WESTERN REGIONAL RESEARCH PUBLICATION

W-1133

Benefits and Costs of Resource Policies Affecting Public and Private Land

Proceedings from the Annual Meeting

San Antonio, Texas, February 22-25, 2006

Nineteenth Interim Report

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Introduction

These proceedings contain selected research papers presented at the 2006 Annual Meeting of the W-1133 Regional Project "Benefits and Costs of Resource Policies Affecting Public and Private Lands" in San Antonio, Texas, February 22-25.

The San Antonio meetings brought together 43 attendees representing academic faculty, graduate students, and Federal Government experts in a collegial and productive environment. As reflected in the paper topics, the challenges associated with resource policies and management of open lands remain manifold and multifaceted. Regional Project members continue to produce cutting-edge research that paves the path for efficient and sustainable resource interventions across the nation. Inter-institutional collaboration, facilitated through the W-1133 umbrella, is integral to the success of these efforts. The Annual Meetings, in turn, constitute an ideal "market place" for the exchange of ideas, experience, and visions for efficient resource policies. As a result, many collaboration efforts are initiated and conceptualized at these meetings.

As acting President for the 2005/2006 Project year, I would like to thank my Executive Officers, Ron Fleming (Secretary Treasurer) and Randy Rosenberger (Vice President) for their invaluable assistance in organizing the meetings. I am also indebted to our advisor Don Snyder (Utah State University) and our administrative liaison Fen Hunt (USDA-CSREES) for their continued effort and support of the W-1133 Project. Last but not least, I would like to extend a heartfelt "thank you" to all of you who, through their active participation, made the San Antonio meetings a truly memorable event.

It was an honor and pleasure to serve as W-1133 President. I am looking forward to the next gathering in 2007!

Sincerely yours,

Klaus Moeltner
University of Nevada, Reno

List of Attendees
2006 W-1133 Meetings
San Antonio

Name	Affiliation
John Bergstrom	University of Georgia
Ryan Bosworth	University of Oregon
Tom Brown	U.S. Forest Service, Rocky Mountain Research Station
Daniel Burghart	University of Oregon
Trudy Ann Cameron	University of Oregon
Patty Champ	U.S. Forest Service, Rocky Mountain Research Station
Alison Davis	University of Nevada, Reno
John Duffield	University of Montana
Jeff Englin	University of Nevada, Reno
Jerry Fletcher	West Virginia University
Nicholas Flores	University of Colorado
Kelly Giraud	University of New Hampshire
Tim Haab	Ohio State University
Michael Hand	University of New Mexico
Michael Hanemann	University of California, Berkeley
LeRoy Hansen	USDA / ERS
Robert Hearne	North Dakota State University
John Hoehn	Michigan State University
Wuyang Hu	University of Nevada, Reno
Fen Hunt	CSREES / USDA
Paul Jakus	Utah State University
Rutherford Johnson	University of Kentucky
Hwa Nyeon Kim	Texas A&M University
Robert Johnston	University of Connecticut
Michael Kaplowitz	Michigan State University
Linda Langner	USDA Forest Service
John Loomis	Colorado State University
Frank Lupi	Michigan State University
Dan McCollum	U.S. Forest Service, Rocky Mountain Research Station
Donald McLeod	University of Wyoming
Klaus Moeltner	University of Nevada, Reno
Joan Poor	St. Mary's College of Maryland
Richard Ready	Pennsylvania State University
Kim Rollins	University of Nevada, Reno
Randall Rosenberger	Oregon State University
Douglass Shaw	Texas A&M University
Steven Shultz	University of Nebraska, Omaha
V. Kerry Smith	North Carolina State University
Donald Snyder	Utah State University
Laura Taylor	Georgia State University
Jennifer Thacher	University of New Mexico
Roger von Haefen	NC State University
John Yoder	Washington State University

W1133 Annual Meetings, San Antonio, TX, Feb. 22-25
Conference Program

Wednesday, Feb. 22:

6:00 – 7:30 Reception / Social gathering, "Presidential Suite", Riverwalk Plaza Hotel

Thursday, Feb. 23: *(* = presenter)*

7:30-8:00	**Registration**
8:00 – 8:10	**"Presidential Address" and Welcome**
8:10 – 10:10	**Session 1: Choices and Policies under Uncertainty** **Chair: Tom Brown**
	"The Value of Information Provided by Fish Consumption Advisories" *Paul Jakus**, Christopher Leggett, Ana Maria Ibanez. *Discussant: Douglass Shaw*
	"An Empirical Investigation of Option Prices for Hunting Permits" To N. Nguyen, *W. Douglass Shaw**, Richard T. Woodward, Robert Paterson, Kevin Boyle *Discussant: Tim Haab*
	"Demands for Government-Sponsored Research with Long-term Uncertain Benefits but Near-Term Costs", Trudy Ann Cameron, *Daniel R. Burghart**, Geoffrey R. Gerdes, *Discussant: Paul Jakus*
10:10 – 10:30	**Break with refreshments**
10:30 – 12:30	**Session 2: Elicitation and Econometric Issues in Non-market Valuation** **Chair: Michael Kaplowitz**
	"Do Fishermen Lie? Measuring Hypothetical Bias Across Response Formats" *John W. Duffield**, Patricia A. Champ, David A. Patterson, Chris J. Neher *Discussant: Laura Taylor*
	"Respondent Uncertainty in Choice Experiments: A Comparison of Real and Hypothetical Choices", *Richard Ready**, Jennifer Lawton, Patricia A. Champ
	"A New Approach to Correct for Hypothetical Bias in Stated Preference Models: The Orbit Procedure", *John Loomis**, Steve Davies
	"Reconsidering the Statistical Gains from Dichotomous Choice Contingent Valuation with Follow-Up Questions", Heechan Kang, *Timothy C. Haab** , *Discussant: Kim Rollins*
12: 30 – 2:00	**Lunch (on your own)**
2:00 – 3:30	**Expert panel: "W1133 – A Synthesis of the Past and the Road Ahead"** **Chair / Moderator: Dan McCollum**
	John Bergstrom, John Hoehn, John Loomis, V. Kerry Smith (15 minutes per panelist, 30 minutes open discussion)
3:30 – 3:50	**Break with refreshments**
3:50 – 5:10	**Session 3: Meta-Analysis, Benefit Transfer and Meta-Questions** **Chair: Linda Langner**
	"Generalization, Measurement, and Publication: Sources of Error in Benefit Transfer and their Management", Randall S. Rosenberger*, Tom D. Stanley, *Discussant: Rob Johnston*
	"Sampling Issues and Value Comparison in the Meta Analysis of Ecosystem Benefit Estimates" *John P. Hoehn**
	"What are Ecosystem Services?" *Thomas C. Brown**, John C. Bergstrom, John B. Loomis

Friday, Feb. 24

7:30-8:00	**Registration**
8:00 – 10:00	**Session 4: Advances and Topics in Valuation Methodology / Chair: Michael Hand**
	"Combining Attitudinal and Choice Data to Improve Estimates of Preferences and Preference Heterogeneity: A FIML, Discrete-Choice. Latent-Class Model", William Breffle, Edward Morey, *Jennifer Thacher**, *Discussant: Wuyang Hu*
	"An Empirical Investigation of Consideration Set Models" *Roger von Haefen**, Wiktor Adamowicz, Matthew Massey
	"The Use of Attitudinal Responses as Explanatory Variables in Choice Experiments" *Robert Hearne**
	" Modeling consumers' intended buying decision of GM food: A simultaneous latent variable approach integrating psychometric and econometric aspects of consumer decision-making" Sanjoy Bhattacharjee, Phil Wandschneider, *Jon Yoder**
10:00 – 10:30	**Break with refreshments**
10:30 – 12:30	**Session 2: Welfare Implications of Agricultural and Environmental Policies** **Chair: Robert Hearne**
	"Estimating the Value of Water Use Permits: A Hedonic Approach Applied to Farmland in the Southeastern U.S.", Ragan Petrie, *Laura Taylor**, *Discussant: Alison Davis*
	"Welfare Implications of the Policy Process: Estimating Context-Sensitive Willingness to Pay for Agricultural and Open-Space Conservation", *Robert J. Johnston**, Joshua M. Duke
	"Is an Ounce of Prevention Worth a Pound of Cure?" Ryan Bosworth, *Trudy Ann Cameron**, J.R. DeShazo
	"Measuring the Economic Benefits of Conservation Efforts in a National Park" *Patricia A. Champ**, George L. Peterson, Yann-Jou Lin
	"Benefit Variables Applied in USDA Agri-Environmental Policy Analyses", *LeRoy Hansen**
12: 30 – 2:00	**Lunch (on your own)**
2:00 – 3:20	**Session 6: Public Preferences for Open Land and Environmental Amenities** **Chair: Joan Poor**
	"Measuring Preferences for Stream Restoration under Uncertainty", *Nicholas E. Flores**
	"Water Projects and Exurban Sprawl: WTP for Increased Water Charges as Derived Demand for Environmental Amenities", *Donald M. McLeod**, Roger Coupal, Scott Lieske
	"The Availability and Geographical Specificity of Agricultural Land Value Data: Implications for Hedonic Studies", *Steven Shultz**
	"Amenities, Wages, and the Use of Stated Preference Surveys" *Michael S. Hand**, Jeff Bjarke, Jennifer Thacher, Daniel McCollum
3:20 – 3:40	**Break with refreshments**
3:40 – 4:40	**Session 7: Topics in Recreation Demand Analysis / Chair: Kelly Giraud**
	"Distributional Consequences of Fees in a Discrete Choice Model of Recreation Demand with Incomplete Data: An Application to Mode-Specific Fishing" *Hwa Nyeon Kim**, Richard T. Woodward, W. Douglass Shaw, Wade L. Griffin
	"A Random Utility Travel Cost Model of Deer Hunting in Michigan" Scott Knoche, *Frank Lupi**, Brent Rudolph
	"Valuing Forest Fires: A Large Scale Approach" *Jeffrey Englin**, Thomas Holmes, Janet Lutz ,Adam Longhorn and Juan Marcos Gonzalez
	"Willingness to Accept by Private Land Owners for Consumptive and Non-Consumptive Uses" Ronald A. Fleming, Angelos Pagoulatos, *Rutherford Johnson**, Roger Brown.

Saturday, Feb. 25

8:00 – noon Business meeting (W1133 members only)

An Empirical Study of Option Prices for Hunting Permits

To N. Nguyen, W. Douglass Shaw, Richard T. Woodward*
Texas A&M University
Robert Paterson
Industrial Economics, Incorporated
Kevin Boyle
Virginia Polytechnic Institute

Abstract
Using data from a 1992 survey of Maine hunters, we estimate the willingness to pay for a program that would eliminate the risk of non-participation in an otherwise lottery-rationed moose hunting system. We develop an empirical model to estimate the option price (OP) hunters have for eliminating this risk, based on survey data. We find an estimated OP to eliminate the non-participation risk associated with the lottery system of over $380. The estimated results are compared with the results of 12 years of management experience. We also provide a modest ex post analysis and overview of an auction of a small number of licenses occurring since 1998. Based on the estimates from the model and survey data, we believe that Moose management strategies in recent years would pass a benefit-cost test.

* The authors thank Paan Jindapon for his comments on this paper and Tim Haab and Kerry Smith, as well as other W-1133 conference participants for their comments on a presentation of it. Nguyen is a graduate student, Shaw is Professor and Woodward is Associate Professor. Paterson is a senior economist, and Boyle is Professor and Department Head.

Introduction

In this paper we develop an empirical model within the expected utility framework to evaluate the benefit of eliminating the risk of not being drawn in an annual hunting lottery. The lottery scheme is one that randomly allocates hunting permits when supply is scarce relative to demand. The program we evaluate effectively increases the the probability of obtaining a permit to a value of one. We provide estimates of the option price (OP) for Maine moose hunting permits using referendum data from a survey in 1992 and discuss an ex-post evaluation of the hunting system. The OP is Graham's (1981) measure of ex ante welfare based on the expected utility framework (see also, Cameron, 2005). The measurement of recreation values can be critical for the economically efficient management of hunting activities, especially when federal funding for wildlife management has diminished while at the same time many states face an expansion of urban residential areas and other human activities. The study of hunters' behaviors under the risks involved with permit lotteries produces additional useful inputs for the management over the standard valuation models that assume there is no such risk.

In the hunting survey that provides our data, hunters were asked whether or not they would be willing to pay a certain amount to guarantee themselves a hunting permit in the next year. If they chose not to pay any sum of money for this program, they could still participate in the annual lottery for the hunting permit with the usual number of granted permits. The estimated OP indicates the individual's ex-ante willingness to pay for a program that eliminates the risk of not being drawn.

This research paper makes two contributions to the literature. First, here we explicitly derive the utility-theoretic relationship between how much the hunters are willing to pay for an elimination of non-participation risk and the resulting appropriate welfare measures, conditional on the individual's perception on his or her chance in the lottery being the same as the objective risks presented to the hunter. Second, it provides an empirical application of a model that uses referendum-style survey data to estimate OP, of which there are only a few examples in the environmental economics literature (see also, Cameron 2005; Riddel and Shaw 2006). While some authors claim to have estimated the OP in the past, it is not clear that they actually did so because of the type of question that was asked in a survey, or because of the way that risk is measured and introduced in the empirical model (see Shaw, Riddel and Jakus 2005 and discussion below). The organization of the remainder of this paper is as follows. Section 1 provides a brief review of the literature on valuing hunting permits and on valuing environmental changes that involve uncertainty with the focus on the relevant econometric estimation methods. Section 2 presents the theoretical and econometric models of the OP for the Maine moose hunting permit. Section 3 describes the survey, and questionnaires. Section 4 discusses data and the empirical results based on these data, and in Section 5 we offer conclusions.

1 Literature review

In this section, we review the hunting valuation literature that most closely pertains to this paper. First, we briefly review the travel cost method (TCM) literature on the valuation of hunting permits under a lottery-rationed system. Next, we briefly discuss the referendum contingent valuation method and the generic model for estimating OP.

1.1 Lottery-rationed hunting valuation with TCM

Within the non-market valuation literature, the estimation of the value of hunting and other recreational activities under a lottery-rationed system has been studied using various approaches. In such studies the hunting value is different than the usual values for resources or recreational activities because the supply of permits is constrained through a lottery. Loomis (1982), Boxall (1995) and Scrogin, Berrens, and Bohara (2000) propose variants on the travel cost framework to model the demand at aggregate or individual level. As an alternative to the standard travel cost method, a hedonic regression model is presented in Buschena, Anderson, and Leonard (2001) for obtaining the marginal value of a hunting permit.

Traditionally, the estimation of expected Marshallian consumer surplus for a hunting activity follows the standard travel cost method (TCM). The TCM utilizes the total number of trips actually taken as the dependent variable, with no risk or uncertainty prevalent in the model. It is implicitly assumed that the individual hunter knows everything with certainty, including how many trips he or she will take, environmental and stock conditions at the hunting areas, etc. However, this certainty approach is inappropriate in the context of a lottery-based hunting system because the lottery introduces an element of risk in participating in the activity. For example, Loomis (1982) showed that the standard TCM would result in biased estimation when a lottery system for hunting permits pertains, and suggested a modified version of the TCM that specifies per capita hunting permit applications in zones of origin as the dependent variable. This modified model follows the zonal TCM structure, which refers to the use of zonal level of data as against individual level. Scrogin, Berrens, and Bohara (2000) also essentially apply a zonal TCM, in which total zonal hunting permit applications for each site were treated as counts within a count-data model. They use their data to estimate expected consumer surplus associated with lottery-rationed hunting permits.

As an alternative to the zonal structure, Boxall (1995) presented the discrete choice TCM using data on individual choices of alternative lottery-rationed hunts for estimation of compensating surplus for a permit and for changes in site attributes. At the individual level, applications for hunting permits at specific hunting sites (destinations) were appropriately modeled as a discrete choice among a limited set of sites. Boxall's model estimation follows the multinomial logit approach. Further, in realizing the effect of uncertainty in getting a permit, Boxall's model specified permit applicants' site choices based on their expected utilities. In addition, hunters were assumed homogeneous in their perception about the chance of being drawn. The chances were based on the probabilities of obtaining permits in the previous year.

More recently Scrogin and Berrens (2003) investigated a discrete choice model estimated in two stages. In their first stage of their model, individual expected access probabilities were estimated for the alternative lotteries by modeling the observed binary outcomes of being drawn or not drawn. Explanatory variables for the model of expected access probabilities include the probability of being drawn in the previous season and participant characteristics. In the second stage, the lottery choice model was developed by following the multinomial logit framework, conditioned on the first stage estimates of the access probabilities.

With the prevalence of using individual level of data, the discrete choice travel cost models seem to have emerged as the preferred approach to derive the value of lottery-rationed hunting and other similar recreational activities. However, as recognized in Boxall (1995), Scrogin and Berrens (2003), and Akabua et al. (1999) the key and challenging task in the analysis of these models is the specification of the hunters' individual perceived probability. This problem continues to be a concern in the literature.

Quite recently, Ananda and Herath (2005) discuss the importance of including a degree of uncertainty, attributed to lack of information on forest ecosystems and their processes, into decision-making and management of forest lands. The risks associated with lotteries that randomly allocate hunting permits are different than those associated with ecosystem information, but these risks are certainly related to one another. Just like managers of lands and habitat, those charged with managing species in which rights are allocated through a lottery must consider behavior under uncertainty.

In the next section we carefully explain the option price concept for the benefit of those readers not familiar with Graham's (1981) paper, and refer to the referendum-style contingent valuation method (CVM) literature to set the stage for our own model.

1.2 Option price and referendum contingent valuation method

1.2.1 Option price

The OP instead of other measures of ex ante welfare, such as the option value or expected surplus, has been shown to be the appropriate measure for valuing environmental changes under conditions involving risk (Graham, 1981). To clarify the meaning of the OP, first consider the example of a public project or policy that will improve on the quality (or level) of environmental service. Assume the quality of environmental service (Q) takes a value of Q_0 or Q_1 contingent on state of nature ω (e.g.: weather), either good (ω=1) or bad (ω=0) respectively. The benefit of the project is generated from increasing the quality from Q_1 to Q_1' in the good state of nature and from Q_0 to Q_0' in the bad state. For example, Graham (1981) considers weather-related events such as droughts and floods and the impact of providing a dam on the effects of either negative event.

Assume further that the probability of the good state is π and that of the bad state is (1-π). These probabilities are also assumed to be well-known to individuals, unlike the case where there are conditions of "uncertainty" or ambiguity (see Riddel and Shaw 2006). We thus far have:

$$Q(\omega)=\begin{cases} Q_1 \quad if\ \omega=1\ \ (good\ state),\ prob=\pi \\ Q_0 \quad if\ \omega=0\ (bad\ state)\ \ ,\ prob=1-\pi \end{cases}$$

Next, let $U(Q_j, M)$ where j = 0, 1 be the ex-post indirect utility function that is common to the individuals and M be monetary income.

The expected surplus E(S) measure associated with this utility function is defined as the probability weighted sum of the compensating surpluses in the cases that the state of nature is good or bad. Let the surplus for an individual be S_1 in the good state and S_0 in the bad state. Then, the expected surplus is calculated as: $\pi S_1 + (1-\pi) S_0$. The values of S_1 and S_0 for an individual can be obtained by asking for the sure payment he or she is willing to pay for the project when the state of nature is observed. Formally, they are solutions of the equations:

$$U(Q_1, M) = U(Q_1', M - S_1) \qquad \text{for good state} \qquad [1]$$

and

$$U(Q_0, M) = U(Q_0', M - S_0) \qquad \text{for bad state} \qquad [2]$$

As noted in the introduction, it may well be that in some previous work, while some authors claimed to have estimated the OP, they may have in fact simply estimated the E(S).

Now we define the individual's OP. It is defined as the maximum amount that the individual is willing to pay for the project regardless of the state of nature tomorrow. For a formal definition of OP, let the expected utility of the individual at the status quo (without the project being undertaken) be V*, then we have:

$$V^* = \pi U(Q_1, M) + (1-\pi) U(Q_0, M) \qquad [3]$$

For an individual who is assumed to be expected-utility maximizing, the amount of payment is chosen such that his or her new expected utility is not less than in the status quo. The values of OP as defined will solve the equation:

$$\pi U(Q_1', M - OP) + (1-\pi) U(Q_0', M - OP) = V^* \qquad [4]$$

where V* is defined in [3].

If the OP is obtained via a survey question, the question must make it clear to the individual that the state of nature that will hold cannot be determined, and that the individual must pay his or her OP whatever the state of nature will occur. In general, the values of E(S) and OP are different. For a more detailed discussion about OP and expected surplus, see Graham (1981), Smith (1992), and Cameron (2005).

A large body of literature studies the issues of the option price and other ex-ante welfare measures under the microeconomic theory, but does not offer a careful discussion of how to actually estimate the ex-ante welfare measure using data (see further discussion in Shaw, Riddel, Jakus, 2005). For this reason, it may be that previous work that reports an OP must be viewed carefully. In some early studies there is no careful derivation of the OP and its resulting form, which is based on the specification of the appropriate risk distribution that will relate to equation [4], as well as the functional form the utility function takes. It is also important to discern whether an OP, rather than the E(S), was sought after in the questions that individuals were asked in any previous survey.

1.2.2 The discrete-choice contingent valuation method

In order to empirically estimate OP as well as in other CVM practices, the use of referendum-style CVM has become very popular. In a typical referendum CVM application to hunting (no lottery involved), respondents might be asked if they are willing to pay to secure an improvement in the species population. Strictly speaking, a referendum format means that individuals are told that there will be a vote, and that the program will not be undertaken unless the majority (or some decision rule) votes for the referendum to support the program. However, the discrete choice style of asking the question (i.e. would you pay $X or not?) is often referred to as the referendum-style CVM even when there is no test of the vote.

Any errors or randomness in the conventional discrete choice or referendum CVM model (one without risk or uncertainty) are assumed to be attributable to the investigator's failure to observe all the dimensions of the problem. These errors are typically introduced in a fashion that leads to estimation using the logit or probit models of discrete choice. Such errors are the conventional "investigator's" error and they are not synonymous with the randomness introduced as part of a known risk.

Hanemann (1984) introduced the use of the referendum CVM and the random utility model (RUM) approach to build logit model for estimation of the Hicksian compensating and equivalent surplus for a hunting permit. Recently, Cameron (2005) used a modified version of the referendum CVM approach, allowing for risk. Her derivation of the resulting OP is quite distinct, involving specification of the expected utility in different states that pertain to global climate change. She estimates individual OP's for global climate change mitigation programs. Note that her resulting model is quite different from just the basic logit or probit CVM that appears in the previous literature. It is clear that a conventional logit, with no modifications, and the resulting Hicksian welfare measures (the expected CV and EV) do not produce the same welfare measure as the OP. This is easily seen in the next section.

Riddel and Shaw (2006) extended the approach taken by Cameron (2005) (actually using an earlier unpublished version of her paper) to cover the effects of ambiguity in analyzing the effects of the risk from transporting nuclear waste on household location decision. Ambiguity arises when the risks the individual faces are poorly understood or for some reason imprecise. We note that the “quasi” OP that Riddel and Shaw (2006) derive and estimate has properties that are under current investigation. This research (see Shaw et al. 2006) concerns whether an OP under assumptions of Knightian uncertainty (ambiguity) has the same properties as the OP based on the more simple expected utility model used by Graham (1981).

2 The empirical model for OP

The objective of this section is to develop a specific econometric model for the OP and derive the equation that allows the calculation of the OP for increasing the probability of obtaining a hunting permit to a value of one, i.e. a guarantee for permit. This elimination of risk inherent in the lottery is what is presented to hunters in the survey questionnaire.

Again, let M be income and the states be specified with the j index (j = 1 if awarded a permit, and j = 0 if not awarded a permit). Suppose the individual derives his or her utility from income and other non-income activities such as hunting. Further assume that the individual utility function is linear in logarithm of income, i.e. assuming individuals are risk averse with respect to risk in income:

$$U(j, M) = \alpha_j + \beta \log(M) \qquad [5]$$

where β denotes marginal utility of a one-percentage increase in income M; α_1 is all non-income utility including the utility obtained from hunting and α_0 is all non-income utility without hunting taking place. Non-income utility differs whether one hunts or not because of the value of this constant term and so $(\alpha_1 - \alpha_0)$ representing the utility purely derived from hunting is expected to have positive sign.

Note that some modelers of value based on the discrete-choice contingent valuation approach assume that the utility function is linear in income and hence no income effect. While the assumption of no income effects is perhaps defensible in models with no risk or uncertainty, we think it is less defensible to assume this in a risk model, as it implies that all individuals are risk neutral with respect to income.

