived using data layers maintained by the regional government's data resources center (Metro Data Resources Center 2009). Median income and proportion white at the census tract level were derived from the 2000 U.S. Census (U.S. Census Bureau 2009).

A walkability index, which is illustrated in Figure 1, was created by staff at the Portland Bureau of Planning and Sustainability as part of the "20-minute neighborhood" concept. The index takes into account several variables, such as the actual walking distance to full service grocery stores, elementary schools and parks, and if streets are steeply sloped. Also, the city was divided into ½ mile by ½ mile grid cells and the number of commercial businesses, the percentage of sidewalk coverage, the number of intersections and the level of public transit in each grid cell was computed, weighted, and then incorporated into the index. Scores range from 1-100 for the city of Portland with the scores for our observations ranging from 1-83.

Figure 1: Walkability Index for City of Portland, Oregon

Because data are not consistently available for urban amenities outside the city of Portland, observations within ½ mile of the city limits may have inaccurate walkability index values, so these observations were dropped from our analysis. The 30,786 property sales used in our analysis are shown in Figure 2.

Figure 2: Property Sales in Study Area

The regression specification includes detailed structural, location and environmental characteristics. The names and descriptions for variables used in the regression are provided in Table 2.

Table 2: Variable Names and Descriptions

Summary statistics for key variables are provided in Table 3. Lot sizes are small, averaging around 7,000 square feet; on average, 44% of our lots are covered by impervious surface area followed by approximately 29% low structure vegetation (grass, shrubs and small trees), and 27% high structure vegetation (trees). Thirty-two properties have water on the property itself, so the average lot coverage for this variable is very small. The land cover percentages remain fairly constant in the buffers (200 feet, 200 feet to ¼ mile and ¼ mile to ½ mile) surrounding our properties. Impervious surface area in the buffers is approximately 46%, low structure vegetation ranges from around 27% to 28%, and high structure vegetation remains steady at 26%.

Table 3: Summary Statistics

V. Results

The theoretical basis for the hedonic price method is firmly established in the literature. The appropriate functional form for estimation is less clear with most researchers using the semi-log.

Results of four semi-log models are presented in Table 4. Models 1 and 2 include just the walkability index and vegetation variables, respectively. Model 3 includes both variables and Model 4 adds interaction terms. Quadratic terms are included for the vegetation variables (high structure and low structure) and for the walkability index because we believe there is some point past which increases in these variables will decrease a property's sale price—a modeling approach informed by research in the study area (Netusil, Chattopadhyay, and Kovacs 2010). We predict that water on a property will always decrease its sales price due to risks of flooding and other hazards, while water in the buffers surrounding a property will always increase sale price. Impervious surface is the excluded variable in both models.

A Breusch-Pagan/Cook-Weisberg test indicates heteroskedasticity in all models, so robust regressions are run. All of the control variables have the expected sign and magnitude, and most are statistically significant. Full regression results are available from the authors.

Table 4: Regression Results (robust standard errors)

Models 1 and 2 are included to compare the effect of modeling these variables individually with models that include both variables (Model 3) and interaction terms (Model 4). The estimated coefficient for walkability is statistically significant at the 10% level in Model 1, but the quadratic term is not significant. The estimated magnitude, significance, and in the case of the quadratic term, the sign of the estimated coefficient, changes in Model 3 when vegetation is included as a control variable providing strong evidence of omitted variable bias in Model 1. Interestingly, a comparison of estimated coefficients for land cover variables Models 2 and 3 does not provide strong evidence of omitted variable bias.

The estimated coefficients on both walkability terms in Model 3 are statistically significant, as are many of the land cover variables. Walkability is estimated to have a positive effect up to 93, which is outside the range of our data (the maximum value is 83), but below the maximum possible value of 100. A one standard deviation increase (13.11 points) in the walkability index for a property, starting from the mean score of 47.79, is predicted to increase its sale price by $9,942 (3.29%) using the average sale price in our data set.

The linear and squared terms for on-property high structure vegetation are statistically significant and predict an optimal on-property high structure vegetation coverage of 33.74%, which is 8.19 percentage points higher than the average in our data set. For the average lot, this represents a 20-25 year old oak tree (McPherson et al. 2002). We estimate that increasing high structure vegetation on a property from the average amount (25.55%), to the amount that maximizes sale price (33.74%) will increase a property’s sale price by $214 (0.07%).

Increasing high structure vegetation in the 200-foot and 200 foot to ¼ mile buffer surrounding a property is predicted to always increase a property’s sale price. The furthest buffer from a property, ¼ mile to ½ mile, shows diminishing returns to high structure vegetation. The estimated coefficients for low structure vegetation are mixed ranging from both the linear and quadratic term being significant (200 foot, ¼ mile to ½ mile buffers), to only the quadratic term (on property) to neither term (¼ mile to ½ mile buffer). Water on a property is significantly negative, as expected, and water in the surrounding buffers is significantly positive, which is also expected.

The fourth model adds interaction terms to Model 3. Nine of the twelve interaction variables are statistically significant providing evidence of a synergistic relationship between walkability and land cover.

Table 5 includes predictions of a one standard deviation increase in the walkability index, from its mean value, evaluated at the 25th, 50th and 75th percentile of high and low structure vegetation for all buffers. Predicted effects range from 1.47% of the mean sale price for properties when high and low structure vegetation are at the 25th percentile to 6.88% when they are at the 75th percentile.

Table 5: Predicted Effect of a One Standard Deviation Increase in Walk Score Evaluated at Mean Sale Price and Mean Walkability Index

Table 6 holds walkability constant at its 25th, 50th and 75th percentile scores and shows the predicted effect from increasing on-property high structure vegetation on a property from the dataset average of 25.55% to the target tree canopy amount (35%-40%) specified for residential property in the city of Portland's Urban Forest Action Plan (Urban Forest Action Plan 2007).

Table 6: Predicted Effect of Increasing On-Property High Structure Vegetation Evaluated at Mean Sale Price and Mean High Structure Vegetation

The amount of high structure vegetation that maximizes a property's sale price varies with its walkability score. Using estimated coefficients from Model 4, the "optimal" amount of high structure vegetation on a property is 18.44% when the walkability index is at the 25th percentile, 36.60% at the 50th percentile, and 51.12% for the 75th percentile. This variation in "optimal" amounts of tree canopy explains why increasing tree canopy, when the walkability index is at the 25th percentile, decreases a property's sale price. Predicted effects for the 50th and 75th percentile are positive, but small.

IV. Policy Implications and Conclusions

This paper has highlighted the importance of two key factors in determining the sale price of residential properties in an urban area: access to urban amenities, captured by a walkability index, and land cover on a property and in the surrounding neighborhood. Models that use one variable or the other likely suffer from omitted variable bias. Models that include both variables, and interaction terms, show that effects on sale price that depend on the other variable's level with increases in high structure vegetation having a negative predicted effect when walkability is held constant at a low level (25th percentile). Increases in walkability, holding high and low structure vegetation constant, is predicted to increase a property's sale price with the largest effect occurring for properties with a high amount of low and high structure vegetation (75th percentile) on the property and in the surrounding buffers.

It is possible that these effects arise from scarcity of walkability and high levels of high structure vegetation in the same area. If an area is very walkable, it may be further from parks and closer to retail areas that have more impervious surfaces. Areas with a high proportion of trees may be less likely to have lots of businesses nearby. Nevertheless, the data does suggest that increasing both of these factors should have statistically and economically significant effects on a property's sale price and that the greatest effect comes from increasing the two in a coordinated effort.

Our results indicate that increasing walkability and vegetation should be pursued, and that both are beneficial for single-family residential property owners, but neither goal should be achieved at the expense of the other. How cities accomplish this goal, and what combination of walkability and vegetation are best for environmental and social goals, remain questions for further research.

Acknowledgements

We greatly acknowledge assistance from Jonathan Kadish, Gary Odenthal, Carmen Piekarski, Zach Levin and Minott Kerr.

Table 1: Summary Statistics for House Sales by Area

Variable	Number of Observations	Mean	Standard Deviation	Minimum	Maximum
realsaleprice (study area)	30,786	302,118	168,435	53,135	3,408,846
realsaleprice (N Portland)	5,165	246,825	81,446	64,795	993,354
realsaleprice (NE Portland)	9,883	316,248	149,998	53,135	1,572,030
realsaleprice (NW Portland)	326	706,618	360,571	107,527	2,750,809
realsaleprice (SW Portland)	2,835	462,116	289,284	93,618	3,408,846
realsaleprice (SE Portland)	12,577	267,173	119,082	58,921	1,903,024

Figure 1: Walkability Index for City of Portland, Oregon

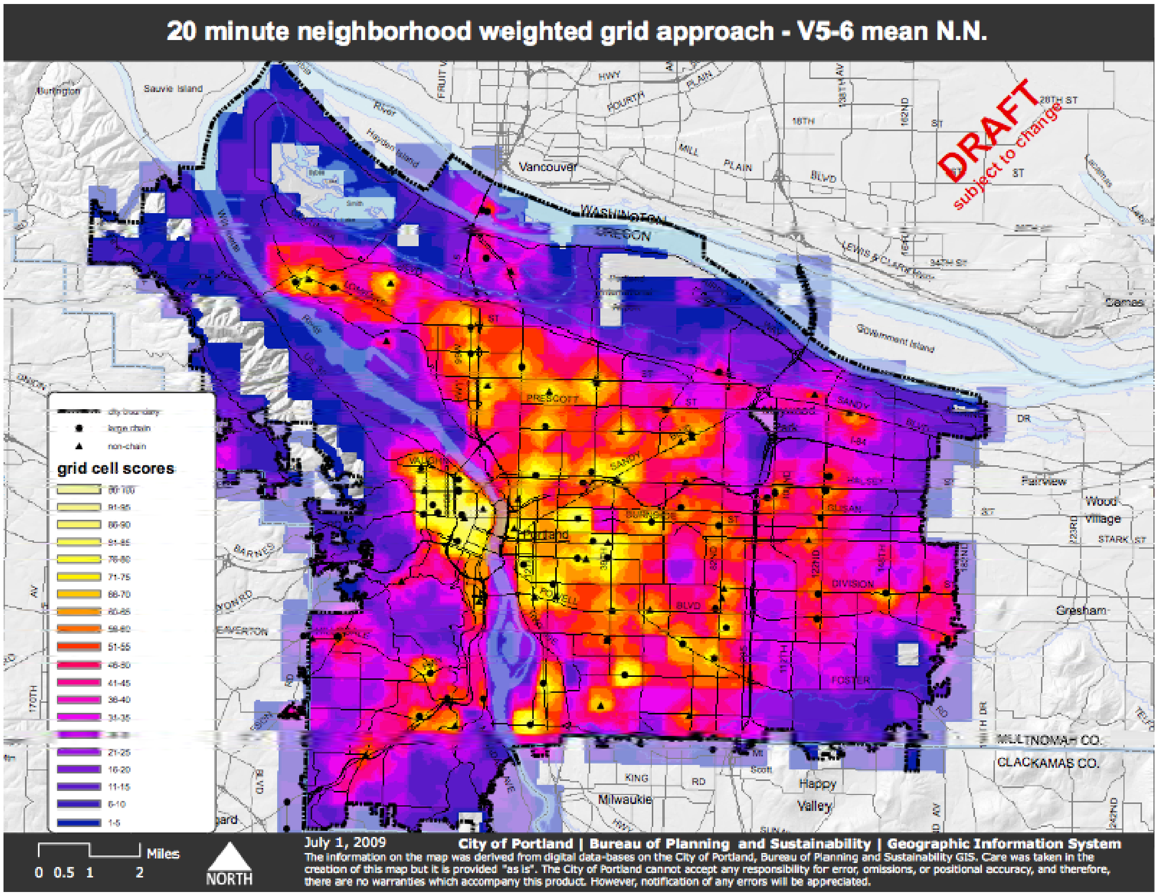


Figure 2: Property Sales in Study Area

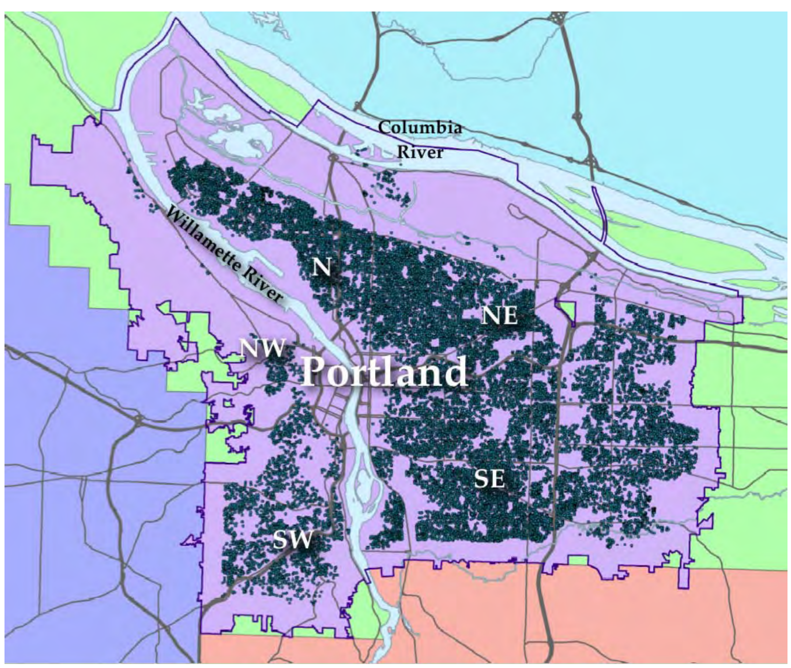


Table 2: Variable Names and Descriptions

Variable Name	Variable Description
walkability	Walkability index score
prop_high	Proportion high structure vegetation on property
prop_low	Proportion low structure vegetation on property
prop_imp	Proportion impervious surface on property
prop_water	Proportion water on property
prop_hv_200	Proportion high structure vegetation within 200 feet
prop_lv_200	Proportion low structure vegetation within 200 feet
prop_imp_200	Proportion impervious surface within 200 feet
prop_wa_200	Proportion water within 200 feet
prop_hv_1320	Proportion high structure vegetation between 200 feet and 1/4 mile
prop_lv_1320	Proportion low structure vegetation between 200 feet and 1/4 mile
prop_imp_1320	Proportion impervious surface between 200 feet and 1/4 mile
prop_wa_1320	Proportion water between 200 feet and 1/4 mile
prop_hv_2640	Proportion high structure vegetation between 1/4 mile and 1/2 mile
prop_lv_2640	Proportion low structure vegetation between 1/4 mile and 1/2 mile
prop_imp_2640	Proportion impervious surface between 1/4 mile and 1/2 mile

prop_wa_2640	Proportion water between 1/4 mile and 1/2 mile
lotsqft	Lot square footage
bldgsqft	House square footage
fullbaths	Number of full bathrooms
halfbaths	Number of half bathrooms
age	Year house was sold minus year house was built
numfire	Number of fireplaces
N, NE, NW, SW, SE	Dummy variables for areas of the city
dist_N, dist_NE, dist_NW, dist_SW, dist_SE (feet)	Area variables interacted with distance to CBD
elevation (feet)	Elevation of property in feet
medianincome ($)	Median income of census tract in dollars
crime	Crime index score
proportionwhite	Proportion of census tract that is white
prop_slope10%	Proportion of property with a slope greater than 10%
Month 1-36	Dummy variables indicating sale month of transaction
maj_art330, maj_art660, maj_art1320, maj_art2640	Dummy variables indicating whether a property is within 330, 660, 1320, or 2640 feet of a major arterial road

fwy 330, fwy660, fwy1320, fwy2640	Dummy variables indicating whether a property is within 330, 660, 1320 or 2640 feet of a freeway

Table 3: Summary Statistics

Variable	Mean	Standard Deviation	Minimum	Maximum
walkability	47.79	13.11	1	83
prop_high	.2555	.2163	0	1
prop_low	.2927	.1923	0	1
prop_imp	.4518	.1927	0	1
prop_water	.0000369	.0016	0	.1418
prop_hv_200	.2481	.1291	0	.9921
prop_lv_200	.2819	.0992	0	.7801
prop_imp_200	.4696	.1201	0	.9565
prop_wa_200	.0003696	.008089	0	.3565
prop_hv_1320	.2436	.1106	.02430	.9213
prop_lv_1320	.2739	.07673	.01124	.6749
prop_imp_1320	.4802	.1073	.04586	.9384
prop_wa_1320	.0022624	.0187	0	.5310
prop_hv_2640	.2426	.1045	.0562	.8355
prop_lv_2640	.2670	.0694	.0054	.6315
prop_imp_2640	.4814	.1042	.0661	.8658
prop_wa_2640	.0090128	.0372	0	.5962
lotsqft	6274.60	5320.13	808	365,750
bldgsqft	1905.76	809.48	396	12,061
fullbaths	1.55	.6612	0	6

halfbaths	.2664	.4704	0	4
age	62.00	29.84	0	137
numfire	.7920	.6706	0	6
dist	24,224.68	9,101.17	3,415.31	49,965.3
elevation	224.20	116.80	10	1,040
medianincome	43,283	12,152	14,091	108,931
crime	2.50	3.14	1	36
lncrime	.5336	.7565	0	3.58
proportionwhite	.7596	.1342	.2943	.9571

Table 4: Regression Results (robust standard errors)

Variables	Model 1	Model 2	Model 3	Model 4
walkability	1.06768e-03*		5.97157e-03***	-4.17659e-03***
	(6.27958e-04)		(6.89291e-04)	(1.46649e-03)
walkability2	22.65259e-06		-3.20117e-05***	-6.27820e-06
	(6.55829e-06)		(6.96361e-06)	(8.81000e-06)
prop_high		7.06845e-02***	6.89150e-02***	-9.92901e-02***
		(1.90705e-02)	(1.89586e-02)	(3.82456e-02)
prop_high2		-.111279***	-.111729***	-9.46229e-02***
		(2.50218e-02)	(2.48523e-02)	(2.49768e-02)
walkability*prop_high				3.27217e-03***
				(6.36739e-04)
prop_low		1.03427e-02	3.85257e-03	-8.60920e-02**
		(2.48553e-02)	(2.46640e-02)	(4.19106e-02)

prop_low2		-7.07229e-02*	-6.14550e-02*	-7.43739e-02**
		(3.63128e-02)	(3.58957e-02)	(3.50870e-02)
walkability*prop_low				1.96878e-03***
				(7.35359e-04)
prop_water		-7.36746***	-7.18829***	-10.701**
		(1.744838)	(1.774594)	(4.249342)
walkability*prop_water				.232681
				(0.167586)
prop_hv_200		.159067***	.184108***	-.265058***
		(4.07884e-02)	(4.05419e-02)	(8.10759e-02)
prop_hv2_200		-8.02222e-02	-.102993	6.22763e-02
		(6.35825e-02)	(6.33478e-02)	(6.87583e-02)
walkability*prop_hv_200				7.88557e-03***
				(1.27284e-03)
prop_lv_200		.443899***	.433219***	.289262***
		(7.22956e-02)	(7.21371e-02)	(0.103347)
prop_lv2_200		-.461151***	-.421295***	-.432532***
		(0.111749)	(0.111743)	(0.113157)
walkability*prop_lv_200				3.14192e-03**

				(1.41205e-03)
prop_wa_200		.846658**	1.06766***	-.563672
		(0.385804)	(0.363878)	(0.641638)
walkability*prop_wa_200				5.59467e-02**
				(2.65298e-02)
prop_hv_1320		.253295***	.330514***	.196147
		(7.08785e-02)	(7.06033e-02)	(0.156796)
prop_hv2_1320		.162962	.226176**	.352361***
		(0.108516)	(0.108366)	(0.135395)
walkability*prop_hv_1320				2.05930e-03
				(2.35815e-03)
prop_lv_1320		.142154	.111889	.410147*
		(0.140576)	(0.140266)	(0.225069)
prop_lv2_1320		-1.55452e-02	.242364	.278726
		(0.221011)	(0.221076)	(0.251169)
walkability*prop_lv_1320				-6.99113e-03***
				(2.56260e-03)
prop_wa_1320		.237102	.345257**	1.1123***
		(0.151544)	(0.148107)	(0.368680)
walkability*prop_wa_1320				-2.29504e-02***

				(8.29671e-03)
prop_hv_2640		.651971***	.597983***	.901364***
		(7.66755e-02)	(7.59835e-02)	(0.144002)
prop_hv2_2640		-.859051***	-.651059***	-.976767***
		(0.120009)	(0.120007)	(0.137130)
walkability*prop_hv_2640				-4.34080e-03**
				(2.04546e-03)
prop_lv_2640		.801175***	.606028***	-.712771***
		(0.166059)	(0.167362)	(0.221581)
prop_lv2_2640		-1.03777***	-.681525**	-.166792
		(0.282672)	(0.285593)	(0.305121)
walkability*prop_lv_2640				2.23610e-02***
				(2.39760e-03)
prop_wa_2640		.418843***	.470244***	.639733***
		(5.16137e-02)	(5.13326e-02)	(0.189230)
walkability*prop_wa_2640				-4.29048e-03
				(3.58124e-03)
Observations	30,786	30,786	30,786	30,786

R-squared	0.756	0.763	0.766	0.769

*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 5: Predicted Effect of a One Standard Deviation Increase in Walk Score Evaluated at Mean Sale Price and Mean Walkability Index

	High and Low Structure Vegetation at 25th Percentile For All Buffers	High and Low Structure Vegetation at 50th Percentile For All Buffers	High and Low Structure Vegetation at 75th Percentile For All Buffers
Predicted increase in sale price of a one standard deviation increase in walkability index	\$4,431 (1.47%)	\$11,746 (3.89%)	\$20,801 (6.89%)

Table 6: Predicted Effect of Increasing On-Property High Structure Vegetation Evaluated at Mean Sale Price and Mean High Structure Vegetation

	Walkability index at 25th percentile (score of 39)	Walkability index at 50th percentile (score of 49)	Walkability index at 75th percentile (score of 57)
Predicted effect of achieving 35% high structure vegetation on property	-$610 -0.02%	$326 0.11%	$1,074 0.36%
Predicted effect of achieving 40% high structure vegetation on property	-$1,130 -0.37%	$301 0.10%	$1,446 0.48%

References

Acharya, Gayatri, and Lynne Lewis Bennett. 2001. Valuing open space and land-use patterns in urban watersheds. *Journal of Real Estate Finance and Economics* 22 (2-3):221-237.

Booth, Derek B., David Hartley, and Rhett Jackson. 2002. Forest Cover, Impervious Surface Area, and the Mitigation of Stormwater Impacts. *Journal of the American Water Resources Association* 38:835-845.

Bureau, U.S. Census. 2009. American FactFinder.

Center, Metro Data Resource. *Regional Land Information System* 2009.

Chen, Hong, Anthony Rufolo, and Kenneth J. Dueker. 1998. Measuring the impact of light rail systems on single-family home values - A hedonic approach with geographic information system application. *Land Use and Transportation Planning and Programming Applications* (1617):38-43.

Development, Oregon Department of Land Conservation and. 2006. Oregon's Statewide Planning Goals & Guidelines Goal 14: Urbanization. edited by O. D. o. L. C. a. Development.

Donovan, Geoffrey H., and David T. Butry. 2010. Trees in the city: Valuing street trees in Portland, Oregon. *Landscape and Urban Planning* 94:77-83.

Garrod, Guy D., and Ken G. Willis. 1992. The Environmental Economic-Impact of Woodland: A 2-Stage Hedonic Price Model of the Amenity Value of Forestry in Britain. *Applied Economics* 24 (7):715-728.

Geoghegan, J, LA Wainger, and NE Bockstael. 1997. Spatial landscape indices in a hedonic framework: an ecological economics analysis using GIS. *Ecological Economics* 23 (3):251-264.

Haight, Abby. 2009. Portland Gets Its Reward: Most Sustainable City. *The Oregonian*.

Lutzenhiser, Margot, and Noelwah R. Netusil. 2001. The Effect of Open Spaces on a Home's Sale Price. *Contemporary Economic Policy* 19 (3):291-298.

Mahan, Brent L., Stephen Polasky, and Richard M. Adams. 2000. Valuing Urban Wetlands: A Property Price Approach. *Land Economics* 76 (1):100-113.

Mansfield, Carol, Subhrendu K. Pattanayak, William McDow, Robert McDonald, and Patrick Halpin. 2005. Shades of Green: Measuring the value of urban forests in the housing market. *Journal of Forest Economics* 11 (3):177-199.

McPherson, E. Gregory, Scott E. Maco, James R. Simpson, Paula J. Peper, Qingfu Xiao, Ann Marie VanDerZanden, and Neil Bell. 2002. Western Washington and Oregon Community Tree Guide: Benefits, Costs, and Strategic Planting. Center for Urban Forest Research, USDA Forest Service, Pacific Southwest Research Station.

Metro. 2008. State of the Watersheds Monitoring Report, Initial report: Baseline watershed conditions as of Dec. 2006.

Metro Data Resource Center. 2009. *2007 High Resolution Landcover* 2007 [cited June 8 2009]. Available from http://rlismetadata.oregonmetro.gov/display.cfm?Meta_layer_id=2303&Db_type=rlislite.

Netusil, Noelwah R., Sudip Chattopadhyay, and Kent Kovacs. 2010. Estimating the Demand for Tree Canopy: a Second-Stage Hedonic Price Analysis in Portland, Oregon. *Land Economics*.

Paterson, Robert W., and Kevin J. Boyle. 2002. Out of Sight, Out of Mind? Using GIS to Incorporate Visibility in Hedonic Property Value Models. *Land Economics* 78 (3):417-425.

Payton, Seth, Greg Lindsey, Jeff Wilson, John R. Ottensmann and Joyce Man. 2008. Valuing the Benefits of the Urban Forest: A Spatial Hedonic Approach. *Journal of Environmental Planning and Management* 51 (6):717-736.

Pivo, Gary and Jeffrey D. Fisher. 2010. The Walkability Premium in Commercial Real Estate Investments. In *Responsible Property Investesting Center, University of Arizona; Benecki Center for Real Estate Studies, Indiana University*.