The discrete-choice CVM question offers the individual the option of buying a permit with certainty at a price R. Hence, the individual chooses between the expected utility if they answer "Yes" and pay the price R, and that obtained if they answer "No", V_y and V_n respectively:

$$V_y = \alpha_1 + \beta \log(M\text{-}R\text{-}C) + \varepsilon_y \qquad [6]$$

$$V_n = \pi [\alpha_1 + \beta \log(M\text{-}C)] + (1\text{-}\pi) [\alpha_0 + \beta \log(M)] + \varepsilon_n \qquad [7]$$

where C is the hunter's travel cost for a trip to the hunting site, π is the probability of being drawn in the lottery and the ε terms reflect components of the utility that are unobserved by the researcher. What is different here from the usual (no risk) model is the expected utility derivation above. When the hunter says yes, he or she is guaranteed a permit, so the probability of obtaining a permit is increased to one. In [6] the hunter receives a permit with certainty; implicitly $\pi = 1$. In [7] the hunter declines the purchase of the guarantee and thus must take his or her chances of obtaining a permit. The first term on the right-hand-side of [7] represents the expected utility associated with being drawn. The second term represents the expected utility associated with not being drawn in the lottery. In this case the hunter keeps all his or her income, not paying the option price nor the travel costs for a trip. As in Graham's application of the expected utility framework, the risk model is state dependent: utility functions differ in their constant term specification in the two states (hunting vs. not hunting).

When offered an option to purchase the hunting permit, a respondent will accept the offer if the expected utility difference $\Delta V = (V_y - V_n) > 0$ and refuse it if otherwise. By subtracting [7] from [6] and rearranging, we reach the binary choice model with allowance for the risk associated with the lottery:

$$\Delta V = V_y - V_n = \alpha - \beta Q + \varepsilon \qquad [8]$$

where: $\varepsilon = (\varepsilon_y - \varepsilon_n)$;

$\alpha = (\alpha_1 - \alpha_0)(1-\pi)$;

and: $Q = [\pi \log(M\text{-}C) + (1\text{-}\pi) \log(M)] - \log(M\text{-}R\text{-}C)$

In fact, in the sample under study travel costs are very small to incomes (C << M) and Q can be approximated as Q ≈ log(M) - log(M-R-C). The term Q is the expected reduction in logarithm of net income when buying the offer. In other words, Q measures, in logarithm term, the expected increase in expenditure for hunting by buying the offer instead of by participating in the lottery. On the benefit side, the constant term α in [8] reflects the gain in expected hunting utility if buying the offer. On the cost side, the term β * Q reflects the loss in expected utility caused by bid price and travel cost if buying the offer.

Assuming ε follows a logistic distribution, we can estimate the parameters α and β in [8] by using a logit model with the observed Yes / No responses to the option offer being the dependent variable. Given the estimated values of α and β, the individual OP can be obtained by setting ΔV in [8] equal to zero and solving for bid R. First, solve for Q from the equation ΔV = 0:

$$\mathrm{Q} = \log\left(\frac{(M)^{1-\pi}(M-C)^{\pi}}{M-C-R}\right) = (\alpha / \beta) + (\varepsilon / \beta)$$

Then take exponents of both sides and solve for bid R to have:

$$\mathrm{OP} = \mathrm{R} = (\mathrm{M} - \mathrm{C}) - (\mathrm{M} - \mathrm{C})^{\pi}\ \mathrm{M}^{1-\pi} \exp[-(\alpha/\beta)]\exp[-\varepsilon/\beta] \qquad [9]$$

Note that the OP is a function of ε and so it is a random variable. Let the variable EOP be the expected value of OP with respect to ε. Take expectation for both sides of [9] to derive EOP, noting that $E_\varepsilon\{\exp[-\varepsilon/\beta]\}$ is moment generating function of logistic distribution at (-1/β) and equal to $Bta(1-\frac{1}{\beta}\,;1+\frac{1}{\beta})$ where Bta( ) is the beta function:

$$\mathrm{EOP} = \mathrm{E}_\varepsilon(\mathrm{OP}) = (M-C) - M^{1-\pi}(M-C)^{\pi} * \exp\left(-\frac{\alpha}{\beta}\right) * Bta\left(1-\frac{1}{\beta}\,;1+\frac{1}{\beta}\right) \qquad [10]$$

In fact, as mentioned previously, C << M and so the equation [10] can be approximated to be [11], in which EOP is presented as a portion of income given appropriate values of α and β:

$$\mathrm{EOP} \approx M * \left\{1-\exp\left(-\frac{\alpha}{\beta}\right) * Bta\left(1-\frac{1}{\beta};1+\frac{1}{\beta}\right)\right\} \qquad [11]$$

It is shown from the EOP equation [10] that the effects of risk (π) on EOP are indirectly through income as well as through hunting utility (α_1-α_0). This is an ex-ante measure of welfare. Other models often seem to produce an ex-post or expected consumer's surplus welfare measure rather than an OP, or expected OP (Shaw, Riddel and Jakus 2005).

For the rest of the paper, we apply the empirical model derived in this section to estimate the OP for the case of the Maine moose hunting lottery. Following that, we conduct a simple ex-post cost/benefit evaluation of the proposal to offer additional permits as options in Maine.

3 Maine moose hunting and the survey

Moose hunting in Maine is regulated much like in other states in the US and in Canada. One must apply for a permit in each year to be able to hunt in one of nineteen Wildlife Management Districts, which cover over 21,000 square miles and include six zones: NW, NE, C, SW, SC, and SE. The applicants take a chance in a public lottery conducted in mid-June of each year. Successful applicants will have a hunting season that is 6 days long. The success rate of hunters (those that killed or "bagged" a moose) in 1992 was 91%. For virtually all moose hunters then, winning the lottery leads to a high chance of bagging a moose.

In 1992 the 900 permits were to be awarded to hunt moose and as a result 69,237 individuals applied to participate in the permit lottery. Thus, the probability of being selected in the lottery (π) was 1.3 percent. This probability is similar to that of preceding years. In that year a random sample of 900 residents who applied for but did not receive a permit were sent a survey asking about a proposal to allow a small group of hunters the right to buy a permit with certainty outside of the lottery.[1] This sample of individuals was drawn using the same procedures as was used to allocate the 900 hunting permits and the response rate for this survey was 78 percent.

We focus on two main sections in this survey. First, there were a number of questions regarding the travel costs the hunter may incur, such as travel distance and time as well. Second, there was an OP question. The respondent was informed that the probability of winning the lottery in the previous year was 1.3%. They were also informed that the Maine Legislature had increased the number of moose hunting permits issued to Maine residents from 900 to 1000. The

[1] The 900 residents who received a permit were also sent a survey. These responses are not relevant to this study about option price.

extra 100 permits were to be sold to resident hunters under the program to cover the current costs of managing Maine's moose herd. Then he or she was offered an amount to guarantee a permit the year after. They were asked to response "yes" or "no" to purchase this guarantee. If they did not want to buy the guarantee, they could still participate in the annual lottery.

The last section of the survey elicits the socio-economic characteristics (age, gender, education, and income) of the individual. Income is categorized into 16 interval ranges and the respondents' income varies from less than $5,000 to more than $100,000. Shown in Table I is a profile for the resident respondents. The data shows that there is only a small portion of respondents, 46 out of the 704 respondents, who have ever hunted in Maine as a permit holder, and 70 other people hunted as a subpermittee, a guest of the permittee without a right to an additional moose. The data also shows that respondents have expended a great deal of effort to obtain a permit. On average, respondents had applied 7.3 times in the annual lotteries during the 1980-1991 period. Within the sample, there are 265 respondents who applied every year during this period. These permits are clearly highly prized, at least based on the effort exerted to get them.

Table I: *A profile of the resident respondents*

Description	Frequency
Number of respondents	704
males	565
Average income	$32,662
Average age	41 years
Hunting experience:	
Ever hunted moose in Maine prior to 1992	116 people
as a permit holder	46
as a subpermittee	70
Hunting as a subpermittee in Maine in 1992	7
Past attempts to get a permit:	
Average years of having applications during 1980-1991	7.30 years
Have applied every year during 1980-1991	265 people

4 Data and estimation results

4.1 Data description

Table II shows the summary statistics of data used for estimation of the logit model [8]. In this table, the response variable (ANSWER) and Q are the two key variables to estimate the logit model [8] while bid price (BID), travel cost (TRAVEL), and income (INC) data are included in the value of Q. The other variables used for the variant models include socio-demographic characteristics (AGE, MALE, and EDUC) and hunting related factors (EVER for hunting experience and APPS for past effort to obtain a permit).

Table II: *Summary statistics of data*

Variable	**Description**	**Mean**	**Std.Dev.**	**Min**	**Max**
ANSWER	Response (1: Yes, 0: No)	0.36	0.48	0	1
BID	Referendum price	1341	1571	9	4320
TRAVEL	Travel cost	66.38	51.81	0	320
INC	Income	32662	21468	5000	105000
Q	Expected total cost of hunting in log term	0.09	0.22	0.00021	2.09378
AGE	Age in years	40.86	15.40	10	86
MALE	Dummy (1: male, 0: female)	0.81	0.39	0	1
EDUC	Ordinal categories (degrees) from 1 to 8	3.41	1.42	1	8
EVER	Dummy for hunting experience (1: ever hunted before 1992 and 0: never)	0.16	0.37	0	1
APPS	Number of applications from 1980 to 1991	7.30	3.81	1	11

Five levels of bids were used, ranging from $9 to $4320. Table III shows how the percentage willing to pay a particular bid tends to decline as the bid level increases.

Table III: *Bids and percentages of YES*

Bid	**Total Cases**	**Yes Responses**	**Percentage of Yes Responses of Total**
9	146	124	84.9%
128	126	80	63.5%
780	147	13	8.8%
1433	144	10	6.9%
4320	140	16	11.4%
	703	243	34.6%

The mean travel cost is about $66 per individual. The travel cost is calculated as product of round-trip distance and estimated per-mile cost $0.32, to the nearest hunting site to the hunter. The average travel cost is much lower than the average referendum price offered to the hunter. Note that in the binary choice model, in a case where travel cost is far below the offered referendum payment, then the payment amount will likely dominate the travel cost in determining the outcome (Yes/No).

4.2 Model estimation

We estimate the model [8] with three variant specifications denoted by M-1, M-2, and M-3 and the results are reported in Table IV. Model M-1 includes constant term and the key variable, Q. M-1 is considered as the basic model while other models are variants. Model M-2 augments M-1 with the two variables of gender and education. Model M-3 augments M-2 with age, hunting experience, and past effort to obtain a permit. The socio-demographic variables (age, gender, and education) are introduced into the variant models as interaction terms with Q, as a result of assuming the β coefficient (marginal utility of a one-percentage increase in income) to be a linear function of these variables. On the other hand, the hunting related variables are introduced into M-3 as interaction terms with $(1-\pi)$ as a result of assuming these factors linearly affecting hunting utility $(\alpha_1-\alpha_0)$[2].

[2] Together, we assume the function form of utility augmented with individual characteristics Z to be: $U(j, M; Z) = \alpha_j^0 + \alpha_j^z Z + (\beta^0 + \beta^z Z) M$. Note that α_j^z, marginal hunting utility of Z, is assumed state-dependent, otherwise it will be canceled when taking the utility difference ΔV.

Table IV: *Model estimation results*

Variable	**M-1** Coef.	[p-val]	**M-2** Coef.	[p-val]	**M-3** Coef.	[p-val]
Constant	2.04	[0.000]	2.08	[0.000]	2.09	[0.000]
(1-π) x EVER					-0.21	[0.646]
(1-π) x APPS					0.0044	[0.924]
(- Q)	154.58	[0.000]	162.97	[0.000]	189.95	[0.003]
(- Q) x AGE					-0.68	[0.265]
(- Q) x MALE			-54.79	[0.076]	-54.05	[0.095]
(- Q) x EDUC			10.91	[0.078]	11.09	[0.098]
Log likelihood	-247		-221		-220	
D.F.	1		3		6	
McFadden's R^2	0.357		0.373		0.375	

The estimation result shows that α and β, the coefficients of constant term and Q respectively, are consistently significant in all three models, with p-values near zero. They take positive signs, as expected according to underlying theory and assumptions. Gender and education interacted with Q, are significant at the 10% level. The negative sign on the interaction term with gender predicts that there's greater chance for a male respondent to accept the offer than a female, assuming the same values for other characteristics. Higher education is expected to have negative effect on the chance to accept the offer for the permit guarantee. Age, hunting experience, and past effort for a permit are statitistically insignificant, as shown in M-3.

Further, the LR-test statistic for M-3 and M-2 is computed to be 1.572 and we fail to reject the null hypothesis that all three additional variables in M-3 are zero simultaneously. The LR-stat for M-2 and M-1 is 51.479 leading to rejecting the null. In terms of goodness of fit to data, R-square of M-2 is a bit better than that of M-1 but R-square of M-3 is not improved considerably compared to M2. In addition, log-likelihood of M-3 is not much different from that of M-2. In all, we prefer to use the model M-2 for estimating OP in the next section.

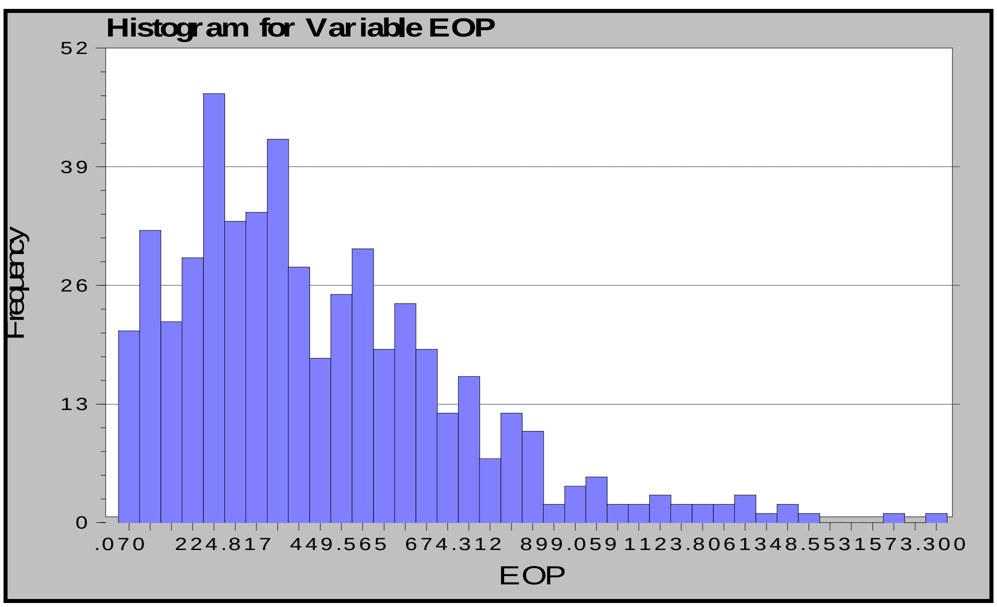
4.3 Estimation results for individual EOP

The individual EOPs over the sample are computed by substituting the estimated coefficients of the model M-2 into the EOP equation [10]. The summary statistics of EOP is reported in Table V and the histogram in Fig-1[3]. We find the average EOPs over the sample is $384.65. This approach finds that more than 80% of respondents have an implied EOP greater than $77 and less than $740.

Table V: *EOP over the sample*

Mean	Std.Dev.	Minimum	Maximum	Cases
384.65	274.62	0.07	1613.64	514

Figure 1: *Distribution of EOP over the sample*



In order to evaluate how well the logit model we estimate predicts the binary responses, we use the prediction rule based on the comparison between individual EOP and referendum price: the predicted response takes value of 1 (Yes to buy the option) if the EOP exceeds the

[3] The sample of EOP is truncated at zero to take out 17 out of 531 EOPs that are negative. The average EOP of the sample without truncation is $370.79.

referendum price and of 0 otherwise. The evaluation result is reported in Table VI. As shown in this table, the model correctly predicts in 443 cases out of 514, or for approximately 84% of the responses.

Table VI: *Actual and predicted responses*

		Predicted		
		"No"	"Yes"	Total
Actual	"No"	276	42	318
	"Yes"	39	157	196
	Total	315	199	514

4.4 An ex-post evaluation for the permit increase program

Ex-post (after the fact) analyses are quite rare in environmental economics. We cannot do a complete one here, but it is interesting to examine some facts that shed some light on whether the permit increase program posed in the survey would lead to positive economic net benefits for Maine hunters. As seen in Table VII, from 1990 to 2000 the number of Moose permits issued in Maine increased substantially, from 1,000 to 3,000. This provides an opportunity to assess the benefits and costs if the state had actually gone through with its plan to auction off some permits as presented in the survey.

The EOP estimation presented above might be useful as an indicator of the benefit input to the evaluation of program, especially if it is costly to actually provide habitat or moose populations consistent with the program. The benefits of this kind of auction can be estimated using the model estimated above. Assume that the highest bidders received the permit, as would be true in an auction of permits. Using individual predictions as presented in Figure 1, we assume that the 100 highest sample bids are relevant in this case. An estimate of the benefits of the program would equal the sum of the highest EOPs, $81,832.

A complete enumeration of the costs of extending the number of permits by 100 cannot be done here, but it is possible to look at one possible cost – the impact of the expansion of the permits on the hunting experience of the remaining hunters. We reviewed the data from the Maine moose hunts in the 1990's, looking at changes in the number of permits and the success rates of the hunters. As shown in Table VII, the success rates in Maine over the years 1990-2000 indicate a sustained rate a bit higher than 90%, on average, despite dramatic changes in the number of permits issued. When the number of permits increases from around 1,000 (the 1992 level) to 2,000, the success rate does not decline from the 1992 level of 91%. However, we note that when permits issued in 1999 and 2000 increase to 3,000, the success rate does decline. We conclude, therefore, that had the State expanded the number of permits by 100 in 1992, this would have had negligible effect on the success rate for Maine hunters considerably. This suggests that a license auction program might have yielded positive net benefits to Maine moose hunters.

An additional piece of evidence is obtained by an actual auction that has been held every year since 1998. Each year five licenses are auctioned off to generate money for youth wildlife and conservation education scholarships in Maine. The auction is conducted in accordance with the first-price sealed-bid format. According to the procedure, an applicant submits the permit bid form along with a nonrefundable $25.00 bidding fee by the deadline. The highest five bidders will be awarded permits and they must pay their bid amount. The winning bids in these auctions are presented in Table VIII. Bids increased significantly during the first three years of the program, but have been relatively stable since that time. In 2004 the winning bids ranged from $8,735 to $11,300. These winning bids are considerably higher than the OP estimates based on the 1992 survey. Reviewing Table III, one can see that the maximum amount offered as payment to our sample of hunters was around $4,300. Even adjusted for inflation, this is still a substantially lower amount than the $11,300 amount observed in the 2004 auction.[4] However, the top bids in the auctions should be viewed differently from our mean bid for several reasons. The predicted bids suggest the best guess as to the OP of an individual based on his or her specific income, travel cost, education, and gender. We would expect 50% of the population to have actual bids above that level and 50% would be below that level. The winning bids in the auction, however, reveal the willingness to pay of only the upper end of the distribution. The winning bids represent only the top five of what might be 69,000 unobserved potential bidders; they represent the upper most tail of the distribution (far fewer than 1 percent). In fact, the error term assumed to have logistic distribution in the logit model allows a positive chance, even though small, for very high bids to be accepted.

Table VII: *Maine statewide moose harvest from 1990-2000* [5]

Season	Harvest	Number of Permits	Success Rate (%)
1990	882	1,000	88
1991	959	1,000	96
1992	908	1,000	91
1993	934	1,000	93
1994	1,130	1,200	94
1995	1,304	1,400	93
1996	1,384	1,400	92
1997	1,374	1,500	92
1998	1,866	2,000	93
1999	2,619	3,000	87
2000	2,552	3,000	85

[4] The purchasing power of $4,300 from 1992 is worth about $5,800 in 2004, using the consumer price index, or CPI.
[5] *Source*: The Maine Department of Inland Fisheries and Wildlife Fisheries & Wildlife Available online at http://www.state.me.us/ifw/hunttrap/hunt_management/moose.htm

Table VIII: *Winning bids in Maine auction of moose permits (1998-2005)*

	Lowest	**Highest**
1998	6,150	8,000
1999	7,100	9,200
2000	8,200	10,201
2001	9,252	11,000
2002	10,101	11,327
2003	9,625	12,501
2004	8,735	11,300
2005	10,150	10,880

Source: Personal communication, Mark Ostermann, Maine Department of Inland Fisheries & Wildlife.

5 Conclusion

In this paper we have presented an empirical model to value the elimination of risk of not being drawn in a lottery that randomly allocates the hunting permits. We provide an estimate of the mean OP for the Maine moose hunting permit from the 1992 survey. The theoretical derivation from the expected utility framework shows that the individual OP reflects the increase in their expected net hunting benefit thanks to risk elimination. The estimation model specifies the significant determinants of the hunters' responses, including the informed probability of being successful in the annual lottery, referendum price and travel cost. Using data just from our specification, the model correctly predicts the responses of 84% of the cases. However, the initial referendum bids given to respondents were far lower than the actual bids made in recent auctions for permits in Maine. This indicates the importance of ascertaining the range of maximum possible bids in a referendum contingent valuation. Based on our analysis, we believe that expanding the number of licenses and making licenses available outside of the lottery would have passed a benefit-cost test, generating positive net benefits to the state and revenue to the government.

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Demand for government-sponsored research with private and public long-term benefits but near-term costs: Climate change adaptation

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Abstract:

Governments often participate in the provision of public goods in the form of sponsored research and development projects. Individual support for such public investments depends upon many factors. We explore an example involving individuals' stated willingness to give up a tax credit in order to devote that amount of money, instead, to support a research program designed to improve technology directed at climate change adaptation: Improving the energy efficiency of air conditioners. These choices are used to estimate a utility-theoretic specification that distinguishes between the expected average air-conditioning costs and any perceived non-pecuniary benefits from the research program. Variability across stated choice contexts in the number of years before the likely cost saving would begin, and in the length of time they would remain in effect, are used to identify heterogeneous, individual-specific (social) discount rates. Variability in the odds of program success and in the chance that the private sector would independently develop the same technology is used to identify heterogeneous, individual-specific relative rates of (financial) risk aversion in this context. Propensities to attend to randomized elicitation format parameters are also introduced and estimated.

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1. Introduction

In recent years, the U.S. government has shifted the focus of its climate policy from climate change mitigation to climate change adaptation. This shifting emphasis may tend to increase interest in government sponsorship of research and development projects related to climate change adaptation. However, there appears to be little available research concerning public preferences for these types of R&D projects. Prospective benefit-cost analysis for proposed public expenditures on such research endeavors requires some sense of the level of support for them—in particular, the public's willingness to incur the costs of these programs.

This study constitutes one example of an assessment of the perceived public benefits from a government-sponsored R&D program—specifically, a program to improve the future energy-efficiency of air conditioners. If climate change is anticipated to produce higher average summer temperatures in parts of the U.S., along with a greater frequency of extreme weather events (i.e. heat waves), one form of adaptation involves making it cheaper for people simply to use more air conditioning to preserve their comfort levels in the face of these changes in climate.

What are likely to be the most important features of an economic model that can produce estimates of society's overall willingness to pay for such a research program? The most defensible models tend to be firmly grounded in neoclassical consumer theory, in particular, upon random utility models of consumer choice. Given that these R&D projects have costs that begin now, but benefits that do not fully materialize until well into the future, discounting behaviors must be a prominent feature of any such choice model. Finally, there are uncertainties involved. In the case we will consider, the most immediate uncertainty concerns the odds of success of the government research program, as well as the chance that the private sector might come up with the same new technologies, with or without the government-sponsored program.

Thus, it will be important to account for varying degrees of risk aversion and to consider an expected utility framework for individuals' choices.[1]

Besides the fact that this appears to be one of the first attempts to provide a framework for assessing the expected discounted social benefits associated with a government-sponsored research program, a number of innovations are offered in this research,. Methodologically, we introduce a nonlinear adaptation of a conventional ordered logit choice model that specifically accommodates heterogeneity in time preferences and heterogeneity in risk aversion across individuals. We also parameterize the amount of attention our study participants appear to devote to statements about the likelihood of success for the public R&D program and the odds that the program will prove to be unnecessary because the private sector will develop the new technology on its own. We show that a specification that allows individuals to attend less than fully to information that is provided about these risks actually seems to do a better job of explaining the choices they make. Tangentially, we also explore the sensitivity of individual's stated choices to systematic variations in the designs of the choice sets used to elicit these preferences, thereby offering a sense of the robustness of our findings to the types of arbitrary research design decisions made by investigators.

Section 2 places this research in the context of several different veins of related research. Then, section 3 reviews our available data (a convenience sample of stated choices elicited from over 2000 respondents), summarizes the choice experiments we employ, and provides descriptive statistics. Section 4 details the structural model behind our empirical specification. We start with a simple linear-in variables form for indirect utility and build up to more-general models that allow for risk aversion (with respect to net income) and for subjective updating of

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[1] An additional important source of uncertainty concerns how severe any changes in climate may turn out to be. This point will be addressed below.

the survey's asserted probabilities (of success for the government research program, and of mere duplication of private sector efforts). Section 5 discusses our estimates, first for some basic models that assume homogeneous preferences, and then for some very complex models that reveal many significant dimensions of heterogeneity in preferences (even within our limited sample of mostly college students). Section 6 offers some caveats and outlines directions for future research, and section 7 concludes.

2. Related literatures

The public choice problem that motivates this study (adaptation to climate change) and the specific application that we consider (publicly funded research to increase air conditioner efficiency), have connections to other literatures. Likewise, there is other existing work that addresses the potential for social benefits associated with government sponsored R&D more generally. Finally, there has been some attention paid to the difference between the perceived private (selfish) versus social (altruistic) benefits from public expenditures. In this section, we briefly review these related literatures.

Climate change and electricity demand for air conditioning. A small literature has begun to appear wherein researchers have assessed the potential impacts of climate change on energy demands. In the economics literature, Mansur, et al. (2005), for example, determine that in the US, climate change will tend to increase average winter temperatures, thereby decreasing demands for heating fuels, but will simultaneously raise average summer temperatures, and thus increase demands for electricity to run air conditioners. The possibility of increased air conditioner energy efficiency is the public good that would be made available through the government-sponsored research program considered by our respondents. If these efficiency increases could be achieved, they would lessen the impacts of climate change on the demand for electricity.

In other research, it has been observed that privatization and deregulation of utilities can be expected to lead to a leaner electricity industry and therefore a need for more careful planning to manage future electricity demands. Hor, et al. (2005) use weather variables, in addition to other regressors, to explain variations in monthly electricity demand in England and Wales, finding that inclusion of degree days, enthalpy latent days, and relative humidity improves demand forecasts during the summer months. Amato, et al. (2005) us degree-day variables to explain historical temperature sensitivity of residential and commercial demands for electricity and heating fuels, and then assess potential future energy demand responses to selected climate change scenarios. Earlier work by Sailor (2001) also explored the range of responses in electricity demand loads in response to climatic variations.

In contrast, Considine (2000) examines the impacts of climate fluctuations on carbon emissions by using monthly models of U.S. energy demand, and finds that lower energy use associated with reduced heating requirements offsets higher fuel consumption to meet increased air-conditioning costs. The net effect appears to have been that warmer climate conditions slightly reduced carbon emissions.

Concerning air conditioning technologies in particular, Sailor and Pavlova (2003) argue that long-term climate change will increase the market saturation of air-conditioners. They conclude that changes in market saturation may be much more important than the role of weather sensitivity in explaining electricity consumption.

Changes in air conditioner saturation to date may also have played an important role in terms of adaptation to extreme weather events. There is a distinct literature on the topic of "excess" summer mortality in U.S. cities, as a consequence of heat waves (e.g. McGeehin and Mirabelli (2001), Yoganathan and Rom (2001), Davis, et al. (2002), Curriero, et al. (2002), Davis, et al. (2003), Davis, et al. (2003)). Most research seems to suggest that the responsiveness of excess summer mortality to extreme weather events was greater in the 1960's and 1970's than it has been recently. It is acknowledged that many such deaths are preventable. The improvement is attributed in part to increased access to air conditioning, as well as improved medical care, education, and infrastructure adaptations. Presumably, greater energy efficiency in air conditioning technologies will mean lower air conditioning costs and will permit greater reliance on air conditioning during extreme summer weather events.

The social value of public investments in R&D. Griliches (1992) provides a review of the empirical evidence for the existence and magnitude of R&D spillovers, concluding that such spillovers are both prevalent and important. Jones and Williams (1998) ask whether there is too much or too little research and development. Working with a growth model, they derive a relationship between the social rate of return to R&D and estimates from empirical analyses. Their conservative estimate suggests that "optimal R&D investment is at least two to four times actual investment." There seem to be substantial positive externalities from research, and the inability of firms to internalize these social benefits results in an undersupply of R&D activities.

Another literature that is relevant to this study concerns the social benefits of *government sponsored* research. For example, Mamuneas (1999) assesses the short-run effects of publicly financed R&D capital on the cost structure of several high-tech manufacturing industries in the U.S., showing that such publicly financed R&D capital reduces the variable production cost in all industries. It also causes output to increase, which suggests that consumers, as well as producers, are made better off. Alternatively, Jaffe (2002) cites "…wide agreement that the high social rate of return to research and innovation justifies government support for research." Some recent empirical work by Bonte (2004), for West German manufacturing industries, considers the evidence concerning spillovers from both privately and publicly financed business R&D. Their results suggest that public funding of R&D in higher-technology industries seems to induce private R&D investments within these industries but that this effect is not necessarily present in all industries.

Finally, the choice scenarios used to elicit preferences in the present study (which presumes that consumers may be willing to give up an individual tax credit so that the money could be used instead to sponsor climate change adaptation research in the form of cheaper air-conditioning technologies) might mirror public willingness to provide, instead, an R&D tax credit for firms. The cost-effectiveness of R&D tax credits has been assessed for the UK by Griffith, et al. (2001). They find that in the long run, the increase in GDP "…far outweighs the costs of the tax credit."

Selfish versus altruistic components of demand. Our application also looks very closely at the distinction between demand for a public good that reflects solely expected individual benefits, versus an altruistic component of demand that is distinct from these private benefits. Goeree, et al. (2002) note that when a public good has a higher value to everyone, this means that the net cost to the individual of contributing towards its provision will be lower, while at the same time the benefits to others from its provision will be higher. These authors decompose the return to a contribution into an "internal" return for oneself and an "external" return to others. They argue that since contributions in their experiment are generally increasing in both the size of the external return and in the size of the group, these external returns are not simply "warm glow." They find heterogeneity in the magnitude of the altruism effect, with more dispersion among male subjects.

A related issue is the separation of "legitimate" demand for a public good versus expressions of demand for the "warm glow" of giving, rather than for the good itself. Recent work in this vein is offered by Nunes and Schokkaert (2003), who devise an empirical approach to net out the warm glow associated with empirical measures of willingness to pay, leaving only "cold" WTP. In a somewhat similar vein, Clark, et al. (2003) consider an array of distinct motivations for expressing willingness to pay for a premium-priced green electricity program (including concerns about ecosystem health, personal health, environmental quality, global warming, and warm-glow satisfaction).

3. Available Data: A Stated-Preference Field Experiment

We have at our disposal a large sample of survey responses that were collected as part of a comprehensive survey of climate change policy preferences. The Global Policy Survey (http://globalpolicysurvey.ucla.edu) includes a variety of stated preference choice experiments designed to measure attitudes towards alternative climate change policies and willingness to incur the potential costs of climate change mitigation and adaptation. It also builds in an unusually wide array of dynamically randomized survey formats that permit assessment of the sensitivity of choices to elicitation strategies. The sample used here consists primarily of college students from 92 different colleges and universities throughout the U.S. and Canada who took the survey over the internet.[2, 3]

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[2] Another sample consisted of a general population mail survey, but that survey allowed for fewer randomizations due to the limitations of the pre-printed paper format. Internet delivery allowed the widest variety of experiments to be conducted because the survey instrument was generated dynamically according to a series of randomizations.

In the particular module of the Global Policy Survey that forms the basis for this paper, subjects were asked if they would be willing to give up a tax credit this year and to direct that money to be spent, instead, on a government-sponsored program to improve air conditioner efficiency. See Figure 1 for one example of this (randomized) module. The benefits from this proposed program are described in terms of the average annual net savings in air conditioning expenses that would be enjoyed for a specified period in the future.[4]

a. Choice framing experiments

We now discuss each of the independent randomizations built into the elicitation format, along with the rationale for selecting the range of values. Table 1 contains summary statistics for these question format differences.

Future average annual air cost savings. Expected average annual air conditioning cost savings quoted to respondents were varied between $5 and $50 (see Table 1 for the full distribution of values). We wanted a program that would produce modest annual private benefits (cost savings) over a long period.

Year future savings will begin. In our mostly-student sample, the vast majority of respondents were less than 25 years of age. The numbers of years until the benefits of the government research program would begin were varied, randomly across respondents, between 5

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[3] We vary the manner in which the question is posed to the respondent because stated choice experiments are often subject to the criticism that the choices elicited from respondents are sensitive to the format and framing of questions. By randomizing the format and framing of choices in our survey, we are able to demonstrate the sensitivity of our results to these different scenario design decisions. In many earlier surveys, just one design would be selected, which precludes this type of sensitivity analysis.

[4] If we were to repeat this survey experiment, we would certainly expand this choice scenario to be explicit about the provision mechanism. Given the data that we have to work with, we can only assume that respondents were making their choices with an understanding that the government-sponsored research program would be put into action if aggregate "check-off" amounts reached some goal and that there would be no program (and "checked-off" amount would be rebated as a tax credit) if the total was not sufficient.

years and 20 years. Since the maximum age of respondents in our sample is only 50 years, this time horizon probably does not exceed anyone's nominal life expectancy.

Duration of savings period. Cost savings due to the government research program could not credibly be described as persisting indefinitely into the future. The asserted duration of the savings period was varied randomly between 10 and 30 years. For respondents aged 25 or younger, the maximum inception time for these benefits could be 20 years, and the maximum duration could be 30 years, meaning that the potential benefits would accrue until the individual is 75 years old. For any respondent aged up to 50 years, the time horizon could reach age 100.

Sizes of tax credits at stake. Each respondent was asked about several different tax credits. The amount of each proposed tax credit was calculated dynamically by the survey software based on the asserted annual cost savings and the time profile of these benefits. The tax credit amounts were based on the present discounted value of the stream of benefits described in the choice scenario, based on conventional exponential discount rates ranging, in most cases, from 1% through 30%.[5] If the implicit discount rate employed by the respondent falls somewhere in this range, they would be expected to prefer the tax credit when the (larger) tax credit amount corresponds to the lowest implicit discount rate and to prefer the government research program when the (smaller) tax credit amount corresponds to the highest implicit discount rate. Tax credit amounts were generally rounded to two significant figures. Table 2 shows descriptive statistics for the dollar ranges of tax credit values associated with each implicit interest rate (with the differences being an artifact of the different sizes and time profiles of the future expected annual cost savings).

Success probability for government research program. Had we posed the choice task to respondents without stating a probability that the government research program would be

[5] The online survey was programmed in Perl and made wide use of the FasTemplate utility.

successful, each respondent would have made their own inferences about this dimension of the choice problem. Most people realize that not all research programs are guaranteed to be successful. By making this factor explicit, we can assess how variations in the likely success of the government research program seem to affect an individual's willingness to sponsor such a research program with public funding. Table 1 shows the range of success probabilities used in the different randomizations of our survey.

Odds of development of same technology by private sector. Likewise, individuals may be concerned that funding a government research program is unnecessary because if the innovation is desirable, the private sector will have a strong incentive to develop the technology on its own, and public funding for the research is unnecessary. To pre-empt unobserved variation across individuals in assumptions about private sector provision, our framing of the choice problem included an explicit statement about the odds of private sector provision. The last panel of Table 1 shows the range of values used across the different randomizations.

b. Sample characteristics

The sample used for this study is a convenience sample of college students. But, unlike the usual case where all of the students are members of one class or taking courses in just one discipline, this online survey reached students at 92 different institutions enrolled in classes taught by 114 different instructors.[6]

The sample used here is clearly not representative of the population at large. Table 3 provides a summary of demographic data available for this sample and corresponding data for the greater U.S. student population (where available). A quick glance at the age distribution

[6] This was a highly complex survey and we are grateful for the huge amount of time, in the aggregate, that these individuals contributed to this research program.

values indicates that our sample is younger and contains more females than the population of U.S. college students. Thus, any conclusions drawn about the magnitudes of parameter estimates cannot necessarily be generalized beyond the scope of the survey sample. However, the use of the relatively young college sample does have benefits. Because most respondents are likely to remain alive and therefore to potentially experience all of the benefits of the project, individual responses should be based primarily on the pecuniary and altruistic benefits accruing to the respondent, and less on any intergenerational bequest motives.

4. Theoretical Framework

For our estimating specifications, we consider a progression of four utility-theoretic choice models, starting with a simple linear model with homogeneous preferences and relaxing this model's restrictions as we progress. All choices posed to respondents involve either money now (a tax credit) or the present value of future expected cost savings (on average, across the population, from improved air conditioning technologies developed by a government-sponsored research program). Along with the utility from the private cost savings, individuals may perceive non-pecuniary benefits from the research program. These may include incidental innovations or non-market benefits associated with the diffusion of the new technology.

Utility is assumed to depend upon the present discounted value of expected net income, where the respondent's basic income is augmented by the amount of the current period tax credit under one alternative. Under the other alternative, income is augmented by the present discounted value of the expected air-conditioning cost savings and possibly also enhanced by the present value of any perceived non-pecuniary benefits. The utility difference between these two states is assumed to drive each individual's choice.

Let the utility for individual i under choice j be U_{iTC} if the tax credit option is chosen, and let it be U_{iRD} if the government-sponsored research program is selected. These utilities can be decomposed into systematic components V_{iTC} and V_{iRD} that are explained by observables, and an associated stochastic components, η_{iTC} and η_{iRD}. If we can assume that these stochastic components are distributed i.i.d. extreme value, then their difference can be assumed to be i.i.d. logistic. For ease of exposition, we develop our theoretical models in terms of the deterministic (observable) utility differences, V_{iTC} and V_{iRD}. Table 4 summarizes the functional forms for these deterministic components for each of our four specifications, to be explained in detail in the subsections to follow. When we consider the utility differences presumed to drive respondent choices, and develop the contributions to the log-likelihood function for each of our different specifications, we will reinstate the random component.

Model 1: Linear; No Uncertainty about Program Success or Redundancy

In our first model, we assume that individuals respond to the tax credit choice question based on (a) their private pecuniary benefits (which depend on the size of the tax credit, the size of the average cost savings, the start time for these benefits, and their duration) and (b) their present discounted non-pecuniary benefits (the utility they derive from the public good dimensions of the program). We assume that no attention is paid to the assertion (in the preamble to the choice exercise) that there is only a chance that the government sponsored research program will be successful (i.e. that the probability of failure is nonzero) and that there also is a chance that the private sector will provide these innovations on its own.

In Model 1, as for our other specifications, β_{0i} is the (estimated) marginal utility of (possibly transformed) net income for individual i, TC_i is the tax credit offered to individual i, and s_i is the advertised stream of annual per-capita private pecuniary benefits if the government-sponsored research program is implemented (i.e. the average air conditioning operating cost savings). The variables t_i and dur_i are the start time and duration for which lower air conditioning operating costs will be realized, respectively, and q_i is an (estimated) individual-specific "social" discount rate.[7] The (estimated) marginal utility β_{1i} reflects the present value of utility from the stream of non-pecuniary or social benefits from the program.

The first model thus involves observable utilities that do not explicitly incorporate uncertainty. Referring to Table 4, we proceed as though the individual compares (a) V_{iTC}^1 , her current-period utility with the income improvement resulting from the tax credit this period, $\beta_{0i}\left(Y_i + TC_i\right)$, to (b) V_{iRD}^1, her current-period utility with the present-value income improvement due to her future expected cooling-cost savings from the R&D program, $\beta_{0i}\left(Y_i + B_i^*\right)$, plus any utility associated with the present value of the non-pecuniary or social benefits associated with the R&D program (β_{1i}). Here, we assume conventional discounting and an individual-specific discount rate, q_i , and we abbreviate the present value of the future cost savings as:

$$B_i^* = s_i \sum_{\tau=t_i}^{t_i+dur_i} \frac{1}{\left(1+q_i\right)^\tau} \qquad (1)$$

[7] The two preference parameters in this model, β_i and q_i , are indexed to the individual because they are incorporated as systematic varying parameters. That is, we write these parameters as $\beta_i \equiv \beta' x_i$ and $q_i \equiv q' z_i$ where x_i and z_i are vectors of explanatory variables, and β and q are vectors of parameters to be estimated. In estimation, we will constrain the social discount rate to be strictly positive by estimating $\log(q_i) = q' Z_i$, so that $q_i = \exp(q' Z_i)$ cannot be negative for any values of Z_i .

This specification, Model 1 in Table 4, is used primarily for testing whether the observed choices are sensitive to the most basic variables that should affect them.[8,9] We initially suppress any heterogeneity and merely attempt to estimate single scalar values for the three key parameters: β_0, β_1, and q.

In addition to these essential utility parameters, there are a number of incidental parameters that are also estimated for each model. These are the thresholds that separate the intervals in the ordered logit models. Some respondents have only two possible answer options to the choice question concerning the tax credit versus the R&D program. In modeling binary outcomes, the single threshold for the latent indirect utility-difference variable underlying a conventional binary logit model is normalized to zero. Zero represents the boundary between "yes" and "no."

In our data, however, some individuals are offered three answer categories ("yes," "not sure," and "no"), four answer categories ("definitely yes," "probably yes," "probably no," and "definitely no"), or five answer categories ("definitely yes," "probably yes," "not sure," "probably no," and "definitely no"). If we normalize the boundary between "yes" and "no" at zero for the four-category case, there are eight non-zero thresholds to be estimated when we pool the answers for all four types of questions. The roles of these incidental threshold parameters are revealed in more detail in Appendix 2, which sets out our full log-likelihood function. Since standard packaged algorithms are not available for models that aggregate ordered data across

[8] Given the discussion by Frederick, et al. (2002) and the model of Laibson (1997) in their investigations of private discount rates, coupled with the social discounting "rules of thumb" advocated by Moore, et al. (2004), we do not consider more flexible discounting formulae because benefits will accrue, at the earliest, in five years.

[9] We apply the term "social discount rate" somewhat loosely: typically, discount rates are used to discount streams of utility rather than streams of income, as we do here. Moreover, because we explicitly incorporate non-pecuniary benefits of program adoption through the parameter β_1, thus our discount rate is probably closer to a private rate of discount than a social rate of discount.

subsamples with differing numbers of answer categories, this likelihood function must be programmed in Matlab (or similar generic function-optimizing software).

For all of our models, the estimated values of these threshold parameters bear the expected signs. Since little else about these estimated threshold parameters is very interesting (provided they are present in each model), we do not report these estimates in the body of this paper.

Model 2: Chances of Success, Private Sector Provision

This second model is somewhat more general. Respondents are informed during the preamble to the choice exercise that the future benefits from the government-sponsored research program are not guaranteed (see the text above the choices in Figure 1). Instead, there is less than a 100% chance that the research program will be successful. There is also a chance that the private sector will develop the same technologies on its own, so that they will be available even without the government-sponsored research program. In light of this information, respondents are making choices over an uncertain stream of future benefits. *Expected* present discounted utility thus seems to be an appropriate modeling framework to examine.

The stated uncertainties about the success of the government-supported research program and the chance of private sector provision are varied randomly across respondents. With this uncertainty, the observable utilities should be written as expected values. Let ξ_i represent the probability that the government research program will be successful and π_i the probability that the technology will be developed independently by the private sector.[10]

[10] It is possible that the effort dedicated to similar research in the private sector may depend on whether a government-sponsored research program is under way. We abstract from any such dependence in this analysis. It proved too difficult to attempt to explain more-complex probabilities to survey respondents.

If the respondent chooses just to take the tax credit, the relevant probability is simply the probability that the private sector will succeed on its own in developing the technology, π_i. If the respondent chooses to fund the government-supported R&D program, the probability that either the government program or the private sector will be successful in developing the technology is the probability of the union of these two (assumed) independent events: $\Xi_i = \xi_i + \pi_i - \xi_i \pi_i$.

Model 2 in Table 4 provides the systematic portions of utility under each alternative for this case. Note that this model assumes that the non-pecuniary benefits of the new technology are assumed to be the same regardless of whether the technology is developed by the government or by the private sector.

Model 3: Subjective Updating of Uncertainty and Cost Saving Information

Next, it makes sense to question whether respondents just passively accept the asserted probabilities of success and the stream of cost savings (income) of the government-sponsored research program (or attainment of the same technologies by the private sector). It seems most natural to entertain a spectrum of possibilities. First note that the asserted probability of success of the government-sponsored research program implies an associated probability of failure: $(1-\xi_i)$. Respondents may pay no attention to (or reject) this stated probability of failure. Or, they may completely accept the scenario outlined in the elicitation format. However, it is most likely that respondents lie somewhere between these two extremes.[11] In this model, we permit respondents to update the advertised failure probability by invoking a parameter, θ_{1i}, that scales this stated probability into an effective (subjective) probability of program failure,

[11] There is also the chance that respondents may over-react by assuming that the probability of failure is greater than advertised.

$\theta_{1i}(1-\xi_i)$. This implies that the corresponding effective (subjectively updated) probability of success is $1-\theta_{1i}(1-\xi_i)$. A second parameter, θ_{2i}, can be used to scale the probability of private sector provision, π_i, stated on the survey instrument, into an effective (subjective) probability of this outcome: $\theta_{2i}\pi_i$.

Our switch from "the probability of government success" to "the probability of government failure" makes the algebra somewhat more complex, but it is useful because it makes the special cases somewhat more intuitive. If $\theta_{1i} = \theta_{2i} = 0$, the respondent assumes zero probability of government failure and zero chance that the private sector will develop the same technology on its own. Put another way, the respondent completely rejects the possibility of government failure. In this instance, the respondent has scaled (updated) the effective probability of government *success* to unity. At the other extreme, if $\theta_{1i} = \theta_{2i} = 1$, the respondent completely accepts both the stated probability of failure of the government program and the chance of private sector provision. If the values of these parameters lie between zero and one, the respondent recognizes the stated chances of government program failure or private provision, but does not completely accept these probabilities.[12]

To simplify notation, we will let Π_i^{TC} denote the subjectively weighted probability of enjoying the equivalent benefits if the tax credit option is taken (so that the benefits are enjoyed only if the private sector achieves the innovations on its own). Let Π_i^{RD} denote the subjectively weighted probability of enjoying the benefits of the R&D program if the respondent chooses to forgo the tax credit and fund the government to pursue the program.)

[12] This product is interpreted as a subjective probability, so it must therefore take on a value that is valid as a probability (i.e. it must lie between zero and one). However, we will merely constrain θ_{1i} to be positive during estimation (by estimating its logarithm). This scaling parameter itself can be greater than one as long as the probability stated on the survey instrument, ξ_i, is less than unity.

There is also a likelihood that respondent may not pay complete attention to the cost savings outlined in the choice preamble. To accommodate the possibility of this inattention we invoke parameter θ_3 on the stream of potential cost savings. Thus, the pecuniary benefits that are assumed to drive choices are now $\theta_3 B_i^*$. Model 3 in Table 4 gives the relevant forms for the systematic portions of utility in the case that respondents pay incomplete attention to the stated probabilities and cost savings in the preamble of the choice scenario.

Model 4: Risk Aversion With Respect to Income

Given that uncertainty has been introduced, we should accommodate the possibility of risk aversion. We note, however, that the amounts of money at stake are relatively small compared to each individual's income, so any risk aversion may be difficult to discern. Nevertheless, suppose we allow for an arbitrary degree of relative risk aversion over pecuniary benefits. This can be accomplished with the Box-Cox transformation, used widely in empirical contexts as a generalization of both linear and logarithmic transformations. We will model observable utility as being linear in a Box-Cox transformation of present discounted net income: $Y^{(\lambda)} = \left(Y^{\lambda} - 1\right)/\lambda$.

The Box-Cox parameter, λ, can be interpreted as one minus the coefficient of relative risk aversion for a utility function that is characterized by constant relative risk aversion (CRRA).[13] Relative risk aversion (RRA) for a utility function $V(Y)$ is defined as $-Y * V''(Y)/V'(Y)$. For a utility function that is linear in a Box-Cox transformation of net income, we have:

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[13] λ can be modeled as a scalar parameter, or it can be modeled as a varying systematically across individuals with a vector of observable characteristics W_i: $\lambda_i \equiv \lambda' W_i$.

$$V(Y) = \beta\left[\frac{Y^{\lambda} - 1}{\lambda}\right] = \beta Y^{(\lambda)} \tag{2}$$

Thus $V'(Y) = \beta\left(Y^{\lambda-1}\right)$ and $V''(Y) = \beta\left(\lambda - 1\right)Y^{\lambda-2}$, so that $RRA = -Y * V''(Y)/V'(Y) = 1 - \lambda$. Compare this to the simple utility function typically used in macroeconomic models where constant relative risk aversion is desired. If we let $\lambda = 1 - \phi$, then the typical form, $Y^{1-\phi}/(1-\phi)$, can be written as Y^{λ}/λ. The Box-Cox transformation, more familiar in applied econometrics, involves a change in location ($-1/\lambda$) and scale (the β parameter), relative to this simpler form. Yet it still exhibits constant relative risk aversion. Econometricians are familiar with the insight that a Box-Cox transformation parameter of $\lambda = 1$ is consistent with a linear model (i.e. risk neutrality corresponds to $\phi = 0$). Note that the smaller the value of λ, the greater the curvature in the utility function (i.e. more risk averse), and as $\lambda \to 0$, the transformation approaches a logarithmic transformation. This accounts for the advantage of using this form, since the conventional variant, Y^{λ}/λ, becomes undefined as the parameter ϕ approaches 1.

Model 4 in Table 4 summarizes the functional forms for the systematic portion of utility under each alternative when utility is nonlinear in net income.