Quality, Oregon Department of Environmental. Oregon Water Quality Index Report for Lower Willamette, Sandy, and Lower Columbia Basins Water Years 1986-2995.

Services, Bureau of Environmental. 1999. Steelhead and Chinook Salmon Listing Portland's Response to the Endangered Species Act.

———. 2010. Portland's Green Infrastructure: Quantifying the Health, Energy, and Community Livability Benefits. edited by B. o. E. Services. Portland.

Song, Y., and G. J. Knaap. 2003. New urbanism and housing values: a disaggregate assessment. *Journal of Urban Economics* 54 (2):218-238.

Sustainability, City of Portland Bureau of Planning and. 2009. Status Report: Twenty-Minute Neighborhoods.

———. 2009. Status Report: Twenty-minute Neighborhoods.

Tyrvainen, Liisa, and Antti Miettinen. 2000. Property prices and urban forest amenities. *Journal of Environmental Economics and Management* 39 (2):205-223.

Urban Forest Action Plan. 2007. Portland: Portland Parks & Recreation.

Species Preservation versus Development:
An Experimental Investigation under Uncertainty

W. Douglass Shaw
Dept.of Agricultural Economics
Texas A&M University
wdshaw@tamu.edu

Therese Grijalva
Department of Economics
Weber State University
tgrijalva@weber.edu

Robert P. Berrens
Department of Economics
University of New Mexico
rberrens@unm.edu

Species Preservation versus Development:
An Experimental Investigation under Uncertainty

Abstract: The safe minimum standard (SMS) is a decision rule to preserve a renewable resource, unless the social costs of doing so are somehow intolerable. While unpersuasive to many, support for the SMS has been advocated by some economists and policy analysts for settings involving irreversibility and a high degree of uncertainty. The objective of this paper is to explore decision-making involving species preservation versus development within an experimental laboratory setting, and involving a particular type of uncertainty (known payoffs and uncertain probabilities). The experimental design implements a number of prior game-theoretic investigations of the SMS (Bishop 1978; Ready and Bishop 1991; Palmini 1999), involving insurance, lottery or combined games against nature. The choices are between species preservation, which possibly provides a cure for a disease, or developing habitat, leading to irreversible depletion. Econometric results from a random parameters logit (RPL) model, using responses from 117 participants (across both US and Mexican university student samples) and 9 treatment choices, indicate that support for the SMS varies across the type of game, the imposed maximum regret condition concerning the relative magnitude of different costs (e.g., cost of disease) and benefits (e.g., net benefits of development), a constructed measure of respondents' risk aversion, and other factors. There is also evidence of highly heterogeneous preferences for preservation even within our relatively homogenous student samples.

Key Words: Safe Minimum Standard, Uncertainty, Experimental, Game against Nature, Endangered Species

Introduction

Choices between species preservation and development are complicated by potential irreversibility and significant uncertainty (Norton and Toman 1997). The safe minimum standard (SMS) is a decision rule to preserve a renewable resource, unless the social costs of doing so are somehow intolerable. While unpersuasive to many, support for the SMS has been advocated by some economists and policy analysts for settings involving irreversibility and a high degree of uncertainty. A persistent question, at least among some environmental and resource economists as well as other scholars, is whether this potential irreversibility and significant uncertainty justify extraordinary decision processes, such as advocacy of safe minimum standard (SMS) approaches, relative to more standard benefit-cost or utilitarian analyses (Ciriacy-Wantrup 1968; Bishop 1978; Randall and Farmer 1995; Randall 1991 and 2007). Similarly, considerable uncertainty around possible worst-case scenarios involving global warming have prompted some to question whether precautionary approaches might be preferred to more traditional discounted present value approaches to decision making (e.g., Tol 2003; Cole 2008).

The relative merits of SMS-type approaches as preferred decision-making criteria, versus more standard utilitarian approaches, have been argued from game theoretic analyses (e.g., Palmini 1999), pluralistic moral philosophy perspectives (Randall and Farmer 1995), or from the legal theory of trusts (Scott 1999). Randall (2007) argues that there may be no single rationale for supporting an SMS approach. The SMS may or may not command consensus in society (Atkinson et al. 2007, p. 6), but from any perspective, many economists and other analysts have found arguments in support of SMS approaches unpersuasive (e.g., see Margolis and Naevdal 2008).

The objective of this paper is to explore decision-making involving species preservation versus development within an experimental laboratory setting, and involving a particular type of uncertainty (known payoffs, but with uncertain probabilities of events). This investigation implements a number of prior game-theoretic investigations of the SMS (Bishop 1978; Ready and Bishop 1991), involving "lottery" or "insurance" games against nature, respectively. Further, we parameterize the basic species preservation versus development dynamic game of Palmini (1999) with corresponding regret conditions of making a wrong choice into an experimental framework.

The experimental design also includes treatments varying: (i) information about the species; and (ii) and the imposed maximum regret condition (regret associated with making a wrong choice). Multiple sessions of the experiments were run with university student subjects in the US and Mexico. As part of each session, time (e.g., Coller and Williams 1999) and risk preferences (e.g., Holt and Laury 2002) were elicited, as both may affect choices (e.g., Anderson et al. 2008) between preservation and development. Finally, we also allow the subjects to exhibit heterogeneity in their tastes for preservation.

Econometric results from a random parameters logit (RPL) model, which allows for individual heterogeneity, demonstrate how support for the SMS preservation choice varies significantly depending on the format of the game against nature, the imposed maximum regret condition, the degree of risk aversion as well as both cultural and individuating factors. While the evidence might be interpreted as supporting the plausibility of SMS-type policy choices, with an underlying minimax-regret rationale or logic, it is also clear that such support is neither absolute nor necessarily commanding of a consensus or majority vote, and might be highly context dependent.

Background Literature

Species Preservation and Safe Minimum Standard Approach

Species have value to society for many reasons including: the possibly that they contain a substance that eventually leads to a cure for a disease; the recognition of their importance as a critical part of an ecosystem and connections to biodiversity; and the current or future values they have from harvest, or for passive or extractive recreation (wildlife viewing, hunting). It has been estimated that one-third of medical prescriptions given out annually in the United States are based on substances derived from nature or 'synthesized in imitation of natural substances' (Lovejoy 1993), and that one-quarter of the drugs marketed in the United States contain active ingredients derived from plants (see Farnsworth 1990; Brewer 1993; National Wildlife Federation 2006, and U.S. Fish and Wildlife Service 2005). Values for many species go beyond use-only related values (Boyle and Bishop 1987; Loomis and Ekstrand 1997).

Real uncertainty relating to the net benefits of developing habitat, or from preserving species, arises for many possible reasons. In the case of development, a developer may not know her benefits from markets that evolve after a development decision occurs, especially when the time it takes to fully develop a project is lengthy. For preserving species uncertainty may arise from the: (i) nature of diseases (current and future unknown ones); (ii) timing of cures, extraction; (iii) future prices and values related to harvest, or development of the habitat; (iv) discovery of substitute resources; and (v) exact knowledge of population and population dynamics.

Economists have used contingent valuation, and a variety of other stated preference approaches, to estimate the value of protecting endangered and at-risk species (e.g. Boyle and Bishop 1987; Loomis and Ekstrand 1997). For example, in an early study of connections between species preservation and information, Samples et al. (1986) find that the species' characteristics and status as endangered as well as features of a proposed investment program influence willingness-to-pay (WTP) for preservation. In their heavily-cited, original meta-analysis of studies valuing rare and endangered species, Loomis and White (1996) find that the annual maximum WTP for 18 species ranges from $6 to $95 per household. More recently, Richardson and Loomis (2009) have updated the original meta-analysis (Loomis and White 1996) of threatened, rare and endangered species. Both the original and the updated meta-analyses present evidence of systematic information about the social benefits of species preservation. However, both Loomis and White (1996) and Richardson and Loomis (2009) stopped short of endorsing the use of such values in strict benefit-cost decision rules, and instead supported SMS approaches in collective choice rules to protect endangered species.

The SMS was first proposed by Ciriacy-Wantrup in 1952 (see 1968 – a later edition reference) as a pragmatic policy tool for protecting critical natural resources, at risk of extinction, rather than any attempt to extend the theory of optimal social choice (Castle and Berrens 1993; Berrens 2001). The intent was to develop a pragmatic tool for collective choice in the face of uncertainty, limited scientific information, and irreversible losses (Castle and Berrens 1993; Castle 1996). However, there have been a number of attempts to operationalize the SMS in game-theoretic terms.

Bishop (1978) originally presented a simple game against nature, referred to as the "insurance game" where society is uncertain whether or not a disease will occur. The cure for the disease is known to be found in a natural species, but there is true uncertainty about whether the disease will occur. Society's mutually exclusive choices are either species preservation or development (with the irreversible loss of the species and the cure). If society follows a minimax decision rule, and minimizes the maximum possible losses, then the preservation strategy would be chosen.[1] Bishop (1978) proposed a "modified minimax rule" that recognized the opportunity costs of foregone development and argued for the SMS, unless the social costs of doing so were "unacceptably large."

Ready and Bishop (1991) presented an alternative to the insurance game known as the "lottery game" where there is certainty that a disease will occur but uncertainty in whether the cure will be found in a natural species. In this alternative game, the minimax decision rule does not lead to the choice of the preservation strategy.

In comparing the insurance and lottery games, Ready and Bishop (1991) argued that the predictions of game theoretic models are ambiguous because results are highly sensitive to the framing of the game against nature. They concluded that the SMS may be without rigorous theoretical foundations, and yet still yield the "right" societal choice. The implication is that support for the SMS approach can only be based on appeals to moral arguments or value judgments.

Nevertheless, Ready and Bishop (1991) did note (p. 311) that a minimax regret criterion (selecting a strategy that minimizes the maximum possible regret of the wrong choice) would support the SMS preservation choice in both game types (insurance or

[1] The mini-max rule is in turn based on early work by Milnor (1964), who suggests that in many cases of risk or uncertainty society wants to minimize the worst that can happen. [In contrast, a maxi-min strategy is one where society tries to get the largest benefits, assuming the worst payoff – the minimum – is drawn.]

lottery). Palmini (1999) pursued this and theoretically showed that the minimax regret criterion provides unambiguous support for the SMS strategy in both types of games in species preservation choices.

Regret can be thought of as the welfare loss from choosing an action, then realizing an alternative outcome. Under the minimax regret (MMR) criterion, society chooses the alternative that costs it the least reduction in welfare if the wrong choice is made. In the lottery game, if society chooses development and a cure was indeed available, it would have foregone a cure for a possibly disastrous disease. Alternatively, the cost of choosing preservation (and then not observing a cure) would be foregone development benefits minus any preservation benefits (e.g., non-market amenities). The preferred choice will depend on assumptions embedded in the game about the relative sizes of the payoffs of different outcomes. But Ciriacy-Wantrup (1968) expected that the forsaken development benefits would not typically exceed the benefits of preservation, and there are some endangered species BCA studies that would support this (e.g., Rubin et al. 1991; Hagen et al. 1992; see Castle and Berrens 1993).

Palmini (1999) develops a schematic as a dynamic game between society and nature, which combines both the insurance and lottery games (and thus combines both types of uncertainty in the mutually exclusive choices of preservation or development). Nature rolls the dice, and the outcomes are determined, and hence, society plays a game against nature. In reality, the probabilities of events are very often unknown, but the payoffs are known. In his game-theoretic presentation, Palmini (1999) concludes that a MMR decision rule yields a consistent outcome for preservation. With the full opportunity cost and regret included in the decision, he theoretically establishes a rational choice criterion for a risk-averse society.

Simply stated, in situations of considerable uncertainty and potential irreversible loss, the SMS approach requires that some safe minimum level of a renewable resource be protected unless the social costs of doing so are somehow intolerable or unacceptably large. It can be viewed as a burden of proof switching device, favoring preservation actions, but providing no trump card to preservation (Randall and Farmer 1995). The determination of what constitutes intolerable is made through the political or administrative process in any particular case (Castle 1996). Such vagueness has caused many critics to dismiss the SMS approach. Determination of intolerable costs through a political process has been likened to the exemption and "God Squad" provision in the U.S. Endangered Species Act (ESA) of 1973, as amended (Berrens 2001). As such, a number of authors have roughly equated the logic of the SMS, preserve a species unless the social costs of doing so are somehow intolerable (Bishop 1978), to preservation policies like the ESA (Bishop 1980; Castle and Berrens 1993; Woodward and Bishop 1997; Bishop and Woodward 2000, etc.).

Drawing from earlier research on axiomatic rational choice models, Woodward and Bishop (1997) argued that in cases of true Knightian uncertainty, decision makers may attach additional weight to avoiding worst case scenarios. There has been a long period of argument about definitions of uncertainty (e.g. LeRoy and Singell 1987), but here maintain that uncertainty means unknown probabilities (e.g., Knight 1921). In contrast, in Woodward and Bishop's "expert panel problem" there is disagreement within a panel of experts about some scientific question (e.g., the probability of extinction, or an extreme climate event). Here the experts may believe they know the probabilities, but disagree, leading the public to experience ambiguity.

In decision settings that involve large and irreversible outcomes, and with no meaningful way to assign probabilities, it might be quite reasonable to focus on the endpoints of the outcome space. The implication is that it might be rational for policy decision makers to adopt precautionary approaches (see Randall 2009; and Weitzman 2007 and 2009), including the SMS (Arrow et al. 2000). Woodward and Bishop (1997) explicitly identified the SMS as such an approach, and endangered species protection under the ESA as a relevant policy setting (Berrens 2001).

Species preservation (versus development) decisions and their relationship to like the ESA of 1973, as amended (in the US), are controversial, and often difficult to explore empirically (for exceptions see: List et al. 2006; Berrens et al. 1998 and 1999; Greenstone and Gayer 2009 and Ferraro et al. 2007). Further, Randall (2007) notes that the ESA, even as amended, may still fall short of the intent of the SMS approach in being largely reactive or late in the game, rather than representing a truly proactive safety approach. That is, the SMS should be implemented as an early warning trigger to help keep the costs of preservation tolerably low (Farmer and Randall 1998; Randall 2007). Here, we are most interested in empirically exploring the SMS approach and consideration of information and uncertainty; that is, we are not trying to draw judgment on a complex piece of legislation (e.g., the ESA) and its implementation. Rather, the results of our study will help identify factors that explain individual preferences for an SMS approach, including different levels of social costs of preservation.

The Experimental Design

Our full experimental design includes: (i) the insurance game of Bishop (1978); (ii) the lottery game of Ready and Bishop (1991); and (iii) the combined dynamic game against nature of Palmini (1999). A simplified form of Palmini's (1999) extensive, dynamic overall game with corresponding regrets matrix is shown in Figure 1 (extrapolated from Goodstein 2008).

For preservation benefits, Palmini assumes that all of the benefits are lumped together as hunting, viewing, etc. These are captured in B_{pres} (benefits of preservation). But the assumption is that if society chooses to develop, they will lose the future B_{pres}, so there is implied discounting (if future benefits are to be discounted at all). In his game, Palmini (1999) maintains the following assumptions: (1) the benefits of development are greater than the benefits of preservation, $B_{dev} > B_{pres}$; and (2) that the cost of the disease, $C_{disease}$, will be extremely severe such that $C_{disease} >> B_{dev}$. Because benefits and costs occur over time, all figures represent present-valued amounts.

Formally, as noted in the earlier discussion, the rules that decision makers use to choose between preservation and development depend on whether they are engaged in a particular strategy. Palmini (1999) argues that in making a decision, the opportunity cost of a wrong choice is included in the decision-process. For example, suppose an individual would prefer to develop a unique, irreplaceable resource potentially containing a cure for a disease. In making this choice, the individual would consider the benefits of development minus foregone preservation benefits as well as well as the opportunity cost of making a wrong choice. When choosing development, the opportunity cost of a wrong choice includes the welfare losses from the disease if indeed a cure would have been available. Including the welfare loss from a wrong choice implies that individuals use a minimax-regret (MMR) decision rule (Palmini 1999, p.470).

In using a MMR decision rule, a comparison of regrets associated with the choices to preserve or develop is needed to determine which option yields the greatest regret so as to minimize that regret. Referring to the regret matrix associated with Figure 1, the maximum regret associated with the decision to develop arises if both a disease occurs and a cure could have been found; whereas the maximum regret for the decision to preserve arises when society in spite of preserving the resource still incurs the cost of the disease (disease and no cure). The optimal choice is to preserve when the maximum regret associated with the development choice *exceeds* the maximum regret associated with the preservation choice, or the following regret condition holds (Palmini 1999)**:**

$$\begin{aligned}&[(B_{dev} - C_{disease}) - B_{pres}] > [(B_{pres} - C_{disease}) - (B_{dev} - C_{disease})]\\&or\\&C_{disease} > 2\cdot(B_{dev} - B_{pres})\end{aligned} \quad . \qquad [1]$$

The full set of nine crossed treatments in our stylized experiments will implement the insurance, lottery and combined games, and will also vary the relationship between $C_{disease}$ versus $2\times(B_{dev} - B_{pres})$: (i) for $C_{\text{disease}} > 2\times(B_{dev} - B_{pres})$, maximum development regret exceeds the maximum preservation regret (labeled hereafter as “COSTGT”); (ii) for $C_{\text{disease}} = 2\times(B_{dev} - B_{pres})$, the regret is the same between preservation and development (labeled hereafter as “COSTEQUAL”); and (iii) for $C_{\text{disease}} < 2\times(B_{dev} - B_{pres})$, the maximum preservation regret exceeds the maximum development regret (labeled hereafter as “COSTLT”, and used as our reference case in the logit specifications). Table 1 presents dollar amounts used in the experiment to represent each of the relationships between the $C_{disease}$, B_{dev} and B_{pres}. While in any experiment there are questions of "parallelism," the amounts used were within the budget, allowed large differences in potential earnings, and attempted to characterize the essential SMS setting.

Experimental Methods

Subjects were college students recruited at two universities: the University of Nayarit (UAN) in Tepic, Mexico, and Weber State University (WSU) in Ogden, Utah. Nayarit is a Pacific coastal state with a number of unique migratory bird estuaries. Further, Nayarit is the poorest state in Mexico. We might expect these two groups of students to be different from one another culturally.

For recruitment all students were told that they would be participating in tasks with some actual monetary payoffs. All subjects were paid a fixed amount for showing up ($5 in USA and $7 pesos in Mexico), with the opportunity to earn more based on the choices they made in the experiment. At the beginning of the experiment, WSU students were told that they could earn between $11 and $50 (USD), while UAN students could earn between $21 and $90 (Pesos).

All earnings associated with the experiment exercises were couched in laboratory dollars (or pesos), where each participant would be paid 10% of laboratory earnings. Further, because a wrong choice in the SMS exercise could lead to a loss, each student was given an endowment ($100 laboratory US dollars and $280 laboratory pesos) to prevent them from losing money (i.e., earning a negative amount). By adjusting the US laboratory dollar amounts by wage rates in Nayarit (as compared to average wages rates in Ogden, UT) and current exchange rates, the earnings of the Mexican subjects were comparable to the earnings of the US subjects. The rounding of these amounts to the nearest tenth figure led to peso amounts that were 1.8 to 2.0 times more than the US dollar amounts.[2]

The primary purpose of the experimental design was to test support for the SMS, within the context of a minimax-regret decision rule. To aid the empirical analysis of SMS choices, we obtain two additional pieces of information from each individual participant: (i) the individual's risk attitude, and (2) their potential rate of discount. There is no reason to believe that all subjects would have the same risk attitude, same rate of discount, or the same preference for goods and services in the dimension of time. There is an expanding literature on ways to obtain both pieces of information in laboratory settings (e.g., see Andersen et al. 2008; Chetan et al. 2008), and empirically estimate risk and discount rates jointly. However, as will be seen below, because we use long time horizons the time elicitation format involved hypothetical choices, and only our risk preference elicitation involved real payoffs; thus, we cannot pursue the joint estimation that others have (e.g. Andersen et al. 2008).

The experiment was organized as follows: (1) subjects were first presented with an introduction about the experiment, instructions, and a practice exercise; (2) utilizing a split-sample approach, some subjects were provided detailed information on species and cures found in nature, while others are presented with limited information; (3) subjects faced a series of paired financial choices over three time horizons (1 year, 5 years and 100 years) (see Table 4); (4) subjects faced the series of risk pairs as shown in Table 2; (5) subjects participated in nine preservation versus development choice exercises (SMS

[2] At the time of the experiment, exchange rates were approximately $14.8 pesos per USD, and wage rates in Nayarit were approximately 1/8 of Ogden wage rates. For example, $100 USD would be presented in pesos as $190 (= $100 ÷ 8 × 14.8 = $185 pesos rounded up to $190 pesos). The calculation of the $280 laboratory peso endowment was based on a $100 laboratory USD endowment plus the $5 participation fee.

choices); and (6) subjects were asked a number of debriefing questions and socio-demographic/economic questions to help explain their choices in (3), (4) or (5).[3] The risk and time exercises are briefly discussed next.

Risk Exercises

We adopt the relatively simple multiple-price list (MPL) approach to elicit risk attitudes (e.g., see Table 2) following the formats used by several researchers (Holt and Laury 2002; Andersen et al. 2006 and 2008; Anderson and Mellor, 2008). A MPL exercise can be used to reveal whether a person is neutral, risk-averse, or risk-loving. In many, but not all experiments, subjects are paid for their risk choices, depending on the realization of outcomes. We pay subjects for one of the choices they make, i.e., for their answer on one of the rows in the MPL (Table 2), which is randomly chosen. To make the draw, in the presence of students, pre-counted candies (pink and white) were placed in an opaque bag (e.g., 90 white to correspond with a 90% probability and 10 pink to correspond to 10% probability), where one student then drew a candy to determine each students' earnings for the risk gamble.

Table 2 shows the risk pairs that each subject evaluates. The subjects are presented with two payments in each: Option A includes \$120 and \$90; and Option B includes \$210 and \$10. The chance of being paid the larger amount in either A or B increases from 1% to 100%, whereas the chance of being paid the smaller amount in either A or B decreases from 99% to 0%. The usual interpretation of choices is that if a person were to choose Option B in the first row, they would have to be especially risk loving, and if they were to choose Option A at the next to last row, they would be

[3] The full set of experiment materials is available at: http://faculty.weber.edu/tgrijalva/experimentalmaterialsforspeciespreservation/materials.htm

especially risk averse. For individuals who are risk neutral, the expectation is the usual one: i.e., that the subject will be indifferent between a gamble and an amount of money paid with certainty that corresponds to the expected outcome. Risk-averse individuals require a higher amount of payment under the gamble, and those who seek risk get utility from the gamble itself, and actually require less of a payment under the gamble than they would a payment with certainty.

Table 3 presents the expected value of each option and the frequency of choices made by the participants (both at WSU and UAN). The exercise is constructed such that expected earnings under A exceed those for B, up until the 7^{th} row, at which point expected earnings for Option A are less than those for Option B. A risk neutral person would switch where the expected earnings are approximately equal, which happens between rows 6 and 7. For Row 12 , one would hope no one would choose column A, but an odd response does allow identification of those who do not understand the exercise.

The mental process the subject uses may involve expected utility, or expected earnings calculations, where the subject does a rough calculation of expected earnings, and decides at what point to switch. There is no law of general human behavior, however, which dictates this will be so. Other possibilities are that a subject weighs larger risks of gains more or less heavily, or weighs low risks of gains more or less heavily than mathematics would dictate, all suggestive of an underlying probability weighting function (e.g. see discussion of probability weighting functions applied to environmental contexts in Shaw and Woodward 2008).

Time Preference Exercises

Benefits from development might take several years to be realized, but almost certainly these benefits would be viewed by players of our game as almost immediate, and accruing to themselves rather than to someone else in the future. In the real world decisions to develop critical, yet unprotected habitat depend on several factors, including concerns that potential designation as legally critical habitat under the ESA may impact the development timing decision (e.g., List et al. 2006).

The benefits from preservation may accrue to future generations. Individuals certainly may wish to provide goods for people other than themselves, although the strength of such altruistic behavior has been questioned (e.g. Laury and Taylor 2008). In any case, discount rates may be important determinants of choice involving trade-offs in the future. While this likely plays an important role in real-life applications, our SMS games provided immediate payments (i.e., present value amounts) at the conclusion of the experiment. As such, the opportunity cost of waiting was not a factor. However, recognizing that some participants may be thinking about preserving for future generations, as a first step, we explored the role of time preferences in our empirical analysis. If our SMS games did involve future payments to participants, a complicated model would need to be employed (e.g., Rosen 1988; McIntosh et al. 2007).

There are several ways of eliciting discount rates (see early efforts by Fuchs 1986; the extensive review of papers that report on efforts to elicit discount rates and theoretical and methodological concerns by Frederick et al. 2002), but most rely on providing individuals with money and time tradeoffs, leaving the researcher to make inferences about individual discount rates implied by those choices. Table 4 shows the trade-off pairs for a one year time horizon, using a $100 current payment, and varying annual, and effective annual interest rates. Because of the long time horizons, subjects are not paid upon realization of these outcomes, and this unfortunately makes this elicitation approach different from the risk task.

The pairs of choices are quite similar to the paired choices made in the risk exercise. Coller and Williams (1999) find that choices in the laboratory may actually be influenced by thoughts about real rates of return in the field and the possibility of earning a higher rate of return in the field than is offered in the laboratory. As such, subjects were presented with information about possible field rates of return. The subjects, even if a random single person is chosen as a winner to be paid, can take their earnings and invest them outside the laboratory, so this is a concern when real money is involved.