Reinstate Random Component and Develop Utility-Differences

Starting with the binary choice format, we model a stated preference for the tax credit as the “1” outcome and a stated preference for devoting the money instead to the government-sponsored R&D program as the “0” outcome.[14] Individuals are assumed to prefer the tax credit

[14] Note that these designations are completely arbitrary. The survey question asked respondents if they would prefer the tax credit, so we take a “no” as a “0” outcome and a “yes” answer as the “1” outcome. We could just as easily have interpreted the question as asking whether the individual would prefer the government research program.

if the net indirect utility from that alternative, versus funding the program, is positive. If the random indirect utility in each case is:

$$\begin{aligned} U_{iTC} &= V_{iTC} + \eta_{iTC} \\ U_{iRD} &= V_{iRD} + \eta_{iRD} \end{aligned} \tag{3}$$

Then the individual utility-difference will be given by:

$$\begin{aligned} \Delta U_i &= (V_{iTC} - V_{iRD}) + \varepsilon_i \\ &= \Delta V_i + \varepsilon_i \end{aligned} \tag{4}$$

where $\varepsilon_i = \eta_{iTC} - \eta_{iRD}$.

For the three-, four-, and five-level response options offered in other variants of the survey, we assume that the same latent continuous individual utility-differences drive the respondent's selection of one of the available ordered response categories. We will maintain the assumption that these latent utility differences are distributed standard logistic, so that the multi-level response options imply ordered logit frameworks.

Finally, it is reasonable in some contexts to assume that the error terms in such a model are all drawn from the identical (standard) logistic distribution. In this case, however, the dispersion of the error term (the noise in the choices) may vary systematically with the design of the choice scenario or with the characteristics of the respondent. We will assume a base error dispersion normalized to 1 (as in the homoscedastic case). However, we will allow departures from this dispersion level for distinct differences in the type of observation. For the baseline category (e.g. the two-level response option) we will assume that the error dispersion is $\exp(0) = 1$. For "other" categories, we will estimate the relative error dispersion as $\exp(\kappa_i)$. If the estimated value of κ_i is positive, that category will have a larger error dispersion than the base category. If κ_i is negative, that category will have a smaller error dispersion.

Estimation

In the previous section we introduced, and then generalized, our specifications for indirect utility that will be used to explain the stated choices from our survey. The development of the log-likelihood specification to be used to estimate the unknown preference parameters (either as scalars, or as systematically varying parameters) is given in detail in Appendix 2.

5. Results and Interpretation

Homogeneous Preferences

Table 5 shows the progression of generalizations across our different models, but maintains the assumption that preferences are completely homogeneous across respondents. Model 1 demonstrates a simple ad hoc approach where the observable utility difference that drives choices is determined simply by the advertised net pecuniary benefits of the program, (ignoring any uncertainty about either kind of “failure”) and a dummy variable that captures the present value of any non-pecuniary public benefits (or “warm glow”) that may be active if the technological improvement is made available. This model demonstrates that each of the main variables that we expect to influence choices does indeed have the anticipated (and a highly statistically significant) effect.[15]

Model 2 shows what happens to the key parameter estimates when respondents are assumed to pay attention to the survey’s statements about the two types of uncertainties about “failure,” and to fully accept these statements. However, they are assumed to be risk-neutral with respect to these uncertainties, so that the utility function remains linear in discounted net

[15] In this and subsequent tables, we omit ancillary parameter estimates including the ordered logit thresholds. All of the threshold values are significant at the 95% level and have the expected signs.

income. An inspection of the maximized log-likelihood value for Model 2, compared to Model 1, however, indicates that the ad hoc Model 1 seems to do a better job of explaining the choices that respondents make. One suspects that this belies some too-restrictive assumptions in Model 2.

We innovate in Model 3 by introducing our “propensity to attend” parameters. As outlined above, these “propensities to attend”(PTAs), if both equal to zero, indicate that the respondent pays *no* attention to the probability that the government program will fail or the probability that the private sector will provide the technology so that the government program is unnecessary (as in Model 1). A value of unity for both PTA parameters suggests complete acceptance of the stated probability (as in Model 2). Not surprisingly, the estimated unconstrained PTAs in Model 3 lie between these two extremes (where we do not constrain these parameters to lie within the unit interval during estimation.)

In Model 4, we relax the assumption that utility is linear in income and thereby allow the specification to accommodate any risk aversion that may be present. This is accomplished by allowing indirect utility to be linear in a Box-Cox transformation of discounted net income. Risk neutrality is inferred if the value of the relative-risk-aversion parameter, λ, is no different from unity. In the homogeneous-preferences specification, we are not able to reject risk neutrality (on average in the sample). Below, however, we find that this outcome may mask heterogeneity in risk preferences across our sample.

In most variations on Models 3 and 4 that we have explored, the propensity to attend (PTA) to the odds of [just duplicating private sector efforts] is larger than the propensity to attend (PTA) to [the odds of failure of the government research program]. That is, respondents pay more attention to the probability that the private sector will render the government sponsored research project unnecessary than to the (stated) likelihood that the government project will fail outright.

One of the intriguing dimensions of our models is that people are making choices over a government program that will have private benefits in the form of expected future air conditioning cost savings. However, the research program may also have some public goods dimensions (e.g. individuals may be paternalistically altruistic or they may anticipate positive spillover effects of the technologies that are developed). We can use any of Models 1, 2, 3, and 4 to produce an estimate of the apparent willingness to pay for the anticipated present discounted non-pecuniary benefits of the program. This willingness to pay may be calculated by taking the ratio of the marginal utility of the program to the marginal utility of income: β_1 / β_0. Since all dollar-denominated values in our empirical section are measured in $1000 units, the parameter estimates imply a willingness to pay for the present-discounted non-private benefits on the order of about $120.00. The marginal utility of income in our models with $\lambda \neq 1$ are technically non-constant, but the extent of the nonlinearity is sufficiently small that the greater generality does not make much of a difference to this estimate.

Heterogeneous Preferences

The next sets of estimation results demonstrate why a formal structural utility-theoretic approach can be beneficial to cost-benefit studies: The interpretation of indirect utility parameters is straightforward, and utility-theoretic models (thanks to Roy's Identity) lend themselves well to the task of inferring willingness to pay for the public good represented by the non-pecuniary benefits of the government-sponsored R&D program in this study.

When introducing heterogeneity in preferences, it is appropriate to begin with a utility-theoretic specification and then to accommodate heterogeneity by converting the scalar parameters of the homogeneous-preferences model into systematically varying parameters that depend upon exogenous individual characteristics that may proxy for differences in preferences. In practice, this amounts to introducing a lot of interaction terms. This strategy is preferable to the more ad hoc approach of simply interacting individual characteristics with alternative-specific dummy variables to produce an ad hoc list of linear and additively separable terms to append to the list of explanatory variables in the choice model.

The potential for heterogeneity in preferences across the population is an important consideration in cost-benefit studies: We do not expect young and old individuals to value a public project in the same manner when it offers long-term benefits but incurs near-term costs for society. To accommodate basic heterogeneity according to age groups and gender, we generalize Model 4 by beginning to allows several of the key parameters in our models (i.e. $\log(q_i)$, where q_i is the discount rate, λ_i, which captures risk aversion, and κ_i, the error dispersion parameter) to vary systematically with observable individual differences. The estimation results in Table 6 detail selected coefficients from three variants of Model 4 which incorporate exogenous individual variation through age and gender differences.

In considering Table 6, note that we use a somewhat non-standard format for our presentation of the three increasingly general specifications: Models 4a, 4b, and 4c. Vertical dotted lines separate the three models. Each specification has scalar estimates for β_0, the marginal utility of the transformed discounted net income variable that captures the anticipated private pecuniary benefits from the R&D program, and for β_1, the utility derived from the program that stems from its public goods aspects—i.e. its benefits to others, any positive externalities associated with the government R&D program, and any other non-pecuniary benefits (such as "warm glow") accruing to the individual. We also constrain the two propensity-to-attend parameters, θ_1 and θ_2, to be identical across respondents. For each model, there is a single column of results for these four scalar parameters. However, each column of estimates then separates into three columns, one for each of the three parameters that we allow to vary systematically: $\log(q_i)$, λ_i, and κ_i. Since we allow the same individual characteristics affect each of these varying parameters, we line up these sub-vectors of coefficients side by side (to permit easy comparisons across models and across the three varying parameters).

What types of heterogeneity are identified? For our university sample, the parameter estimates for our heterogeneous discount rate indicate that this parameter tends to be smaller for older individuals (within this college sample), and that female respondents have lower discount rates than males (the relatively high discount rates of college-aged males is perhaps not surprising).

The relative risk aversion parameter is significantly different for only one age group: persons aged 31 to 50 years have significantly higher rates of relative risk aversion.[16] Thus, in our college sample, respondents under the age of 31 demonstrate relative risk aversion (using a 90% confidence level) that was not observed when a homogeneous risk parameter was estimated. This illustrates that the potential importance of allowing for different preferences across a sample.

Finally, when the error dispersion parameter is allowed to vary systematically, we observe that, compared to males, females seem to make more consistent choices (i.e. display smaller average error dispersion parameters). Younger respondents also seem to make more consistent choices than older ones.

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[16]The statistically significantly larger value of the $\log(\kappa_i)$ parameter for the 31-50 age group implies that this group also demonstrates less consistency in their choice decisions.

Heterogeneous Preferences with Endogenous Regressors, Design Effects

In Table 7, we introduce yet more sources of heterogeneity into the three key parameters of interest in our analysis. Whereas the models in Table 6 used only those variables that are arguably exogenous (age and gender) or randomly assigned (elicitation format), the generalizations of Model 4 examined in Table 7 introduce some (potentially endogenous) behavioral correlates into the systematically varying parameters. These include indicator variables for whether the respondent belongs to any environmental groups, whether they are a business major or a social science major (as opposed to other majors), whether they have ever taken an economics course, whether they self-identify as being “liberal” or “conservative” (relative to a “neutral” option), and whether they are currently employed either full- or part-time.

All three specifications in Table 7 also control for the characteristics of the randomized design of the elicitation format. These controls include dummy variables for (a) the arrangement of answer options with the “Yes” option on the left, (b) decreasing (as opposed to increasing) tax credit amounts, going down the page, (c) relative to the base case with just three offered tax credits, dummy variables for instances of five, seven, or thirteen offered tax credit amounts, all spanning the same range of implied discount rates, and (d) dummy variables for three-, four-, or five-category response options (as opposed to just a two-category (“yes” or “no”) option.

Table 7 reports results for three further-generalized variants of Model 4. The tendency for discount rates to decline with age in this college sample is apparent when we consider only exogenous variation in respondent characteristics. However, when the behavioral correlates are introduced, this tendency vanishes—age was apparently serving as a good proxy for several endogenous behavioral variables. Of the behavioral correlates, we find that (self-selected) business majors have higher discount rates on average, while self reported liberals, students who have taken an economics course, and those respondents who were employed at the time of the survey tend to have lower discount rates.

For our heterogeneous relative risk aversion parameter, the sub-population of 31- to 50-year-olds again demonstrates significantly higher rates of relative risk aversion. Also of note is that business majors tend to have lower rates of relative risk aversion, as do social science majors, compared to all other types of students taken as a group.

With respect to the error dispersion, and as noted previously, increases in age are correlated with decreases in choice consistency and females make less noisy choices. It is also quite clear from the three models that increasing the number of response options tends to lower choice consistency.

All three specifications in Table 7 include the full set of elicitation design variables. We will not concentrate on these controls in this paper, except to note the perhaps-unsettling number of statistically significant shifters on our discounting, risk aversion, and choice-consistency parameters associated with these variables. Putting the “yes” option on the left seems to increase the degree of risk aversion implied by individual’s responses, and arranging the tax credit amounts in descending order may also increase apparent risk aversion. Asking the respondent to consider more different tax credits (increasing task complexity and time requirements) may lower the implied discount rate, decrease apparent risk aversion, and increase the amount of noise in respondent’s choices. The presence of more than two response options seems to decrease apparent discount rates and risk aversion, but to increase the noise in choices. This may be interpreted as the consequences of allowing the respondent to equivocate in their choices.

6. Caveats and Directions for Future Research

The current models introduce heterogeneity very selectively. We have elected to maintain the assumption that the marginal utility of Box-Cox-transformed net income is constant across respondents, and that the non-pecuniary utility conferred by the R&D program, if it is implemented, has the same average value for all respondents. In other words, we assume for now that β_0 and β_1 are constants. It is often challenging to separately identify heterogeneity with respect to the same sets of individual characteristics simultaneously for a parameter like β_0 and another one like $\log(q)$, but it will certainly be important to attempt this, since the parameters are not directly in ratio form. With respect to the β_1 parameter on the non-pecuniary (social) benefits, one might contemplate an assumption that these benefits will span the same time interval as the future private pecuniary benefits, but this is probably not the case. The spillovers from the government research program may both pre-date and post-date the private net savings over existing technologies for air-conditioning specifically. We fall back on the assumption that this "lump" of utility subsumes the central tendency of all the subjective heterogeneity in the sample regarding possibly non-pecuniary benefits of the R&D program.

We have introduced the possibility that individuals may or may not pay full attention to the information in the preamble to the choice tasks (about the odds of success for the government R&D program and the odds that it may be unnecessary due to private sector activity). However, we are still assuming that the other important piece of information in the preamble—concerning the average annual cost savings, s_i —is taken at face value. We assume that the individual merely adopts this specific value as their own annual cost savings.

It is possible, however, that respondents selectively alter the annual cost savings to better reflect what these savings might be from their own perspective. In particular, we could introduce a non-negative scaling factor, say $\exp(\gamma_i)$, that individual respondents apply to the stated average annual cost savings s_i. Their assessment of the likely discounted net pecuniary benefits from the program would then be some revised amount, $\exp(\gamma_i)s_i$, which (depending upon the estimated value of γ_i), could be greater or less than the nominal amount. This scaling parameter γ_i would default to zero, but we have sufficient information in our data to allow it to vary systematically with the historical number of cooling-degree-days in the state/province where the individual tells us they plan to make their permanent home.

The scaling parameter could also vary systematically with the individual's subjective rating of just how bad things are likely to get under a business-as-usual climate policy. Respondents were asked to consider five categories of possible climate change impacts. One of these was "Oceans, Weather-sea levels, frequency and severity of storms". They were asked to rate this possible impact on a scale of -4 to +4, with -4 being "extremely undesirable change" and +4 being "extremely desirable change" with 0 being "no change." Another category was "Human health - and well-being, including diseases," which was rated between "extremely harmed" and "extremely improved." These different ratings across individuals provide an opportunity to control for some heterogeneity with respect to subjective severity of climate change consequences.

7. Conclusions

In this study, we have applied utility theoretic models to choices made in an online survey. This survey examined support for a proposed government research and development program that facilitates climate change adaptation by lowering future air conditioner operating costs. Our computerized survey employs several elicitation format randomizations which permit us to control for frequently cited biases in stated preference survey designs. Additionally, we accommodate many types of heterogeneity in some of the model parameters of interest. Since we are examining public support for a project that will have a near-term private cost and both private and public long-term benefits, in addition to differing degrees and types of uncertainty, we model preferences over pecuniary outcomes using discounting in an expected utility framework.

We also attempt to measure respondents' perceived non-pecuniary (public and/or other-regarding) benefits of the program. Our data suggest that the typical present value of the current and future non-pecuniary benefits of the research program may be on the order of $120.

We also innovate by demonstrating the potential importance of explicitly accommodating participant inattention to selected survey-provided information. The use of a choice theoretic framework makes incorporating the parameters necessary to model this inattention relatively simple. We label these inattention parameters as "propensities to attend." Typically, researchers assume a priori that respondents pay complete attention to all survey information. This may be too ambitious. Our estimates indicate that it is statistically unlikely that this assumption is always valid. Here, respondents appear to weight by about 0.25 the implied odds of failure of the government research program, and to weight by about 0.5-0.6 the odds that the private sector will develop the same technology, making the government program redundant.

Utility-theoretic choice modeling frameworks that accommodate individual heterogeneity in discount rates, risk aversion, and error variances may prove to be valuable for other studies as well. For this non-representative sample, we find mean discount rates on the order of about 0.06

and minimal risk aversion (for income amounts this small). For example, the researcher can explicitly identify heterogeneous risk and time preferences which permits a clearer interpretation of parameter estimates in the context of rational decision making theory. This will permit parameter estimates from stated preference studies to be compared with those estimated from revealed preference data in analogous situations. This allows researchers (in some cases) to assess the external validity of these types of studies. Additionally with the use of choice theoretic models, the incorporation of model parameters that capture propensities-to-attend is straightforward.

Concerning public demand for government programs that may help people adapt to climate change, we have demonstrated that people appear to look beyond just their individual private benefits to anticipate some social benefits as well. The approximate mean value of willingness to pay for these additional social benefits appears to be somewhat over $100. If respondents fully accepted the statement about their likely annual cost savings from the program, this willingness to pay is in addition to those likely savings.

Table 1

Distributions of Choice Scenario Design Variables

	Freq.	Percent
Future average annual savings (U.S. $)		
5	215	10.73
10	219	10.93
15	208	10.38
20	206	10.28
25	253	12.63
30	227	11.33
35	229	11.43
40	214	10.68
50	232	11.58
Total	2,003	100
Years until the future savings will begin		
5	504	25.16
10	515	25.71
15	511	25.51
20	473	23.61
Total	2,003	100
Duration of savings period, in years		
10	502	25.06
15	514	25.66
20	507	25.31
30	480	23.96
Total	2,003	100
Percent chance that the gov't R&D program will be successful		
50	303	15.13
60	313	15.63
70	351	17.52
80	358	17.87
90	353	17.62
95	325	16.23
Total	2,003	100
Percent chance that private companies would provide this technology		
10	388	19.37
20	393	19.62
30	417	20.82
40	406	20.27
50	399	19.92
Total	2,003	100

Table 2
Tax credit amounts quoted to respondents
under each implied discount rate

Implied discount rate (%)	Instances	Mean	Std. Dev.	Min	Max
1	2003	\$382	\$257	\$38	\$1227
2	2003	306	203	30	1014
3	2003	248	164	23	845
4	2003	203	135	18	710
5	2003	167	114	14	602
6	2003	139	97	11	514
7 [a]	1671	118	84	9	442
8	2003	99	74	7	383
9 [a]	1671	84	65	5	333
10	2003	72	58	4	292
11 [a]	1671	62	52	3	257
12	2003	53	47	2	228
15	2003	36	35	1	163
20 [a]	332	19	23	0	100
25 [a]	332	11	15	0	65
30 [a]	332	7	11	0	44

[a] After the survey had been in progress, it became clear that there was minimal information in the credits associated with very high implicit discount rates, so these “credits” were replaced with amounts corresponding to 7%, 9% and 11% discount rates. Not all of these credits were displayed for respondents receiving surveys with only 3, 5 or 7 credit amounts.

Table 3

Descriptive statistics for respondent characteristics, survey behavior (n = 2003)

Variable	Mean	Std. Dev.	Min	Max	2001 U.S. Data
Individual characteristics					
Age 20 = aged 20 or less (baseline category)	0.4119				0.3355*
Age 21-25 = aged 21 to 25	0.4588				0.3057*
Age 26-30 = aged 26 to 30	0.0754				0.1216*
Age 31-50 = aged 31 to 50	0.0539				0.2362*
Female	0.5052				0.5670*
Family income (based on bracket)	67,260	38,890	8,000	125,000	67.2**
>=1 environmental groups	0.2361				
Major = business	0.3500				
Major = social science	0.3010				
>=1 economics class	0.8807				
Liberal (self-identified)	0.4334				0.2690**
Conservative (self-identified)	0.2526				0.1910**
Work (full- or part-time)	0.4353				
Annual family income now (U.S. $)		Freq.	Percent		
(complete distribution)	8,000	99	4.94		
	15,000	152	7.59		
	25,000	192	9.59		
	40,000	352	17.57		
	62,500	394	19.67		
	87,5000	363	18.12		
	125,000	451	22.52		
	Total	2,003	100		

* Source: U.S. Statistical Abstract, 2003. Age distribution estimated based on readily available age groups
** Source: U.S. Statistical Abstract, 2003. Data based on a survey of freshman in 2001. The income figure is a median value.

Table 4

Model specifications: systematic portions of indirect utility under each alternative

Model	Description	Alt.	Systematic Utility
1	Linear in net income, certainty	TC	$V_{iTC}^{1}=\beta_{0i}\left(Y_i+TC_i\right)$
		RD	$V_{iRD}^{1}=\beta_{0i}\left(Y_i+B_i^{*}\right)+\beta_{1i}$
2	Linear, chance of private sector provision, chance of govt program failure	TC	$V_{iTC}^{2}=\pi_i\left[\beta_{0i}\left(Y_i+TC_{ij}+B_i^{*}\right)+\beta_{1i}\right]+\left(1-\pi_i\right)\left[\beta_{0i}\left(Y_i+TC_{ij}\right)\right]$
		RD	$V_{iRD}^{2}=\Xi_i\left[\beta_{0i}\left(Y_i+B_i^{*}\right)+\beta_{1i}\right]+\left[1-\Xi_i\right]\left[\beta_{0i}\left(Y_i\right)\right]$
3	Uncertain benefits (subjectively weighted)	TC	$V_{iTC}^{3}=\Pi_i^{TC}\beta_{0i}\left[\left(Y_i+\theta_{3i}B_i^{*}\right)+\beta_{1i}\right]+\left(1-\Pi_i^{TC}\right)\beta_{0i}\left[\left(Y_i+TC_{ij}\right)\right]$
		RD	$V_{iRD}^{3}=\Pi_i^{RD}\left\{\left[\beta_{0i}\left(Y_i+\theta_{3i}B_i^{*}\right)\right]+\beta_{1i}\right\}+\left[1-\Pi_i^{RD}\right]\left[\beta_{0i}\left(Y_i\right)\right]$
4	Uncertain benefits, plus risk aversion	TC	$V_{iTC}^{4}=\Pi_i^{TC}\beta_{0i}\left[\left(Y_i+TC_i+\theta_{3i}B_i^{*}\right)^{(\lambda_i)}+\beta_{1i}\right]+\left(1-\Pi_i^{TC}\right)\beta_{0i}\left[\left(Y_i+TC_i\right)^{(\lambda_i)}\right]$
		RD	$V_{iRD}^{4}=\Pi_i^{RD}\beta_{0i}\left[\left(Y_i+\theta_{3i}B_i^{*}\right)^{(\lambda_i)}+\beta_{1i}\right]+\left(1-\Pi_i^{RD}\right)\beta_{0i}\left[\left(Y_i\right)^{(\lambda_i)}\right]$

Where TC=take tax credit, no R&D program; RD=forgo tax credit, implement R&D program

$$B_i^{*}=s_i\sum_{\tau=t_i}^{t_i+dur_i}\frac{1}{\left(1+q_i\right)^{\tau}};\quad \Xi_i\equiv\xi_i+\pi_i-\xi_i\pi_i;\quad Y^{(\lambda)}=\left(Y^{\lambda}-1\right)\big/\lambda;\quad \Pi_i^{TC}\equiv\theta_{2i}\pi_i;\quad \Pi_i^{RD}\equiv\left[1-\theta_{1i}(1-\xi_i)\right]+\theta_{2i}\pi_i-\left[1-\theta_{1i}(1-\xi_i)\right]\theta_{2i}\pi_i$$

Table 5
Homogenous Preferences, Homoscedastic, Introducing Parameters

	Model 1	**Model 2**	**Model 3**	**Model 4**
Features	Ad-hoc, No failure	Utility-theoretic, Chance of failure	Utility-theoretic, Propensities to Attend	Utility-theoretic, Propensities to attend, Non-risk-neutral
MU(Income) (β_0)	6.04 (265.)**	5.977 (265.)**	6.056 (270.)**	6.06 (269.)**
MU(Social Benefits) (β_1)	0.7192 (18.0)**	0.8691 (24.4)**	0.7137 (18.6)**	0.7098 (18.5)**
PTA: Outright Failure (θ_{1i})	0	1	0.3496 (4.21)**	0.3401 (4.11)**
PTA: Private Provision (θ_{2i})	0	1	0.5031 (6.72)**	0.4937 (6.55)**
Log(discount rate) = $\log(q_i)$	-2.579 (-81)**	-2.987 (-69.8)**	-2.828 (-70.3)**	-2.830 (-70.5)**
λ_i (where $RRA = 1 - \lambda_i$)	1	1	1	1.034 (37.3)**
Maximized Log-Likelihood	-14846.33	-14872.16	-14813.82	-14813.03
Observations	2003	2003	2003	2003

* - Significant at the 90% confidence level; ** - Significant at the 95% confidence level ; Ordered logit thresholds and error dispersions have been suppressed due to space constraints. These suppressed parameter estimates are all significant at the 95% level and are available from the authors. Estimated using MATLAB 7.1 R14 on a 64-bit Linux workstation.

Table 6
Heterogeneous Preferences (using only exogenous characteristics)

	Model 4a			**Model 4b**			**Model 4c**		
Scalar parameters									
MU(pecuniary benefits) (β_0)	6.309 (134.)**			6.322 (141.)**			6.305 (126.)**		
MU(non-pecuniary) (β_1)	0.8382 (16.9)**			0.8524 (16.9)**			0.8309 (16.0)**		
PTA: Outright Failure (θ_{1i})	0.2963 (3.55)**			0.2698 (3.14)**			0.2525 (2.93)**		
PTA: Private Provision (θ_{2i})	0.5071 (6.86)**			0.5407 (7.26)**			0.5663 (7.83)**		
Varying parameters	$\log(q_i)$	λ_i	$\log(\kappa_i)$	$\log(q_i)$	λ_i	$\log(\kappa_i)$	$\log(q_i)$	λ_i	$\log(\kappa_i)$
Constant	-2.659 (-55.8)**	1.003 (36.4)**	0	-2.639 (-54.9)**	0.9236 (19.7)**	0	-2.644 (-54.8)**	0.9279 (19.7)**	0
Age 21-25	-0.2055 (-5.69)**	-	-	-0.2177 (-5.97)**	0.07456 (1.26)	-	-0.2533 (-6.88)**	0.09179 (1.57)	0.07599 (2.92)**
Age 26-30	-0.1127 (-1.58)	-	-	-0.1393 (-1.96)*	-0.06965 (-0.764)	-	-0.2485 (-3.09)**	-0.1165 (-1.15)	0.2348 (4.31)**
Age 31-50	-0.3707 (-4.84)**	-	-	-0.3974 (-4.81)**	0.4519 (3.09)**	-	-0.5282 (-4.9)**	0.4446 (2.31)**	0.3745 (6.14)**
Female	-0.08995 (-2.7)**	-	-	-0.0956 (-2.85)**	0.08207 (1.48)	-	-0.0503 (-1.46)	0.06704 (1.21)	-0.131 (-5.35)**
Incidental parameters not reported:	Ordered logit thresholds			Ordered logit thresholds			Ordered logit thresholds		

Sample avg. of varying parameters	0.05917[a]	1.003	0	0.05964[a]	1.018	0	0.05887[a]	1.019	0.006581
Max Log L		-14770.32			-14761.79			-14714.48	
Observations		2003			2003			2003	

* - Significant at the 90% confidence level; ** - Significant at the 95% confidence level ; Ordered logit thresholds and error dispersions have been suppressed due to space constraints. These suppressed parameter estimates are all significant at the 95% level and are available from the authors. Estimated using MATLAB 7.1 R14 on a 64-bit Linux workstation. a – Exponentiated average of point estimates from logarithmic specification

Table 7
Heterogeneous Preferences (including Endogenous Characteristics and Elicitation Differences)

	Model 4d			Model 4e			Model 4f		
Scalar parameters									
MU(pecuniary benefits) (β_0)	6.359 (100.)**			6.36 (91.7)**			6.377 (91.0)**		
MU(non-pecuniary) (β_1)	0.9669 (13.7)**			0.9889 (13.4)**			1.021 (12.6)**		
PTA: Outright Failure (θ_{1i})	0.2418 (2.86)**			0.1531 (1.73)*			0.1890 (2.13)**		
PTA: Private Provision (θ_{2i})	0.6286 (9.01)**			0.6393 (9.17)*			0.6442 (9.05)**		
Varying parameters	$\log(q_i)$	λ_i	$\log(\kappa_i)$	$\log(q_i)$	λ_i	$\log(\kappa_i)$	$\log(q_i)$	λ_i	$\log(\kappa_i)$
Constant	-2.467 (-33.9)**	0.7603 (8.28)**	0	-2.162 (-22.4)**	0.9266 (8.13)**	0	-2.186 (-21.9)**	0.946 (8.15)**	0
Exogenous characteristics:									
Age 21-25	-0.2206 (-5.95)**	0.08114 (1.4)	0.09765 (3.71)**	-0.1746 (-4.53)**	0.1584 (2.64)**	0.05879 (2.23)**	-0.173 (-4.35)**	0.1692 (2.82)**	0.04709 (1.74)*
Age 26-30	-0.292 (-3.87)**	-0.2186 (-2.22)**	0.2618 (4.79)**	-0.1155 (-1.44)	-0.02791 (-0.264)	0.2128 (3.91)**	-0.07739 (-0.935)	-0.01052 (-0.103)	0.1763 (3.2)**
Age 31-50	-0.5874 (-4.9)**	0.4864 (2.64)**	0.3967 (6.49)**	-0.3671 (-3.05)**	0.4964 (2.62)**	0.3721 (6)**	-0.3797 (-3.09)**	0.5017 (2.62)**	0.3346 (5.31)**
Female	-0.08862 (-2.49)**	0.1495 (2.71)**	-0.1393 (-5.64)**	-0.001472 (-0.0382)	0.05245 (0.908)	-0.1583 (-6.33)**	0.02095 (0.526)	0.04227 (0.73)	-0.167 (-6.6)**

Potentially endogenous characteristics:									
Member environmental group?	-	-	-	-0.1795 (-4.16)**	-0.007705 (-0.0943)	-	-0.2281 (-4.72)**	0.007027 (0.0838)	0.07451 (2.36)**
Business Major?	-	-	-	0.1729 (4.47)**	-0.4695 (-7.77)**	-	0.1631 (4)**	-0.4639 (-7.73)**	0.004389 (0.155)
Social Science Major?	-	-	-	-0.2814 (-7.16)**	-0.2116 (-3.24)**	-	-0.3221 (-7.4)**	-0.23 (-3.42)**	0.06932 (2.34)**
Had Econ. Class?	-	-	-	-0.24 (-3.9)**	-0.03 (-0.344)	-	-0.2078 (-3.2)**	-0.05802 (-0.657)	-0.01535 (-0.396)
Self Reported Liberal	-	-	-	-0.2153 (-5.09)**	0.1078 (1.41)	-	-0.2206 (-5.00)**	0.1362 (1.80)*	-0.01156 (-0.383)
Self Reported Conservative	-	-	-	0.0843 (1.69)*	0.08923 (1.22)	-	0.06378 (1.19)	0.1254 (1.65)*	0.1226 (3.62)**
Currently Employed	-	-	-	-0.134 (-3.68)**	-0.01208 (-0.209)	-	-0.1548 (-4.02)**	-0.002385 (-0.0407)	0.05571 (2.19)**
Elicitation format:									
"Yes" option on left	0.0305 (0.88)	-0.16 (-2.95)**	-0.02397 (-0.973)	0.04213 (1.18)	-0.1029 (-1.84)*	-0.008596 (-0.347)	0.04209 (1.15)	-0.1116 (-1.99)**	-0.01086 (-0.437)
Descending tax credits	0.0218 (0.622)	-0.1448 (-2.53)**	0.05259 (2.12)**	0.02944 (0.822)	-0.05306 (-0.919)	0.04959 (1.98)**	0.03515 (0.955)	-0.02978 (-0.518)	0.04856 (1.94)*
Five tax credit amounts	-0.3427 (-6.75)**	0.08948 (1.24)	0.1394 (4.01)**	-0.2951 (-5.76)**	0.1471 (1.84)*	0.1209 (3.46)**	-0.2964 (-5.62)**	0.133 (1.65)*	0.1167 (3.34)**
Seven tax credit amounts	-0.2078 (-4.45)**	0.2889 (4.23)**	0.01248 (0.361)	-0.1832 (-3.81)**	0.1836 (2.33)**	0.01565 (0.442)	-0.1805 (-3.64)**	0.1414 (1.79)*	0.002304 (0.0648)
Thirteen tax credit amounts	-0.03168 (-0.65)	0.07857 (0.948)	0.1035 (2.9)**	-0.03639 (-0.734)	0.07415 (0.899)	0.1094 (3.05)**	-0.0394 (-0.774)	0.0682 (0.824)	0.107 (2.97)**

Three response options	-0.3777 (-4.45)**	0.2557 (3.06)**	0.14 (2.02)**	-0.2943 (-3.63)**	0.1757 (2.03)**	0.05698 (0.827)	-0.2816 (-3.33)**	0.1719 (1.96)*	0.04008 (0.606)
Four response options	-0.007472 (-0.136)	0.2788 (3.35)**	0.1831 (2.95)**	0.05954 (0.997)	0.1781 (2.10)**	0.1949 (2.87)**	0.07719 (1.26)	0.1643 (1.89)*	0.1706 (2.71)**
Five response options	-0.01107 (-0.125)	0.02147 (0.284)	0.3641 (5.59)**	0.03056 (0.307)	0.03571 (0.446)	0.3615 (5.39)**	0.05016 (0.488)	0.03146 (0.388)	0.3231 (5.17)**
Incidental parameters not reported:	Ordered logit thresholds			Ordered logit thresholds			Ordered logit thresholds		
Sample avg. of varying parameter	**0.0561**[a]	**0.9769**	**0.0926**	**0.0596**[a]	**0.9741**	**0.0642**	**0.0599**[a]	**0.9821**	**0.1199**
Maximized Log Likelihood		-14635.04			-14483.24			-14469.47	
Observations		2003			2003			2003	

* - Significant at the 90% confidence level; ** - Significant at the 95% confidence level ; Ordered logit thresholds have been suppressed due to space constraints. These suppressed parameter estimates are all significant at the 95% level and are available from the authors. Estimated using MATLAB 7.1 R14 on a 64-bit Linux workstation.

a – Exponentiated average of point estimates from logarithmic specification

Figure 1
Sample choice set (most-extensive variant)

Other trade-offs over time

Some trade-offs involve future benefits to *society*, rather than future money coming *just to you* as an individual. FAQ

Suppose that you are being asked to choose between two policy options:

1. a **one-time tax credit, *this year only*, for each household**, OR
2. having the total amount of the proposed tax credit for all households spent on government subsidies for research and development (R&D) for more energy-efficient air conditioners. If successful, this technology will save an average of **$50 per household per year** for the period **between 20 and 40 years** from now.

Other details:

- There is a **70% chance** that the government-subsidized R&D program will be successful.
- There is a **30% chance** that without the program, private companies would provide this technology.

For each row in the table below, *please click **one** button.* Respond just as you would if a *real* tax credit, in each amount, was at stake. Keep in mind the other things you might otherwise use this tax credit money to pay for.

If your one-time tax credit would be:	**Would you prefer this tax credit, rather than the R&D program?**				
	Definitely yes	**Probably yes**	**Not sure**	**Probably no**	**Definitely no**
$19					
$49					
$81					
$136					
$234					
$411					
$739					
	Definitely yes	**Probably yes**	**Not sure**	**Probably no**	**Definitely no**

OK

Figure 2

Pertinent respondent characteristics

How can you be described?

Click on one button, or check one or more boxes.

FAQ

1. What is your age?

20 or less	21-25 years	26-30 years	31-40 years	41-50 years	51-64 years	65 or more

2. What is your gender?

male	female

3. Highest education level <u>completed</u>?

highschool or less	some college	college graduate	master's degree	doctoral degree	trade school	professional degree

4. If you have attended any college, how many years of college have you <u>completed</u>?

<1 year	1 year	2 years	3 years	4 years	>4 years	not applicable

5. If you have attended any college, what is (was) your major field of study? (Check as many as apply.)

physical sciences	life sciences	social sciences	arts and humanities	engineering	business	other

…

7. Have you ever taken a college course in economics?

yes	no	not sure

8. Which categories describe your current status? (Check as many as apply.)

work full-time	work part-time	student	non-paid work	retired	childcare/ eldercare provider	other/ undecided

…

10. "I consider myself to be..."

liberal	moderately liberal	moderate	moderately conservative	conservative

…

12. "The annual income bracket for my family is:"

less than $10,000	$10,000 to $20,000	$20,000 to $30,000	$30,000 to $50,000	$50,000 to $75,000	$75,000 to $100,000	more than $100,000

…

16. To how many environmental groups or organizations do you belong?

0	1	2	3	4	5	6 or more

…

OK

.

Appendix 1

Randomization of Question Formats & Sample Exclusion Criteria

Randomization of Question Formats

Number of answer options offered. Some respondents were allowed only to indicate "yes" or "no" to the question of whether they would prefer the offered tax credit to the future air conditioning cost savings described in the choice scenario. For these respondents, the number of columns is just two (nc2=1). Other respondents were allowed to express uncertainty, with three answer options: "yes," "not sure," and "no" (nc3=1). A third group was not allowed to sit on the fence with the "not sure" option, but was allowed to express their degree of certainty about their yes and no answers with four options: "definitely yes," "probably yes," "probably no," and "definitely no" (nc4=1). A final quarter of the sample was allowed these four options plus a "not sure" option (nc5=1). One-quarter of the sample was allocated to each of these treatments. The two-option case is the base case, and we consider shifts in a number of different estimated quantities for each large number of answer options.

Number of tax credit amounts to be considered. The complexity of the choice occasion is generally expected to interact with the respondent's cognitive capacity to potentially affect choice consistency. It is also possible that the amount of detail included in the choice set can systematically affect the implied preference parameters or the social discount rates that we elicit. Roughly one-quarter of the sample was asked to consider a full set of 13 different possible tax credits (bid13=1), one-quarter each considered seven (bid7=1), five (bid5=1), or just three different credit levels (bid3=1). These indicator variables are all zero otherwise.

Yes-to-No versus No-to-Yes (left-to-right ordering of answer options). Across respondents, we randomly assigned answer options that varied between “yes” on the left (lright=1) for about half of the respondents and “yes” on the right (lright=0) for the other half.

Tax credit amounts small-to-large or vice versa (top to bottom). Half the survey designs had their tax credit amounts arranged in increasing order and half had the tax credit amounts decreasing from top to bottom. It was determined to be too cognitively challenging for respondents to present the tax credit amounts in randomized order.

Sample Exclusion Criteria

The modal response pattern is what we label as “plausible” and “monotonic.” This response pattern is when individual take large tax credits, but choose to let the government proceed with the R&D project when the tax credits are smaller. To qualify as monotonic, responses must demonstrate a non-decreasing affinity (as dictated by their response) to an increasing tax credit, with at least one choice that demonstrates strictly increasing affinity for an increased tax-credit. This last qualification makes the monotonic choice category distinct from respondents who made constant choices. Reverse monotonic choices are classified as demonstrating a non-increasing affinity to an increasing tax-credit, with at least one choice demonstrating strictly decreasing affinity.

The possibility that individuals might “get the question backwards” was not detected in the survey design phase. We asked if they would prefer each tax credit to the stated time profile of cost savings. However, some individuals seem to be answering “yes” or “no” in a manner consistent with the question having asked whether they would prefer the stated time profile of cost savings to the offered tax credit. Thus, these “backwards” responses were reverse coded and used in the study.

Another potentially problematic pattern was those respondents making constant choices. This module occurred relatively late in the CPS. This pattern could be an indication that the respondent is not making selections that reflect their preferences for the tax credit versus the future cost savings, but instead they merely wish to finish the survey and preserve their eligibility for the prize drawing. However, it could also be the case that the respondent is accurately selecting responses, but that their implicit discount rate lies outside the range reflected in the survey design. We retain constant choices.

Exclusion criteria were utilized based on systematic response patterns identified in the data. A final category of responses were those labeled as "inconsistent" (i.e. non-monotonic in either the increasing or decreasing implied discount rate). These responses are likely due to participant disinterest or confusion. For these reasons, observations exhibiting inconsistency were dropped from our estimating sample.

APPENDIX 2 - Log-likelihood Function

The formula for the log-likelihood function, as always, involves discrete indicators for each answer option. All of the following binary indicators are "zero otherwise":

$C2Y_i$, $C3Y_i$	=1 if respondent chooses "yes" in 2- or 3- alternative cases
$C2N_i$, $C3N_i$	=1 if respondent chooses "no" in 2- or 3-alternative cases
$C3M_i$, $C5M_i$	=1 if respondent chooses "not sure" in 3- or 5-alternative cases
$C4DY_i$, $C5DY_i$	=1 if respondent chooses "definitely yes" in 4- or 5-alternative cases
$C4PY_i$, $C5PY_i$	=1 if respondent chooses "probably yes" in 4- or 5-alternative cases
$C4PN_i$, $C5PN_i$	=1 if respondent chooses "probably no" in 4- or 5-alternative cases
$C4DN_i$, $C5DN_i$	=1 if respondent chooses "definitely no" in 4- or 5-alternative cases

Each different answer format offered on the survey implies a different set of probability formulas. These probabilities, by number of answer levels, are as follows:

2-level:

$$P2Y_i = \frac{1}{1+\exp[(\alpha_{20}-\Delta V_i)/\exp(\kappa_i)]} \qquad \text{YES}$$

$$P2N_i = \frac{\exp[(\alpha_{20}-\Delta V)_i/\exp(\kappa_i)]}{1+\exp[(\alpha_{20}-\Delta V_i)/\exp(\kappa_i)]} \qquad \text{NO}$$

3-level:

$$P3Y_i = \frac{1}{1+\exp[(\alpha_{31}-\Delta V_i)/\exp(\kappa_i)]} \qquad \text{YES}$$

$$P3M_i = \left(\frac{\exp[(\alpha_{31}-\Delta V_i)/\exp(\kappa_i)]}{1+\exp[(\alpha_{31}-\Delta V_i)/\exp(\kappa_i)]}\right)-\left(\frac{\exp[(\alpha_{30}-\Delta V_i)/\exp(\kappa_i)]}{1+\exp[(\alpha_{30}-\Delta V_i)/\exp(\kappa_i)]}\right) \qquad \text{NOT SURE}$$

$$P3N_i = \frac{\exp[(\alpha_{30}-\Delta V_i)/\exp(\kappa_i)]}{1+\exp[(\alpha_{30}-\Delta V_i)/\exp(\kappa_i)]} \qquad \text{NO}$$

4-level:

$$P4DY_i = \frac{1}{1+\exp[(\alpha_{42} - \Delta V_i)/\exp(\kappa_i)]} \qquad \text{Def. YES}$$

$$P4PY_i = \left(\frac{\exp[(\alpha_{42} - \Delta V_i)/\exp(\kappa_i)]}{1+\exp[(\alpha_{42} - \Delta V_i)/\exp(\kappa_i)]}\right) - \left(\frac{\exp[(\alpha_{41} - \Delta V_i)/\exp(\kappa_i)]}{1+\exp[(\alpha_{41} - \Delta V_i)/\exp(\kappa_i)]}\right) \qquad \text{Prob. YES}$$

$$P4PN_i = \left(\frac{\exp[(\alpha_{41} - \Delta V_i)/\exp(\kappa_i)]}{1+\exp[(\alpha_{41} - \Delta V_i)/\exp(\kappa_i)]}\right) - \left(\frac{\exp[(\alpha_{40} - \Delta V_i)/\exp(\kappa_i)]}{1+\exp[(\alpha_{40} - \Delta V_i)/\exp(\kappa_i)]}\right) \qquad \text{Prob. NO}$$

$$P4DN_i = \frac{\exp[(\alpha_{40} - \Delta V_i)/\exp(\kappa_i)]}{1+\exp[(\alpha_{40} - \Delta V_i)/\exp(\kappa_i)]} \qquad \text{Def. NO}$$

5-level:

$$P5DY_i = \frac{1}{1+\exp[(\alpha_{53} - \Delta V_i)/\exp(\kappa_i)]} \qquad \text{Def. YES}$$

$$P5PY_i = \left(\frac{\exp[(\alpha_{53} - \Delta V_i)/\exp(\kappa_i)]}{1+\exp[(\alpha_{53} - \Delta V_i)/\exp(\kappa_i)]}\right) - \left(\frac{\exp[(\alpha_{52} - \Delta V_i)/\exp(\kappa_i)]}{1+\exp[(\alpha_{52} - \Delta V_i)/\exp(\kappa_i)]}\right) \qquad \text{Prob. YES}$$

$$P5M_i = \left(\frac{\exp[(\alpha_{52} - \Delta V_i)/\exp(\kappa_i)]}{1+\exp[(\alpha_{52} - \Delta V_i)/\exp(\kappa_i)]}\right) - \left(\frac{\exp[(\alpha_{51} - \Delta V_i)/\exp(\kappa_i)]}{1+\exp[(\alpha_{51} - \Delta V_i)/\exp(\kappa_i)]}\right) \qquad \text{NOT SURE}$$

$$P5PN_i = \left(\frac{\exp[(\alpha_{51} - \Delta V_i)/\exp(\kappa_i)]}{1+\exp[(\alpha_{51} - \Delta V_i)/\exp(\kappa_i)]}\right) - \left(\frac{\exp[(\alpha_{50} - \Delta V_i)/\exp(\kappa_i)]}{1+\exp[(\alpha_{50} - \Delta V_i)/\exp(\kappa_i)]}\right) \qquad \text{Prob. NO}$$

$$P5DN_i = \frac{\exp[(\alpha_{50} - \Delta V_i)/\exp(\kappa_i)]}{1+\exp[(\alpha_{50} - \Delta V_i)/\exp(\kappa_i)]} \qquad \text{Def. NO}$$

A little extra intuition is involved when pooling data from two-, three-, four-, and five-alternative simple or ordered logit specification. In a two-outcome model, it is natural to normalize the threshold level of the latent variable, α_{20} above, to zero. When pooling all four types of data, it makes sense to normalize the distinction between a “yes” answer and a “no” answer to zero. For the three- and five-outcome cases, however, there is no bright line between “yes” and “no,” due to the presence of the “not sure” option. Thus we normalize α_{20} and α_{41} to zero, but allow all of the other thresholds (α_{30}, α_{31}, α_{40}, α_{42}, and α_{50}, α_{51}, α_{52}, and α_{53}) to take on whatever values the data seem to dictate, with the expectation that α_{30}, α_{40}, α_{50}, and α_{51} should be negative and the others should be positive.

Log-likelihood Function

Denote the number of observations in each subsample with 2, 3, 4, and 5 answer options *as N2, N3, N4 and N5*. The log-likelihood function for each model considered in this paper will be determined by the format of the relevant systematic utility-difference function, ΔV_i, is simply the sum of the relevant components that apply for each type of response format. The preference parameters to be estimated are embodied in this utility difference. The ordered logit threshold parameters, α_{mn} and the differentials in the error dispersion, relative to the base case, are captured by the systematically varying parameter κ_i:

Log L

$$=\sum_{i=1}^{N2}\left[C2Y_i\ln(P2Y_i)+C2N_i\ln(P2N_i)\right] \qquad \ldots \text{ sample with pairwise choices}$$

$$+\sum_{i=1}^{N3}\left[C3Y_i\ln(P3Y_i)+C3M_i\ln(P3M_i)+C3N_i\ln(P3N_i)\right]$$

… sample with three-alternative choices

$$+\sum_{i=1}^{N4}\left[C4DY_i\ln(P4DY_i)+C4PY_i\ln(P4PY_i)+C4PN_i\ln(P4PN_i)+C4DN_i\ln(P4DN_i)\right]$$

… sample with four-alternative choices

$$+\sum_{i=1}^{N5}\begin{bmatrix}C5DY_i\ln(P5DY_i)+C5PY_i\ln(P5PY_i)+C5M_i\ln(P5M_i)\\ +C5PN_i\ln(P5PN_i)+C5DN_i\ln(P5DN_i)\end{bmatrix}$$

… subsample with five-alternative choice

Optimization method

The unknown preference parameters (either as scalars, or as systematically varying parameters) are estimated by conventional maximum likelihood methods. Since the problem at hand is non-standard, however, the parameters cannot be estimated using packaged econometric software. We maximize the log-likelihood using Matlab 7.1. We employ numeric derivatives and the BFGS algorithm, but recalculate the full Hessian matrix at the optimum using Greene's formulas. Asymptotic t-test statistics are based on a symmetrized version of the Hessian, where the off-diagonal elements of the numeric Hessian, if different, are averaged.

Participating Universities and Colleges

Alfred University
American River College
Arizona State University
Bemidji State University
Boston College
Brandeis
Brock University
Brown University
California State University Monterey Bay
California State University, Long Beach
Colorado School of Mines
Columbia University
Dartmouth College
Duke University
Fairfield University
Georgia State University
Iowa State University
Kansas State University
Lewis and Clark College
Louisiana State University
Macalester College
Michigan State University
Millersville University
North Carolina State University
Northern Arizona University
Northwestern University
Ohio State University
Oregon State University
Pennsylvania State University
Reed College
SUNY College of Environmental Science …
San Diego State University
Simon Fraser University
Stanford University
State University of New York at Binghamton
State University of New York at Stonybrook
Syracuse University
Texas A&M University
Trinity University (San Antonio, Texas)
University of Alabama
University of Alaska, Anchorage
University of Alaska, Fairbanks
University of Alberta
University of British Columbia
University of Calgary
University of California, Berkeley
University of California, Irvine
University of California, Los Angeles
University of California, San Diego
University of California, Santa Barbara
University of Colorado, Boulder
University of Connecticut
University of Delaware
University of Florida
University of Guelph
University of Hawaii, Manoa
University of Illinois, Urbana-Champaign
University of Iowa, Iowa City
University of Kentucky
University of Maine
University of Maryland, Baltimore County
University of Maryland, College Park
University of Massachussetts, Amherst
University of Miami
University of Michigan, Ann Arbor
University of Minnesota
University of Nevada, Las Vegas
University of Nevada, Reno
University of New Hampshire
University of North Carolina at Asheville
University of North Carolina, Chapel Hill
University of North Carolina, Wilmington
University of Oregon
University of Ottawa
University of Pennsylvania
University of Pittsburgh
University of Rochester
University of Saskatchewan
University of Southern Mississippi
University of Tennessee
University of Texas at Austin
University of Virginia
University of Washington
University of West Virginia
University of Wyoming
Utah State University
Warren Wilson College
Washington State University
Washington University St. Louis
Westminster College
Yale University
York University

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Do Fishermen Lie? Measuring Hypothetical Bias Across Response Formats

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Abstract

This paper presents the results of a study comparing question format effects in a contingent valuation experiment. Four treatments are implemented on samples of Montana licensed anglers comparing hypothetical and actual payments using a dichotomous choice format, and hypothetical and actual payments using a payment card question format. This paper is relevant to the W-1133 objective to Estimate Benefits of Ecosystem Management of Forests and Watersheds. The study replicates major elements of an earlier (1989-90) field experiment which solicited hypothetical and actual donations to benefit instream flows for Montana fisheries. Extensions of the earlier work include: repeat contacts to increase response rate, follow-up of the contingent valuation question to explore respondent certainty, and several question format treatments (payment card, as in the original study, and dichotomous choice). The partner for the cash treatment in the current study is Montana Trout Unlimited. Methods include interpretation of welfare measures based on an interval response model for both payment card and dichotomous choice. The study design allows investigation of some standard, and not yet fully resolved, major issues in stated preference methods including the extent of hypothetical bias, and how bias varies across question format (e.g. "what question format reveals the truth about public good values").