SMS Exercises

In regards to the SMS choices, the treatments consist of a presentation of several versions of the insurance, lottery, and combined games against nature. The subjects were presented first with a lottery game with disease certain, but a cure to be uncertain. No probabilities of diseases or cures were presented to the subjects to characterize risks. In the second treatment subjects played an insurance game with the disease being uncertain, but with insurance leading to a certain cure. In the last treatment subjects were presented with uncertainty for both the disease and the cure. In all games against nature, the costs of the disease ($C_{disease}$) and the benefits of development or preservation (B_{dev} and B_{pres}) varied to represent cases where $C_{disease}$ where either equal, greater than or less than $2\times(B_{dev} - B_{pres})$. The payoff matrix and the corresponding regrets matrix presented to the subjects reflects Figure 1. As in the risk tasks, students earned money based on one of their choices in the SMS exercises. Similarly, numbers 1 through 9 written on white ping-pong balls were placed in an opaque bag to determine which of the nine SMS exercises would be selected for the basis of earnings.

To determine outcomes and to introduce uncertainty for the SMS exercises, we used opaque bags containing pink and white pieces of candy. The students were told that a research assistant placed the candies in the bag and that no one knew the exact contents of each bag, including the instructor. Further, the students were explicitly told that: (1) an unknown total number of candies was in each bag (e.g., could be 2 or could be 1000); (2) an unknown number of each color of candy was in each bag; (3) there was at least one pink and one white candy in each bag; (4) a random student would be selected to choose the color associated with either a disease or a cure (e.g., cure = pink or white); and (5) a different random student would be selected to draw a piece of candy from the bag. Thus, it was hoped that students could see that we could not know what color any student would choose. Students were informed that at the conclusion of the experiment they could look inside each bag to confirm the integrity of the experimental design. Prior to conducting the final version of the experiment, a pretest was conducted at Utah State University using 8 students. Debriefing of the students indicated that 1 or 2 students did associate a 50-50 probability with the outcome, while a majority said that they felt uncertain. Further, these students indicated that they had no preconceived notion of a cure being associated with the color pink or white.

The full experimental design for this study also includes the following. First, we have two crossed split-sample treatments: (i) US versus Mexican university students; and (ii) absence or presence of additional information provided on species preservation benefits. The former tests for cultural differences across distinct populations, as found in some experimental studies (Cummings et al. 2009). The latter tests for information effects, as found in the species valuation literature (Samples et al. 1986). Then, each participant faces nine choices that vary by: (i) the game against nature (lottery, insurance, combined); and (ii) the relationship between the $C_{disease}$ and $2\times(B_{dev} - B_{pres})$ (see equation [1], which expresses the maximum regret condition of Palmini, 1999).

Econometric Methods and Results

As an initial investigation, a simple comparison of frequencies for the preservation choice across the nine treatment scenarios is presented in Table 6 (see Table 5 for variable definitions). The top panel in Table 6 is for the full sample, while the bottom panel is for a restricted sample of 34 participants (9 choices × 34 participants providing 302 observations) that are the most highly risk averse (measured by RISKHAT, the risk attitude prediction). For some authors (e.g., Bishop 1978; Palmini 1999), risk aversion is a key assumption for understanding SMS behavior.

All other things equal, one expects that the higher the $C_{disease}$ relative to $2\times(B_{dev} - B_{pres})$, the more likely one will be to preserve. Further, the maximum regret condition [1] (labeled COSTGT, where $C_{disease} > 2\times(B_{dev} - B_{pres})$) is imposed by Palmini (1999) for expecting the SMS preservation choice under a MMR strategy. Generally, the regret condition appears to clearly matter. Under the Palmini (1999) regret condition (see equation [1]), where development imposes the maximum regret, we observe a large majority (85%) of participants choosing the preservation choice, for *both* the lottery and the insurance games. There also appears to be general support, as predicted in the original Bishop (1978) lottery game work, for an SMS preservation strategy; but, the equivalence of 85% in the two game forms is otherwise unexpected (e.g., Ready and Bishop 1991), where the preservation choice would be expected to be less likely under the insurance game against nature. Thus, the Ready and Bishop (1991) rejection of the original Bishop (1978) argument based on the game against nature (location of the uncertainty) is not clear in the absence of any significant difference in support for the SMS choice. This equivalence might appear to be consistent with Palmini's (1999) argument for a MMR strategy (and see Bishop 1978, p. 311). However, when we move to the combined games, with both types of uncertainty present, we see the proportion choosing the preservation choice dropping significantly to less than a majority. Thus, even under the most favorable regret condition [1], majority support for the SMS preservation choice is not always observed.

What happens when we move to our more restrictive sample of the most risk-averse participants (and risk aversion is emphasized by Palmini 1999)? Here, a more than 80% majority is observed for both the lottery and insurance games, and a 56% (the average frequency of COSTGT and BOTHGTGT) majority is observed for the combined game form (Palmini 1999), edging the SMS preservation choice back over a majority.

Finally, as shown in Table 6, while there is sensitivity to the regret condition (COSTGT, BOTHGTGT, COSTEQUAL, COSTLG), this occurs primarily when the location of the maximum regret is reversed between development (COSTGT) and preservation (COSTLT). Some relaxing of the Palmini (1999) maximum regret condition in equation [1] required to support the SMS preservation choice is possible, as observed by the COSTEQUAL frequencies that are always relatively closer to the COSTGT condition frequencies. Given this initial picture of the simple frequency of choosing the preservation option, we turn to more detailed modeling, controlling for other factors.

To further explore preservation choices we estimate several variants of a random parameters logit (RPL) model to preserve or develop habitat. The RPL allows for taste heterogeneity in that each parameter estimate can be individual-specific, and is also quite general in allowing for patterns of correlations that can break the assumption of the independence of irrelevant alternatives (IIA). Because each subject answers three questions for three treatments, one would suspect the possibility of correlations over responses in the error term, presuming that the choices are identical in nature, or at least strongly similar (i.e. they address the choice for the same good). A panel approach is one alternative to dealing with possible correlation, but the RPL is another and richer in that it allows for correlations in the parameters, and heterogeneity in any parameter allowed to be random as opposed to fixed. Students are sometimes thought to be a fairly homogenous group of people, but in expressing preferences that relate to environmental trade-offs they might be quite different from one another. As will be seen, several of the explanatory variables have influences that indicate heterogeneity exists.

Four model specifications are explored. All model specifications are based on 1000 Halton draws and assuming a normal distribution of parameter estimates. Further, it is assumed that there are nine periods in the model to represent the nine choices made by each subject. Variable names and definitions are presented in Table 5. Model I includes the treatment variables in the experimental design only. Model II includes treatment variables plus a number of individuating and socio-economic characteristics (e.g., GENDER, AGE, saving for retirement (RETIREMENT), KIDS, and an income measure (LOG(USPPPY)), as well as a dummy variable indicating whether an individual believed the outcome of a disease or cure was represented by a 50-50 probability (FIFTY50). Model III includes all the variables from Model II plus a measure of risk aversion (RISKHAT).[4] Following prior work on the importance of risk aversion in supporting SMS preservation choices (Bishop 1978; Palmini 1999), we expect the sign on RISKHAT to be significant and positive. Lastly, Model IV includes all the variables included in Model III as well as the use of a binary measure of whether or not a participant exhibited a declining discount rate for longer time horizons, which is proxied by whether a subject chose a lower discount rate in the 100 year MPL than the 1 year MPL (referred to as DECLINERATE).

Tables 7A-7D present the results of the four RPL specifications. For each model specification the estimated coefficients for the set of explanatory variables, the estimated implied standard deviations to test for evidence of heterogeneous parameters, and the estimated marginal effects are included. A quick glance at each table shows that several variables have statistically significant standard deviations, supporting the use of the RPL (as opposed to the fixed parameter logit), and significant marginal effects.

Across all model specifications, many of the treatment variables are statistically significant and robust. The results show evidence of a cultural difference between the two university student samples. The estimated coefficient on MEX is negative and

---

[4] Choices in the risk exercises are used to estimate a modified exponential power (EP) function (e.g., Holt and Laury 2002; and Harrison and Rutsröm 2008). The EP function is given as:

$$v(x) = \frac{1 - \exp\left(-\alpha x^{1-r}\right)}{\alpha} \text{ for } x > 0,$$

where x represents the income from the experimental choice, or the experimental prize. The terms α and r are parameters to be estimated. See Harrison and Rutsröm 2008 for a full presentation of the utility function estimated by maximum likelihood methods. Using the results of this model, a term RISKHAT representing relative risk aversion ($= r + \alpha(1 - r)x^{(1-r)}$) is created and used in the RPL model. A complete description of the model and the results are available upon request from the lead author.

significant (at either the 0.01 or 0.05 levels). The information treatment (INFO) variable tests whether additional details about the species (e.g., Samples et al. 1986) in a contingent valuation context) affects the probability of choosing preservation. The results show that INFO does not appear to be a significant determinant of the preservation choice; however, the implied standard deviation of the random parameter on INFO is significant across all models, as evidence of a source of heterogeneous preferences.

Consistent with the evidence in Table 6, across all model specifications, both the game form (location of the uncertainty, i.e., disease or cure) and the imposed regret condition (Palmini 1999) clearly affect the preservation versus development choice. Relative to the residual category of the combined game (Palmini 1999) with both types of uncertainty, participants are significantly more likely to choose the SMS preservation choice under both the lottery (LOTTERY) and insurance (INSURANCE) games. Further, participants are significantly more likely to choose preservation, under the case when development maximizes regret (COSTGT), and the case where the maximum regret between the preservation and development are equal (COSTEQUAL), relative to the residual category where preservation maximizes regret (COSTLT). In the game that contained uncertainty in both the disease happening and whether a cure would be found, a different cost relationship is additionally explored (BOTHGTGT). In this situation, the $C_{disease}$ is set to be twice the size of B_{dev} to determine whether the cost of a bad outcome can significantly offset the perhaps the ‘double’ uncertainty situation. The coefficient on BOTHGTGT is positive and significant at the 0.01 level.

Across Models II-IV, the coefficients on many of the individual or socioeconomic factors retain their signs and significance levels. For the full model specification (Model

IV) presented in Table 7D, the majority of coefficients in the model have either a significant estimated coefficient, or implied standard deviation, or both. (The exceptions are for INFO*MEX, KID, KID2, and DECLINERATE.)

The estimated coefficient for risk aversion (RISKHAT) is significant and positive, indicating that more risk averse individuals are more likely to support the SMS preservation choice (as expected, following Palmini 1999).[5] Further, there is evidence of heterogeneity in the significance of the implied standard deviation of the random parameter for RISKHAT. This is intuitive; the role that risk preference plays in making the choice to preserve may well be stronger or weaker for certain individuals. Across Models II-IV, the coefficient on FIFTY50 is positive and significant at the 0.01 level.

In terms of individuating, and socio-economic characteristics of the participants, the estimated coefficient on gender (MALE) is negative and significant (women are more likely to support preservation). The estimated coefficient on our income measure, LOG(USPPPY), for this sample of students, is negative and significant in Models III and IV, while our measure of far-sightedness, and wealth planning (RETIREMENT) is a significant positive determinant of preservation. Finally, in terms of the estimated implied standard deviations, MALE, LOG(USPPPY), and RETIREMENT are shown to be significant sources of heterogeneity in preservation preferences. Across all students, these factors vary across individuals in terms of their influence on the choice.

Conclusions and Future Research

The objective of this paper is to examine the support for SMS preservation choices, given our stylized experimental design (which builds on prior game theoretic

---

[5] Anderson and Mellor (2008) find that risk aversion measures from similar experimental risk elicitation trade-off designs are good predictors for a number of health-related field behaviors.

explorations of the SMS) and a particular type of uncertainty (known payoffs and unknown probabilities). While the evidence might be interpreted as supporting at least the plausibility of SMS-type policy choices, with an underlying regret rationale, it is also clear from the results that such support is neither absolute nor necessarily commanding of a consensus or even majority vote, and might be highly context dependent. As to whether SMS-type approaches might be likely to garner broad public support, there are certainly a variety of ways this might be investigated (e.g., large public surveys, etc.).

Our modeling illustrates that there is heterogeneity in making the choice to preserve species habitat. There may well be other motives than minimizing maximum regret for making choices such as the ones faced by our subjects. For example, new explorations consider whether people simply try to avoid feelings of guilt when they make a decision (e.g. Li et al. 2008; Wubben et al. 2009).[6] We cannot rule this out, but such feelings would likely be more prevalent when decisions are publicly displayed, rather than private, as in our study. In this experimental case study, we find some mixed results, but significant heterogeneity. And despite our cultural, split-sample treatment (which turns out to be significant), our sample of university students is arguably more homogenous than a large general population sample that includes people of all ages.

Under imposed regret conditions in [1], we find that a large majority of participants would choose an SMS preservation option for both the lottery and insurance games. The SMS option would fail a majority-rule referendum when the two types of uncertainty are combined in the dynamic game form (unless the $C_{disease}$ are perhaps significantly great), although we do observe a small majority in the most highly risk

[6] We thank Jay Shogren for drawing our attention to this emerging literature on guilt, but we were not aware of it at the time of study design.

averse of our sample. Support for the SMS is shown to be somewhat sensitive to the imposed regret condition in the game-theoretical developments; that is, we can relax the regret condition somewhat, but not too much. Support for the SMS-type preservation option is also shown to be affected by a number of individuating factors, such as the degree of risk aversion, gender, culture/geographic location, and a number of socio-economic variables. Finally, there is significant evidence of heterogeneous preferences for the SMS preservation choice in our RPL model. While it might be argued that under certain conditions an SMS approach might garner a significant consensus or even majority support, the evidence from this experimental investigation would appear to indicate that such a result is highly sensitive to some identifiable factors.

Randall and Farmer (1995) state that society will not always reach consensus concerning difficult preservation choices, but argue that when consensus does emerge it will often involve an SMS-type approach. Theoretical arguments for SMS-type approaches may or may not be persuasive to many economists and policy analysts (see Gollier et al. 2000; Gollier and Triech 2003), and as shown here, may or may not be able to capture majority public support, but our initial experimental results add to an understanding of what likely affects public support, and further add to the debate over how important extreme/catastrophic outcomes (e.g., as expressed here in our imposed regret condition) should be counted in social choices with a high degree of uncertainty (e.g., Randall 2009; Weitzman 2009). That is, there may need to focus more attention in future research on extreme outcomes (of even a greater magnitude than parameterized in this experiment) and possible regret scenarios for significant environmental problems that involve potential irreversibility and uncertainty.

Table 1: Values of $C_{disease}$, B_{dev}, and B_{pres}

	Amount
BOTHGTGT: $C_{disease} = 2 \times B_{dev}$	
$C_{disease}$	\$200
B_{dev}	\$100
B_{pres}	\$40
COSTGT: $C_{disease} > 2 \times (B_{dev} - B_{pres})$	
$C_{disease}$	\$150
B_{dev}	\$100
B_{pres}	\$40
COSTEQ: $C_{disease} = 2 \times (B_{dev} - B_{pres})$	
$C_{disease}$	\$120
B_{dev}	\$100
B_{pres}	\$40
COSTLT: $C_{disease} < 2 \times (B_{dev} - B_{pres})$	
$C_{disease}$	\$100
B_{dev}	\$100
B_{pres}	\$40

Table 2: Risk Tradeoff Table (English version)

	Indicate A or B Below	Option A	Option B
1	**A B**	a 1% chance of earning $120 and a 99% chance of earning $90	a 1% chance of earning $210 and a 99% chance of earning $10
2	**A B**	a 5% chance of earning $120 and a 95% chance of earning $90	a 5% chance of earning $210 and a 95% chance of earning $10
3	**A B**	a 10% chance of earning $120 and a 90% chance of earning $90	a 10% chance of earning $210 and a 90% chance of earning $10
4	**A B**	a 20% chance of earning $120 and a 80% chance of earning $90	a 20% chance of earning $210 and a 80% chance of earning $10
5	**A B**	a 30% chance of earning $120 and a 70% chance of earning $90	a 30% chance of earning $210 and a 70% chance of earning $10
6	**A B**	a 40% chance of earning $120 and a 60% chance of earning $90	a 40% chance of earning $210 and a 60% chance of earning $10
7	**A B**	a 50% chance of earning $120 and a 50% chance of earning $90	a 50% chance of earning $210 and a 50% chance of earning $10
8	**A B**	a 60% chance of earning $120 and a 40% chance of earning $90	a 60% chance of earning $210 and a 40% chance of earning $10
9	**A B**	a 70% chance of earning $120 and a 30% chance of earning $90	a 70% chance of earning $210 and a 30% chance of earning $10
10	**A B**	a 80% chance of earning $120 and a 20% chance of earning $90	a 80% chance of earning $210 and a 20% chance of earning $10
11	**A B**	a 90% chance of earning $120 and a 10% chance of earning $90	a 90% chance of earning $210 and a 10% chance of earning $10
12	**A B**	a 100% chance of earning $120	a 100% chance of earning $210

Table 3: Expected Values and Frequency of Choices from Risk Tradeoff exercise (based on the first line that Option B was selected)

	English Version ($ USD)		**Spanish Version ($ pesos)**	
	Expected Value of Option A and (Percentage Choosing Option A)	Expected Value of Option B and (Percentage Choosing Option B)	Expected Value of Option A and (Percentage Choosing Option A)	Expected Value of Option B and (Percentage Choosing Option B)
1	$90.30 (87%)	$12.00 (13%)	$170.50 (96%)	$23.70 (4%)
2	$91.50 (77%)	$20.00 (23%)	$172.50 (96%)	$38.50 (4%)
3	$93.00 (69%)	$30.00 (31%)	$175.00 (91%)	$57.00 (9%)
4	$96.00 (65%)	$50.00 (35%)	$180.00 (87%)	$94.00 (13%)
5	$99.00 (55%)	$70.00 (45%)	$185.00 (84%)	$131.00 (16%)
6	$102.00 44%)	$90.00 (56%)	$190.00 (71%)	$168.00 (29%)
7	$105.00 (23%)	$110.00 (77%)	$195.00 (53%)	$205.00 (47%)
8	$108.00 (18%)	$130.00 (82%)	$200.00 (36%)	$242.00 (64%)
9	$111.00 (11%)	$150.00 (89%)	$205.00 (18%)	$279.00 (82%)
10	$114.00 (8%)	$170.00 (92%)	$210.00 (7%)	$316.00 (93%)
11	$117.00 (5%)	$190.00 (95%)	$215.00 (4%)	$353.00 (96%)
12	$120.00 (0%)	$210.00 (100%)	$220.00 (0%)	$390.00 (100%)

Table 4: One Year Time Preference Table

Payoff alternative	Option A (Pays amount shown now)	Option B (Pays amount shown in 1 year)	Annual interest rate	Effective annual interest rate	Preferred Option (circle A or B)	
1	$100	$101.00	1%	1.0025%	**A**	**B**
2	100	102.01	2%	2.0100%	**A**	**B**
3	100	103.02	3%	3.0225%	**A**	**B**
4	100	105.06	5%	5.0625%	**A**	**B**
5	100	110.25	10%	10.2500%	**A**	**B**
6	100	115.56	15%	15.5625%	**A**	**B**
7	100	121.00	20%	21.0000%	**A**	**B**
8	100	126.56	25%	26.5625%	**A**	**B**
9	100	132.25	30%	32.2500%	**A**	**B**
10	100	138.06	35%	38.0625%	**A**	**B**
11	100	144.00	40%	44.0000%	**A**	**B**
12	100	150.00	45%	50.0625%	**A**	**B**
13	100	156.25	50%	56.2500%	**A**	**B**

Note: a similar layout was used for 5 years and 100 years.

Table 5 Estimating Variable names, definitions and mean or frequencies

Variable Name	Definition	Mean/frequency
MEX	Dummy variable (DV)* for subject from Mexico	0.47
LOTTERY	DV indicator of lottery game	0.33
INSURANCE	DV Indicator of insurance game	0.33
INFO	DV Indicator of the version with more information	0.50
INFO*MEX	MEX and INFO interaction	0.24
COSTGT	DV - Disease cost is more than 2 times greater than development <u>net</u> benefits (NB = benefits of development – benefits of preservation).	0.27
COSTEQUAL	DV- Disease cost equal to development <u>net</u> benefits	0.33
BOTHGTGT	DV – Cost of the disease is two times greater than Development benefits and applied only to the situation when <u>both</u> a cure and disease are uncertain.	0.06
AGE	Age of subject	24
RETIREMENT	DV indicating that respondent has a retirement account	0.10
KIDS	Children in household	0.27
$KIDS^2$	KIDS squared for quadratic specification	0.77
LOG(USPPPY)	Log of household income in U.S. PPP dollars (2009)	2.77
MALE	DV indicating male gender	0.46
FIFTY50	DV indicating respondent believed the probability of a disease or a cure was 50%.	0.44
DECLINERATE	DV indicating respondent's demonstrated a lower discount rate for longer time horizons	0.49

Dummy variable = 1 for category, = 0 otherwise. All means based on all 117 subjects.

Table 6: Frequencies of Choices to Preserve across the Experimental Design Cells

Full Sample			
	Lottery (disease certain, cure uncertain)	Insurance (disease uncertain, cure certain)	Both (disease and cure both uncertain)
BOTHGTGT: $C_{disease} = 2 \times B_{dev}$	—	—	45%[a]
COSTGT: $C_{disease} > 2 \times (B_{dev} – B_{pres})$	85%	85%	40%[a]
COSTEQUAL: $C_{disease} = 2 \times (B_{dev} – B_{pres})$	66%	77%	36%
COSTLT: $C_{disease} < 2 \times (B_{dev} – B_{pres})$	39%	43%	27%
* Percentage of 1053 responses for 117 subjects; 55 had COSTGT and 62 had BOTHGTGT			

[a] When both the disease and cure were uncertain, a total of 62 students faced an increase in the $C_{disease}$ to be twice the size of development benefits; this is represented by BOTHGTGT. The average frequency of choosing PRESERVE of COSTGT and BOTHGTGT when both the disease and cure are uncertain is 43%.

ONLY THOSE WITH RISKHAT[a] >= MEAN			
	Lottery (disease certain, cure uncertain)	Insurance (disease uncertain, cure certain)	Both (disease and cure both uncertain)
BOTHGTGT: $C_{disease} = 2 \times B_{dev}$	—	—	62%[b]
COSTGT: $C_{disease} > 2 \times (B_{dev} – B_{pres})$	82%	85%	46%[b]

COSTEQUAL: $C_{disease} = 2 \times (B_{dev} - B_{pres})$	68%	71%	32%
COSTLT: $C_{disease} < 2 \times (B_{dev} - B_{pres})$	41%	53%	12%
* Percentage of 306 responses for 34 subjects; 13 had COSTGT and 21 had BOTHGTGT.			

[a] RISKHAT is estimated from the model examining risk preferences. A larger estimate for RISKHAT indicates greater risk aversion.
[b] When both the disease and cure were uncertain, a total of 62 students faced an increase in the $C_{disease}$ to be twice the size of development benefits; this is represented by BOTHGTGT. Of these 62, 21 had a RISKHAT prediction that was greater than the mean. The average frequency of choosing PRESERVE of COSTGT and BOTHGTGT when both the disease and cure are uncertain is 56%.

Table 7A: Results of Random Parameters Logit (Preserve = 1) for Panel Data, Model I

	Mean parameter estimate (t-ratio)	Implied standard deviations of random parameters (t-ratio)	Marginal effects (t-ratio)
CONSTANT	–1.11***	0.77***	–0.27***
	(–12.876)	(15.61)	(–12.95)
MEX	–0.40***	0.12	–0.10***
	(–4.10)	(1.54)	(–4.10)
INFO	0.05	0.60***	0.01
	(0.52)	(7.82)	(0.52)
INFO*MEX	0.12	0.84***	0.03
	(0.85)	(7.51)	(0.85)
COSTGT	1.52***	0.82***	0.37***
	(17.97)	(7.12)	(18.00)
COSTEQUAL	0.87***	0.01	0.21***
	(11.69)	(0.12)	(11.72)
LOTTERY	1.00***	0.07	0.25***
	(12.69)	(0.77)	(12.73)
INSURANCE	1.20***	0.31***	0.30***
	(15.52)	(3.30)	(15.59)
BOTHGTGT	0.93***	0.24	0.23***
	(6.77)	(1.21)	(6.81)
LnL		–601.72	
Π^2 (restricted model is Logit)		45.16 (p-value = 0.00)	

Table 7B: Results of Random Parameters Logit (Preserve = 1) for Panel Data, Model II

	Mean parameter estimate (t-ratio)	Implied standard deviations of random parameters (t-ratio)	Marginal effects (t-ratio)
CONSTANT	–0.98***	0.39***	–0.24***
	(–6.74)	(7.37)	(–6.78)
MEX	–0.22**	0.38***	–0.05**
	(–2.23)	(5.06)	(–2.23)
INFO	0.07	0.56***	0.02
	(0.76)	(7.29)	(0.76)
INFO*MEX	0.10	0.31***	0.02
	(0.68)	(2.80)	(0.68)
COSTGT	1.53***	0.92***	0.38***
	(17.53)	(7.89)	(17.53)
COSTEQUAL	0.87***	0.01	0.21***
	(11.54)	(0.05)	(11.56)
LOTTERY	1.01***	0.13	0.25***
	(12.68)	(1.39)	(12.68)
INSURANCE	1.23***	0.17*	0.30***
	(15.84)	(1.75)	(15.92)
BOTHGTGT	0.81***	1.58***	0.20***
	(4.82)	(5.26)	(4.83)
MALE	–0.51***	0.02	–0.13***
	(–6.73)	(0.20)	(–6.73)
KIDS	–0.12	0.03	–0.03
	(–0.90)	(0.41)	(–0.90)
$KIDS^2$	0.01	0.01	0.01
	(0.36)	(0.44)	(0.36)
RETIREMENT	0.54***	1.22***	0.13***
	(3.74)	(6.31)	(3.74)
LOG(USPPPY)	-0.06	0.18***	–0.01
	(-1.45)	(9.82)	(–1.45)
FIFTY50	0.32***	0.05	0.08***
	(4.45)	(0.60)	(4.53)
LnL		–582.50	
Π^2 (restricted model is Logit)		37.37 (p-value = 0.00)	

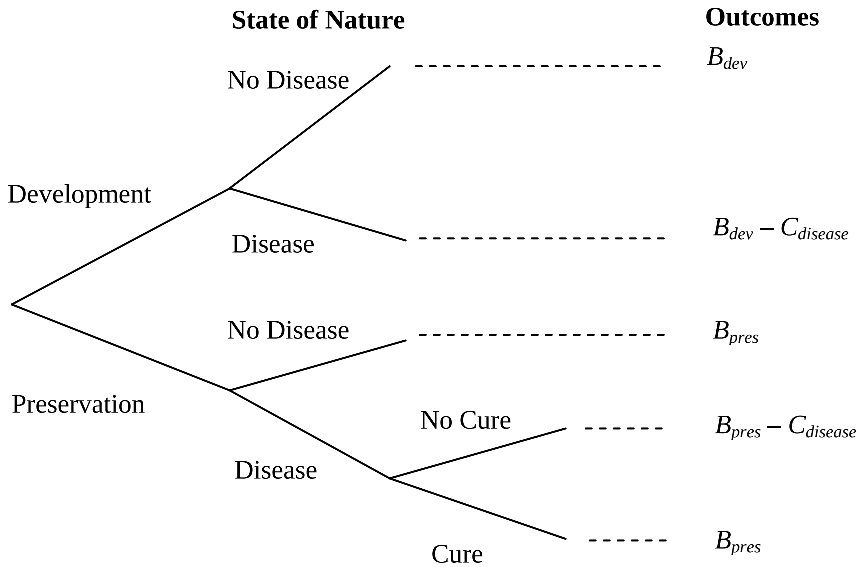
Table 7C: Results of Random Parameters Logit (Preserve = 1) for Panel Data, Model III

	Mean parameter estimate (t-ratio)	Implied standard deviations of random parameters (t-ratio)	Marginal effects (t-ratio)
CONSTANT	–5.06***	0.05	–1.25***
	(–2.64)	(1.01)	(–2.64)
MEX	–0.22**	0.15**	–0.05**
	(–2.15)	(2.00)	(–2.15)
INFO	0.08	0.47***	0.02
	(0.79)	(6.15)	(0.79)
INFO*MEX	0.08	0.13	0.02
	(0.58)	(1.22)	(0.58)
COSTGT	1.53***	0.90***	0.38***
	(17.54)	(7.72)	(17.55)
COSTEQUAL	0.87***	0.01	0.21***
	(11.55)	(0.07)	(11.57)
LOTTERY	1.01***	0.07	0.25***
	(12.68)	(0.76)	(12.68)
INSURANCE	1.23***	0.09	0.30***
	(15.90	(0.98)	(15.95)
BOTHGTGT	0.79***	1.91***	0.19***
	(4.45)	(5.60)	(4.45)
MALE	–0.49***	0.01	–0.12***
	(–6.39)	(0.11)	(–6.39)
KIDS	–0.24	0.10	–0.06
	(–1.60)	(1.59)	(–1.60)
KIDS^2	0.03	0.01	0.01
	0.88)	(0.17)	(0.88)
RETIREMENT	0.51***	1.64***	0.13***
	(3.47)	(7.66)	(3.47)
LOG(USPPPY)	–0.10**	0.08***	–0.02**
	(–2.23)	(4.49)	(–2.23)
FIFTY50	0.34***	0.02	0.08***
	(4.68)	(0.22)	(4.77)
RISKHAT	0.09**	0.01***	0.02**
	(2.16)	(12.33)	(2.16)
LnL		–582.01	
Π^2 (restricted model is Logit)		38.35 (p-value = 0.00)	

Table 7D: Results of Random Parameters Logit (Preserve = 1) for Panel Data, Model IV

	Mean parameter estimate (t-ratio)	Implied standard deviations of random parameters (t-ratio)	Marginal effects (t-ratio)
CONSTANT	–4.90***	0.01	–1.21***
	(–2.46)	(0.03)	(–2.46)
MEX	–0.22**	0.16**	–0.05**
	(–2.17)	(2.05)	(–2.17)
INFO	0.06	0.54***	0.02
	(0.59)	(6.89)	(0.59)
INFO*MEX	0.09	0.14	0.02
	(0.63)	(1.26)	(0.63)
COSTGT	1.53***	0.92***	0.38***
	(17.53)	(7.89)	(17.53)
COSTEQUAL	0.87***	0.01	0.22***
	(11.58)	(0.06)	(11.59)
LOTTERY	1.02***	0.04	0.25***
	(12.66)	(0.44)	(12.66)
INSURANCE	1.24***	0.23**	0.31***
	(15.84	(2.41)	(15.88)
BOTHGTGT	0.74***	2.26***	0.18***
	(3.98)	(5.80)	(3.98)
MALE	–0.52***	0.02	–0.13***
	(–6.60)	(0.30)	(–6.60)
KIDS	–0.23	0.10	–0.06
	(–1.58)	(1.58)	(–1.58)
$KIDS^2$	0.03	0.01	0.01
	0.76)	(0.16)	(0.76)
RETIREMENT	0.54***	1.45***	0.13***
	(3.63)	(7.03)	(3.62)
LOG(USPPPY)	–0.11**	0.04**	–0.03**
	(–2.49)	(1.95)	(–2.49)
FIFTY50	0.32***	0.06	0.08***
	(4.38)	(0.76)	(4.45)
RISKHAT	0.09**	0.01***	0.02**
	(2.00)	(12.11)	(2.01)
DECLINERATE	0.08	0.27***	0.02
	(1.01)	(3.38)	(1.01)
LnL		–581.61	
Π^2 (restricted model is Logit)		37.02 (p-value = 0.00)	

Figure 1: Game Against Nature



Note: B_{dev}, B_{pres}, and $C_{disease}$ represent the benefits of development, the benefits of preservation, and the cost of the disease, respectively.

The Regrets Matrix

	No Disease	**Disease/Cure**	**Disease/No Cure**
Development	$B_{dev} – B_{pres}$	$(B_{dev} – C_{disease}) – B_{pres}$	$(B_{dev} – C_{disease}) – (B_{pres} – C_{disease})$
Preservation	$B_{pres} – B_{dev}$	$B_{pres} – (B_{dev} – C_{disease})$	$(B_{pres} – C_{disease}) – (B_{dev} – C_{disease})$

(Cells in gray are the maximum in absolute values)
Source: Goodstein, 2008 (adapted from Palmini, 1999)

References

Anderson L, Mellor J (2008) Predicting health behaviors with experimental measures of risk preference. Journal of Health Economics 27:1260-1274.

Andersen S, Harrison G W, Lau M I, Rutström E E (2006) Elicitation using Multiple Price List formats. Experimental Economics 9: 383-405.

Andersen S, Harrison G W, Lau M, Rutström E (2008) Eliciting Risk and Time Preferences. Econometrica 76: 583-618.

Arrow K, Daily G, Dasgupta P, Levin S, Maler K G, Maskin E, Starrett D, Sterner T, Tietenberg T (2000) Managing ecosystem services. Environmental Science and Technology 34:1401-1406.

Atkinson G, Dietz S, Neumayer E (2007) Introduction. In: Atkinson G, Dietz S, Neumayer E (Eds) Handbook of Sustainable Development. Edward Elgar, Cheltenham UK, p 1-26.

Berrens R (2001) The safe minimum standard of conservation and endangered species: a review. Environmental Conservation 28(2):104-116.

Berrens R, Brookshire D, McKee M, Schmidt C (1998) Implementing the safe minimum standard approach: Two case studies from the U.S. Endangered Species Act. Land Economics 74:147-161.

Berrens R, McKee M, Farmer M (1999) Incorporating distributional considerations in the safe minimum standard approach: endangered species and local impacts. Ecological Economics 30:461-474.

Bishop R (1978) Endangered species, irreversibility, and uncertainty: the economics of a safe minimum standard. American Journal of Agricultural Economics 60:10-18.

Bishop R (1980) Endangered species: An Economic Perspective. Transactions of the North American Wildlife Conference 45:208-218.

Boyle K J, Bishop R C (1987) Valuing Wildlife in Benefit-Cost Analyses: A Case Study Involving Endangered Species. Water Resources Research 23(5):943-50.

Brewer S J (1993) Search and discovery of plant-derived bioactive chemicals at Monsanto-Searle. Testimony to the Subcommittee on Environment and Natural Resources, Committee on Merchant Marine and Fisheries, US Congress, House of Representatives Ser. No. 103-74, 9 November 1993, p 35–41.

Castle E (1996) Pluralism and pragmatism in the pursuit of sustainable development. In: Adamowicz W, Boxall P, Luckert M, Phillips W, White W (Eds) Forestry, Economics and the Environment. CAB International, Wallingford UK, pp 1-9.

Castle E, Berrens R (1993) Economic analysis, endangered species, and the safe minimum standard. Northwest Environmental Journal 9:108-130.

Chesson H, Viscusi W K (2000) The Heterogeneity of Time-risk Tradeoffs. Journal of Behavioral Decision Making 13(2):251-58.

Chetan D, Eckel C, Johnson C, Rojas C (2008) Eliciting Risk Preferences: When is Simple Better? Discussion paper, U of Texas-Dallas.

Ciriacy-Wantrup S V (1968) Resource Conservation: Economics and Policies 3rd ed. (1st ed. 1952). University of California Press, Berkeley, CA.

Cole D (2008) The Stern Review and Its Critics: Implications for the Theory and Practice of Benefit Cost Analysis. Natural Resources Journal 48(1):53-90.

Coller M, Williams M B (1999) Eliciting Individual Discount Rates. Experimental Economics 2:107-27.

Cummings R, Martinez-Vazquez J, McKee M, Torgler B (2009) Tax morale affects tax compliance: Evidence from surveys and an artefactual field experiment. Journal of Economic Behavior and Organization 70:447-457.

Farmer M, Randall A (1998) The rationality of a safe minimum standard of conservation. Land Economics 74:287-302.

Farnsworth N R (1990) The role of ethnopharmacology in drug development. In: Chadwick D J, Marsh J (Eds.) Bioactive Compounds from Plants. Ciba Foundation Symposium 154, pp 2–11.

Ferraro P, McIntosh C, Opsina M (2007) The effectiveness of the US endangered species act: An econometric analysis using matching methods. Journal of Environmental Economics and Management 54(3):245-262.

Frederick S, Loewenstein G, O'Donoghue T (2002) Time Discounting and Time Preference: A Critical Review. Journal of Economic Literature XL: 351-401.

Fuchs V R (1986) Time Preference and Health. In: Fuchs V R (ed.) The Health Economy. Harvard University Press, Cambridge MA, pp 214-42.

Gilboa I, Schmeidler D (1989) Maxmin Expected Utility with a Non-Unique Prior. Journal of Mathematical Economics 18:141-53.

Gollier C, Treich N (2003) Decision-making Under Scientific Uncertainty: The Economics of the Precautionary Principle. Journal of Risk and Uncertainty 27(1): 77-103.

Gollier C, Jullien B, Treich N (2000) Scientific Progress and Irreversibility: an Economic Interpretation of the Precautionary Principle. Journal of Public Economics 75:229-53.

Goodstein, E (2008). Game Theory and the Safe Minimum Standard Appendix 7A. In: Economics and the Environment 5th edn. John Wiley & Sons, Hoboken NJ, p 139.

Greenstone M, Gayer, T (2009) Quasi-experimental and experimental approaches to environmental economics. Journal of Environmental Economics and Management 57:21-44.

Hagen D, Vincent J, Welle P (1992) Benefits of Preserving Old-Growth Forests and the Spotted Owl. Contemporary Policy Issues 10:13-26.

Harrison G W, Rutström E E (2008) Risk Aversion in the laboratory. In: Cox J, Harrison G W (eds) Research in Experimental Economics 12:41-196.

Holt C A, Laury S K (2002). Risk Aversion and Incentive Effects. American Economic Review 92(5):1644-5.

Knight F (1921) Risk, Uncertainty, and Profit. Hart, Schaffner, and Marx, Boston MA.

Laury S, Taylor L (2008) Altruism Spillovers: Are Behaviors in Context-Free Experiments Predictive of Altruism toward a Naturally Occurring Public Good? Journal of Economic Behavior and Organization 65:9-29.

LeRoy, S. and L.D. Singell (1987) Knight on Risk and Uncertainty. Journal of Political Economy 95 (2): 394-406.

Li H, Rosenzweig M, Zhang J (2008) Altruism, Favoritism, and Guilt in the Allocation of Family Resources: Sophie's Choice in Mao's Mass Send Down Movement. Working paper No. 54, Yale University.

List J A, Margolis M, Osgood D E (2006) Is the Endangered Species Act Endangering Species? NBER working paper #12777.

Loomis J, Ekstrand E (1997) Economic Benefits of Critical Habitat for the Mexican Spotted Owl: A Scope Test Using a Multiple-Bounded Contingent Valuation Survey. Journal of Agricultural and Resource Economics 22(2):356-66.

Loomis J B, White D S (1996). Economic Benefits of Rare and Endangered Species: Summary and Meta-Analysis. Ecological Economics 18:197-206.

Lovejoy T (1993) Statement to the Subcommittee on Environment and Natural Resources, Committee on Merchant Marine and Fisheries, US Congress, House of Representatives Ser. No. 103-74, 9 November 1993, pp 3–5.

Margolis M, Naevdal E (2008) Safe Minimum Standards in Dynamic Resource Problems: Conditions for Living on the Edge of Risk. Environmental and Resource Economics 40:401-23.

McIntosh C R, Shogren J F, Finnoff D C (2007) Invasive Species and Delaying the Inevitable: Results from a Pilot Valuation Experiment. Journal of Agricultural and Applied Economics 39: 81-93.

Milnor J (1964) Games against nature. In: Shubik M (ed) Game Theory and Related Approaches to Social Behavior. Wiley Press, New York City NY, pp 20-31.

National Wildlife Federation (2006) Medicinal Benefits of Endangered Species. Http://www.nwf.org/wildlife/pdfs/medicinalbenefits9-06.pdf. Cited 8 December 2009.

Norton B, Toman M (1997) Sustainability: Ecological and economic Perspectives. Land Economics 73:553-585.

Palmini D (1999) Uncertainty, risk aversion, and the game theoretic foundations of the safe minimum standard: a reassessment. Ecological Economics 29:463-472.

Randall A (1991) The value of biodiversity. Ambio 21:63-68

Randall A (2007) Benefit-Cost Analysis and a Safe Minimum Standard of Conservation. In: Atkinson G, Dietz S, Neumayer E (Eds) Handbook of Sustainable Development. Edward Elgar, Cheltenham, UK, pp 91-108.

Randall A (2009) We already have risk management -- Do we really need the precautionary principle? International Review of Environmental and Resource Economics 3(1):39-74.

Randall A, Farmer M (1995) Benefits, costs and a safe minimum standard of conservation. In: Bromley D(ed) Handbook of Environmental Economics, Blackwell, Oxford UK, pp 26-44.

Ready R C, Bishop R C (1991) Endangered species and the safe minimum Standard. American Journal of Agricultural Economics 73:309-12.

Richardson L, Loomis J (2009) The Total Economic Value of Threatened, Endangered and Rare Species: An Updated Meta-analysis. Ecological Economics 68(2):1535-1548.

Rosen, S. 1988. “The Value of Changes in Life Expectancy.” *J. of Risk and Uncertainty* **1**: 285-304.

Rubin J, Helfand G, Loomis J (1991) A benefit-cost analysis of the northern spotted owl. Journal of Forestry 90:25-29.

Samples K C, Dixon J A, Gowen M M (1986) Information Disclosure and Endangered Species. Land Economics 62(3):306-312.

Scott A (1999). Trust law, sustainability and responsible action. Ecological Economics 31:139-154.

Shaw W D, Woodward R T (2008) Why Environmental and Resource Economists Should Care about Non-Expected Utility Models. Resource and Energy Economics 30: 66-89.

Thaler R (1981) Some Empirical Evidence Dynamic Inconsistency. Economics Letters 8(3):201-07.

United States Fish and Wildlife Service (2005) Why Save Endangered Species. http://www.fws.gov/endangered/pdfs/why_save_end_species_july_2005.pdf. Cited 8 December 2009.

Tol R (2003) Is the uncertainty about climate change too large for expected cost-benefit analysis? Climatic Change 56(3):265-289.

Viscusi W K (1990) Do Smokers Underestimate Risks. Journal of Political Economy 98(6):1253-69.

Weitzman M (1998) Why the Far-Distant Future Should be Discounted at its Lowest Possible Rate. Journal of Environmental Economics and Management 36:201-08.

Weitzman M (2007) The Stern Review of the Economics of Climate Change. Journal of Economic Literature 45(3):703-24.

Weitzman M (2009) On modeling and interpreting the economics of catastrophic climate change. Review of Economics and Statistics 91(1):1-19.

Woodward R T, Bishop R C (1997) How to Decide When Experts Disagree: Uncertainty-Based Choice Rules in Environmental Policy. Land Economics 73(4):492-507.

Wubben M J J, Cremer D, Van Dijk E (2009) When and How Communicated Guilt Affects Contributions in Public Good Dilemmas. Journal of Experimental Social Psychology 45:15-2.

What is the Value of a Trip to a National Park? Searching for a Reference Methodology

Dr. John Duffield, Dr. David Patterson, and Chris Neher
Department of Mathematical Sciences
University of Montana
Missoula, MT 59812

Abstract

This paper contributes to the literature on the accuracy and reliability of primary nonmarket valuation methods by comparing observed price response (to entrance fee increases) at Yellowstone National Park to predicted price response based on several alternative stated preference methods. We implement a 2 x 2 experimental design with payment card and dichotomous choice question format and travel cost and entry fee payment vehicles. The data is from a 2005 survey of Yellowstone National Park visitors with 1,512 completed surveys and a 64.4% response rate. Based on a censored regression model, we find that the payment vehicle effect is consistently greater than the elicitation format effect across three independent (spring, summer, fall) samples. Significant differences in welfare estimates were identified across both payment vehicle and question format. For example, median per trip values per travel group for the summer 2005 sample are $27 for payment card/entry fee, $110 for payment card/travel cost, $76 for dichotomous choice/entry fee, and $304 for dichotomous choice/travel cost. We test the hypothesis that the observed price response and the predicted price response (for each method) are the same. The null hypothesis is rejected for both of the methods using the entry fee payment vehicle.

Keywords: nonmarket valuation, recreation, contingent valuation, elicitation format, validation, national parks

JEL classification: D61, Q51, Q25.

Introduction

This paper contributes to the literature on the accuracy and reliability of primary nonmarket valuation methods by comparing observed price response (to entrance fee increases) at Yellowstone National Park to predicted price response based on several commonly applied stated preference methods. We implement a 2x2 experimental design that compares the payment card and dichotomous choice question formats and a travel cost and entry fee payment vehicle on a 2005 sample of Yellowstone National Park visitors. The price response from these four models is compared to the observed response for an increase (actually a doubling) in the entry fee to Yellowstone National Park (implemented in January 1997) from $10 to $20 per vehicle.

One motivation for this paper is that the economics literature reports a several orders of magnitude range in estimated willingness to pay (WTP) for visits to national parks, from around $7 to in excess of $700 (Kaval and Loomis 2003). While some of this difference can likely be attributed to differences in the visitor populations and length and quality of the visit experience across parks, benefit transfer functions fitted to these the available estimates for NPS sites (Duffield, Patterson, Neher, and Loomis 2010), as well as for other recreation sites (Rosenberger and Loomis 2001), indicate that methodology is the primary factor explaining the variation in WTP. To the extent variation in WTP is driven by methods, this obscures the effects of the covariates that would be of greatest utility for benefit transfer: core economic variables (Bergstrom and Taylor 2006) and site characteristics. Going forward, it would be very useful to public agencies to identify a reference or standard methodology that could be consistently applied in future work. This is, in fact, the strategy of the U.S. Fish and Wildlife Service in its National Survey of Fishing, Hunting and Wildlife-related Recreation. However, there has been no systematic use of a consistent methodology for the valuation of National Park visits or visits to the National Forest system.

We test several key methodological design elements for stated preference: question format and payment vehicle. There is a substantial economics literature that compares the influence of these methodological choices on welfare estimates. Champ and Bishop (2006) review some of this literature. However, to our knowledge, there has been no previous study that controls for both influences. The literature is also thin with respect to validation of nonmarket methods by comparison to actual market price or quality changes. Bishop and Heberlein (1979) provide one example; Ready (2003) provides a more recent paper and cites other related literature.

Specifically with respect to payment vehicle, it has long been thought that entry fees might elicit responses having more to do with perception of a “reasonable” entry fee (perhaps based on actual fees at substitute sites) than with measuring WTP (Mitchell and Carson 1989). Nonetheless, this payment vehicle is still applied; an example of a recent application is Leggett et al. (2003), where the WTP for visiting Fort Sumpter National Historic Park is reported to be $8.26 for a payment card question format and an entry fee payment vehicle. (Parenthetically, in this study’s pretest, the dichotomous choice question format was used with an entry fee vehicle and estimated WTP averaged across modes of survey administraton was $14.19). Is $8.26 per visit to Fort Sumpter a meaningful estimate?

The NPS fee demonstration experiment, which began in January 1997, implemented very substantial changes in entry fees at selected parks. These fee changes provide an opportunity to compare state preference methods with observed price response. At Yellowstone, as an example, the standard 7-day entry fee per vehicle was doubled and increased from $10 to $20.

Several Yellowstone visitor surveys that included nonmarket valuation, one implemented in 1998-1999 and another in 2005, provide data sets to compare observed and predicted price response. In this paper we focus on the 2005 data. The 2005 survey was a unique year long probability sample conducted at entrance stations using Dillman methods, with a total of 2,406 surveys distributed, 59 undeliverable, and 1,512 completes for an overall response rate of 64.4%. A 2x2 design was implemented to test both question format (contingent valuation and payment card) and payment vehicle (entry fee and travel cost). A censored regression model was estimated using a maximum likelihood interval approach, which supported pooling the payment card and dichotomous choice data. Significant differences in welfare estimates based on this model were identified across both payment vehicle and question format; as described below the estimates vary across methods (combined effect of payment vehicle and elicitation format) by an order of magnitude. Which of these estimates (if any) tells us the truth about the value of a visit to Yellowstone?

With respect to observed price response, total visitation to Yellowstone dropped by about 4% in 1997 relative to 1996. However, use rebounded to 1996 levels the following year. A regression model was fit to time-series visitation for Yellowstone and a parameter was estimated for trend and a post-price change indicator variable. We test the hypothesis that the observed price response and the predicted price response (for each model) are the same (2009).

The primary contributions of this paper are: 1) testing for both question format and payment vehicle effects using widely applied nonmarket valuation methods on three independent samples, 2) comparing price response from nonmarket models to observed price response for a nationally significant recreation resource, and 3) demonstration of a model that pools payment card and dichotomous choice responses and measures the relative effect of these two design elements.

Methods

Nonmarket valuation models for visitor trips to Yellowstone National Park were estimated using the contingent valuation method (Champ, Boyle, and Brown 2003.). Observed price response was based on the observed change in Yellowstone National Park visitation in 1997 relative to 1996, and from a fitted time series model with an indicator variable for years 1997 and after. We test the hypothesis that the observed price response and nonmarket methods predicted price response are significantly different. Because the survey was implemented after the observed price change in 1997 from $10 to $20, with the survey we explore the next nearest $10 increment, which for the entry fee payment vehicle is from $20 to $30, and for the travel cost payment vehicle is a $10 increment. The specific null hypothesis tested depend on the assumed shape of the demand function: 1) if linear demand, observed price response equals predicted: H_o: $\triangle Q$ obs = $\triangle Q$ model or 2) if demand is convex to origin, observed greater than predicted: H_o: $\triangle Q$ obs $\geq$ $\triangle Q$ model.

In contingent valuation potential respondents are asked about their willingness to pay for the particular service at issue. For current trip values, several question formats (dichotomous choice and payment card) and payment vehicles (travel cost and entrance fee) were used to examine the impact of survey methodology on estimated values. The estimation of willingness to pay models was implemented using a maximum likelihood interval approach (Welsh and Poe 1998; Cameron and Huppert 1989).

Respondents were asked to choose the highest amount he or she was willing to pay from a list of possible amounts. It was inferred that the respondent's true willingness to pay was some amount located in the interval between the amount the respondent chose and the next highest amount presented. Let X_{iL} be the maximum amount that the ith person would be willing to pay and X_{iU} be the lowest presented amount that person would not pay. Given this, WTP (willingness to pay) must lie in the interval $\left[X_{iL}, X_{iU}\right]$ If $F\left(X_i;\beta\right)$ is the statistical distribution function for WTPi, with parameter vector β then the probability that WTPi lies between two given payment bid amounts is $F\left(X_{iU};\beta\right)-F\left(X_{iL};\beta\right)$ and the associated log-likelihood function is:

$$\ln(L)=\sum_{i=1}^{n}\ln\left[F\left(X_{iU};\beta\right)-F\left(X_{iL};\beta\right)\right]$$

The SAS statistical procedure LIFEREG was used to estimate the parametric model of willingness to pay based on the underlying payment card responses.

The survey instrument included an initial section that queried the respondent on the current trip to Yellowstone including activities, previous experience, and preferences. A following section included questions on trip expenditures and the contingent valuation questions; the last section collected socio-economic data on the respondent. The actual wording of the valuation questions are included in Appendix A.

Data Collection

The 2005 Yellowstone National Park Visitor Survey was a year-long survey of park visitors. The overall focus of the survey was on the economic impact of wolf recovery

(Duffield, Neher and Patterson 2008), however the survey was also used as an opportunity to examine the influence of nonmarket valuation methodology. This survey had two distinct target populations: 1) all park visitors entering through park entrances, and 2) park visitors who were stopped along the road within the portion of the Lamar valley most commonly associated with wolf watching. The focus here is on the sample of all park visitors contacted at entrance stations.

The 2005 Yellowstone Visitor Survey was designed as a year-long random survey of park visitors. The primary target population for the 2005 survey was the year-round population of Yellowstone National Park visitors. The sampling plan for this group was designed to survey a generally equal number of park visitors at park entry gates in each of the four seasons. In order to achieve this, the sampling interval was adjusted for each season to account for the very large differences in total park visitation in the different seasons. The goal of balanced sample sizes across seasons was chosen to yield sample sizes in non-summer seasons that would allow meaningful comparison of trip and visitor characteristics across seasons.