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Do Fishermen Lie? Measuring Hypothetical Bias Across Response Formats

I. Introduction

This paper describes preliminary results from a field experiment designed to compare responses to a contingent valuation instrument to actual cash donations. This study is in part a replication of an earlier experiment (Duffield and Patterson 1991) aimed at measuring values for provision of a public environmental good. The resource in the 1990 survey was increased streamflow in several potentially important spawning tributaries for two endangered fisheries: a fluvial population of Arctic grayling and a population of Yellowstone cutthroat trout.

A limitation of the 1990 study was that the two treatments of most interest were implemented as one-time mailings to simulate typical fund-raising solicitations. Both of the latter went out under The Nature Conservancy letterhead and were designed to be very similar in content and wording. As a result of the single mail contact, the response rates were relatively low to these treatments, particularly for the cash response. There was a third treatment (contingent valuation) that paralleled the first two, but went out under University of Montana letterhead and included repeat mail contacts (a total of four) and achieved high response rates (74% and 77% for resident and nonresident anglers respectively). The University of Montana treatment was used to characterize the population and provide a contrast between a "typical" academic contingent valuation and the other treatments.

The objectives in replicating the 1990 survey in 2005 included achieving higher response rates in the comparable cash and contingent valuation treatments to provide a better measure of potential differences in real and hypothetical economic commitments for this resource and setting. It was also anticipated that the replication over the span of 15 years would provide an opportunity to measure changes in values, and insights into what, if any, measures of attitudes, preferences, or socio-economic status and characteristics might explain any changes found. A previous paper (Duffield, Neher, Patterson, and Champ 2005) provided a preliminary summary of the results comparing the payment card question format responses in 1990 and 2005 across two quite different angler populations: resident Montana anglers and licensed nonresident anglers.[1]

The focus of the current paper is on the question format effects (for dichotomous choice and payment card), across both actual and hypothetical treatments. There are two strands of related literature here: studies that have investigated cash and hypothetical payments, and studies that have investigated question format effects. The literature at the intersection of these two sets is quite limited. Brown et al. 1996 provided the first such study, comparing dichotomous choice and open-ended formats with both actual and hypothetical payments. Champ and Bishop (2001, 2006) investigate three treatments: dichotomous choice hypothetical and actual, and payment card actual. To our knowledge, the current study is the first to compare dichotomous choice and payment card formats using both actual and hypothetical treatments.

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[1] Overall welfare measures for the payment card treatments based on a simple average of the bid amounts indicated were: resident cash $2.78 and resident CV $5.38 (ratio of 0.516 cash/CV), and nonresident cash $13.18, CV $29.28 (ratio 0.45). The resident values for both cash and CV treatments were about 20% of the nonresident values, consistent with some major differences between the two angler populations. Nonresidents were much more specialized (70 percent fly fish only versus 23 percent for residents), avid, had higher income (were two brackets higher than Montana residents), more likely to be male (86 percent versus 74 percent), older (mid-50's versus mid-40's) and much more likely to be a member of a conservation, sportfishing, or boating organization (48% versus 19%). In real (constant 2005 dollar) terms the 2005 values were similar (and for residents almost identical) compared to the 1990 estimates.

The remainder of this paper includes a brief summary of literature and methods, followed by a description of the preliminary results.

II. Literature

The comparison of real economic commitments with contingent valuation responses had its beginning in the work of Bohm (1972) and Bishop and Heberlein (1979). There have since been a number of laboratory and field experiments. Studies specifically investigating donation payment mechanisms include Duffield and Patterson 1991, Navrud 1992, Seip and Strand 1992, Brown et al. 1996, Champ et al. 1997, Byrnes et al. 1999, Champ and Bishop 2001, and Champ and Bishop 2004. The general finding of this literature is that hypothetical payments generally exceed actual payments. Other things equal, this provides evidence of hypothetical bias.

There is a substantial literature on question format effects, as summarized in Table 2. Only a handful of these studies (six) include actual payments, while 20 report contingent valuation results. Most of the studies focus on the comparison of dichotomous choice to open-ended. The most common result is that $WTP_{DC}>WTP_{OE}$. With respect to dichotomous choice and payment card, the consistent finding based on 10 studies is that dichotomous choice estimates are greater than (8 studies) or equal to (two studies) payment card based estimates. The one study comparing actual payments (Champ and Bishop 2006) is typical and shows the ratios of dichotomous choice to payment card estimated mean WTP to be 2.25 (using a linear logit model and Hanemann mean) to 2.10 (nonparametric). This summary is simplistic given the great variation in methods and resources across these studies including estimation, choice of welfare measure, split or combined samples, and public versus private goods.

III. Methods

The general finding from the literature is that hypothetical payments generally exceed actual payments. As noted, and other things equal, this provides evidence of hypothetical bias. The latter is more or less the Achille's heel of the contingent valuation method: "ask a hypothetical question and you get a hypothetical answer". Conversely, the holy grail for the field might be in identifying a contingent valuation procedure that consistently identifies the underlying latent willingness to pay, as presumably measured by actual payments. There are a number of promising approaches in the literature to developing such procedures. These include "cheap talk" (e.g. Cummings and Taylor 1999), and strategies for identifying respondents who are more certain about their hypothetical responses (e.g. Champ et al. 1997, Ready et al. 2001). [2] The approach here is focused on reconsidering the question Brown et al. 1996 posed: "Which response format reveals the truth about donations to a public good?". Brown et al.'s answer with respect to dichotomous choice and open-ended formats was "neither". The candidate being examined here is the payment card approach, again being compared to dichotomous choice.

The specific hypothesis we test measure the equivalence of question formats and response to hypothetical and actual donation payment vehicles: 1) Is the response to willingness to pay questions, overall contribute or not contribute, equivalent across treatments, and 2) Are willingness to pay estimates equivalent based on measures of central tendency?

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[2] The current study includes a followup "certainty" question based on the Champ et al. 1997 methods. These results are not covered here.

The resource examined here is a public environmental good in that many of the services provided by the resource in question are not excludable. It is anticipated that existence and bequest motives (Krutilla 1967) relating to instream flow in these streams and the associated passive use are significant relative to direct use. In fact it is not very likely that any given angler respondent will ever fish any of the several small streams described in the 1990 and 2005 studies, or experience significantly improved angling in the larger rivers fed by these small tributaries. Nonetheless, direct use may still be an important motive. In any case, the specific payment vehicle used here is anticipated to capture both passive and direct use in a total valuation framework (Randall and Stoll 1983). The choice to make a donation can be modeled in the context of an indirect utility function framework (e.g. Boyle and Bishop 1987). The willingness to pay (donate) amount that will just make an individual ambivalent between the current level of services and one with adequate streamflow defines a Hicksian compensating variation welfare measure. Cameron and Huppert (1989, 1991) provide an empirical model for estimating WTP from payment card interval data. Symmetric parametric or nonparametric methods can be applied for both question formats.

The choice of a donation payment vehicle raises problems in interpretation due to a lack of incentive compatibility relative to a referendum format (Carson, Groves and Machina 2000). Nonetheless, a donation payment vehicle is the most plausible approach for the public environmental good at issue here, and can arguably provide a lower bound on the relevant Hicksian surplus (Champ et al. 1997). It is apparent that the general recommendation of the NOAA panel (Arrow et al. 1993) to utilize the referendum format for passive use valuation is too broad, since there are many cases where referendums are not feasible or plausible.

Table 1 summarizes and compares study methods between the1990 and 2005 experiments. An important change in survey methods was to use Dillman method repeat mail contacts. The 2005 study included five contacts: an intial letter, first survey mailing, reminder postcard, second survey mailing, and a third survey mailing.

The basic structure (and most of the original questions) of the 1990 survey instrument was retained for 2005. The sequence is as follows: initial set of questions on angling use, questions designed to measure attitudes and preferences, valuation question sequence, and questions addressing respondent socioeconomic characteristics. The decision was made to use the same set of payment card amounts as in 1990 (10, 25, 50, 100, 250, other) for both payment card and dichotomous choice formats. Information on the resource and the Montana Streamflow Fund initiative were provided in the initial letter and prior to the donation questions in the survey instrument. The text for the donation question for all four treatments is provided in Figure 1.

The revised instrument was pretested in fall of 2004 with a mailing to a sample of 300 anglers. One important finding from the pretest was that the subsample of the 2003-2004 nonresident season angler license list made available to the researchers by Montana Fish, Wildlife and Parks included nonresidents who held season licenses by virtue of a “combination” elk and/or deer hunting license that included season fishing. The latter group had very low response rates to the 2005 pretest, and had not been included in the 1990 sample frame. For the main 2005 survey, this group was also excluded from the nonresident season license subsample.

The initial contact letter for the 2005 survey was mailed on January 21. The reminder postcard went out February 8, the first survey package January 27- 31, second survey package on February 25, and third survey package on April 13.

Table 3 summarizes the allocation of the total initial mailing list (of 2,500 nonresident anglers) across the four treatments, and response rates. Based on the pretest and 1990 study response rates, cash treatments were oversampled relative to contingent valuation in anticipation of lower relative response rates.

IV. Results

Table 4 provides means of respondent characteristics by subsample (proportions for binary variables). Characteristics include angler specialization, days fishing, preferences across fishery types, general environmental attitudes (intended to measure bequest and existence motives), knowledge of trust funds, and socioeconomic characteristics (including age, income, gender, and education). Inspection of these means indicates that the subsamples are generally similar with respect to these measures. One-way ANOVA's on all variables in that table of means found only one statistically significant difference in "priority is cutthroat/bull trout" (P=.009). This variable measured respondent priorities for the type of stream to get additional instream flow resources; PC hypothetical was higher than the other three. To summarize, only one of the 20 measures of respondent characterisitics showed a significant difference across the subsamples. This supports the interpretation that any differences found across question format are likely treatment effects rather than due to differences between subsamples.

With respect to response rates, using five mail contacts in 2005 (compared to one in 1990) significantly improved survey participation. The overall response rate is 47%. Relative response rates across treatments could provide an indication of the relative difficulty respondents have in answering a given question format or an actual compared to a hypothetical donation question. The cash response rates average about 85% of the corresponding contingent valuation treatment response rate. For both the payment card format and the dichotomous choice format, these differences were significant (P=.010 and P=.042, respectively, Table 5). The dichotomous choice response rates were also systematically lower (and also in about an 85% ratio) compared to the corresponding nonresident payment card response both overall, across cash treatments and across hypothetical treatments (P=.000, P=.003, and P=.013, respectively).

Item nonresponse to the donation question across treatments is as follows: DC actual 6.7%, DC hypothetical 6.9%, PC actual 13.2%, and PC hypothetical 5.8%. Combining response rates and item nonresponse for the treatments to estimate aggregate nonresponse shows the following percentile of useable surveys relative to delivered surveys: DC actual 40.2%, DC hypothetical 46.3%, PC actual 44.2%, and PC hypothetical 55.7%. This tabulation counts cash responses as "missing" if the donation question was not marked. However, in a number of cases respondents donated cash amounts other than the bid amount for both question formats, particularly for dichotomous choice. For purposes of estimating willingness to pay, missing donation question responses were coded as "no" in the dichotomous choice format and "zero" in the payment card format. Sample sizes for each treatment are close to the study goal of about 200 in each cell for the contingent valuation treatments and well in excess of that number for the cash treatments (Table 3).

Turning to our specific hypothesis, overall response to the donation question, in terms of contributing or not, is shown for all four treatments in Table 6. The hypothesis of equivalent responses across cash and hypothetical treatments for the dichotomous choice format is rejected (chi-square test statistic 14.608, P=.000), as well as for the payment card format (test statistic 25.705, P=.000). The percentage of respondents contributing some amount is greater in the hypothetical payment treatment compared to the actual payment treatment for both question formats. For actual and hypothetical respondents, the percentage of respondents contributing is significantly higher for the payment card respondents (cash chi-square 4.90, P=.027; hypothetical 5.73, P=.017).

Response distributions for both question formats are shown in Table 7. For dichotomous choice the data displayed is the usual percent "yes" by bid level. For payment card, the reported parameter is cumulative indicating the percent willing to pay that amount or greater. These distributions are plotted in Figure 2. By inspection, the dichotomous choice hypothetical response distribution stands out from the other three treatments as not converging to near zero; this appears to be evidence of the oft-noted "fat tails" problem. In fact, the divergence is not just in the tails. Interestingly, the absolute differences between cash and hypothetical "yes" response are fairly stable in absolute terms. For example, the ratios at $10, $50 and $250 are quite similar (0.235, 0.218, and 0.189). With respect to the payment card data, the CV response proportions drop off very significantly at the highest bid level in the payment card (only 3.1 percent check this amount in the hypothetical payment card treatment, compared to 19.4 percent answering "yes" to this bid amount in the hypothetical dichotomous choice). Actual percentages at this highest bid are quite similar to the hypothetical payment card response: 0.9 % for actual payment card at the $250 bid and 1.6% for actual dichotomous choice. An interesting twist is that the payment card CV responses intersect and appear to fairly closely match the cash dichotomous choice responses (Figure 2).

Turning to estimated WTP results, Table 8 summarizes bivariate log logistic models for the dichotomous choice treatments and Tables 8 and10 give the estimated welfare measures. The estimated truncated mean (truncated at the highest bid amount) is $76.17 in the hypothetical treatment and $36.90 in the cash treatment. The standard error of differences between truncated means is 10.56, the z-statistic is 3.72, and differences are significant (P=.0002). Medians are $25.43 and $8.86 are also significantly different (SE of difference equals 7.46, z equals 2.22 and P=.026).

The distribution of bid amounts selected for the payment card treatments is shown in Table 9. The simple mean of these bids is $29.28 for the hypothetical donation and $13.18 for the actual donation.

Table 10 provides welfare measures for both dichotomous choice and payment card estimated WTP. Table 10 provides results from a log logisitic model using the payment card interval data estimated by maximum likelihood (Cameron and Huppert 1989). The estimated truncated mean for the payment card hypothetical is $43.27, which is significantly greater than the truncated mean for payment card actual of $21.94 (SE of the difference equals 10.56, z equals 3.72 and P=.0002). The estimated medians are also significantly different (SE of the difference is 2.23, z equals 3.91, and P=.0001).

Comparing question formats, the DC hypothetical truncated mean of $76.17 is significantly greater than the payment card hypothetical mean of $43.27 (SE of the difference is 10.11, z equals 3.26, P=.00011). The differences for actual truncated means are also significantly different ($36.90 for dichotomous choice and $21.94 for payment card), SE of the difference 5.28, z equals 2.83, and P=.0046. The differences between the medians are only marginally statistically significant.

Interestingly, the payment card hypothetical truncated mean of $43.27 and the dichotomous choice cash truncated mean of $36.90 are not statistically significantly different (SE of the difference 6.19, z=1.03, P=.30).

A final study result relating to question format effects is the effect on overall participation. This has implications for which of the response formats is most efficient for creating effective demand for public goods. From the standpoint of fund raising, the resident payment card approach generated $1.13 per initial list and 4.1 percent contributed, nonresident PC $6.00, and 14 percent, and nonresident DC $3.06 and 8.6 percent. As a one-time fund raising drive, the two question formats are actually potentially quite similar in total take, it's just that the dichotomous choice needs to be done in two stages: a pretest to identify the optimal bid, and then a mailing to implement at that one bid level. For the case at hand, a dichotomous choice mailing just asking for donations of $100 would return $5.88 per initial list name, statistically identical to the $6.00 for the payment card. The advantage of the payment card, however, is much higher participation (total percent of contributors is 5.9 percent for the second stage of a DC design versus 14.0 percent for a PC design). For creation of a pool of donors to draw on in the future, the PC yields a much more valuable list. By the standards of the direct mail world, the nonresident list is obviously very lucrative. In marginal returns per mailing, there was a smooth decline as contacts progressed, but the nonresident mailing was still more than breaking even on the third mailing against costs ($0.60 per address marginal return), while the resident mailing was only cost effective for one mailing (and slipped to earning 4 cents on the third mailing).

V. Discussion

With respect to the comparison between hypothetical and actual donation responses, the findings of this study are consistent with the existing literature. Hypothetical contributions were found to be significantly greater than actual contributions for both question formats. What we found in addition, similar to the findings of Brown et al. 1996, was that the tendency for contingent valuation to overestimate WTP seems to be exacerbated by use of the dichotomous choice question format.

However, the interesting finding here is that the payment card question format does relatively well in terms of more closely matching the actual WTP distributions, particularly in the tail of the distribution. If one is willing to accept the view that either of the actual WTP distributions (payment card or dichotomous choice) is an equally plausible measure of the true latent WTP, then it is noteworthy that the payment card hypothetical response provides a reasonable approximation to one of these measures, the actual dichotomous choice responses. Needless to say, this result is limited to the current study, and for the case where a donation payment vehicle is used to value a public environmental good. Further studies would be needed to support the proposition that the payment card question format can be used to reliably identify actual latent willingness to pay – at least within the range of our ability to measure this construct. In any case, these findings suggest that the payment card may be the most promising of the three basic question format approaches (dichotomous choice, payment card, open-ended) for at least a subset of public environmental goods. Future research should continue to explore the influence of question format, perhaps most usefully in conjunction with the other approaches being developed to minimize hypothetical bias, including cheap talk and correction for respondent certainty.

Do fishermen lie? Yes, but the story you get depends on how you ask the question.

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Table 1. Comparison of Study Methods: 1990 and 2005 Studies

Study characteristic	**1990 Study**	**2005 Study**
Resource examined	Instream Flows / Threatened Fisheries	Instream Flows / Montana Fisheries
Cooperating group	The Nature Conservancy	Trout Unlimited
CV Question format	Payment Card	Payment Card and Dichotomous Choice
Surveys mailed	7,662	3,750
Survey contacts	One	Five
Sample Frame	Licensed anglers	Licensed anglers

Table 2: Recent Elicitation Studies since 1990

Authors	**The Good**	**Public or Private Good**	**Response Formats**	**Results**
Actual Payment Studies				
Champ and Bishop (2006)	Wind generated electricity	Public	DC, PC	$WTP_{DC} > WTP_{PC}$
Fyrkblom and Shogren (2000)	A Swedish national atlas	Private	DC, OE	$WTP_{DC} = WTP_{OE}$[a]
Cadsby and Maynes (1999)	Tokens which are converted into Canadian Dollars	Public	DC, OE	$WTP_{DC} < WTP_{OE}$
Lunander (1998)	Preview of a movie	Private	DC, OE	$WTP_{DC} > WTP_{OE}$
Loomis et al. (1997)	Art Print	Private	DC, OE	$WTP_{DC}=WTP_{OE}$
Brown et al. (1996)	Road removal in the North Rim of Grand Canyon	Public	DC, OE	$WTP_{DC}>WTP_{OE}$
Contingent Valuation Studies				
Blaine et al. (2005)	Curbside Recycling	Public	DC, PC	$WTP_{DC} > WTP_{PC}$

Ready, Navrud, and Dubourg (2001)	Avoidance of an episode of illness	Private	DC, PC[3]	$WTP_{DC} > WTP_{PC}$
Cameron, Poe, Ethier, Schulze (2002)	Green Power Program	Public	DC, OE, PC	$WTP_{DC} > WTP_{PC} > WTP_{OE}$
Reaves, Kramer, and Holmes (1999)	Recovery of an endangered species	Public	DC,[4] OE, PC	$WTP_{DC} = WTP_{OE} = WTP_{PC}$
Bohara et al. (1998)	Protection of Instream Flows	Public	DC, OE	$WTP_{DC} \geq WTP_{OE}$[5]
Lunander (1998)	Preview of a movie	Private	DC, OE	$WTP_{DC} > WTP_{OE}$
Welsh and Poe (1998)	Reduced fluctuations in Glen Canyon Dam releases	Public	DC, PC	$WTP_{DC} > WTP_{PC}$
Kramer and Mercer (1997)	Tropical Rain Forest Protection	Public	DC, PC	$WTP_{DC} = WTP_{PC}$
Loomis et al. (1997)	Art Print	Private	DC, OE	$WTP_{DC} = WTP_{OE}$
Donaldson, Thomas, and Torgerson (1997)	A bone mineral density scan	Private	OE, PC	$WTP_{PC} > WTP_{OE}$
Boyle et al. (1996)	Ex post WTP to hunt moose in Maine	Private	DC, OE	$WTP_{DC} = WTP_{OE}$
	WTP of individuals who applied for a moose hunt permit but did not get one	Private	DC, OE	$WTP_{DC} > WTP_{OE}$

[3] Ready, Navrud and Dubourg refer to the payment card treatment as open ended. However the treatment was like a payment card in that respondents were shown a card with offer amounts and asked to check the amount they would like.

[4] The DC question used a double-bounded format where respondents who said yes to the initial offer were asked a follow-up question with a higher offer amount and respondents who said no to the initial offer amount were asked a follow-up question with a lower offer amount.

[5] $WTP_{DC} > WTP_{OE}$ when a log normal distribution was used and $WTP_{DC} = WTP_{OE}$ when a Weibull or Gamma distribution was used.

	Creation of a local response center to clean up oil spills	Public	DC, OE	$WTP_{DC} = WTP_{OE}$
Brown et al (1996)	Road Removal in the North Rim of the Grand Canyon	Public	DC, OE	$WTP_{DC} > WTP_{OE}$
Ready, Buzby, Hu (1996)	Food safety improvements	Private	DC, PC	$WTP_{DC} > WTP_{PC}$
Holmes and Kramer (1995)	Protection of a forest ecosystem	Public	DC, PC	$WTP_{DC} > WTP_{PC}$
McFadden (1994)	Wilderness Preservation	Public	DC, OE	$WTP_{DC} > WTP_{OE}$
Kriström (1993)	Protection of forest areas in Sweden	Public	DC, OE	$WTP_{DC} > WTP_{OE}$
Kealy and Turner (1993)	Candy Bar	Private	DC, OE	$WTP_{DC} = WTP_{OE}$
	Reduction in acid rain damage in Adirondacks	Public	DC, OE	$WTP_{DC} > WTP_{OE}$
Haefele, Kramer and Holmes (1992)	Forest quality	Public	DC, PC	$WTP_{DC} > WTP_{PC}$
Johnson, Bregenzer, and Shelby (1990)	Permit for one whitewater recreation trip on the Rogue River	Private	DC, OE	$WTP_{DC} > WTP_{OE}$

Table 3. Response Rate Characteristics, 2005 Survey

Sample	Surveys mailed	Bad Addresses	Delivered	Surveys returned	Response rate
Nonresident Payment Card					
- Cash sample	850	89	761	387	50.9%
- Hypothetical Sample	400	48	352	208	59.1%
Subtotal-Nonresident PC	1250	137	1113	595	53.5%
Nonresident Dichotomous Choice					
- Cash sample	850	122	728	314	43.1%
- Hypothetical sample	400	50	350	174	49.7%
Subtotal-Nonresident DC	1250	172	1078	488	45.3%

Table 4. Means of respondent characteristics by subsample (proportions for binary variables).