Sampling allocation and sampling intervals were based on total park recreational visitation, as estimated by the NPS, totaling approximately 2.8 million visitors. The vast majority of those visitors (almost 2 million) visited during the three summer months of June, July, and August. The 2004 Yellowstone National Park visitation was used as a basis for both allocating survey effort throughout the survey year, and for weighting final survey responses to more closely represent the distribution of actual visitation across seasons and entrances. The procedure followed in administering the survey included a 4-step process.

1. Yellowstone entrance station personnel (and Lamar survey personnel), following a specified schedule and sampling interval would intercept visitors and ask them to participate in the survey. Those who agreed were asked to supply their name and mailing information. This information was collected by the park personnel and periodically forwarded to the researchers in Missoula, MT.
2. The visitor contact information was entered into a database and an initial survey mailing was made including an explanatory letter, survey booklet, and postage paid return envelope.
3. Following the Dillman (2001) survey procedure, a reminder postcard was sent to respondents approximately one week after the survey.
4. A second complete survey package was mailed to those visitors who had not responded to the first two mailings

Based on previous survey experience with this population, and the desire to minimize survey costs, it was anticipated that a good response rate could be achieved with just the three Dillman-method contacts and no financial incentive. There were 12 survey waves in total over the survey year which began on December 18, 2004 and ran through December 17, 2005 for the park entrance sample. A total of 2,406 surveys were mailed, 59 were undeliverable, and 1,512 were returned for an overall response rate of 64.4%. Parenthetically, survey response rates were significantly higher for visitors contacted in the Lamar Valley sample than for the general entrance station contacts. This likely reflects the greater interest the Lamar respondents had in the primary subject of the survey (wolf presence in the park). Overall, approximately 74% of visitors in the Lamar sample responded to the survey while 64% of visitors in the entrance station sample returned completed surveys. Because of changes in entrance procedures for winter visitors (primarily snowmobile riders), the winter sample goal was not achieved. However, the target samples for spring, summer and fall were achieved and make for an interesting data set that can be used to test hypothesis relating to elicitation format and payment vehicle on not one, but three independent samples.

While every effort was made to gather a sample of Yellowstone National Park visitation which mirrored the actual distribution of recreational visitation to the park in 2005, variations in distribution and response rates across months and entrances led to some over and under sampling of visitors during certain periods and at certain entrances. Prior to analyzing the survey responses, the sample distribution was examined and responses were weighted to correct for any over or under-sampling. Responses were also weighted to correct for disproportionate probabilities of selection to participate in the survey. A second weight for the entrance sample was constructed which considered the number of times the respondent had entered the park on their trip, and the number of people in their vehicle when they were sampled.

Survey responses were also analyzed for non-response bias. Gender and place of residence was collected for all potential respondents at the entrance stations and compared to survey respondents for both variables. Non-response bias occurs when those individuals who responded to the survey are significantly different (have significantly different responses) from those who chose not to respond. No significant differences were identified between the two groups for these measures.

Results

The results highlighted here concern the size of the observed price response, the predicted price response based on the nonmarket valuation methods, the test of the null hypothesis of no significant difference, and measurement of the relative effect of payment vehicle versus elicitation format using a censored regression interval model.

Observed price response. With respect to the observed price response, Figure 1 shows a plot of total annual visitation to Yellowstone National Park for the period 1990-2009. A simple visual inspection suggests that visitation was fairly stable in this period with small increases and decreases throughout the period. The NPS fee demonstration experiment, which began in January 1997, implemented very substantial changes in entry fees at selected parks. At Yellowstone, as an example, the standard 7-day entry fee per vehicle was doubled and increased from $10 to $20.

Economic theory would predict that, other things equal, visitation to Yellowstone National Park would drop in 1997 relative to 1996 due to the primary entrance fee having doubled. Visitation did decrease in 1997 (relative to 1996) from 3,012,171 to 2,889,513 or a decrease of 122,658 visits, a 4.1% decrease. A regression model was also fit to time-series visitation for Yellowstone and a parameter was estimated for trend and a post-price change indicator variable. The parameter on the price change indicator was, as theory would predict, negative (with a value of –102,694 and a standard error of 111,062). Based on this simple model, the fitted effect of the price change was a –3.7% decline in visitation in the year of the price change and thereafter. The confidence interval on this estimate includes zero (-11.0% to 4.4%).

A more complex model of this park's visitation would include other potential covariates such as road conditions, wildfire, visitor income, and gasoline prices. For our purposes here, both the simple observed change and the fitted change clearly support the proposition that the price effect of doubling entrance fees is relatively small and possibly approaching zero. This is certainly plausible given that Yellowstone is a nationally significant resource and that many visitors are spending more than $1,000 on primary destination trip to visit this remarkable park, which is not only the nation's but also the world's first national park. Based on the 2005 data set, the mean trip expenditure by local (17-county Greater Yellowstone Area residents in Idaho, Montana, Wyoming) summer visitors was $117 per travel group and $709 per travel group for non-locals. For the summer season 94% of visitors are non-local. It is *a priori* unlikely that many such visitors would be deterred from visiting by a $10 increase in the nominal entrance fee.

Nonmarket valuation findings. This section focuses on several issues: price response, hypothesis tests comparing observed and survey-based price response, and interpretation of the fitted model with respect to elicitation format and payment vehicle effects.

For purposes of identifying price response, we focus on the response revealed by the raw nonmarket valuation response data. This approach avoids the issue of model specification and the question of whether the model fit is adequate. The raw data set is also for the same time frame (annual) as the observed price response. For example, with respect to dichotomous choice, the key data is the proportion responding “no” to the question of whether they would visit if the entry fee increased from $20 to $30 (entry fee payment vehicle) or if their travel costs increased by $10 (travel cost payment vehicle). For the payment card format, the relevant data is the proportion who would also not visit if entry fee (or travel cost) increased by this amount.

Table 1 summarizes the response proportions for a $10 increase in travel costs or a $10 increase in entrance fee across question formats. The basic finding is that the change predicted based on the travel cost payment vehicle is relatively small for both question formats, a 7% decrease in visitation. The responses predicted by the entry fee payment vehicle are significantly greater, a 21% decline in visits for the dichotomous choice question format, and a 28% decline predicted by the payment card-entry fee method.

Hypothesis test. The results of the hypothesis test (comparison to the fitted observed price change) are reported in Table 2. The null hypothesis (that the observed price response and the predicted price response are the same) is rejected for both the payment card-entry fee method (P< .00001) and the dichotomous choice-entry fee method (P =.0216). Clearly a 21% or a 28% price response does not appear to have occurred between 1996 and 1997 either based on the simple observed decline of –4.1% or the fitted model estimate of –3.4%. On the other hand, the null hypothesis for the predicted price response from the travel cost payment vehicle (for either question format) cannot be rejected.

<u>Censored regression interval model.</u> To explore the influence of the alternative stated preference methods for welfare estimates, a censored regression interval model was successfully fitted to the pooled (across question format and payment vehicle) valuation response data (Table 3). Parameters are highly significant for three independent samples (spring, summer and fall seasons), including indicator variables for the payment vehicle and elicitation format effects. A consistent finding across samples is that for our methods, the payment vehicle effect is greater than the elicitation format effect. This is an interesting finding given the emphasis in the literature on elicitation format, with relatively little attention paid to payment vehicle.

A plot of predicted response probabilities across bid levels for the 2005 Yellowstone National Park summer sample is shown in Figure 2 (for the bid range $0 to $500, the upper limit of the entry fee bid range) and Figure 3 (for the bid range $0 to $2000, applicable only to the travel cost payment vehicle applications). As expected, and consistent with the previous economic literature, this figure indicates substantial differences across both payment vehicle and question format . These differences are reflected in the welfare measures. Estimates were developed for two parameters of the WTP distributions for all three seasons, including medians (Table 3) and truncated means (Table 4). The welfare estimates, for example for medians for summer, differ by more than an order of magnitude from $27 for payment card/entry fee to $304 for dichotomous choice/travel cost.

The method-specific values and ranking are relatively stable across these independent (season specific) samples. This supports the interpretation that these are fairly robust and stable differences.

Conclusions

This paper tested the null hypothesis that observed price response for visits to Yellowstone National Park is the same as the price response predicted by several commonly applied nonmarket valuation methods. These included two question formats, dichotomous choiee and payment card, and two payment vehicles, entry fee and travel cost. The main finding is that the null hypothesis is rejected for both of the methods using an entry fee payment vehicle. The entry fee vehicle predicts a price response that is quite large (20% to 28%) relative to the observed (3% to 4%) response. The null hypothesis could not be rejected for either of the question formats used in conjunction with the travel cost payment vehicle.

Another finding, consistent with the literature, is that welfare estimates vary considerably across the methods, for example, median per trip values per travel group for the summer 2005 sample are $27 for payment card/entry fee, $110 for payment card/travel cost, $76 for dichotomous choice/entry fee, and $304 for dichotomous choice/travel cost. Which of these estimates (if any) tells us the truth about the value of a visit to Yellowstone? It appears that for our application welfare estimates based on an entry fee vehicle (as suggested generally many years ago by Mitchell and Carson (1989)) may not be reliable. By extension, other entry fee-based estimates in the literature (for example Leggett et al (2003) estimates for Fort Sumpter) may also not be reliable. Given the consistent direction of bias indicated by the current results, it would appear that the entry fee-based estimates are overly conservative.

The findings also provide some support for the conclusion that the travel cost payment vehicle used with either a payment card or dichotomous choice format are potential candidates for use as a reference methodology in future applications.

A limitation of the study is that the observed price change preceded the nonmarket valuation survey by eight years. This is a long enough period of time for changes in the underlying demand. Additionally, the decision was made in study design to not correct for inflation in the bid design and to use the same nominal price levels observed in 1997 . However, this only strengthens the hypothesis test in that in real (constant 1997 dollars) the $10 price change response measured in 2005 dollars understates the change in real 1997 dollar terms (about a $12 dollar interval) and would lead to even larger overstatement of the price response by the entry fee payment vehicle. Another limitation is that only a small portion of the response function is being tested, which is in the lowest part of the bid range. This limits the strength of generalizations about the reliability of the rest of the stated preference response functions.

There are several extensions to this work that would be useful to undertake. As indicated earlier, a data set (limited to dichotomous choice with a travel cost payment vehicle) for Yellowstone also is available for 1998-1999, much closer to the time of the actual price change. It would be of interest to also test the null hypothesis for this data set and compare to the corresponding 2005 estimates. Both the earlier and 2005 data set also include the relevant information to support an individual travel cost model estimate. For example, a negative binomial count data model following Shaw (1988) and recent applications by Heberling and Templeton (2009) and Bowker et al. (2010) could be estimated and also tested against the observed price change. These stated preference and revealed preference methods could be compared from the standpoint of convergent validity. Since the comparison would be feasible over the entire bid range, this would help inform the choice of question format.

<u>Acknowledgements</u>

We gratefully acknowledge the financial support provided by Yellowstone Park Foundation. Glenn Plumb was Project Director for the 2005 survey effort, and helped coordinate NPS participation in the project. Many Yellowstone National Park staff members played crucial roles in providing advice, data, personnel hours, and valuable feedback on the project during including Doug Smith, Wayne Brewster, John Varley, Tammy Wert, and the YNP Entrance Station Staff. We are especially indebted to Becky Wyman for her work on the Lamar Valley data collection.

Table 1. 2005 Raw Data Stated Preference Response Proportions for a $10 Increase in Travel Costs or a $10 Increase in Entry Fees.

Question Format	**Payment Card**	**Dichotomous Choice**
Entry Fee	89/318 = .2800 (SE=.0252)	8/39 = .2051 (SE=.0647)
Travel Cost	21/300 = .0700 (SE=.01473)	2/29 = .0690 (SE=.0471)

Table 2. Hypothesis Tests: Observed Price Response vs. Raw Data Predicted Response

Question Format	Observed	Predicted	Difference (SE)	Z	P
PC-EF	.0341 (.0369)	.2800 (.0252)	.2459 (.0446)	5.51	<.00001
DC-EF	.0341 (.0369)	.2051 (.0647)	.1710 (.0744)	2.30	.0216
PC-TC	.0341 (.0369)	.0700 (.01473)	.0331 (.0397)	0.90	.37
DC-TC	.0341 (.0369)	.0690 (.0471)	.0349 (.0598)	0.58	.56

Table 3. Estimated Censored regression model Analysis Results of Yellowstone NP Visitor WTP, by Season.

Parameter / Statistic	Estimated Coefficients (Standard Error)		
	Spring Sample	**Summer Sample**	**Fall Sample**
Intercept	4.9155* (0.0793)	4.6970 (0.0928)	4.5096 (0.1016)
DC	0.7647 (0.1032)	1.0204 (0.01203)	0.9560 (0.1320)
EF	-1.6389 (0.0996)	-1.3890 (0.1159)	-1.1529 (0.1285)
Scale	0.5927 (00.0285)	0.6461 (0.0323)	0.6427 (0.0353)
Sample Size	418	328	286
Distribution	Log-Logistic	Log-Logistic	Log-Logistic

All estimated parameters are significant at the 99% level of confidence.
DC: Indicator variable=1 if elicitation format is Dichotomous Choice; 0 if Payment Card
EF: Indicator variable=1 if payment vehicle is Entry Fee; 0 if Travel Cost

Table 4. Estimated Median WTP per Group, by Season and Question Format

Median WTP per Group

FALL	**PC**	**DC**
ENTRY FEE	$ 28.53	$ 74.51
TRAVEL COST	$ 90.75	$ 237.01

SPRING	**PC**	**DC**
ENTRY FEE	$ 26.47	$ 56.91
TRAVEL COST	$ 136.39	$ 293.18

SUMMER	**PC**	**DC**
ENTRY FEE	$ 24.46	$ 83.37
TRAVEL COST	$ 117.81	$ 401.58

Table 5. Estimated Truncated Mean WTP per Group, by Season and Question Format

Truncated-mean WTP per Group ($480 truncation limit)	PC	DC
FALL		
ENTRY FEE	$ 53.54	$ 120.16
TRAVEL COST	$ 140.05	$ 263.97
SPRING		
ENTRY FEE	$ 46.25	$ 91.65
TRAVEL COST	$ 184.29	$ 296.72
SUMMER		
ENTRY FEE	$ 51.50	$ 121.93
TRAVEL COST	$ 161.22	$ 299.94

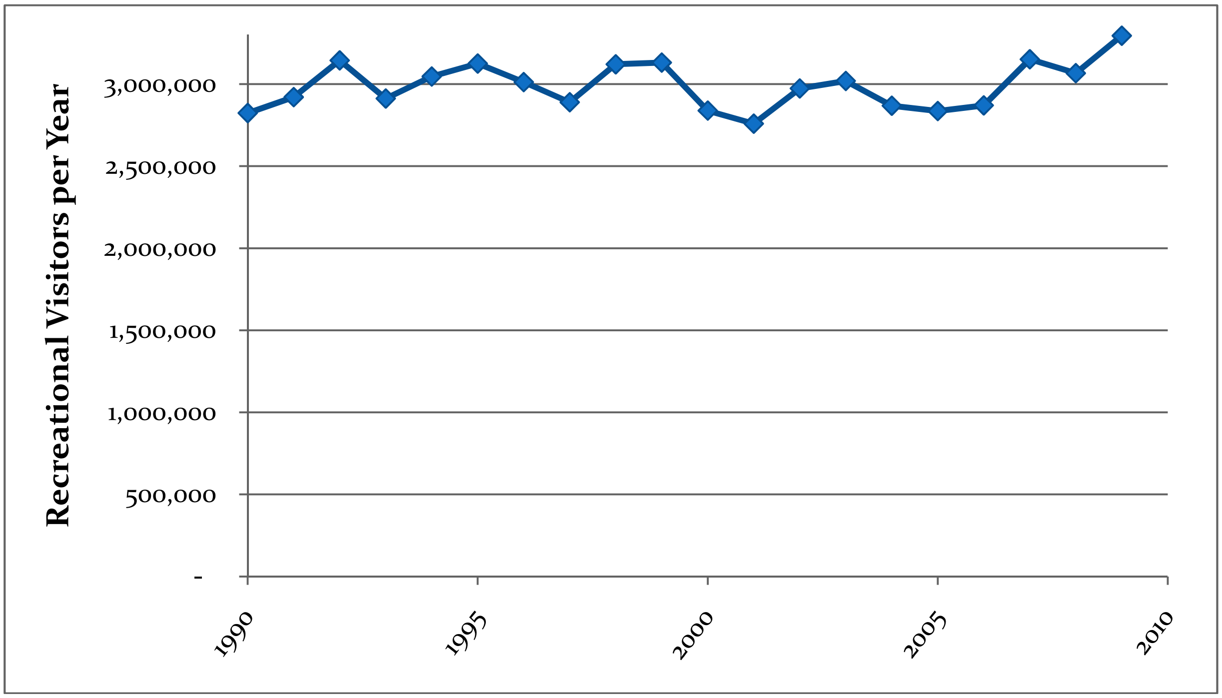


Figure 1. Yellowstone NP Annual Recreational Visitation: 1990-2009

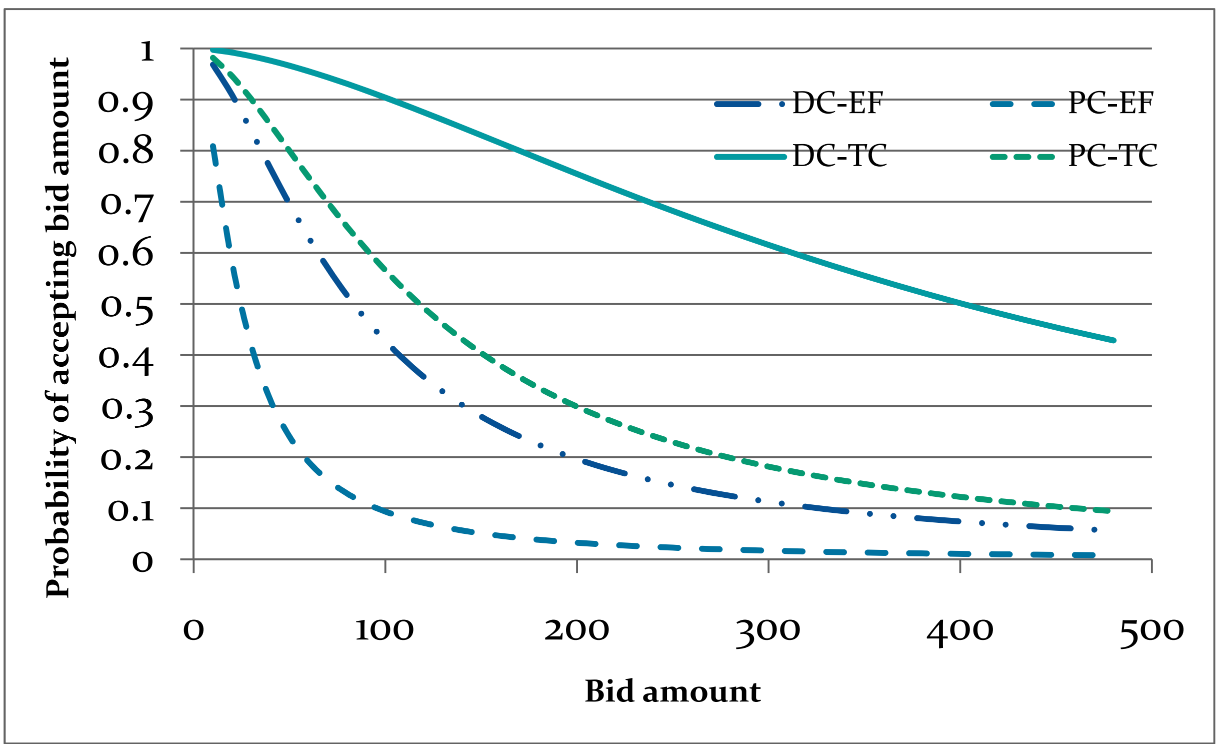


Figure 2. 2005 YNP Data: Predicted Response Probabilities at Alternative Bid Levels (Summer sample)

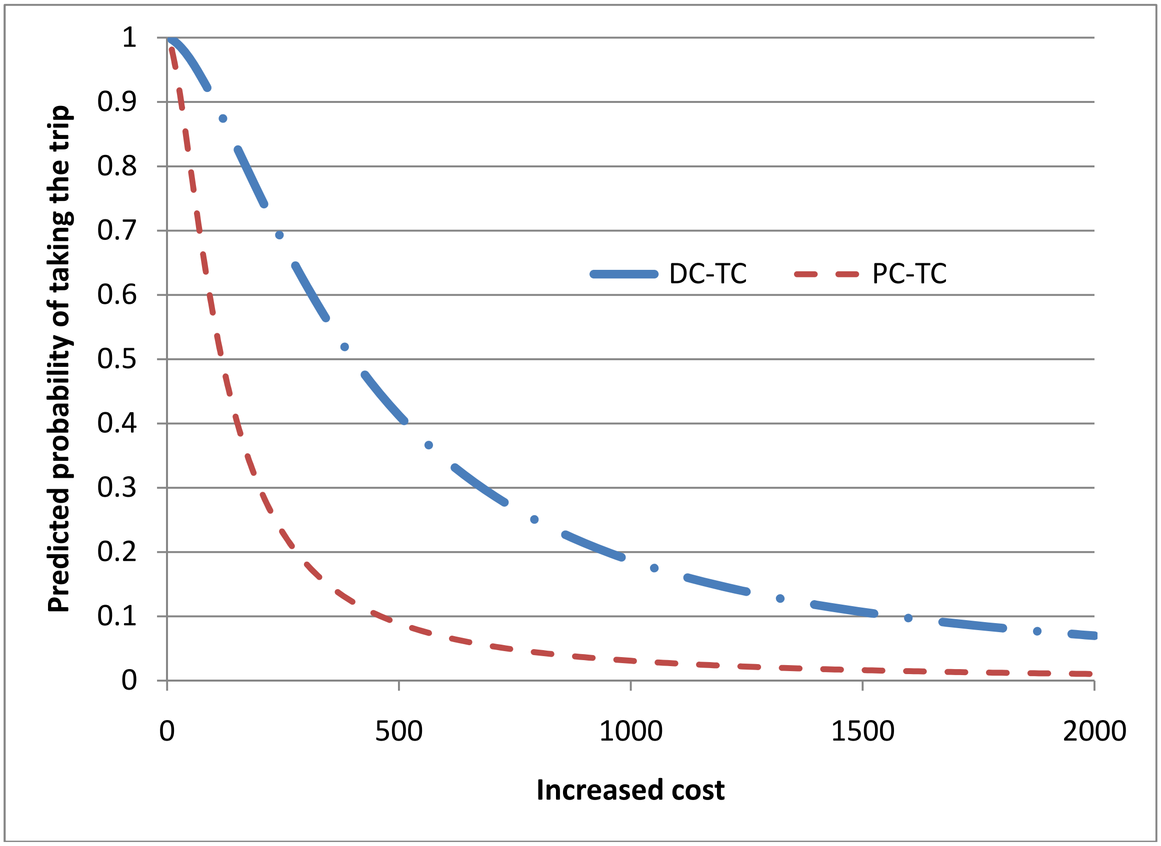


Figure 3. 2005 YNP Data: Predicted Response Probabilities at Alternative Cost Levels (Summer sample)

References

Bishop, Richard and Tom Heberlein. 1979. Measuring values of extra market goods: Are indirect measures biased? *American Journal of Agricultural Economics* 61:926-930.

Bowker, J., Starbuck, C., English, D., Bergstrom, J., Rosenberger, R., & McCollum, D. 2009. *Estimating the Net Economic Value of National Forest Recreation: An Application of the National Visitor Use MOnitoring Data Base.* Athens, GA: University of Georgia, Department of Agricultural and Applied Economics.

Cameron, Trudy Ann, and Daniel D. Huppert. 1989. "OLS versus ML Estimation of Non-market Resource Values with Payment Card Interval Data." *Journal of Environmental Economics and Management* 17:230-246.

Champ, Patricia and Richard Bishop. 2006. "Is Willingness to Pay for a Public Good Sensitive to Elicitation Format?" Land Economics 82(2) 162-73.

Champ, Patricia, Kevin Boyle, and Thomas Brown. (Eds.), 2003. *A Primer on Nonmarket Valuation*. Kluwer Academic Publishers, The Netherlands.

Dillman, D. 2000. Mail and Internet Surveys: the Tailored Design Method, New York. John Wiley and Sons.

Duffield, John, David Patterson, and Chris Neher. 2008. Economics of Wolf Recovery in Yellowstone. *Yellowstone Science* 16(1):20-25.

Duffield, John, David Patterson, Chris Neher and John Loomis. 2010. Valuation of Selected Ecosystem Services in the National Parks: Estimating Models for Benefit Transfer. Working paper.

Heberling, M. and J. Templeton. 2009. Estimating the Economic Value of National Parks with Count Data Models Using On-Site, Secondary Data: The Case of the Great Sand Dunes National Park and Preserve. *Environmental Management* 43:(4): 619-627.

Christopher G. Leggett, Naomi S. Kleckner, Kevin J. Boyle, John W. Duffield, and Robert Cameron Mitchell (2003). "Social Desirability Bias in Contingent Valuation Surveys Administered Through In-Person Interviews**"** *Land Economics* 79(4): 561-575

Mitchell, R.C., and R.T. Carson. 1989. Using Surveys to Value Public Goods: the Contingent Valuation Method. Resources for the Future, Washington, D.C. 463pp

Ready, Richard, Donald Epp, and Willard Delavan. 2005. A comparison of revealed, stated and actual behavior in response to a change in fishing quality. *Human Dimensions of Wildlife* 10: 1, 39-52.

Shaw, D. (1988). On-site Samples Regression: Problems of Non-negative Integers, Truncation and Endogenous Stratification. *Journal of Econometrics* 37: 211-223.

Welsh, M. and G. Poe. 1998. "Elicitation Effects in Contingent Valuation: Comparisons to a Multiple-bounded Discrete Choice Approach." *Journal of Environmental Economics and Management* 36(2): 170-185.

APPENDIX A: Contingent Valuation Survey Questions

<u>(1) Travel Cost-Payment Card Question Format :</u> The costs of visiting and recreating in national parks change over time. For example, gas prices and other travel costs rise and fall.

What is the largest increase in travel costs the group traveling in your vehicle would have paid to visit Yellowstone National Park during this trip? (Circle the amount)

$0 (would not pay more)	$10	$20
$30	$55	$80
$130	$180	$230
$480	$1000	$2000

<u>(2) Travel Cost Dichotomous Choice Question Format :</u> The costs of visiting and recreating in national parks change over time. For example, gas prices and other travel costs rise and fall. Would the group traveling in your vehicle still have chosen to make this trip if the total group costs due to visiting Yellowstone National Park were $_______ <u>more</u> than the amount your group had to pay? (Please check one)

__ YES __ NO

What is the main reason for your answer?____

(3) Entry Fee Payment Card Question Format: Visitors to Yellowstone National Park currently pay an entry fee of $20 per vehicle for a seven-day pass. The National Park Service is not currently thinking of increasing this fee. However, there are other recreational experiences where the access price is quite high (for example, a week of golf green fees or ski lift tickets). Out of fairness to the public, park entrance fees will never increase to those levels. In this question we use entry fee increases only to learn how much visiting Yellowstone National Park is worth to you.

Some people would not pay more than the current fee of $20 per vehicle to visit Yellowstone National Park and would go elsewhere if the fee were higher. Other people would pay more to visit the park, if necessary, because it has this much value to them. What is the highest entry fee per vehicle the group traveling in your vehicle would have paid to visit Yellowstone National Park during this trip? (Please circle the amount) (If your group has a multi-park pass, please answer as if the pass were not valid for Yellowstone National Park)

	$20 (current fee)	
$30	$40	$50
$75	$100	$150
$200	$250	$500

(4) Entry Fee Dichotomous Choice Question Format: Visitors to Yellowstone National Park currently pay an entry fee of $20 per vehicle for a seven-day pass. The National Park Service is not currently thinking of increasing this fee. However, there are other recreational experiences where the access price is quite high (for example, a week of golf green fees or ski lift tickets). Out of fairness to the public, park entrance fees will never increase to those levels. In this question we use entry fee increases only to learn how much visiting Yellowstone National Park is worth to you.

Some people would not pay more than the current fee of $20 per vehicle to visit Yellowstone National Park and would go elsewhere if the fee were higher.

Other people would pay more to visit the park, if necessary, because it has this much value to them.

Would the group traveling in your vehicle still have chosen to visit Yellowstone National Park on this trip if the park entry fee per vehicle were $_____ ? (If your group has a multi-park pass, please answer as if the pass were not valid for Yellowstone.) (Please check one)

__YES __NO

What is the main reason for your answer?__________________________

Rounding in Recreation Demand Models: A Latent Class Count Model

Keith Evans[1] Joseph A. Herriges
Iowa State University Iowa State University

July 29, 2010

[1]Contact author information: 260 Heady Hall, Department of Economics, Iowa State University, Ames, IA 50011. email: *ksevans@iastate.edu*. Phone: 515-294-6173

Abstract

A commonly observed feature of visitation data, elicited via a survey instrument, is a greater propensity for individuals to report trip numbers that are multiples of 5's, relative to other possible integers (such as 3 or 6). One explanation of this phenomenon is that some survey respondents have difficulty recalling the exact number of trips taken and instead choose to round their responses. This paper examines the impact that rounding can have on the estimated demand for recreation and the bias that it may induce on subsequent welfare estimates. We propose the use of a latent class structure in which respondents are assumed to be members of either a *nonrounding* or a *rounding* class. A series of generated data experiments are provided to illustrate the range of possible impacts that ignoring rounding can have on the estimated parameters of the model and on the welfare implications from site closure. The results suggest that biases can be substantial, particularly when then unconditional mean number of trips is in the range from two to four. An illustrative application is provided using visitation data to Saylorville Lake in central Iowa.

JEL Codes: Q51, C5

keywords: recreation demand, rounding

1 Introduction

Models of recreation demand are used extensively to value both access to and potential changes in environmental amenities at recreation facilities, such as lakes, rivers and beaches. Analysts link visitation patterns to the cost of traveling to a site, consumer characteristics and the attributes of the available sites using a range of modeling frameworks, including discrete choice Random Utility Maximization (RUM) models, count data models, and the structural Kuhn-Tucker model. Key to all of these approaches, of course, are data on the numbers of trips to the sites of interest. Trip data most often take the form of counts of the trips taken over a fixed time horizon (e.g., a summer season or calendar year) elicited via a survey instrument, asking the individual to recall (or in some applications to forecast) their numbers of trips. A commonly observed feature of these counts is a greater propensity for individuals to report trip numbers that are multiples of 5's, relative to other possible integers (such as 3 or 6). One explanation of this phenomenon is that some survey respondents have difficulty recalling the exact number of trips taken and instead choose to round their responses.[2] While the apparent clumping of trip data around specific integers is a familiar pattern in recreation demand data, we are aware of no efforts in the literature to date that attempt to account for this pattern. Instead, practitioners treat the reported counts as an accurate reflection of the trips taken by the survey respondent. Even in the broader survey literature, attempts to account for rounding in survey data analyses are rare. Manksi and Molinari [8] provide one of the few exceptions, developing an approach to partially identify patterns in probabilistic expectations elicited via survey instruments.

The purpose of this short paper is to examine the impact that rounding can have on the estimated demand for recreation and the bias that it may induce on subsequent welfare estimates. In particular, we propose a latent class count data model of visitations to a single site in which respondents are assumed to be members of either a *nonrounding* or a *rounding* class, with the latter group providing censored responses to trip questions by rounding their trip counts to the nearest multiple of five. We are agnostic as to why the latter group chooses to round. As Manski and Molinari [8] suggest, "…[t]here are no established conventions for rounding survey responses. Hence, researchers cannot be sure how much rounding there may be in survey data. Nor can researchers be sure whether respondents round to simplify communication or to convey partial knowledge" (p. 219). We go on to suggest the use of an expectation-maximization (EM) algorithm for the estimation of the model. A series of generated data experiments are then provided to illustrate the range of possible impacts

[2] Similar phenomena have been observed in other survey settings. For example, Dominitz and Manski [4] note that in surveys eliciting probabilistic expectations (e.g., the probability of loosing one's job or living to a specific age), responses tend to bunch around multiples of 5%. See Manksi and Molinari [8] for additional discussion of this phenomenon.

that ignoring rounding can have on the estimated parameters of the model and on the welfare implications from site closure. The results suggest that biases can be substantial, particularly when the unconditional mean number of trips is in the range of two to four. Finally, an illustrative application is provided using data on the visitations to Saylorville Lake, a popular recreational site and reservoir in central Iowa. The paper closes with an overall summary of our findings and a discussion of possible extensions of the modeling framework.

2 The Model

We begin this section by formally defining the assumed latent class structure and developing the necessary notation. Latent class models have emerged in recent years as a popular approach to incorporating preference heterogeneity in discrete choice models, both in recreation demand (e.g., [2],[10],[11]) and in the broader literature (e.g., [5],[6],[7]). In our application, the heterogeneity lies in the individual's propensity to round. We then propose an EM algorithm for use in the estimation of the model.

2.1 The Latent Class Count Data Model

The starting point in our approach to examining the impact of rounding in the modeling of recreation demand is to assume that individuals fall into one of two *latent classes*: *Non-rounders* (N) or *Rounders* (R). Individual class membership (denoted by $C_i^* = N$ or R) is unknown to the analyst. For each class, the actual number of trips (y_i^*) taken to the site in question is assumed to be drawn from a Poisson distribution, though the underlying parameters of the Poisson distribution are allowed to vary by class. Specifically, we assume that:

$$Pr(y_i^* = k | C_i^* = c) = \frac{\exp(-\lambda_{ic})\lambda_{ic}^k}{k!} \quad i = 1, \ldots, I; c = N, R, \tag{1}$$

where

$$\lambda_{ic} = \exp(\boldsymbol{X}_i' \boldsymbol{\beta}_c) \tag{2}$$

denotes the conditional mean trips for individuals in class c given characteristics $\boldsymbol{X}_i$ and the parameter vector $\boldsymbol{\beta}_c$. For individuals in the nonrounding class, the reported trips y_i are assumed to be the same as the actual number of trips (i.e., $y_i = y_i^*$). Thus, *conditional* on knowing that $C_i^* = N$, the individual's choice probability is simply:

$$L_{iN}(y_i, \boldsymbol{X}_i; \boldsymbol{\beta}_N) = \frac{\exp(-\lambda_{iN})\lambda_{iN}^{y_i}}{y_i!} \tag{3}$$

In contrast, for individuals in the rounding class, reported trips are assumed to be rounded to the nearest multiple of five for trips greater than 2.[3] Let $\mathcal{I}_5$ denote the set of positive integers that are multiples of five. In this case, *conditional* on knowing that $C_i^* = R$, the individual's choice probability is simply:

$$L_{iR}(y_i, \boldsymbol{X}_i; \boldsymbol{\beta}_R) = \begin{cases} \frac{\exp(-\lambda_{iR})\lambda_{iR}^{y_i}}{y_i!} & y_i = 0, 1, 2 \\ \sum_{j=-2}^{2} \frac{\exp(-\lambda_{iR})\lambda_{iR}^{y_i+j}}{(y_i+j)!} & y_i \in \mathcal{I}_5 \\ 0 & \text{otherwise.} \end{cases} \tag{4}$$

Let $s_R \in [0, 1]$ denote the probability of being in the nonrounding class. Since class membership is not known, the unconditional choice probability for individual i becomes:

$$L_i(y_i, \boldsymbol{X}_i; \boldsymbol{\theta}) = s_R L_{iR}(y_i, \boldsymbol{X}_i; \boldsymbol{\beta}_R) + (1 - s_R) L_{iN}(y_i, \boldsymbol{X}_i; \boldsymbol{\beta}_N), \tag{5}$$

where $\theta = (\boldsymbol{\beta}'_N, \boldsymbol{\beta}'_R, s_R)'$ denotes the combined parameters of the model. The parameter vector θ can be obtained using standard maximum likelihood gradient based methods. However, latent class models are notorious for their difficulty in estimation, particularly since the class labels are themselves arbitrary. In our generated data experiments and application, we instead employ the EM algorithm described below.

2.2 The EM Algorithm

EM algorithms were introduced by Dempster, Laird and Rubin [3] as a means dealing with missing data and have subsequently been adapted to a variety of estimation problems in which some piece of information in a model is missing.[4] In the current application, the missing piece of information we focus on is the class membership variable C_i^*. As with all EM algorithms, the procedure is iterative. Let θ^t denote value of the parameter vector at iteration t. Following the notation in [13], the next iteration on θ (i.e., θ^{t+1}) is obtained for our latent class model by maximizing:

$$\mathcal{E}(\theta|\theta^t) = \sum_{i=1}^{I} \sum_{c=N}^{R} h_{ic}^t \ln \left[s_c L_{ic}(y_i, \boldsymbol{X}_i; \boldsymbol{\beta}_c) \right] \tag{6}$$

[3] We assume that when actual trips (y_i^*) equal 1 or 2, they are not rounded down to zero by the survey respondent, even in the case of the rounding class. It seems reasonable to us that, in reporting trips, the rounding individual distinguishes taking a trip from staying at home even when the number of trips is small.

[4] Train [13], chapter 14, provides an excellent overview of EM algorithms, while McLachlan and Krishnan [9] provide a review of applications.

where

$$h_{ic}^t = h(C_i^* = c|y_i, s^t) = \frac{s_c^t L_{ic}(y_i, \boldsymbol{X}_i; \boldsymbol{\beta}_c^t)}{s_N^t L_{iN}(y_i, \boldsymbol{X}_i; \boldsymbol{\beta}_N^t) + s_R^t L_{iR}(y_i, \boldsymbol{X}_i; \boldsymbol{\beta}_R^t)} \tag{7}$$

denotes the probability that individual i belongs to class c conditional on the observed choice of the individual. Given the structure of the problem in (6), this is equivalent to separately maximizing:

$$\mathcal{E}(s|\theta^t) = \sum_{i=1}^{I} \left[h_{iR}^t \ln(s_R) + h_{iN}^t \ln(1 - s_R)\right] \tag{8}$$

with respect to s_R and (for both $c = R$ and N) maximizing

$$\mathcal{E}(\beta_c|\beta^t) = \sum_{i=1}^{I} h_{ic}^t \ln\left[L_{ic}(y_i, \boldsymbol{X}_i; \boldsymbol{\beta}_c)\right] \tag{9}$$

with respect to β_c. The solution to the maximization of (8) yields:

$$s_R^{t+1} = \frac{\sum_{i=1}^{I} h_{iR}^t}{\sum_{i=1}^{I} (h_{iN}^t + h_{iR}^t)}. \tag{10}$$

The specific steps involved in the EM algorithm are then:

1. With t denoting the current iteration, set $t = 0$ and specify the initial values for both the share of rounders (i.e., s_R^0) and the parameters of the two classes (i.e., β_N^0 and β_R^0). We set $s_R^0 = 0.5$. Similar to the approach suggested by [13] in the context of a latent class logit model (p. 360), the starting values for the parameters of our two latent classes are obtained by randomly partitioning the sample into two groups (N and R) and maximizing (9) for each subsample to obtain β_N^0 and β_R^0.

2. For each observation i and each class c, the probability h_{ic}^t that individual i belongs to class c conditional on the observed choice of the individual is computed using (7).

3. The updated class share of rounders (s_R^{t+1}) is obtained using (10).

4. The updated parameters for the two latent classes (β_N^{t+1} and β_R^{t+1}) are obtained by maximizing (9).

5. Check for convergence. If convergence has not been achieved, then t is incremented by 1 and the algorithm returns to step 2. Otherwise, the algorithm ends and the standard errors for the parameters can be calculated. We use bootstrapped standard errors, but an alternative approach would be to use the converged values from the EM algorithm as starting values in a maximum likelihood estimation of θ using (5).

3 Generated Data Experiments

In order to investigate the potential impact that rounding can have on both parameter estimates and subsequent welfare calculations, we conduct a series of generated data experiments. In all of the experiments, the conditional mean number of trips for each class is assumed to be a linear exponential function of travel cost to the site (denoted by P_i), individual income (denoted by Y_i), and a demographic variable (Z_i) . Specifically, we assume that:

$$\lambda_{ic} = \exp(\beta_{c0} + \beta_{cP} P_i + \beta_{cY} Y_i + \beta_{cZ} Z_i), \quad c = N, R. \tag{11}$$

Across the experiments we vary two factors: (1) the share of rounders (i.e., s_R) and (2) the mean trips for the two classes (by varying β_{N0} and β_{R0}).[5] The emphasis on s_R is obvious, with our model reducing to the standard count data model when $s_R = 0$. Five values for s_R were considered ($s_R = 0.1,\ 0.25,\ 0.5,\ 0.75$, and 0.9). We focus on mean trips by class, since the propensity for individuals to round will depend, in part, on their trip frequencies. If the vast majority of the sample takes 0, 1, or 2 trips, then there will be little room for rounding to occur. Along these lines, we consider two basic experiments. In experiment #1, we fix $\phi_0 \equiv \beta_{R0}/\beta_{N0} = 2$ (making rounders correspond to somewhat more avid trip takers) and vary β_{N0} from 0.5 to 1.5 in steps of 0.25 (increasing the overall trip taking by the two groups). The resulting intercepts are listed part a of Table 1.[6] In experiment #2, we fix $\bar{\beta}_0 = \frac{1}{2}(\beta_{N0} + \beta_{R0})$ (i.e., the simple average of the two type intercepts), varying ϕ_0 from 0.5 to 2.0. When $\phi_0 = 0.5$, rounders are less frequent trip takers than non-rounders, whereas when $\phi_0 = 2$ the opposite is true. The resulting intercepts are listed in part a of Table 2. In both experiments, we assume that the price, income and demographic coefficients are the same across the two classes (i.e., $\beta_{cP} = \beta_P$, $\beta_{cY} = \beta_Y$, and $\beta_{cZ} = \beta_Z$ for $c = N, R$). In total, twenty-five generated data settings were analyzed for each experiment. In all of the experiments, the sample size was set at I=5000.

Formally, 100 generated data sets (with 5000 observations in each data set) were constructed for each experiment/setting as follows:

1. Vectors of travel cost $(P_1, \ldots, P_I)$, income $(Y_1, \ldots, Y_I)$ and the demographic variable $(Z_1, \ldots, Z_I)$ were drawn from uniform distributions (i.e., $P_i \sim U[0,1]$, $Y_i \sim U[0,1]$, and $Z_i \sim U[0,1]$).

[5] We also investigated the impact of varying both the overall price coefficient and differences between β_{NP} and β_{RP}, but found that this had relatively little impact on the bias induced by rounding.

[6] Variations in these intercepts will induce variations in a group's unconditional mean number of trips. Table 1 also provides (in square brackets) the corresponding unconditional mean trips for each group and parameter setting given the assumed data generating process. For example, with $\beta_{0R} = 1$, the corresponding unconditional mean trips would be 1.40.

2. Using β_{R0} and β_{N0} for the given setting, along with $\beta_P = -0.75$, $\beta_Y = 0.25$, and $\beta_Z = -0.25$, λ_{ic} was computed for each individual and latent class using (11).

3. Each individual in the sample was randomly assigned to either the nonrounding ($C_i^* = N$) or rounding ($C_i^* = R$) latent class with probabilities s_N and s_R, respectively, using a draw from uniform distribution; i.e.,

$$C_i^* = \begin{cases} N & u_i < s_N \\ R & \text{otherwise,} \end{cases} \tag{12}$$

where $u_i \sim U[0,1]$.

4. Using

$$\lambda_i = 1(C_i^* = N)\lambda_{iN} + 1(C_i^* = R)\lambda_{iR}, \tag{13}$$

where $1(\cdot)$ is the indicator function, the individual's actual trips (y_i^*) were drawn from a Poisson distribution with conditional mean λ_i.

5. Reported trips (y_i) were then constructed as

$$y_i = \begin{cases} y_i^* & y_i = 0,1,2 \\ 1(C_i^* = N)y_i^* + 1(C_i^* = R)\text{rnd}_5(y_i^*) & \text{otherwise,} \end{cases} \tag{14}$$

where $\text{rnd}_5(x)$ is the function censoring x to the nearest integer of five.

For each experiment, two models were estimated: (1) The latent class count data model (LCCM) outlined above and (2) the standard (single class) (SCCM) count data model in which no rounding is assumed. The resulting parameter estimates are available from the authors upon request. However, as the ultimate goal of the recreation demand model is typically for use in policy analysis, we focus our attention here on the potential bias on subsequent welfare calculations that results from ignoring respondent rounding. Specifically, we consider the welfare impact of the complete loss of access to the site as measured by consumer surplus (CS).[7] In a Poisson count data model with the linear exponential representation of mean trips, the change in consumer surplus resulting from the elimination of the site is given by:

$$CS_i = \frac{\lambda_i}{\beta_{iP}}, \tag{15}$$

where

$$\lambda_i = \exp(\beta_{i0} + \beta_{iP}P_i + \beta_{iY}Y_i + \beta_{iZ}Z_i) \tag{16}$$

[7]Similar results are obtained if either compensating variation (CV) or equivalent variation (EV) are used instead to measure the welfare impact.

denotes the mean number of trips and $\boldsymbol{\beta}_i = (\beta_{i0}, \beta_{iP}, \beta_{iY}, \beta_{iZ})$ denotes individual i's true parameter vector. The true welfare loss measures for individual i in the generated data sample are computed using equation (15). Averaged across the individuals yields the mean true welfare loss for the sample (denoted $\overline{CS}^{Tr}$) for the r^{th} generated data set.

For the single class count data model, the estimated welfare loss measures were computed for each individual i using the fitted parameter vector from the SCCM specification for the r^{th} generated data set. Averaged across the individuals yields the mean welfare loss for the sample predicted using the SCCM specification (denoted $\overline{CS}^{Sr}$).

For the latent class count data model, the predicted welfare loss for individual i is a weighted average of the welfare loss predicted for each latent class; i.e.,

$$\widehat{CS}_i^{Lr} = (1 - \hat{s}_R^r)\widehat{CS}_i^{Nr} + \hat{s}_R^r \widehat{CS}_i^{Rr} \tag{17}$$

where

$$\widehat{CS}_i^{cr} = \frac{\exp(\hat{\beta}_{c0}^r + \hat{\beta}_{cP}^r P_i + \hat{\beta}_{cY}^r Y_i + \hat{\beta}_{cZ}^r Z_i)}{\hat{\beta}_{cP}^r}, \quad \text{for } c = N,\ R \tag{18}$$

and $\hat{\beta}_{ck}^r$ $(k = 0, P, Y, Z)$ and $\hat{s}_R^r$ denote the fitted parameter estimates from the LCCM using the r^{th} generated data set. Averaged across the individuals yields the mean welfare impact for the sample predicted using the LCCM specification (denoted $\overline{CS}^{Lr}$). For each experiment/setting, we compute the percentage error of each model in predicting the true consumer surplus. Tables 1b and 2b provide a summary of our findings for experiments 1 and 2, respectively.

Starting with experiment 1, several patterns emerge. First, as we would expect, the LCCM model does well in predicting the mean welfare loss stemming from the elimination of the site, since it is the correct specification of the data generating process. In general, the average error is less than one percent. The errors are typically larger when the share of rounders s_R is small, leaving relatively few observations with which to estimate parameters for the rounding class. Second, the bias in welfare predictions from ignoring rounding (and using the standard SCCM) can be substantial. Consumer surplus is overstated by as much as 37%. Indeed, the extent to which the SCCM consumer surplus measure overstates the overall welfare loss appears to increase with the latent percentage of rounders in the sample, but does not increase monotonically as the average number of trips increase. Indeed, the largest bias occurs when the unconditional mean number of trips for the rounding class is just over two. This may simply be because, when few trips are taken by the rounding class, there is little opportunity for rounding, whereas when the rounding class takes many trips (e.g., with an unconditional mean of 10.32), the percentage error in reported trips is smaller (e.g., rounding 7 trips to 5 is a larger percentage error than when rounding 47 trips to 45).

Turning to the second experiment in Table 2b, we again see that percentage error resulting from ignoring rounding increases with the size of the rounding class, with the bias being largest when the unconditional mean trips for the rounding class is in the range from 2 to 3. Even when the coefficients are identical for the two latent classes (i.e., $\phi_0 = 1$), the SCCM welfare measures are biased; consumer surplus is overstated by as much 36%. This is due to the fact that, within the rounding class, there will be a larger percentage of individuals rounding up than rounding down (e.g., a larger percentage of the population will have actual trips of 3 and 4 relative to those having actual trips of 6 and 7). Thus, reported trips will be a biased indicator of actual trips for the rounding class, with $E(y_i|\boldsymbol{X}_i, C_i = R) > E(y_i^*|\boldsymbol{X}_i, C_i = R)$.

4 Application

As illustration of our proposed methods, we employ data from the Iowa Lakes Valuation Project. The Iowa Lakes Project, funded by the Iowa Department of Natural Resources and the US EPA, was a four year effort to gather panel data on the recreational lake usage patterns of Iowa households. Beginning in 2002, trip counts for the 132 primary recreational lakes in the state were elicited from a random sample of 8000 state residents. After accounting for nondeliverables, the overall response rate to the mail survey was approximately 62%.[8] In the current paper, we limit our attention to visits to a single site, Saylorville Lake, a reservoir in central Iowa locate just north of the state capital, Des Moines. We also restrict our attention to households within a 100 mile radius of the site, leaving a total of I=1395 observations for use in our analysis. Table 3 provides basic summary statistics for the sample.[9] As Table 3 indicates, the mean number of trips taken to Saylorville Lake in 2002 is 1.66, with approximately 69.4% of the sample choosing not visiting the site that year.

Table 4 provides the parameter estimates for the SCCM and two versions of the LCCM specification. In version 1 of the LCCM, we constrain the parameters of the rounding and non-rounding groups to be the same (and in doing so focus on rounding alone as a source of bias), whereas version 2 relaxes restriction. In general, all of the parameter estimates are statistically significant. The SCCM model finds, as expected, that travel cost negatively impacts the mean number of visits to the site. The results also suggest that trips increase with income, but decrease with the individual's age and education. Similar results are

[8] Additional details regarding the Iowa Lakes Project can be found in [1]

[9] Travel cost P_i is computed assuming an out-of-pocket trip cost of \$0.25 per mile times the individual's round trip distance to the site and a time cost of one-third the individual's hourly times the round trip travel time to the site. Travel distance and travel time were computed using the software package *PCMiler*.

found in the constrained LCCM model, though the impact of education is now somewhat larger. The estimated share of rounders is approximately one-third of the population. The mean consumer surplus associated with closure of Saylorville Lake is approximately 5.3% higher using the SCCM model ($22.49) compared to estimates based on the constrained LCCM specification ($21.36), which is line with our generated data experiment. With the unconditional mean number of trips of 1.66, there is relatively little room for rounding to impact the results.

Turning to the unconstrained LCCM specification, while the general sign of the marginal effects are similar to the other two specification, the parameters differ somewhat between the nonrounding and rounding latent class. Trips are more responsive to age and education for the rounding class, but less responsive to price and income. As was the case in the constrained LCCM model, under forty percent of the population is found to belong to the rounding class. Despite the similarities with the other two model, the unconstrained specification yields a substantially higher estimate of the consumer surplus loss due to the closure of the site ($43.41). While this is certainly possible, we believe that some caution would be appropriate in using the unconstrained LCCM specification. Examining the summary statistics in Table 3, it is clear that the data exhibits a form of overdispersion, since the unconditional mean number of trips (1.66) is much less than the corresponding unconditional variance (24.2). Intuitively, it seems possible that the unconstrained LCCM results may be using the rounding class to compensate for overdispersion. A generalization of the LCCM specification (e.g., using the negative binomial as the base distribution) could be used to examine this issue further.