	DC		PC	
	Cash (*n* =314)	Hypo (*n*=174)	Cash (*n*=387)	Hypo (*n*=208)
How often do you participate in river-related recreation (1=Never to 5=Very frequently)	3.73	3.90	3.84	3.93
Preferred type of water				
Lakes/reservoirs	.22	.22	.18	.21
Smaller streams	.25	.29	.26	.29
Rivers	.53	.49	.56	.51
Fished a Montana stream or river in last 3 years	.80	.86	.80	.87
Days fishing in Montana in 2004	10.39	10.84	10.80	13.06
Use flies only	.66	.70	.65	.70
Rate fishing (1=favorite to 4=prefer other activities)	1.80	1.67	1.82	1.76
Member of any conservation, sportfishing or boating organization	.51	.56	.48	.51
Own or lease recreational property in Montana	.25	.29	.24	.25
Importance of adequate streamflows for Montana fisheries (1=very important to 4=not important)	1.24	1.14	1.24	1.20
Priority rainbow/brown	.45	.43	.41	.37
Priority cutthroat/bull	.27	.29	.28	.40
Attitudes (1=strongly agree to 5=strongly disagree)				
• I enjoy knowing my friends can visit rivers for recreation	1.51	1.47	1.50	1.42
• I have little concern for endangered species	4.29	4.24	4.25	4.32
• I'm glad there's wilderness in Montana even if I never get to see it	1.51	1.47	1.44	1.48
• I feel I should be doing more for Montana's rivers and streams	2.60	2.48	2.61	2.48
• Protecting the environment should be responsibility of state and federal government	2.99	2.83	2.94	2.69
• Private conservation organizations should play a major role in protecting environmental resources	2.09	2.02	2.12	2.18
• I think most Montana rivers already have enough water in them to be a healthy resource	3.73	3.71	3.71	3.77
• Rivers have spiritual or sacred values for me	2.61	2.56	2.62	2.53
• I would be willing to contribute money or time to help Montana rivers even if I could never visit them	2.90	2.70	2.83	2.68
Trust fund knowledge (1=never heard of them to 4=know a great deal about them)	2.57	2.68	2.55	2.71

Heard of Trout Unlimited	.90	.92	.91	.93
Member of Trout Unlimited	.34	.34	.35	.38
Heard of TU projects	.54	.58	.58	.57
Age (years)	55.35	53.98	55.24	54.06
Male	.87	.86	.85	.84
Education (1=some grade school to 8=finished postgraduate)	6.26	6.31	6.17	6.09
Income level (1=less than $15,000 to 9=$150,000+)	6.03	5.61	5.52	5.61

Table 5. Comparison of response rates across subsamples.

Table 5a. Nonresident, Payment card, cash v. hypothetical

		Responded to survey		
		No	Yes	Total
Treatment	Cash	374	387	761
		49.1%	50.9%	100.0%
	Hypo	144	208	352
		40.9%	59.1%	100.0%
Total		518	595	1113
		46.5%	53.5%	100.0%

Table 5 b. Nonresident, Dichotomous choice, cash v. hypothetical

		Responded to survey		
		No	Yes	Total
Treatment	Cash	414	314	728
		56.9%	43.1%	100.0%
	Hypo	176	174	350
		50.3%	49.7%	100.0%
Total		590	488	1078
		54.7%	45.3%	100.0%

Table 6: Response to Willingness to Pay Question, Contribute or not Contribute across treatment.

	Dichotomous Choice		Payment Card	
	Cash (n=314)	Hypo (n=174)	Cash (n=387)	Hypo (n=208)
Yes	23.2%	39.7%	30.7%	51.9%
No	76.8%	60.3%	69.3%	48.1%

Table 7. Response distributions. For dichotomous choice, percent responding "yes". For payment card, percent indicating that amount or greater.

	Dichotomous choice		Payment card	
Amount($)	Cash	Hypo	Cash	Hypo
10	44.1	70.6	35.4	55.1
15			25.6	
20			25.3	47.4
25	32.8	44.4	24.1	46.4
50	18.2	36.4	13.7	25.0
100	16.4	29.4	6.3	12.8
250	1.6	19.4	0.9	3.1
500				0.5
n	345	178	387	208

Table 8. Logit models, dichotomous choice.

Table 8a. Hypothetical treatment

Variable / statistic	**coefficient**	**S.E.**	**p.**
Intercept	2.1987	0.6159	0.0004
LN(BID)	-0.6795	0.1577	0.00001
N	173		
median	$25.43		
T-mean ($250)	$76.17		
S.E. of T-mean	$9.36		

Note: SE of mean simulated using 10,000 iterations.

Table 8b. Cash treatment

Variable / statistic	**coefficient**	**S.E.**	**p.**
Intercept	1.8355	0.5090	0.0003
LN(BID)	-0.8414	0.1456	0.00001
N	314		
median	$8.86		
T-mean ($250)	$36.90		
S.E. of T-mean	$4.88		

Note: SE of mean simulated using 10,000 iterations.

Table 9. Payment card question format, response distribution by bid level and means of selected bid levels.

Amount($)	cash	hypo
0	69.3	48.1
10	8.5	7.2
15	0.3	
20	1.0	1.0
25	9.0	20.2
50	6.5	11.5
100	4.7	9.1
250	0.8	2.4
500		0.5
n	387	208
mean	13.18	29.28

Table 10. Estimated (SE) of median and truncated mean WTP based on log-logistic model for WTP. SE's based on 1000 bootstraps.

	n	Median	Mean truncated at $250
DC-Hypo	173	25.43 (7.11)	76.17 (9.36)
DC-Cash	314	8.86 (2.26)	36.90 (4.88)
PC-Hypo	208	12.78 (2.13)	43.27 (3.81)
PC-Cash	387	4.08 (0.65)	21.94 (2.02)

Figure 1. Willingness to donate questions.

A. Dichotomous choice, cash treatment.

4. We would like to know how much you would be willing to contribute to Trout Unlimited's Montana Streamflow Fund.

<u>Every dollar contributed to this fund would go directly to increasing streamflows in Montana trout streams through the purchase or lease of water rights on Watkins Creek, a rainbow and cutthroat tributary of the Madison River and Sweet Grass Creek a stream that will benefit recruitment of brown trout in the Yellowstone River.</u>

All administrative costs as well as the costs of this survey are being covered by other sources. These specific waters, on which Trout Unlimited is currently working to purchase water rights, are further described on the back of the cover letter.

Are you willing to make a donation of $________to the Montana Streamflow Fund to help purchase water rights for instream flows on these streams? **(Please check one.**)

☐ yes→ **Please complete the enclosed pledge form and return it with the survey**.

☐ no

B. Dichotomous choice, hypothetical treatment.

4. We would like to know how much you would be willing to contribute to Trout Unlimited's Montana Streamflow Fund. As this survey is part of a research project, we are not asking you to make a donation. Nonetheless, we would like you to answer the following question as you would a solicitation for an actual donation.

<u>Every dollar contributed to this fund would go directly to increasing streamflows in Montana trout streams through the purchase or lease of water rights on Watkins Creek, a rainbow and cutthroat tributary of the Madison River and Sweet Grass Creek, a stream that will benefit the recruitment of brown trout in the Yellowstone River.</u>

All administrative costs as well as the costs of this survey are being covered by other sources. These specific waters, on which Trout Unlimited is currently working to purchase water rights, are further described on the back of the cover letter.

If you were asked today, would you be willing to donate $_______ to the Montana Streamflow Fund to help purchase water rights for instream flows on these streams? **(Please check one.)**

☐ yes

☐ no→Please skip to Question 6

C. Payment card, cash treatment.

4. We would like to know how much you would be willing to contribute to Trout Unlimited's Montana Streamflow Fund.

<u>Every dollar contributed to this fund would go directly to increasing streamflows in Montana trout streams through the purchase or lease of water rights on Watkins Creek, a rainbow and cutthroat tributary of the Madison River and Sweet Grass Creek a stream that will benefit recruitment of brown trout in the Yellowstone River.</u>

All administrative costs as well as the costs of this survey are being covered by other sources. These specific waters, on which Trout Unlimited is currently working to purchase water rights, are further described on the back of the cover letter.

How much are you willing to donate to the Montana Streamflow Fund to help purchase water rights for instream flows on these streams? **(Please check one)**

☐ \$10 ☐ \$25 ☐ \$50 ☐ \$100 ☐ \$250 ☐ \$______Other

☐ \$0, I would choose not to make a donation at this time

If you are making a donation**:**

Please complete the enclosed pledge form and return with the survey.

D. Payment card, hypothetical treatment.

4. We would like to know how much you would be willing to contribute to Trout Unlimited's Montana Streamflow Fund. As this survey is part of a research project, we are not asking you to make a donation. Nonetheless, we would like you to answer the following question as you would a solicitation for an actual donation.

<u>Every dollar contributed to this fund would go directly to increasing streamflows in Montana trout streams through the purchase or lease of water rights on Watkins Creek, a rainbow and cutthroat tributary of the Madison River and Sweet Grass Creek, a stream that will benefit the recruitment of brown trout in the Yellowstone River.</u>

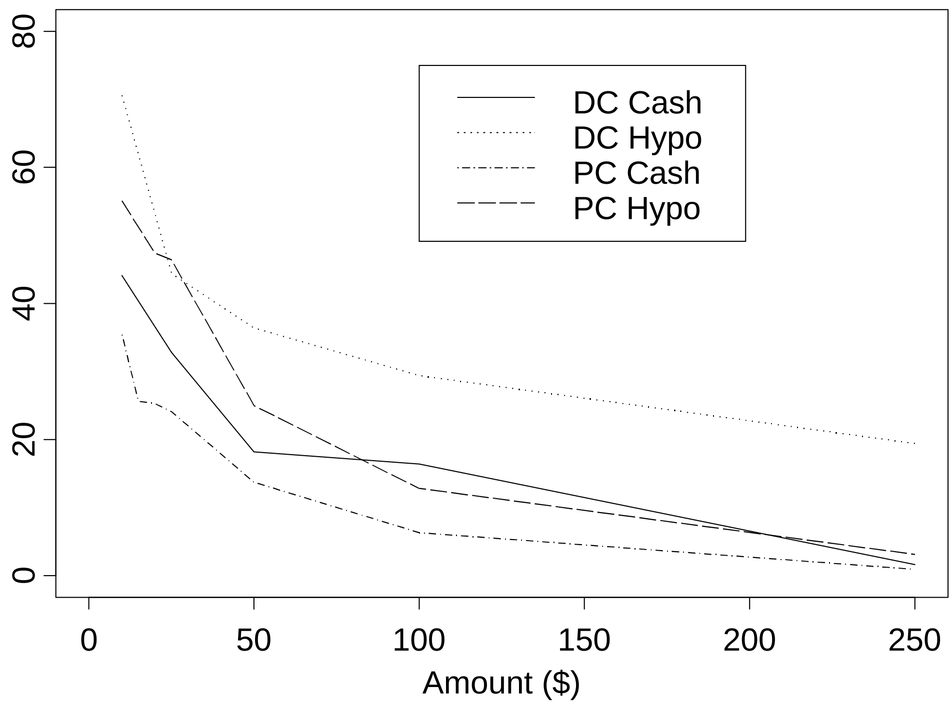
All administrative costs as well as the costs of this survey are being covered by other sources. These specific waters, on which Trout Unlimited is currently working to purchase water rights, are further described on the back of the cover letter.

If you were asked today, how much would you be willing to donate to the Montana Streamflow Fund to help purchase water rights for instream flows on these streams? **(Please check one.)**

☐ $10 ☐ $25 ☐ $50 ☐ $100 ☐ $250 ☐ $______Other

☐ $0, I would choose not to make a donation at this time

Figure 2. Plot of percent "yes" for dichotomous choice, and percent indicating that amount or greater for payment card.



Reconsidering the Statistical Gains from Dichotomous Choice Contingent Valuation with Follow-Up Questions

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Abstract:
The contingent valuation method elicits statements of willingness to pay for changes in the resource allocation of non-market goods such as recreation, wildlife and environmental quality. Since the work of Carson, Hanemann and Mitchell; Hanemann, Loomis and Kanninen; and Cameron and Quiggin, many, if not most, contingent valuation practitioners have operated under the premise that the dichotomous choice with follow-up question format offers significant statistical gains over a single dichotomous choice question. In an attempt to fill a gap in the dichotomous choice contingent valuation literature, we inadvertently discovered that, in many cases, the significant efficiency gains of the dichotomous choice with follow-up question format simply do not exist. We therefore call into question the continued use of the dichotomous-choice with follow-up format.

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As the contingent valuation method gained popularity, incentive incompatible open-ended questions of the form 'How much are you willing to pay,' were quickly dismissed in favor of potentially incentive-compatible dichotomous choice questions of the form 'Would you be willing to pay $5?' Bishop and Heberlein demonstrated that dichotomous choice questions can provide unbiased estimates of willingness to pay. Because dichotomous choice questions are easy for respondents to answer, more market-like than open-ended questions and potentially incentive compatible (Loomis; Hoehn and Randall) they are the presumptive choice for contingent valuation applications.

Despite the behavioral advantages, dichotomous choice contingent valuation responses provide much less information about each respondent's willingness to pay than open-ended responses resulting in decreased efficiency (higher variance) in the estimates of expected willingness to pay. In an effort to extract more information from each respondent, Carson, Hanemann and Mitchell introduced the dichotomous choice with 'follow-up question'. In the dichotomous choice with follow-up protocol, respondents receive an immediate follow-up question to an initial dichotomous choice question, with a higher or lower bid depending on the response to the initial question. Hanemann, Loomis and Kanninen showed that offering the follow-up question significantly increases the statistical efficiency of willingness to pay estimates.

Hanemann, Loomis and Kanninen assumed that a respondent's responses to the initial and follow-up questions originate from an underlying willingness to pay distribution that is the same across both questions. Numerous studies have found that willingness to pay distributions and estimates from the follow-up question are substantially different from the estimates from the first question only (McFadden and Leonard; Cameron and Quiggin; Kanninen; Herriges and

Shogren; Alberini, Kaninnen and Carson; Burton et al). These studies found significant bias and heteroskedasticity in estimated willingness to pay and non-perfect correlation across the two dichotomous responses thereby raising serious doubts that each individual's responses to multiple questions come from a single distribution.

To allow for the possibility of different distributions of willingness to pay across the initial and follow-up question, Cameron and Quiggin proposed the use of a bivariate probit model. They assume that the two distributions are correlated but not necessarily identical as implied by the model of Hanemann, Loomis and Kanninen. Cameron and Quiggin argue that failure to allow for non-perfect correlation may result in biased estimates of mean willingness to pay from the Hanemann, Loomis and Kanninen interval-data model. Alberini compared the performance of the *bivariate probit model* and *interval-data model* using Monte-Carlo simulations and showed that the estimates of mean or median willingness to pay from the interval-data model can be very robust to departures from perfect correlation.

Rationale

Here, we intend to fill a gap in the literature on the statistical properties of dichotomous choice with follow-up questions. In doing so, we call into question the future use of such questions. The natural progression of the dichotomous choice contingent valuation literature led researchers to overlook an important comparison between models. The original intent of the dichotomous choice with follow-up question format was to increase the efficiency of estimates of willingness to pay over that achieved by a single dichotomous choice question (Hanemann, Kanninen and Loomis). The interval-data model achieved this goal. The bivariate probit model relaxed some of the restrictive assumptions of the interval-data model and proved to alleviate the potential bias caused by those assumptions. As we will show, relaxing the restrictive assumptions of the interval-data model introduces the possibility that the follow-up question may decrease the statistical efficiency of estimated willingness to pay when compared to an estimate of willingness to pay from the initial dichotomous choice question alone. This comparison is missing in the literature.

It is possible that estimation techniques using follow-up information such as the bivariate probit model and interval-data model will yield more efficient estimates than a single bounded model, because gains in statistical efficiency arise from the series of willingness to pay questions that allow the researcher to bracket respondents' willingness to pay between two of the monetary bid amounts. But, if the correlation between the first and second responses deviates from one, the efficiency gains afforded by the second question diminish rapidly and in some circumstances become negative.

The Set-up

Consider a simple model for dichotomous choice with follow-up responses proposed by Cameron and Quiggin and Alberini. Define the underlying true willingness to pay for individual i for the first response as: $WTP_{i1}^{*} = \mu_1 + \varepsilon_{i1}$, where μ_1 is the deterministic part of willingness to pay for the first response and ε_{i1} is a normally distributed error term with mean zero and constant variance σ_1^2. Similarly, true underlying willingness to pay for the second response is: $WTP_{i2}^{*} = \mu_2 + \varepsilon_{i2}$. In the typically applied form, the error terms ε_{i1} and ε_{i2} are assumed to be bivariate normally distributed with correlation ρ: $(\varepsilon_{i1}, \varepsilon_{i2}) \sim BVN(0,0,\sigma_1,\sigma_2,\rho)$.

In the dichotomous choice with follow-up format, respondents are randomly offered a payment, b_{1i}, to either accept or reject. The respondent accepts the bid if $WTP_{1i}^{*} \geq b_{1i}$ and rejects the bid otherwise. If the respondent accepts the first bid she receives a second bid, b_{2i}, higher than b_{1i}. The second bid is lower than b_{1i} if the first bid is rejected. The respondent accepts the second bid if $WTP_{2i}^{*} \geq b_{2i}$ and rejects it otherwise. The interval-data model of Hanemann, Loomis and Kanninen arises if $WTP_{i1}^{*} = WTP_{i2}^{*}$, implying equal means, equal variances and perfect correlation—i.e. identical error terms—for WTP_{i1}^{*} and WTP_{i2}^{*}.

Cameron and Quiggin's and Alberini's models arise from relaxing one or more of these assumptions. In the most general form, the bivariate probit model allows for the estimation of different means, different variances and non-perfect correlation between the two distributions[1].

Simulations

To examine both the statistical efficiency and potential bias from dichotomous choice responses with and without follow-up questions, we begin with a series of simulations. For the simplest case we assume the true data generating model is a bivariate probit model with identical means and identical variances but non-perfect correlation across the first and second responses. This data generating process is based on the results reported in Cameron and Quiggin. They tested the hypothesis of both identical means ($\mu_1 = \mu_2$) and identical variances ($\sigma_1 = \sigma_2$) from their dichotomous choice with follow-up data, and could not reject it.

We draw pairs of willingness to pays from a bivariate normal distribution with means of \$250, standard deviations of \$70, and correlation of .5. We vary the sample size across

[1] Haab and McConnell provide an overview of the estimation of these models.

simulations with n= (200, 400, 600, 800 and 1000). The model specification can be summarized as follows:

$$\begin{cases} WTP_{1j} = \mu + \varepsilon_{1j} \\ WTP_{2j} = \mu + \varepsilon_{2j} \end{cases} \text{ where } \mu = 250 \text{ and } (\varepsilon_{1j}, \varepsilon_{2j}) \sim BVN(0,\, 0,\, 70^2, 70^2, 0.5)$$ [2]

To simulate responses we randomly draw a bid from a predetermined set of bid values[3] (150, 200, 250, 300 and 350) and compare each drawn bid to the generated first willingness to pay ($= WTP_{1j}$). A 'yes' is recorded when the first willingness to pay exceeds the randomly chosen bid. To simulate the follow-up response, we double the first bid for an initial 'yes' responses and halve the first bid for an initial 'no' responses. We then compare the doubled (halved) bids with the generated second willingness to pay ($= \mathrm{WTP}_{2j}$) yielding the follow-up dichotomous response (yes or no).

Based on each generated response set, we estimate a single-bounded probit model using the first response only, an unrestricted bivariate probit model using both responses[4] and an interval-data model restricting the mean and variance to be equal and imposing perfect error correlation. The bivariate probit model provides two means and two variances so that the researcher must decide which estimates to use to report willingness to pay. Throughout this paper, we report the first mean and variance allowing that the second mean and variance are potentially distorted by the offered first bid and are of less interest.

Table I summarizes the estimation results for the first simulation.

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[2] To generate bivariate normal distribution of error terms, we utilize following formulation: First, we generate independent two univariate normal distributions having means zero and variances one such as $z_1 \sim N(0,1)$ and $z_2 \sim N(0,1)$. And, second we set $\varepsilon_{1j} = \sigma_1 * z_1 = N(0, \sigma_1^2)$ and $\varepsilon_{2j} = \sigma_2 * (\rho * z_1 + \sqrt{1-\rho^2} * z_2)$, which will jointly provide $(\varepsilon_{1j}, \varepsilon_{2j}) \sim BVN(0, 0, \sigma_1^2, \sigma_2^2, \rho)$ where ρ is correlation coefficient.

[3] These bid values are selected around 250 uniformly.

[4] We choose to use an unrestricted bivariate probit model as our naïve model. One might be tempted to use a restricted bivariate probit model with equal means and equal variances, but the literature is unclear as to whether this assumption is warranted a priori.

TABLE I

Comparison of estimates when $\mu_1 = \mu_2 = 250, \ \sigma_1 = \sigma_2 = 70, \ \rho = 0.5$

Sample Sizes	Parameters	Single bounded	Dichotomous Choice with Follow-up: Bivariate probit	Dichotomous Choice with Follow-up: Interval data
N=200	$\hat{\mu}_1$ (st_d)*	253.25 (6.88)	253.71 (6.74)	255.05 (5.87)
	$\hat{\sigma}_1$ (st_d)	63.79 (7.46)	62.46 (7.06)	66.50 (4.73)
N=400	$\hat{\mu}_1$ (st_d)	253.41 (4.79)	253.15 (4.79)	253.57 (4.21)
	$\hat{\sigma}_1$ (st_d)	62.04 (5.05)	62.33 (4.95)	67.65 (3.43)
N=600	$\hat{\mu}_1$ (st_d)	251.60 (4.20)	251.56 (4.20)	249.45 (3.39)
	$\hat{\sigma}_1$ (st_d)	69.26 (4.76)	69.32 (4.78)	72.23 (2.93)
N=800	$\hat{\mu}_1$ (st_d)	244.08 (3.42)	244.12 (3.42)	247.78 (2.88)
	$\hat{\sigma}_1$ (st_d)	63.19 (3.68)	63.42 (3.63)	65.66 (2.31)
N=1000	$\hat{\mu}_1$ (st_d)	247.27 (3.23)	247.28 (3.23)	251.07 (2.62)
	$\hat{\sigma}_1$ (st_d)	68.62 (3.66)	68.75 (3.63)	67.00 (2.11)

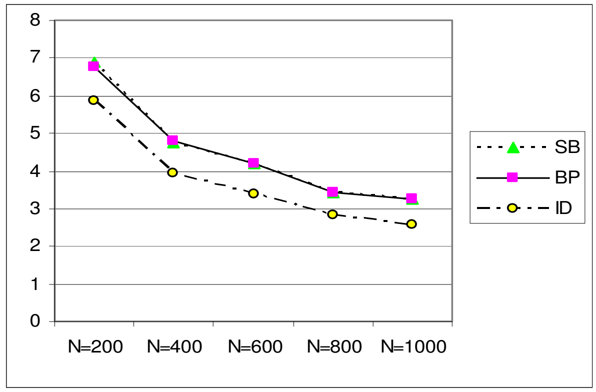
* Standard deviation reported in parenthesis

As expected, the estimated means ($\hat{\mu}_1$) and variances ($\hat{\sigma}_1$) are close to their true values ($\mu_1 = 250, \ \sigma_1 = 70$) for all models, consistent with other findings in the literature (Cameron and Quiggin, Alberini). The interval-data model shows the smallest estimated standard deviation for mean willingness to pay despite the model being misspecified. By assuming perfect correlation, the interval-data model claims unwarranted efficiency gains. Perhaps more surprising, when nonperfect correlation is allowed in estimation the estimated standard deviations of mean willingness to pay from the single bounded model and from the bivariate probit model are almost identical for all but the smallest sample.

Figure 1 plots the standard deviations of the estimated mean willingness to pay from the single bounded model, interval-data model and bivariate probit model as the sample size increases (200→400→600→800→1000). The dotted line from single bounded model almost coincides with the line from bivariate probit model for all sample sizes. In contrast, the line from the interval-data model is always lower.

FIGURE I

Movements of standard deviation of the estimated mean willingness to pay with different sample size[5]



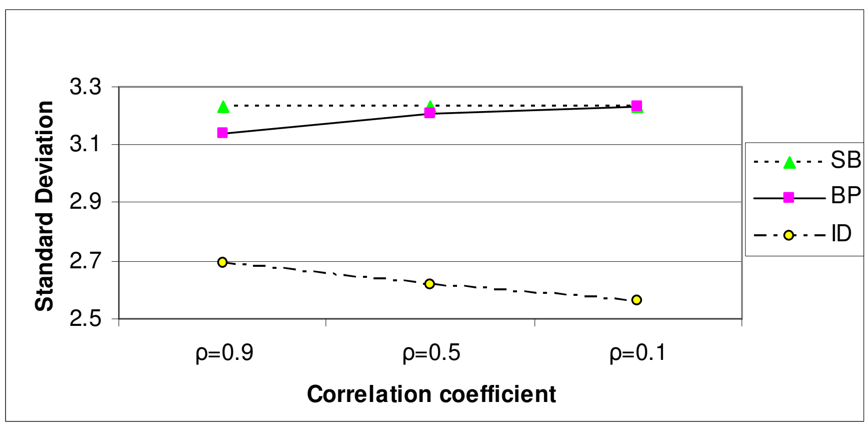
Although the defense of a follow-up question has typically been efficiency gains, the results from our first simulation indicate that the efficiency gains from introducing the follow-up question may be negligible when we allow for flexibility in the model specification. Meanwhile, it is confirmed that if we ignore imperfect correlation, we can falsely increase efficiency by employing the interval-data model.

Haab and McConnell describe three reasons for efficiency gains from a follow-up question. First, the answer sequences of yes-no or no-yes put tighter bounds on willingness to pay. Second, the yes-yes pairs and the no-no pairs, even though they do not completely bound willingness to pay, constrain the part of the distribution where the respondent's willingness to pay can lie. Finally, the number of responses effectively doubles—two responses per person--so that a given function is fitted with more observations. However, our initial results show that the above explanations might not be true for the dichotomous choice with follow-up questions with non-perfect correlation. Presumably, non-perfect correlation may blur the bound on willingness to pay for yes-no or no-yes answer sequence, and may widen the bound for yes-yes or no-no answer sequence and finally, does not effectively double the sample size.