5 Summary and Possible Extensions

The objective of this paper was to illustrate the potential bias that rounding can have on both the characterization of trip demand and the subsequent welfare estimates derived from a count data model of recreation demand. We propose a latent class model to allow for rounding by a subset of the population. Both our generated data experiments and an application to recreation demand at Saylorville Lake in central Iowa suggest that the potential bias can be substantial.

There are a number of possible directions for future research. First, our latent class model assumes that the share of rounders (s_R) is a constant. However, it seems reasonable that the propensity for individuals to round might depend upon their characteristic (e.g., age, gender, etc.), as well as the circumstance under which the survey is conducted (e.g., involving near-term recall versus recall for time periods further into past). Numerous authors have made the class membership probability in the latent class model of function of respondent attributes (e.g., using a logit specification). Second, our latent class model allows for only one type of rounding (i.e., to the near integer multiple of five). The framework can readily be generalized to allow for a variety of rounding behaviors (e.g., rounding to multiples of ten) by introducing addition latent classes. Finally, the Poisson count model underlying our latent class model specification carries with it the often criticized assumption of equidispersion (with the conditional mean of the trips being equal to the conditional variance). The latent class approach used above could, however, be readily generalized by assuming the each class has actual trips that are from a more general count data distribution allowing for overdispersion (e.g., the negative binomial or zero inflated Poisson).

	Table 1: Welfare Performance - Experiment #1									
	a. Parameter Setting[a]									
β_{0R}	1.00 [1.40]		1.50 [2.30]		2.00 [3.80]		2.50 [6.26]		3.00 [10.32]	
β_{0N}	0.50 [0.85]		0.75 [1.09]		1.00 [1.40]		1.25 [1.79]		1.50 [2.30]	
	b. Mean Percentage Error in CS									
s_R	LCCM	SCCM	LCCM	SCCM	LCCM	SCCM	LCCM	SCCM	LCCM	SCCM
0.10	2.4	2.9	1.8	7.9	1.2	5.9	1.3	2.0	-0.1	-0.2
0.25	0.6	6.1	1.9	16.8	0.5	12.0	0.1	2.9	0.3	0.5
0.50	0.5	11.0	0.6	26.0	0.2	19.2	-0.4	2.0	0.1	1.3
0.75	0.9	15.6	-0.2	32.9	0.5	23.8	0.2	3.2	0.3	0.4
0.90	1.2	17.3	0.0	36.7	0.1	25.6	0.1	2.7	0.0	0.1

[a]Corresponding unconditional group mean trips in square brackets.

	Table 2: Welfare Performance - Experiment #2									
	a. Parameter Setting[b]									
ϕ_0	0.50		0.75		1.00		1.50		2.00	
β_{0R}	1.00 [1.40]		1.29 [1.86]		1.50 [2.30]		1.80 [3.11]		2.00 [3.80]	
β_{0N}	2.00 [3.80]		1.71 [2.85]		1.50 [2.30]		1.20 [1.71]		1.00 [1.40]	
	b. Mean Percentage Error in CS									
s_R	LCCM	SCCM	LCCM	SCCM	LCCM	SCCM	LCCM	SCCM	LCCM	SCCM
0.10	0.9	1.1	1.3	2.0	1.2	3.6	1.4	6.4	1.2	5.9
0.25	0.1	1.7	1.0	6.0	1.0	9.3	1.5	14.1	0.5	12.0
0.50	0.0	5.0	0.7	12.5	0.8	19.0	1.4	24.8	0.2	19.2
0.75	0.3	9.6	-0.1	19.9	0.3	28.8	0.1	31.1	0.5	23.8
0.90	0.4	14.7	0.8	27.0	0.7	35.8	0.6	35.9	0.1	25.6

[b]Corresponding unconditional group mean trips in square brackets.

Table 3: Summary Statistics					
Variable	Model Variable	Mean	Std. Dev.	Min	Max
Total Day Trips (2002)	y_i	1.664	4.924	0.000	60.000
Travel Cost ($10's)	P_i	3.057	2.095	0.185	17.067
Income ($10000's)	Y_i	6.300	5.904	0.500	32.500
Age (10 years)	$Z_{Age,i}$	5.290	1.744	1.550	8.750
College	$Z_{Educ,i}$	0.396	0.489	0.000	1.000

Table 4: Parameter Estimates							
Model	Class	$\hat{\beta}_0$	$\hat{\beta}_P$	$\hat{\beta}_Y$	$\hat{\beta}_{Z,Age}$	$\hat{\beta}_{Z,Educ}$	$\hat{s}_R$
SCCM		2.664	-0.740	0.033	-0.170	-0.030	n.a.
		(0.006)	(0.002)	(0.003)	(0.001)	(0.004)	
Constrained		2.626	-0.741	0.033	-0.171	-0.469	0.361
LCCM		(0.084)	(0.023)	(0.004)	(0.014)	(0.046)	(0.097)
LCCM	$C_i = N$	3.193	-0.816	0.032	-0.079	-1.621	
		(0.017)	(0.006)	(0.001)	(0.003)	(0.033)	
LCCM	$C_i = R$	0.866	-0.750	0.014	-0.258	4.093	0.386
		(0.125)	(0.039)	(0.002)	(0.018)	(0.062)	(0.008)

References

[1] Azevedo, C., K. Egan, J. Herriges, and C. Kling (2003). *Iowa Lakes Valuation Project: Summary and Findings from Year One.* Final Report to the Iowa Department of Natural Resources, August.

[2] Boxall, A., and W. Adomowicz (2002). "Understanding Heterogeneous Preferences in Random Utility Models: A Latent Class Approach," *Environmental and Resource Economics*, Vol. 23, pp. 421-442.

[3] Dempster, A., N. Laird, and D. Rubin (1977), "Maximum Likelihood from Incomplete Data via the EM Algorithm," *Journal of the Royal Statistical Society B* Vol. 39, pp. 138.

[4] Dominitz, J., and C. Manski (1997). "Perceptions of Economic Insecurity: Evidence From the Survey of Economic Expectations," *Public Opinion Quarterly*, Vol. 61, pp. 261-287.

[5] d'Uva (2005). "Latent Class Models for Use of Primary Care: Evidence from a British Panel," *Health Economics*, Vol. 14, pp. 873-892.

[6] d'Uva (2006). "Latent Class Models for Utilisation of Health Care," *Health Economics*, Vol. 15, pp. 329-343.

[7] Greene, W., and D. Hensher (2003). "A Latent Class Model for Discrete Choice Analysis: Contrasts with Mixed Logit," *Transportation Research, Part B*, Vol. 37, pp. 681-698.

[8] Manski, C., and F. Molinari (2010). "Rounding Probabilistic Expectations in Surveys," *Journal of Business & Economic Statistics*, Vol. 28, No. 2, pp. 219-231.

[9] McLachlan, G. and T. Krishnan (1997), *The EM Algorithm and Extensions*, John Wiley and Sons, New York

[10] Morey, E., J. Thacher, and W. Breffle (2006). "Using Angler Characteristics and Attitudinal Data to Identify Environmental Preference Classes: A Latent-Class Model," *Environmental & Resource Economics*, Vol. 34, pp. 91-115.

[11] Provencher, B., K. Baerenklau, and R. Bishop (2002). "A Finite Mixture Logit Model of Recreational Angling with Serially Correlated Random Utility," *American Journal of Agricultural Economics*, Vol. 84, No. 4, pp. 1066-1075.

[12] Train, K. (2008). "EM Algorithms for Nonparametric Estimation of Mixing Distributions," *Journal of Choice Modelling*, Vol. 1, No. 1, pp. 40-69.

[13] Train, K. (2009). *Discrete Choice Methods with Simulation*, Cambridge University Press, 2nd edition.

Capturing Preferences Under Incomplete Scenarios Using Elicited Choice Probabilities.

Subhra Bhattacharjee, Iowa State University
Joseph A. Herriges[1], Iowa State University
Catherine L. Kling, Iowa State University

Preliminary Draft - Please do not Quote without Permission

July 22, 2010

[1]Contact author information: 260 Heady Hall, Department of Economics, Iowa State University, Ames, IA 50011. email: *jaherrig@iastate.edu*. Phone: 515-294-4964

Abstract

Manski [9] proposed using elicited choice probabilities instead of the standard dichotomous choice responses when the choice comparisons of interest are only incompletely described in the available survey instrument. This allows the survey respondent to express their uncertainty regarding the alternatives they face by revealing *ex ante* the odds that an alternative will be their preferred option *ex post*. Recently, Blass *et al.* [1] provided a strategy for analyzing elicited choice probabilities and an empirical example. This paper extend this literature by providing preliminary findings from a split sample comparison of the elicited choice probability and stated choice elicitation formats using data from the 2009 Iowa Lakes Survey. In addition, we examine the impact of different information treatments on the two survey format responses.

JEL Codes: Q51, C25

keywords: nonmarket valuation, dichotomous choice, choice probabilities.

1 Introduction

The Random Utility Maximization (RUM) model provides the foundation of most empirical analyses of contingent valuation (CV) and choice experiment exercises used to elicit the value of environmental amenities and other nonmarket goods. However, a fundamental premise of the RUM model is that individuals, in making a choice among the available alternatives, know *exactly* what utility they will receive from each alternative.[2] The error term in the model reflects, not the respondent's uncertainty, but rather missing information on the part of the analyst. This missing information can take the form of unobserved factors affecting the choice, measurement error, or misspecification of the conditional indirect utility function itself. The analyst then makes some assumption about the empirical distribution of the error term, allowing them to specify conditional choice probabilities for each individual and to then estimate the parameters associated with the assumed distribution.

The assumption that individuals have no uncertainty about the choices they face in a stated preference survey, while convenient, seems tenuous at best, particularly when individuals are asked to evaluate goods with which they have little past experience. Indeed, a number of CV studies have attempted to capture this uncertainty in their survey design by adding "probably yes," "probably no," "uncertain", and similarly equivocating options to the list of possible responses (e.g., Wang, [12]; Ready *et al.* [10]) or by asking respondents to rate the *certainty* of their answers on a numerical scale (e.g., Johannesson *et al.* [4, 5], Li and Mattsson [6]). The problem with these approaches is that it is no longer clear which response one should use in defining the choice probabilities and the associated welfare measures. While a number of studies have sought to calibrate CV responses using parallel "real" experimental transactions data (e.g., [4, 5],[2],[7]), a consensus has yet to be reached on the form that such calibrations should take.[3]

Manski [9] proposed an approach for dealing with a particular form of uncertainty in discrete choice settings, one stemming from the incomplete description of the choice scenarios. Space constraints and concerns regarding respondent fatigue lead researchers to provide only a skeletal depiction of the alternatives, highlighting those attributes the researcher views as essential. Yet these descriptions leave much to the imagination of the survey respondent,

[2]One can using the standard RUM model to incorporate preference uncertainty by assuming that the individual's choice is made on the basis of expected utility and that the conditional utility function itself is quadratic. The choice between alternatives in the this case would be a function of the perceived mean and variance of each alternative's utility. To out knowledge, however, this approach has not been used to date, in part because of the difficulty in eliciting each individual's perceptions regarding the distribution of their own conditional utilities.

[3]There is also the broader and more fundamental issue as to what decision rule survey respondents use when answering discrete choice questions under uncertainty (e.g., Wilcox [13, 14]).

both in terms of the alternatives directly and in terms of their own situation at the time when a real choice might arise. For example, a CV survey might ask a respondent to choose between two alternative lakes with differing levels of water quality. While the survey might describe the alternatives in terms of average water quality measures (such as Secchi Transparency or Phosphorous levels), the respondent is left with considerable uncertainty in terms how these broad scenario descriptions translate into conditions they care about (e.g., fish catch rates, water safety, etc.) at the time they are actually faced with choosing between the two lakes. As Blass *et al.* [1] note, "…[w]hen scenarios are incomplete, stated choices cannot be more than point predictions of actual choices." Masnki [9] suggests capturing the respondent's uncertainty by eliciting choice probabilities rather than a discrete choice. The idea itself is simple, yet elegant. In essence, Manski suggests viewing the survey respondent much like the standard RUM model treats the analyst. The survey respondent *ex ante* (i.e., at the time they are asked to express a preference over, say, option A versus option B) have incomplete information. As such, they can only express the probability that they would *ex post* (i.e., once their information uncertainties are resolved) prefer option A over option B. Blass, *et al.* [1] further develop the approach and present the first empirical estimation of a random utility model using elicited choice probabilities.

This paper extends the literature in two directions. First, Manski [8] suggests that, faced with a discrete choice question, individuals will compute their subjective choice probability for each alternative and choose that alternative with the highest choice probability. Using recent data from the 2009 Iowa Lakes Project, we investigate the convergent validity of the discrete choice and elicited choice probability formats using a split sample. Half of the individuals in the survey were asked to choose between two hypothetical lakes (Lake A and Lake B) with differing attributes, while the other half of the sample were asked to indicate the probability that they would prefer Lake A over Lake B. Second, the model in Blass, *et al.* [1] assumes that the individuals in the sample all have the same underlying distribution characterizing their informational uncertainties. We investigate the realism of this assumption by splitting our samples yet again, with half of the sample receiving a high information treatment, while the other half receives a low information treatment. In this paper, we present our preliminary results from this study, comparing the choice responses in the four treatment groups. We also compare welfare estimates for the four groups using both the model in Blass, *et al.* [1] and the standard logit specification.

2 Modeling Discrete Choices and Elicited Choice Probabilities

We begin by describing the underlying modeling framework for both the standard discrete choice problem and the elicited choice probability setting. In doing so, we parallel a similar presentation in Blass, *et al.* [1], though we pay particular attention to the nature of what is observable by the survey respondent at the time the survey is administered.

2.1 Discrete Choices

In the standard RUM model of a binary choice from among two options ($j = A, B$), it is assumed that the individual i *knows* the utility that they would receive from each option (U_{ij}) and simply chooses that option that maximizes their utility. The stochastic nature of the problem is in the eyes of the analyst alone, who observes only a subset of the factors influencing the individual's decision. For example, suppose that

$$U_{ij} = \alpha_j + \boldsymbol{\beta}_x \boldsymbol{x}_{ij} + \boldsymbol{\beta}_z \boldsymbol{z}_{ij} \tag{1}$$

where both $\boldsymbol{x}_{ij}$ and $\boldsymbol{z}_{ij}$ are known to the decision-maker, but only $\boldsymbol{x}_{ij}$ is observed by the analyst. The outcome that option A is chosen (denoted $y_i = 1$) is determined by the individual by comparing U_{iA} and U_{iB}, with

$$y_i = \begin{cases} 1 & U_{iA} \geq U_{iB} \\ 0 & U_{iA} < U_{iB}. \end{cases} \tag{2}$$

For the analyst, however, the outcome (y_i) is random variable, since $\boldsymbol{z}_{ij}$ is unknown. The utility that individual i receives from alternative j takes the form:

$$\begin{aligned} U_{ij} &= \alpha_j + \boldsymbol{\beta}_x \boldsymbol{x}_{ij} + \tilde{\epsilon}_{ij} && (3) \\ &= \tilde{V}_{ij} + \tilde{\epsilon}_{ij} && (4) \end{aligned}$$

where $\tilde{V}_{ij} \equiv \alpha_j + \boldsymbol{\beta}_x \boldsymbol{x}_{ij}$ and $\tilde{\epsilon}_{ij} \equiv \boldsymbol{\beta}_z \boldsymbol{z}_{ij}$ captures the unobservable factors influencing U_{ij}. Without knowledge of $\boldsymbol{z}_{ij}$ (and hence $\tilde{\epsilon}_{ij}$), the analyst can only make probabilistic statements about the choice between options A and B. Specifically, the conditional probability that option A is chosen (denoted by P_{iA}) is given by

$$\begin{aligned} P_{iA} &= Pr(y_i = 1 | \boldsymbol{x}_{ij}) && (5) \\ &= Pr(U_{iA} \geq U_{iB} | \boldsymbol{x}_{ij}) && (6) \\ &= Pr(\tilde{V}_{iA} + \tilde{\epsilon}_{iA} \geq \tilde{V}_{iB} + \tilde{\epsilon}_{iB} | \boldsymbol{x}_{ij}) && (7) \\ &= Pr(\tilde{\epsilon}_i \leq \tilde{V}_i | \boldsymbol{x}_{ij}), && (8) \end{aligned}$$

where

$$\tilde{V}_i \equiv \tilde{V}_{iA} - \tilde{V}_{iB} \quad (9)$$
$$= (\alpha_A - \alpha_B) + \boldsymbol{\beta}_x(\boldsymbol{x}_{iA} - \boldsymbol{x}_{iB}) \quad (10)$$
$$= \alpha + \boldsymbol{\beta}_x \boldsymbol{x}_i, \quad (11)$$

with $\alpha \equiv \alpha_A - \alpha_B$ and $\boldsymbol{x}_i \equiv \boldsymbol{x}_{iA} - \boldsymbol{x}_{iB}$, and

$$\tilde{\epsilon}_i \equiv \tilde{\epsilon}_{iB} - \tilde{\epsilon}_{iA}. \quad (12)$$

Different assumptions about the unobservables (i.e., the $\tilde{\epsilon}_{ij}$) yields different functional forms for the choice probabilities. For example, if the $\tilde{\epsilon}_{ij}$'s are assumed to be *iid* Type I extreme value random variables, then a logistic model results, with

$$P_{iA} = \frac{exp(\tilde{V}_i)}{1 + exp(\tilde{V}_i)} = \frac{exp(\alpha + \boldsymbol{\beta}_x \boldsymbol{x}_i)}{1 + exp(\alpha + \boldsymbol{\beta}_x \boldsymbol{x}_i)}. \quad (13)$$

More general RUM models result if we assume that there are unobserved individual attributes (say s_i) that interact with $\boldsymbol{x}_{ij}$ in determining U_{ij}.[4] In this case, we might have

$$U_{ij} = \alpha_j + \boldsymbol{\beta}_x \boldsymbol{x}_{ij} + \boldsymbol{\beta}_z \boldsymbol{z}_{ij} + \boldsymbol{\gamma}_{xz} \boldsymbol{x}_{ij} s_i \quad (14)$$
$$= \alpha_j + (\boldsymbol{\beta}_x + \boldsymbol{\gamma}_{xz} s_i)\boldsymbol{x}_{ij} + \boldsymbol{\beta}_z \boldsymbol{z}_{ij} \quad (15)$$
$$= \alpha_j + \boldsymbol{\beta}_{xi} \boldsymbol{x}_{ij} + \tilde{\epsilon}_{ij} \quad (16)$$

where $\boldsymbol{\beta}_{xi} \equiv \boldsymbol{\beta}_x + \boldsymbol{\gamma}_{xz} s_i$ is a random parameter from the analyst's perspective, capturing heterogeneity in consumer preferences induced by s_i. If the $\tilde{\epsilon}_{ij}$'s are again assumed to be *iid* Type I extreme value random variables, then the mixed logit model results (see, e.g., Train [11]), with

$$P_{iA} = \int \frac{exp(\alpha + \boldsymbol{\beta}_{xi} \boldsymbol{x}_i)}{1 + exp(\alpha + \boldsymbol{\beta}_{xi} \boldsymbol{x}_i)} f(\boldsymbol{\beta}_{xi}) d\boldsymbol{\beta}_{xi}, \quad (17)$$

where $f(\boldsymbol{\beta}_{xi})$ is the assumed distribution of the random parameter $\boldsymbol{\beta}_{xi}$.

2.2 Elicited Choice Probabilities

In Manski's [9] elicited choice probabilities setting, the problem is similar to the discrete choice problem, except that now we allow for uncertainty on the part of both the analyst

[4]For ease of notation, we specify these unobserved attributes as a scalar, though this can easily be generalized.

and the decision-maker. Specifically, it is assumed that there are aspects of the choice alternatives that are incompletely described in the survey and about which the decision-maker forms subjective probability distributions.[5] Suppose that $\boldsymbol{z}_{ij}$ is segmented into these *certain* and *uncertain* components, with $\boldsymbol{z}_{ij} = (\boldsymbol{z}_{ij}^{c\,\prime}, \boldsymbol{z}_{ij}^{u\,\prime})'$. The conditional utility that individual i *anticipates* receiving from choosing alternative j described in (1) now becomes:

$$U_{ij} = \alpha_j + \boldsymbol{\beta}_x \boldsymbol{x}_{ij} + \boldsymbol{\beta}_c \boldsymbol{z}_{ij}^c + \boldsymbol{\beta}_u \boldsymbol{z}_{ij}^u \tag{18}$$

$$= V_{ij} + \epsilon_{ij}, \tag{19}$$

where $V_{ij} \equiv \alpha_j + \boldsymbol{\beta}_x \boldsymbol{x}_{ij} + \boldsymbol{\beta}_c \boldsymbol{z}_{ij}^c$ and $\epsilon_{ij} \equiv \boldsymbol{\beta}_u \boldsymbol{z}_{ij}^u$. To fix ideas, suppose we are again considering a dichotomous choice CV question in which respondents are asked to evaluate two competing hypothetical lakes, described in terms of their water quality conditions (say, Secchi Transparency) and the cost of visiting each lake. In this case, $\boldsymbol{x}_{ij}$ would include the choice attributes as described in the survey, along with individual socio-demographic factors elicited via the survey instrument. The $\boldsymbol{z}_{ij}^c$ would include factors known to the decision-maker, but unknown to the analyst, such as their general interest in fishing, whether or not they own a boat, the age of their children, etc. Finally, $\boldsymbol{z}_{ij}^u$ would include those aspects of the alternatives and individual, unknown to both the decision-maker and the analyst, that arise because the survey paints only an incomplete picture of the choice alternatives. For example, $\boldsymbol{z}_{ij}^u$ might include fishing or weather conditions at the respective sites on the day the individual would actually be choosing where to recreate, how they might feel on the day in question, etc. The assumption is that these factors, while unknown to the respondent *ex ante* when the survey is administered, would be resolved *ex post*, when actually making the site selection decision.

Because $\boldsymbol{z}_{ij}^u$ is unknown to the decision-maker, they can no longer identify with certainty which alternative will yield the highest utility. Instead, at the time the analyst elicits choice probabilities, the individual can only reveal their subjective assessment as to which alternative will maximize their utility. With a choice between two alternatives (i.e., $j = A, B$), individual i's subjective choice probability that alternative A would be preferred is given by:

$$q_{iA} = Pr\left[U_{iA} > U_{iB}\right] \tag{20}$$

$$= Pr\left[V_{iA} + \epsilon_{iA} > V_{iB} + \epsilon_{iB}\right] \tag{21}$$

$$= Pr\left[\epsilon_i < V_i\right] \tag{22}$$

where

$$V_i \equiv V_{iA} - V_{iB} \tag{23}$$

$$= \alpha + \boldsymbol{\beta}_x \boldsymbol{x}_i + \boldsymbol{\beta}_c \boldsymbol{z}_i^c, \tag{24}$$

[5] Blass *et al.* [1] emphasize that the uncertainty in this setting is *resolvable uncertainty*; i.e., that the individual anticipates knowing the actual state of the world when eventually faced with choosing among the available alternatives.

with $\boldsymbol{z}_i^c \equiv \boldsymbol{z}_{iA}^c - \boldsymbol{z}_{iB}^c$, and

$$\epsilon_i \equiv \epsilon_{iB} - \epsilon_{iA}. \tag{25}$$

Blass *et al.* [1] assume that $\epsilon_{ij} \overset{iid}{\sim}$ Type I extreme value, in which case the elicited choice probabilities take the familiar logistic form

$$q_{iA} = \frac{exp(V_i)}{1 + exp(V_i)} = \frac{exp(\alpha + \boldsymbol{\beta}_x \boldsymbol{x}_i + \boldsymbol{\beta}_c \boldsymbol{z}_i^c)}{1 + exp(\alpha + \boldsymbol{\beta}_x \boldsymbol{x}_i + \boldsymbol{\beta}_c \boldsymbol{z}_i^c)}. \tag{26}$$

There are several comments that are worth making regarding these subjective choice probabilities. First, the underlying assumption that the ϵ_{ij} are *iid*, while convenient, is a relatively strong one, requiring that all individuals share the same subjective assessments regarding the unobserved attributes of the alternatives presented in the survey scenarios (i.e., the $\boldsymbol{z}_{ij}^u$). Second, while the subjective choice probabilities in (26) are similar in structure to those for the discrete choice setting in (13), they differ in that the subjective choice probabilities are themselves random variables from the analyst's perspective, depending as they do on the attributes $\boldsymbol{z}_{ij}^c$, which are unobservable by analyst, but known to the decision-maker. Blass *et al.* [1] suggest estimating the parameters associated with $\boldsymbol{x}_i$ by using a log-odds transformation of (26), yielding

$$ln\left(\frac{q_{iA}}{q_{iB}}\right) = \alpha + \boldsymbol{\beta}_x \boldsymbol{x}_i + \eta_i, \tag{27}$$

where $\eta_i \equiv \boldsymbol{\beta}_c \boldsymbol{z}_i^c$. Blass, *et al.* [1] suggest estimating equation (27) using a LAD estimator.[6]

As was the case with the discrete choice setting, the elicited choice model can be generalized to allow for preference heterogeneity by introducing interaction terms associated with $\boldsymbol{s}_i$, individual specific factors observed by the decision-maker, but not the analyst. The conditional utility in equation (18) becomes

$$\begin{aligned}
U_{ij} &= \alpha_j + \boldsymbol{\beta}_x \boldsymbol{x}_{ij} + \boldsymbol{\beta}_c \boldsymbol{z}_{ij}^c + \boldsymbol{\beta}_u \boldsymbol{z}_{ij}^u + \boldsymbol{\gamma}_x \boldsymbol{x}_{ij} \boldsymbol{s}_i + \boldsymbol{\gamma}_c \boldsymbol{z}_{ij}^c \boldsymbol{s}_i + \boldsymbol{\gamma}_u \boldsymbol{z}_{ij}^u \boldsymbol{s}_i & (28)\\
&= \alpha_j + (\boldsymbol{\beta}_x + \boldsymbol{\gamma}_x \boldsymbol{s}_i)\boldsymbol{x}_{ij} + (\boldsymbol{\beta}_c + \boldsymbol{\gamma}_c \boldsymbol{s}_i)\boldsymbol{z}_{ij}^c + (\boldsymbol{\beta}_u + \boldsymbol{\gamma}_u \boldsymbol{s}_i)\boldsymbol{z}_{ij}^u & (29)\\
&= \alpha_j + \boldsymbol{\beta}_{xi} \boldsymbol{x}_{ij} + \boldsymbol{\beta}_{ci} \boldsymbol{z}_{ij}^c + \boldsymbol{\beta}_{ui} \boldsymbol{z}_{ij}^u & (30)\\
&= V_{ij} + \epsilon_{ij}, & (31)
\end{aligned}$$

where now $V_{ij} \equiv \alpha_j + \boldsymbol{\beta}_{xi} \boldsymbol{x}_{ij} + \boldsymbol{\beta}_{ci} \boldsymbol{z}_{ij}^c$ has parameters that vary over individuals and $\epsilon_{ij} \equiv \boldsymbol{\beta}_{ui} \boldsymbol{z}_{ij}^u$, where $\boldsymbol{\beta}_{ui} = \boldsymbol{\beta}_u + \boldsymbol{\gamma}_u \boldsymbol{s}_i$. Note that now, even if individuals share the same subjective beliefs about the uncertain site characteristics $\boldsymbol{z}_{ij}^u$, the error term ϵ_{ij} will be heteroskedastic due to differences in $\boldsymbol{\beta}_{ui}$. As an example of this, suppose that $\boldsymbol{z}_{ij}^u$ represents the fishing

[6] The LAD estimator is proposed to deal with a practical problem in elicited choice probability settings, namely the problem with the log-odds transformation in those cases in which $q_{iA} = 0$ or 1. The LAD estimator is not sensitive to outliers, allowing these extreme cases to be handled by replacing $q_{iA} = 0$ or 1 with δ and $1 - \delta$, respectively, where δ is a small number.

conditions at site j and s_i represents an index on the unit interval indicating an individual's general interest in fishing. For an individual who cares about fishing, s_i will be close to one and the corresponding $\boldsymbol{\beta}_{ui}$ will be relatively large. Because they like fishing, any uncertainty they have about fishing conditions at site j ($\boldsymbol{z}^u_{ij}$) induces substantial uncertainty in terms of the utility they anticipate receiving from visiting site j. In contrast, an individual who does not care about fishing will have an s_i close to zero and the corresponding $\boldsymbol{\beta}_{ui}$ will be relatively small. For this non-fisherman, even if they share exactly the same subjective beliefs about the fishing conditions at site j as the avid fisherman, the uncertainty does not translate into uncertainty about U_{ij} since they do not care about the fishing conditions.

The implication of this heteroskedasticity is that the identified parameters of the subjective choice probabilities will now vary by individual. To see this, consider the case in which $\boldsymbol{z}^u_{ij}$ is a scalar, with the z^u_{ij}'s assumed to be *iid* Type I extreme value random variables. Then $\epsilon_{ij} \equiv \beta_{ui} z^u_{ij}$ and the subjective choice probabilities become:

$$\begin{aligned} q_{iA} &= \frac{exp(V_i/\beta_{ui})}{1+exp(V_i/\beta_{ui})} && (32)\\ &= \frac{exp\left(\frac{\alpha+\boldsymbol{\beta}_{xi}\boldsymbol{x}_i+\boldsymbol{\beta}_{ci}\boldsymbol{z}^c_i}{\beta_{ui}}\right)}{1+exp\left(\frac{\alpha+\boldsymbol{\beta}_{xi}\boldsymbol{x}_i+\boldsymbol{\beta}_{ci}\boldsymbol{z}^c_i}{\beta_{ui}}\right)} && (33)\\ &= \frac{exp(\tilde{\alpha}_i+\tilde{\boldsymbol{\beta}}_{xi}\boldsymbol{x}_i+\tilde{\boldsymbol{\beta}}_{ci}\boldsymbol{z}^c_i)}{1+exp(\tilde{\alpha}_i+\tilde{\boldsymbol{\beta}}_{xi}\boldsymbol{x}_i+\tilde{\boldsymbol{\beta}}_{ci}\boldsymbol{z}^c_i)}. && (34) \end{aligned}$$

where

$$\tilde{\alpha}_i \equiv \frac{\alpha}{\beta_{ui}}, \qquad \tilde{\boldsymbol{\beta}}_{xi} \equiv \frac{\boldsymbol{\beta}_{xi}}{\beta_{ui}}, \qquad \text{and} \qquad \tilde{\boldsymbol{\beta}}_{ci} \equiv \frac{\boldsymbol{\beta}_{ci}}{\beta_{ui}}. \tag{35}$$

Note that both $\tilde{\boldsymbol{\beta}}_{xi}$ and $\tilde{\boldsymbol{\beta}}_{ci}$ will vary by individual, even if the corresponding $\boldsymbol{\beta}_{xi}$ and $\boldsymbol{\beta}_{ci}$ do not. The corresponding log-odds equation used for estimation becomes:

$$\begin{aligned} ln\left(\frac{q_{iA}}{q_{iB}}\right) &= \tilde{\alpha}_i+\tilde{\boldsymbol{\beta}}_{xi}\boldsymbol{x}_i+\tilde{\boldsymbol{\beta}}_{ci}\boldsymbol{z}^c_i && (36)\\ &= a+\boldsymbol{b}_x\boldsymbol{x}_i+\tilde{\eta}_i && (37) \end{aligned}$$

where a and $\boldsymbol{b}_x$ denote the mean values of $\tilde{\alpha}_i$ and $\tilde{\boldsymbol{\beta}}_{xi}$, respectively, and

$$\tilde{\eta}_i \equiv (\tilde{\alpha}_i - a) + (\tilde{\boldsymbol{\beta}}_{xi} - \boldsymbol{b})\boldsymbol{x}_i + \tilde{\boldsymbol{\beta}}_{ci}\boldsymbol{z}^c_i. \tag{38}$$

3 The Iowa Lakes Data

The data used in this paper are drawn from the 2009 Iowa Lakes Survey. The survey is part of an ongoing research effort (funded jointly by the Iowa Department of Natural Resources and the U.S. EPA) to understand recreational lake usage in the state and the value residents place in the site and water quality attributes of Iowa's primary recreational lakes. The project began in 2002 with a mail survey of 8000 Iowa households selected at random. The survey elicited the respondents' visitation rates to each of 132 primary lakes, as well as socio-demographic information for each household. Similar surveys were administered to the same households over the next three years, providing a unique panel data picture of lake usage.[7]

The most recent survey, administered in late 2009, was mailed to a total 10,000 Iowa households, consisting of the respondents to the 2005 Lakes Survey (approximately 4500 households) and an additional random sample of Iowa households. As with earlier surveys, respondents were asked to recall their numbers of day- and overnight-trips to each to the 132 primary lakes over the past year, along with providing socio-demographic information. In addition, Section 2 of the survey consisted of a contingent valuation (CV) exercise. It is this section provides the basis for our analysis below.

A total of four versions of the CV exercise were used in the 2009 survey. In all four versions, respondents were asked to compare two hypothetical lakes (A and B). The lakes differed in terms of their water quality attributes, with Lake B being substantially cleaner than Lake A, and in terms of each lake's the distance from the respondent's home and the associated entrance fee. Figure 1 provides the illustration used in both Versions 1 and 3 of the survey. In addition to the illustration, a textual description of each lake was provided. Versions 2 and 4 of the survey also asked respondents to compare Lakes A and B, however less information was provided in both the text and the illustration regarding each lake's condition, especially in terms its fishing conditions. The purpose of these *low information* versions of the survey was induce greater uncertainty for the survey respondent, which should induce corresponding shifts in the estimated preference parameters.

The other distinguishing feature of the four CV versions was the evaluation format employed. Versions 1 and 2 elicited choice probabilities, as suggested by Manski [9], using the text:

> *Assume that you have to choose between visiting one of the two lakes described on the previous page. What are the chances in percentage terms that you would choose to visit Lake A rather than Lake B? The chance of each alternative should be a number between 0 and 100 and the chances given to the two alternatives should add up to 100. For example, if you give a 5% chance to one alternative it means that there is almost no possibility that you will choose that alternative. On*

[7] In 2003, the surveys were sent to respondents to the 2002 survey (approximately 4500 households) and to an additional random sample of households used to return the total sample size once again to 8000 household. In 2004 and 2005, the surveys were sent only to those household that responded in the previous year).

the other hand, if you give an 80% or higher chance to an alternative it means that almost surely you would choose it.

Versions 3 and 4 of the survey, on the other hand, asked respondents to simply choose their preferred alternative. Table 1 summarizes the four versions of the CV exercise in terms of the information and value elicitation formats. Each survey also included a second paired comparison (Lakes C and D), following the same format as the first paired comparison. The second two lakes were identical to the earlier lakes in terms of water quality and site attributes. The only changes were in terms of the distances and entrance fees associated with the two lakes.[8] The overall survey sample was split evenly between the four versions, with 2500 observations randomly assigned to each version. The overall response rate to the survey was approximately sixty percent.

4 Results

In our preliminary analysis of the CV data from the 2009 Lake Survey, we focus our attention on two modeling approaches. First, we provide a direct comparison of the elicited choice and discrete choice survey responses by converting the former into a discrete choice outcome and estimating a simple logit model for both data sources. Second, we employ the LAD estimator proposed by Blass *et al.* [1] to exam the impact of the information treatment on the elicited choice responses.

4.1 Logit Model Comparison

While the elicited choice probabilities format allows respondents to reveal their uncertainty regarding the preferred alternative in a contingent valuation setting, Manksi [8] suggests that there is a direct link between the two elicitation formats. In particular, he argues that when faced with *resolvable uncertainty* in a stated-choice questionnaire, the respondent "...computes his subjective choice probability for each alternative and reports the one with

[8] The distances and entrance fees were varied across individual surveys. Distance were set at one of three levels (10, 30 and 60 miles), while the entrance fees were set at one of three levels (0, 10, and 20 dollars). A balanced design was used, including all possible combinations of the distance and entrance fees for Lakes A and B, excluding those combinations that would designate the cleaner lake (B) as *as closer or closer* and *as cheap or cheaper* when compared to the dirtier lake (A). The distance and entrance fee combinations were similarly assigned for Lakes C and D.

the highest probability" [1, p. 5]. Specifically, it is assumed, in a binary choice setting, that:

$$y_i = 1[q_{iA} \geq q_{iB}], \tag{39}$$

where $1[\cdot]$ is the standard indicator function. There are two reasons to question this logic. First, while the binary choice referendum format has been argued to be incentive compatible, under certain conditions concerning the consequentiality of the survey, there is no comparable result (to our knowledge) for the elicited choice probability format. Second, while the conversion in (39) is intuitively appealing, it is not clear how risk aversion would alter the individual's choice revelation under scenario uncertainty.

In this subsection, we examine the convergent validity of the elicited choice and stated-choice formats by converting the elicited choice probabilities to a binary outcome using (39) and estimating logit models for both data sets. Separate models are estimated for the first (AB) and second (CD) paired comparisons. Three alternative model specifications are considered. The first model pools the data from the high and low information treatments. In this simple specification, it is assumed that $\tilde{V}_i$ in (13) takes the form:

$$\tilde{V}_i = \alpha + \beta_{DO} ODist_i + \beta_{CO} OCost_i \tag{40}$$

where $ODist_i$ and $OCost_i$ denote the additional distance and additional entrance cost associated with the cleaner lake. Since $y_i = 1$ denotes the choice of the dirtier lake in each paired comparison, we would anticipate both β_{DO} and β_{CO} to be positive. The second model controls for potential information effects, distinguishing the marginal impacts of the distance and cost variables for the low and high information treatments. In particular, (40) is generalized to

$$\tilde{V}_i = \alpha + \gamma D_{Li} + (\beta_{DO} + \delta_D D_{Li}) ODist_i + (\beta_{CO} + \delta_C D_{Li}) OCost_i \tag{41}$$

where D_{Li} is a dummy variable indicating that individual i received the low information treatment. Thus, γ, δ_D, and δ_C denote the differential effect for the low information treatment. Constraining these parameters to zero yields the simple model in (40). Finally, one concern in asking multiple questions in a stated preference survey is that the respondents will react, not only to conditions of the current question, but will *anchor* their responses to the other questions in the survey (See, e.g., Herriges and Shogren [3]). The third specification allows for cross-question effects, generalizing the simple model (40) to allow an individual's choice to depend, not only on the distance and entrance fee comparisons presented in the paired comparison, but on the distance and entrance fee presented in the "other" paired comparison. Specifically, we set

$$\tilde{V}_i = \alpha + \beta_{DO} ODist_i + \beta_{CO} OCost_i + \beta_{DC} CDist_i + \beta_{CC} CCost_i \tag{42}$$

where $CDist_i$ and $CCost_i$ denote the distance and cost differentials in the "other" paired comparison. Thus, when modeling the AB- paired comparison, $CDist_i$ and $CCost_i$ denote the distance and cost differentials in the CD-paired comparison. If there are no spillover effects, we would anticipate $\beta_{DC} = \beta_{CC} = 0$. If this condition does not hold, then respondents are making their choices based, not only on the choice in front of them, but on the conditions outlined in other alternatives.

The results from estimating these three models are presented in Tables 2 and 3 for the converted elicited choice probabilities and the stated-choices, respectively. Starting with the simplest model, we see that both data sets yield very similar results. In both cases, the distance and entrance fees have the anticipated positive signs and are statistically significant at a 5% level in both the AB- and CD-paired comparisons. The coefficients are similar for both elicitation formats, though somewhat smaller when the stated-choice format is used. A formal test has yet to be conducted to determine if these differences are statistically significant.

Turning to the model with information effects, we see relatively little evidence that the differing information treatments altered the choices made by survey respondents. Indeed, only in the CD-paired comparison for the elicited choice probabilities format do we see an impact. In this case, δ_D is positive and significant at a 5% level, suggesting that respondents are more responsive to the distance cost of the cleaner lake when less information is provided regarding conditions at the two lakes. The corresponding entrance fee coefficient δ_C is also positive, though significant only at the 10% level.

Finally, estimates of the third model in (42) suggests little evidence of cross-question effects. None of the estimates of β_{DC} and β_{CC} differ significantly from zero at a 5% level or lower. Only in the single case of the CD-paired comparison for the stated choice data do we find a significant impact for the cross-entrance fee at the 10% level, and then the effect is relatively small compared to the own-entrance fee coefficient.

4.2 Least Absolute Deviation Model

While the conversion of the elicited choice probabilities to a discrete choice outcome provides a direct comparison to the state-choice elicitation format, doing so censors much of the information contained in the choice probabilities. In this section, we focus our attention on the choice probabilities data, providing least absolute deviation estimates for the parameters of the log-odds model in (27). Table 4 provides estimates for the three models, analogous in structure to those used in the previous section. It should be kept in mind, however, that the parameters in Table 4 are not directly comparable to those in Tables 2 and 3 in that different parameter scalings underly the identified parameters.

Starting with the simplest model, the LAD estimates reveal very similar results to those obtained using the logit transformation. In particular, we again find that the distance and entrance fee differentials associated with the cleaner lake have a positive and statistically significant impact on the respondents propensity to choose the dirtier lake. As was the case in Table 2, the entrance fee parameters are roughly three times those for the distance variables. This suggests are marginal implicit mileage cost of approximately \$0.16 (since each distance differential induces two miles of additional round-trip travel).

The information effects, as revealed in Table 4, appear to be substantially stronger for the AB-paired comparison using the elicited choice probabilities than when using their censored discrete choice counterparts (in Table 2). All three information parameters are statistically significant at the 5% level, indicating in general that households are more responsive to the distance and entrance fee treatments when they have relatively little information regarding the alternatives than when they are given more detailed water quality and fishing information. The information effects, however, do not appear to be as large or statistically significant in the CD-paired comparisons.

Finally, the LAD log-odds model confirms the earlier results regarding cross-equation effects. None of the cross-equation terms are statistically significant at the 5% level and only one is significant at the 10% level. Even in this case, the cross-equation effect is substantially smaller than the corresponding own-equation parameter.

5 Future Directions

The results described in this paper represent preliminary findings regarding the use of elicited choice probabilities to capture preferences under incomplete CV scenarios. Further research is needed to formally test some of the differences between elicited choice probabilities and stated-choice responses. In addition, it would be preferable to jointly model the AB- and CD-paired comparisons, allowing for correlation between responses to the two scenarios. Finally, the models presented here do not control for observable individual characteristics or for heterogeneity in individual preferences and/or subjective assessments of the uncertainty associated with the CV scenarios. Mixture models (both continuous and discrete) provide one logical avenue for examining these issues.

6 Tables and Figures

Table 1: CV Survey Treatment Options

	Information Treatment	
Elicitation Method	High Information	Low Information
Choice Probabilities	Version 1	Version 2
Discrete Choice	Version 3	Version 4

Table 2: Logit Models Using Converted Elicited Choice Probabilities

	Models					
	Simple Model		Information Effects		Cross-Question Effects	
Parameter	AB	CD	AB	CD	AB	CD
α	-1.39**	-1.36**	-1.40**	-1.28**	-1.39**	-1.42**
	(0.06)	(0.06)	(0.08)	(0.07)	(0.07)	(0.07)
β_{DO}	0.009**	0.010**	0.007**	0.007**	0.009**	0.010**
	(0.002)	(0.001)	(0.002)	(0.002)	(0.002)	(0.001)
β_{CO}	0.028**	0.033**	0.031**	0.027**	0.028**	0.033**
	(0.004)	(0.004)	(0.005)	(.006)	(0.004)	(0.004)
γ			0.01	-0.20		
			(0.11)	(0.11)		
δ_D			0.004	0.007**		
			(0.003)	(0.003)		
δ_C			-0.006	0.012*		
			(0.008)	(0.008)		
β_{DC}					-0.0008	0.0020
					(0.0010)	(0.0014)
β_{CC}					0.003	0.005
					(0.004)	(0.004)

Table 3: Logit Models Using Stated Choices

	Models					
	Simple Model		Information Effects		Cross-Question Effects	
Parameter	AB	CD	AB	CD	AB	CD
α	-1.53**	-1.28**	-1.51**	-1.26**	-1.57**	-1.31**
	(0.06)	(0.05)	(0.09)	(0.07)	(0.07)	(0.06)
β_{DO}	0.008**	0.011**	0.007**	0.013**	0.008**	0.011**
	(0.002)	(0.001)	(0.002)	(0.002)	(0.002)	(0.001)
β_{CO}	0.037**	0.032**	0.040**	0.034**	0.038**	0.032**
	(0.004)	(0.004)	(0.006)	(.005)	(0.004)	(0.004)
γ			-0.06	-0.04		
			(0.13)	(0.11)		
δ_D			0.001	-0.004		
			(0.003)	(0.003)		
δ_C			-0.004	-0.005		
			(0.009)	(0.007)		
β_{DC}					0.0011	-0.001
					(0.0015)	(0.001)
β_{CC}					0.004	0.007*
					(0.004)	(0.004)

Table 4: LAD Parameter Estimates for the Log-Odds Model

	Models					
	Simple Model		Information Effects		Cross-Question Effects	
Parameter	AB	CD	AB	CD	AB	CD
α	-1.48**	-1.49**	-1.42**	-1.55**	-1.44**	-1.47**
	(0.05)	(0.05)	(0.04)	(0.06)	(0.05)	(0.07)
β_{DO}	0.005**	0.008**	0.003**	0.008**	0.005**	0.009**
	(0.002)	(0.002)	(0.001)	(0.002)	(0.001)	(0.002)
β_{CO}	0.014**	0.025**	0.013**	0.029**	0.015**	0.026**
	(0.004)	(0.004)	(0.003)	(.005)	(0.003)	(0.005)
γ			-0.24**	0.11		
			(0.06)	(0.09)		
δ_D			0.008**	0.004		
			(0.002)	(0.003)		
δ_C			0.014**	-0.000		
			(0.005)	(0.007)		
β_{DC}					-0.0021*	-0.001
					(0.0011)	(0.002)
β_{CC}					-0.001	-0.005
					(0.003)	(0.005)

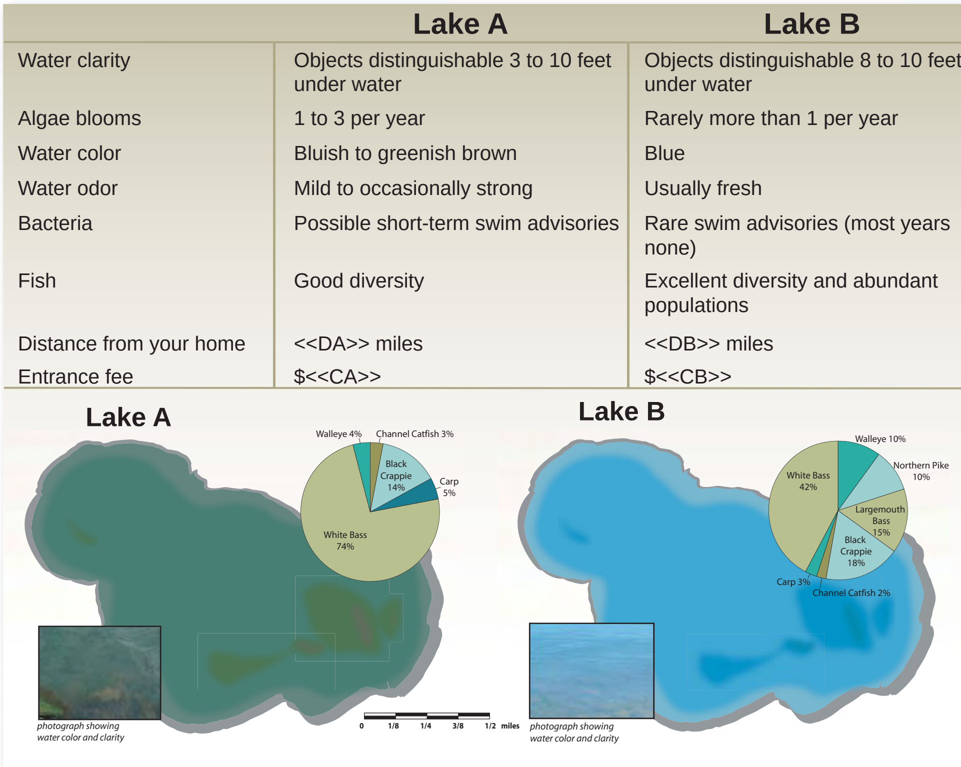


Figure 1: CV Illustration (High Information Treatment - Versions 1 and 3)

References

[1] Blass, A., S. Lach and C. Manski (2010), "Using Elicited Choice Probabilities to Estimate Random Utility Models: Preferences for Electricity Reliability," *International Economic Review*, Vol. **51**(2): 421-440.

[2] Champ, P. A., R. C. Bishop, T.C. Brown, and D. W. McCollum (1997), "Using Donation Mechanisms to Value Nonuse Benefits from Public Goods," *Journal of Environmental Economics and Management*, **33**: 151-162.

[3] Herriges, J.A., and J. Shogren (1996) "Starting Point Bias in Dichotomous Choice Valuation with Follow-up Questioning," *Journal of Environmental Economics and Management*, **30** (1): 112-131

[4] Johannesson, M., G. C. Blomquist, K. Blumenschein, P-. O. Johansson, B. Liljas and R. M. O'Conor (1996), "Calibrating Hypothetical Willingness to Pay Responses," *Journal of Risk and Uncertainty*, **18**1: 21-32.

[5] Johannesson, M., B. Liljas, and P.O. Johansson (1998), "An Experimental Comparison of Dichotomous Choice Contingent Valuation Questions and Real Purchase Decisions," *Applied Economics*, **30**: 643-647.

[6] Li, C.Z., and L. Mattsson (1995), "Discrete Choice Under Preference Uncertainty: An Improved Structural Model for Contingent Valuation,' *Journal of Environmental Economics and Management*, **28**: 256-269.

[7] Loomis, J., and E. Ekstrand (1998), "Alternative Approaches for Incorporating Respondent Uncertainty when Estimating Willingness to Pay: The Case of the Mexican Spotted Owl," *Ecological Economics*, **27**: 29-41.

[8] Manski, C."The Use of Intentions Data to Predict Behavior: A Best Case Analysis," *Journal of the American Statistical Association* **85**: 934-40.

[9] Manski, C. (1999), "Analysis of Choice Expectations in Incomplete Scenarios," *Journal of Risk and Uncertainty*, **19**: 49-66.

[10] Ready, R.C., J. C. Whitehead, and G. C. Blomquist (1995), "Contingent Valuation When Respondents are Ambivalent," *Journal of Environmental Economics and Management*, **29**: 181-196.

[11] Train, K. E. (2010), *Discrete Choice Methods with Simulation, 2nd edition*, Cambridge University Press.

[12] Wang, H. (1997), "Treatment of 'Don't-Know' Responses in Contingent Valuation Surveys: A Random Valuation Model," *Journal of Environmental Economics and Management*, **32**: 219-232.

[13] Wilcox, N. T. (2008), "Stochastic Models for Binary Discrete Choice Under Risk: A Critical Stochastic Modeling Primer and Econometric Comparison," in Cox, J.C., and G.W. Harrison (eds.) *Research in Experimental Economics, Vo. 12: Risk Aversion in Experiments.* Emerald, Bingley, U.K., pp. 197-292.

[14] Wilcox, N. T. (2010), "'Stochastically More Risk Averse:' A Contextual Theory of Stochastic Choice Under Risk," *Journal of Econometrics*, forthcoming.