[5] SB: Single bounded model, BP: Bivariate probit model, and ID: Interval data mode

To verify our thinking on the relationship between non-perfect correlation and efficiency gains from a follow-up question, we conduct another simulation. In this simulation, we fix the sample size at 1000 but vary the correlation between 0.9 to 0.1. The other parameter values are the same as in the first simulation. Figure II shows the change in the standard deviation of estimated mean willingness to pay from the three different models as the true correlation coefficient varies.

FIGURE II
The relationship between correlation coefficient and efficiency gain from follow-up data



The standard deviation of estimated mean willingness to pay from the bivariate probit model is lower than that from single bounded model when the correlation coefficient is close to one ($\rho = 0.9$), but the two converge as the correlation coefficient deviates further from one ($\rho = 0.1$). This is not too surprising since a correlation coefficient of zero corresponds to independent probits on the first and second responses. The more interesting result is that the standard deviation of estimated mean willingness to pay from the interval-data model becomes smaller as the correlation coefficient departs from one. When estimating an interval-data model, we automatically assume that the correlation coefficient is one. By placing an incorrect restriction on the model, we increase the perceived efficiency gain when the more general model is losing efficiency.

Other Data Generating Processes

Numerous studies show that the assumption of equal mean and variance need not hold in dichotomous choice with follow-up responses. In this section, we compare the same three models when the true data generating process has different means or different variances and non-perfect correlation. We examine two stylized cases: a downward shift in the second mean willingness to pay ($\mu_1 > \mu_2$ and $\sigma_1 = \sigma_2$) and higher uncertainty in the second responses ($\mu_1 = \mu_2$ and $\sigma_1 < \sigma_2$). Both anomalies are frequently observed. The first case mimics anchoring where the offering of the initial bid shifts the willingness to pay distribution for the second response to the left. Cameron and Quiggin, Harrison and Kristöm, Herriges and Shogren, Alberini, Kaninnen and Carson, McLeod and Bergland, and Burton *et al* all show evidence of downward bias in the estimated willingness to pay from the second response. The second case mimics potential confusion introduced by the follow-up question. Cameron and Quiggin find that the variance of the second willingness to pay estimate is greater than that of the first willingness to pay estimate. Alberini, Kaninnen and Carson argue that excessively high or low follow-up bids can increase the variance of the second willingness to pay.

For the first case, we generate data on willingness to pay from a bivariate normal distribution with different means, identical variances and non-perfect correlation: $BVN(250,\ 150, 70^2, 70^2, 0.5)$ for three different sample sizes (N= 400, 600 and 1000). Dichotomous responses are simulated in the same way as in the previous section.

TABLE II

Comparison of Estimates among three models when $\mu_1 = 250,\ \mu_2 = 150,\ \sigma_1 = \sigma_2 = \sigma^* = 70,\ \rho = 0.5$

Sample size	Parameters	Single bounded	Dichotomous choice with follow-up	
			Bivariate probit	Interval data
N=400	$\hat{\mu}_1$ (st_d)	253.42 (4.81)	253.41 (4.80)	214.0[6] (5.44)
	$\hat{\sigma}_1$ (st_d)	62.0 (5.01)	62.0 (5.00)	94.8 (5.31)
	$\lvert \sigma^* - \hat{\sigma} \rvert$	8.00	8.00	24.87
N=600	$\hat{\mu}_1$ (st_d)	251.58 (4.21)	251.61 (4.22)	211.5(4.42)
	$\hat{\sigma}_1$ (st_d)	69.3 (4.81)	69.3 (4.79)	95.7 (4.34)
	$\lvert \sigma^* - \hat{\sigma} \rvert$	0.75	0.75	25.77
N=1000	$\hat{\mu}_1$ (st_d)	247.28 (3.21)	247.33 (3.18)	207.5 (3.51)
	$\hat{\sigma}_1$ (st_d)	68.6 (3.71)	68.6 (3.70)	96.7 (3.43)
	$\lvert \sigma^* - \hat{\sigma} \rvert$	1.38	1.38	26.66

Table II summarizes the results. Once again, the estimates from the bivariate probit model are almost identical to those from single bounded model for all sample sizes implying that we may get unbiased mean estimates from the bivariate probit model but we get no additional statistical efficiency beyond what we get from a single response. Of particular importance, the estimates of the interval-data model are no more efficient than the single-bounded model or the bivariate probit model. In fact, the estimated standard deviation of mean willingness to pay from interval-data model is always greater than that from the other two models for all sample sizes.

The efficiency loss from the interval-data model becomes meaningless when we note, predictably, that the estimated means willingness to pay from interval-data model are all shifted downward relative to the true value. More seriously, we note another bias in the estimated dispersion parameter ($\hat{\sigma}$) from the interval-data model. As Alberini pointed out, this sort of bias is prominent for large departures of the correlation coefficient from one even though the true model is assumed to have identical means and variances.

[6] It is obvious that the mean willingness to pay from interval-data model is more likely to be downward biased because this model purportedly restricts two mean willingness to pays to be identical although the true model has lower mean in the second response

To show this we calculate the absolute differences between the true dispersion parameter and estimated dispersion parameters $\Delta = |\sigma^* - \hat{\sigma}|$. Since the true model assumes identical variances ($\sigma_1^2 = \sigma_2^2 = \sigma^{*2}$), we would expect the estimated dispersion parameter ($\hat{\sigma}$) from each model to be close to the true value (σ^*), that is, Δ should be close to zero. As can be seen in Table II, $\Delta = |\sigma^* - \hat{\sigma}|$ from the single bounded model and the bivariate probit model approach zero as the sample size increases. In contrast, those from interval-data model significantly differ from zero and do not decrease as the sample size increases (24.87 (n=400)→25.77 (n=600)→26.66 (n=1000)). Compared to the previous simulation with identical means and variances (see Table I), the absolute difference between the true and estimated dispersion parameters ($\Delta = |\sigma^* - \hat{\sigma}|$) in Table II becomes larger for the interval-data model. Consequently, we think the discrepancy in mean willingness to pay between responses amplifies the bias in the estimated dispersion parameters in the presence of non-perfect correlation.

Finally, we generate dichotomous response data to simulate the case where there is significantly greater uncertainty in the second-round responses. We draw the true willingness to pay from a bivariate normal distribution with equal means but different variances and nonperfect correlation: $BVN(250, 250, 70^2, 120^2, 0.5)$ for three different sample sizes (N= 400, 600, 1000). Table III summarizes the estimation results.

We confirm that the statistical gain of the bivariate probit model is negligible. Also, the estimated dispersion parameters ($\hat{\sigma}_1$) from the interval-data model appear to shift upward. Once again, we find that the estimated standard deviation of mean willingness to pay from the interval-data model is always greater than that from the single bounded model or bivariate probit model.

Even when the interval-data model provides accurate estimates of mean willingness to pay, we can lose efficiency when there is higher uncertainty in the second responses.

TABLE III

Comparison of Estimates among three models when $\mu_1 = \mu_2 = 250, \quad \sigma_1 = 70, \quad \sigma_2 = 120, \ \rho = 0.5$

Sample size	Parameter	Single bounded	Dichotomous Choice with follow-up	
			Bivariate probit	Interval data
N=400	$\hat{\mu}_1$ (St_d)	253.4 (4.8)	253.5 (4.8)	258.0 (5.3)
	$\hat{\sigma}_1$ (St_d)	62.0 (5.0)	62.2 (5.0)	91.3 (4.5)
N=600	$\hat{\mu}_1$ (St_d)	251.6 (4.2)	251.5 (4.2)	252.3 (4.3)
	$\hat{\sigma}_1$ (St_d)	69.3 (4.8)	69.0 (4.8)	92.6 (3.8)
N=1000	$\hat{\mu}_1$ (St_d)	247.3 (3.2)	247.3 (3.2)	250.4 (3.3)
	$\hat{\sigma}_1$ (St_d)	68.6 (3.7)	68.8 (3.7)	91.9 (2.9)

Applications

In the simulations we reinforced the finding that the interval-data model may not always be an appropriate alternative for single bounded model because apparent efficiency gains come only with identical means and identical variances between the first and the second willingness to pay, a questionable assumption at best. More significantly, we found no significant efficiency gains from a follow-up question relative to a single dichotomous choice question when we allow non-perfect correlation between the initial and follow-up responses. This is in stark contrast to the accepted thinking that follow-up questions add statistical information. To test these findings on actual data sets we now use two examples drawn from data sets published in the literature.

The first example is from the contingent valuation survey conducted by the Australian Resource Assessment Commission in 1990 as part of a benefit-cost analysis effort to evaluate options for the use of resource of Kakadu Conservation Zone available in Carson *et al* (1994). Only the sub-sample that administrated a moderate environmental impact scenario ("minor impact") is used. The second example is data from the contingent valuation survey conducted in 1992 to measure the loss of passive use benefits caused by the 1989 Exxon Valdez oil spill in Prince William Sound, Alaska available in Carson *et al* (1992). Table IV presents descriptive statistics for the Kakadu and Alaska data. The total number and percentage of respondents who stated that they would be willing to pay for the project at each bid level is reported.

TABLE IV
Response summary for dichotomous choice contingent valuation with follow-up question

Kakadu contingent valuation study (N=1013)

First bid (Second bid)	Total	Y	N	YY	YN	NY	NN
5(20,2)	253	167	86	150 (59.3%)	17 (6.7%)	7 (2.8%)	79 (31.2%)
20(50,5)	252	156	96	136 (54.0%)	20 (7.9%)	11 (4.4%)	85 (33.7%)
50(100,20)	255	145	108	124 (48.6%)	23 (9.0%)	15 (5.9%)	93 (35.5%)
100(250,50)	253	136	117	105 (41.5%)	31 (12.3%)	17 (6.7%)	100 (39.5%)

Alaska contingent valuation study (N=1043)

First bid (Second bid)	Total	Y	N	YY	YN	NY	NN
10(5,30)	264	179	85	118 (44.7%)	61 (23.1%)	7 (2.7%)	78 (29.5%)
30(10,60)	267	138	129	69 (25.8%)	69 (25.8%)	31 (11.6%)	98 (36.7%)
60(30,120)	255	129	126	54 (21.2%)	75 (29.4%)	25 (9.8%)	101 (39.6%)
120(60,250)	257	88	169	35 (13.6%)	53 (20.6%)	30 (11.7%)	139 (54.0%)

Table V presents results for a single bounded model, bivariate probit model and interval-data model for both data sets. According to a likelihood ratio (LR) test, the most appropriate model for the Kakadu data[7] is a bivariate probit model with identical mean and identical variances and non-perfect perfect correlation across two dichotomous responses (as reported in Cameron and Quiggin). Because the correlation coefficient is very close to one and the means and variances are not statistically different, the interval-data model shows a significant efficiency gain and very little bias in the mean estimate relative to the more general bivariate probit. Both models show an efficiency gain relative to the single-bounded model.

[7] LR statistic for identical means across two responses is 0.45 <3.84, 95% significance level with 1 d.f.; for identical variances across two responses is 2.12<3.84; finally LR statistic for both identical means and identical variances across two responses is 5.92<5.99, 95% significance level with 2 d.f.

TABLE V
Summary of estimation of selected three surveys from three different models

	Single bounded model	Bivariate probit model	Interval data model
Kakadu Conservation Zone contingent valuation survey			
$\hat{\mu}_1$	123.16 (30.16)*	128.77 (27.60)	115.42 (10.85)
$\hat{\mu}_2$	-	146.06 (24.95)	-
$\hat{\sigma}_1$	317.14(110.37)	339.51 (96.55)	273.60 (19.59)
$\hat{\sigma}_2$	-	510.63 (107.77)	-
$\hat{\rho}$	-	0.95 (0.01)	-
Log L	-678.35	-1080.86	-1114.29
Exxon Valdez contingent valuation Survey			
$\hat{\mu}_1$	58.66 (5.71)	58.72 (5.83)	45.20 (3.88)
$\hat{\mu}_2$	-	-22.96(18.56)	-
$\hat{\sigma}_1$	144.07 (19.97)	146.87 (19.93)	108.78 (4.62)
$\hat{\sigma}_2$	-	249.90(40.55)	-
$\hat{\rho}$	-	0.69 (0.04)	-
Log L	-696.06	-1299.72	-1393.83

*standard deviation reported in parenthesis

In contrast, for the Exxon Valdez data, while the estimates from both the bivariate probit and single bounded model are similar, the standard deviation of the first mean willingness to pay from the bivariate probit model is greater than that from the single bounded model (5.83 from bivariate probit >5.71 from single bounded model). We lose efficiency by adopting the dichotomous choice with follow-up survey when there is apparent discrepancy in means or variances across the two responses. As for the interval-data model, since the mean estimates clearly differ from the corresponding estimates in the more general models, the apparent efficiency gain is meaningless.

Conclusions

Dichotomous choice with follow-up survey designs were developed to improve the accuracy of willingness-to-pay estimates relative to one-shot dichotomous choice questions. By eliciting more information on individual's willingness to pay, researchers were thought to narrow the range and precision of estimates, making dichotomous choice contingent valuation more useful for benefit-cost analysis and policy.

Using both simulated and actual data sets we examine both statistical efficiency and bias from a dichotomous choice data with and without a follow-up question. We find that standard interval-data models improve efficiency only if both the first and the second responses have the same means and variances regardless of the correlation coefficient. Otherwise, it is found that the estimates from interval-data model data are potentially biased and less efficient than even a single choice question. In addition, we find that the bivariate probit model does not always perform favorably in terms of efficiency gain when compared to single bounded models. The presence of non-perfect correlation between the first and second responses virtually eliminates any efficiency gains from the follow-up question and in some cases actually causes efficiency losses relative to a simple single dichotomous choice question.

We therefore have serious reservations about the continued use of the dichotomous-choice with follow-up format. We do not view this as an overly negative conclusion. Since the follow-up question offers little in the way of statistical gain, dismissal of such questions frees the researcher to concentrate on the behavioral properties of the single dichotomous choice question without the additional burden of more complicated econometric models.

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Measurement, Generalization, and Publication: Sources of Error in Benefit Transfers and Their Management

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Abstract:

Convergent validity tests of benefit transfer accuracy show errors to range from a few percentage points to 100% and more. This paper discusses three potential sources of errors that affect the accuracy of benefit transfers. (1) The measurement of values is subject to random errors and the caprices that arise from the many judgments and technical assumptions required by the researchers who conduct the primary studies. *Measurement error* occurs when researchers' decisions affect the transferability of measures of value or as the result of sampling. (2) *Generalization error* occurs when a measure of value is generalized to unstudied sites or resources. Generalization error is inversely related to the correspondence between study sites and policy sites. And, (3) *publication selection bias* occurs when the objectives for publishing research limit benefit transfer applications of research outcomes. Criteria for selecting which research results are published may be at odds with the needs of benefit transfer practitioners. Several means for overcoming these sources of error are offered: standardized application of tested methods; closer adherence to benefit transfer protocol; the establishment of an e-journal with explicit criteria for fully recording, reporting, and disseminating research, which has the primary objective of estimating empirical measures of value.

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Introduction

There are two primary sources for resource values – primary research and benefit transfers. Benefit transfer is the “application of values and other information from a ‘study’ site where data are collected to a ‘policy’ site with little or no data” (Rosenberger and Loomis, 2000: 1097). The evolution of benefit transfers began with the transfer of unadjusted individual or aggregate point estimates of value. However, Loomis (1992) argues that transferring the entire demand or benefit function increases the validity and reliability of the transfer. By transferring the demand function the practitioner can make needed adjustments to value estimates based upon specific characteristics of the policy site.

More recently, meta-regression analysis (MRA) has been used to combine and integrate an entire body of empirical evidence relevant to environmental values (Rosenberger and Loomis, 2001; 2003). Meta-regression analysis may further isolate and measure the relationships among the estimates of value and various moderator variables representing research design and site characteristics in the form of a statistical function (Stanley and Jarrell, 1989). This statistical function, the MRA model, becomes the link between the knowledge derived from applied research and its application to policy settings.

Meta-analysis is the statistical analysis of research outcomes from previous studies; i.e., it is the analysis of analyses (Glass, 1976). Meta-analyses can serve three purposes: research synthesis, hypothesis testing, and benefit transfer (Smith and Pattanayak, 2002). Meta-analysis has been widely used in the medical and social sciences, but its application to economics has been more recent. For benefit transfer, meta-regression analyses assume that there exists an underlying meta-valuation function that relates the magnitude of empirical estimates of value to characteristics of the study site, market, and research methods (Rosenberger and Phipps, 2002, 2006; Woodward and Wui, 2001). Primary research, within its context, defines relationships between characteristics and values; i.e., part of the underlying meta-valuation function. Meta-regression analysis combines these and other research characteristics that are reported in the literature to construct the entire function. Variability across estimated parameters or values from primary research studies may be due to differences in context (i.e., movements along the function) and/or errors in their estimation (i.e., deviations from the function). Several meta-regression analyses have been conducted in environmental and natural resource economics (Bateman and Jones, 2003), beginning with the evaluation of recreation benefits (Smith and Kaoru, 1990a; Walsh et al., 1990) and price elasticities of recreation demand (Smith and Kaoru, 1990b), and more recently the evaluation of woodland recreation values (Bateman and Jones, 2003) and surface water quality values (Johnston et al., 2003).

Many studies have expressed concern about the accuracy of benefit transfers and have attempted to measure the error involved in both value and function transfers (Table 1). These evaluations focus on the difference between the 'actual' value for a given policy site and a transferred value to this policy site.[1] This 'actual' value of the policy site is but an estimate derived from an original study conducted specifically for this site. Factors that may affect the accuracy of benefit transfers include the quality of the study site data, the methods used in modeling and interpreting the study site data, analysts' judgments regarding research design and implementation and the closeness between the study site and its policy target (Bergland et al., 1995; Boyle and Bergstrom, 1992; Brouwer, 2000; Desvousges et al., 1992). Close correspondence seems to be a necessary, but not a sufficient criterion for accurate benefit transfers (Brouwer and Spanninks, 1999; Chattopadhyay, 2003). In fact, even if the process of benefit transfer were without error, the transferred value would be expected to differ from the actual value by the square root of the sum of the estimation variances of these two sites. Rosenberger and Loomis (2001; 2003) offer strategies that cope with the imperfections of benefit transfers.

[1] All of the percent errors reported in Table 1 are derived from primary research of that site using the calculation of the percent difference between a transfer value (V_T) and the actual value for the policy site (V_A). The formula is: $[(V_T-V_A)/V_A] * 100$. What defines an acceptable level of error depends on the context of the transfer application and the judgments of practitioners and users of the information. However, as noted by an anonymous reviewer of this paper, other statistical tests of transfer accuracy might prove useful. For example, the tests in Table 1 assume the null hypothesis to be $V_T = V_A$. Kristofersson and Navrud (2005) suggest an equivalency test as an alternative to this conventional null hypothesis of equality. An advantage of their test is that an acceptable level of transfer error may be specified prior to conducting the validity test.

This paper discusses three possible sources of errors that affect the accuracy of benefit transfers: (1) generalization error; (2) measurement error; and (3) publication selection bias. Generalization error arises from the benefit transfer application itself. Measurement error is endogenous to primary research and can only be weakly controlled by the benefit transfer analyst. Publication selection bias arises in a body of knowledge (the literature) if selection criteria favor statistically significant results. Publication selection bias can skew the stock of knowledge from which benefit transfer analysts draw information. A means for overcoming these sources of errors is offered; namely an e-journal. The primary objective of this internet journal is to widely disseminate all economic estimates of value, 'publishable' or not.

Generalization error

Generalization errors arise when estimates from study sites are adapted to represent different policy sites. These errors are inversely related to the degree of correspondence between the study site and the policy site. Benefit transfer assumes that there is an underlying meta-valuation function that links the values of a resource (such as wetlands) or an activity (such as downhill skiing or camping) to the characteristics of markets and sites, across space and over time. If we view study sites as a sample from this meta-function, the meta-valuation function becomes an envelope of a set of study site functions that relates site values to characteristics or attributes associated with each site, including market characteristics, physical site characteristics, spatial characteristics, and time (Rosenberger and Phipps, 2002; 2006). Although the degree that any of these sets of factors affects benefit transfer accuracy is an empirical question, one might suppose that the greater the correspondence (or similarity) of the policy site with the study site, the smaller the expected generalization error (Boyle and Bergstrom, 1992; Desvousges et al., 1992).

Several of the studies listed in Table 1 support the hypothesis that the greater the correspondence, or similarity, between the study site and the policy site, the smaller the expected error in benefit transfers. Lower transfer errors resulted from in-state transfers rather than from across-state transfers (Loomis, 1992; VandenBerg et al., 2001).

Presumably, transfer from within states or political regions entail lower socio-economic, socio-political, and socio-cultural differences than across states. Loomis et al. (1995) find states in the same region have smaller transfer error than generalizing across regions. In particular, multi-site lake recreation models of Arkansas and Tennessee performed better in benefit transfers between the two states (percent errors ranging from 1% to 25% with a nonlinear least squares models and 5% to 74% with the Heckman models) than either one when transferred to California (percent errors ranged from 106% to 475% for the nonlinear least squares models and from 1% to 113% for the Heckman models). This suggests that the similarity between the southeastern models implicitly accounted for site characteristic effects. Similarly, VandenBerg et al. (2001) find better agreement within communities that have shared experiences of groundwater contamination than transferring across states, within states, or to previously unaffected communities. Piper and Martin (2001) show that transfer errors for rural water supply values are smaller when transfers occur between similar sites than dissimilar sites. In a repeated sampling test of benefit transfers, Chattopadhyay (2003) finds that benefit transfer performed poorly across randomly drawn, similar sub-groups of housing data in a hedonic property pricing study, whether transfers were of values or functions.

Researchers also find that generalization errors are reduced by transferring the full functions instead of point estimates or values (Table 1). Even in those cases where function transfers do not outperform value transfers, they, nonetheless, do no worse either (Chattopadhyay, 2003; Ready et al., 2004). Transferring functions better reflects differences between sites because such differences are explicitly accounted for (Loomis, 1992; Parsons and Kealy, 1994; Bergland et al., 1995; Kirchhoff et al., 1997 (birdwatching); Brouwer and Spaninks, 1999; VandenBerg et al., 2001 (pooled data models); Piper and Martin, 2001; Chattopadhyay, 2003). However, it appears that the gains in accuracy may be more a function of the similarity between sites than the calibration of site characteristics that function transfers permit. Nevertheless, Chattopadhyay (2003) finds that function transfers outperform value transfers under conditions where dissimilarities are forced. Often, valuation functions do not include variables measuring the physical differences between the sites or socio-economic differences between the markets because many of the physical differences important for calibrating values across sites are unmeasured in the original functions (Rosenberger and Phipps, 2006). Researchers assume such differences are captured in the price coefficient (Downing and Ozuna, 1996), or that their effect could be relegated to the error term without affecting the parameter(s) of interest. Jiang et al. (2005) show model specifications that include attitudinal information or reflect policy relevant characteristics outperformed other model specifications in predicting choices and estimating welfare measures. As in any regression analysis, the accuracy and reliability of transferring functions depends upon having a wide domain upon which to fit the transfer function, the correct specification of this function, and a high explanatory power.

Measurement error

The measurement of values necessarily entails random errors and many research judgments that can affect the results of the primary studies. In particular, the empirical estimation of a theoretical model includes decisions about which data are most relevant, which estimation strategies are least biased, about how data should be adjusted, and which assumptions to rely upon when connecting the data to the model (Hanemann, 2000). Measurement error occurs when researchers' decisions affect the accuracy of the transferability of values. Often, meta-analysis finds that the methodological choices researchers make in the analysis and estimation of values have a statistically significant effect on its findings. For example, several methodological factors have been found to be statistically significant in a previous meta-regression analysis of recreation use values, including: valuation method, elicitation method, survey design, and units of measurement (Rosenberger and Loomis, 2001). Typically these methodological factors are held constant at the mean level of their use in the literature when applied to meta-valuation functions for benefit transfers. Though necessary, such practices merely avoid potential sources of measurement error without reducing or mitigating these errors.

Limited access to information further complicates the use of meta-analytic techniques in estimating a meta-valuation function for benefit transfer purposes. Florax et al. (2002) argue that although providing incomplete or insufficient information may not be detrimental to the outcome of an original study, it compromises secondary analyses that must compare results across different studies. A comprehensive database is the foundation for quality meta-analyses in particular and benefit transfers in general. Rosenberger and Loomis (2000) show that empirical valuation studies do a poor job at recording and reporting characteristics of the sites and characteristics of the sample populations. For example, out of the 131 studies included in the Rosenberger and Loomis (2000) recreation use values database, about 3% of the studies reported average income for their samples; less than 1% reported average education level; about 16% reported gender proportions; and only 61% bother to report their sample size (Table 2). In the meta-analyses tested in Kirchhoff (1998), Rosenberger and Loomis (2000) and Shrestha and Loomis (2001), none included market characteristics for the underlying samples in the original studies. Yet as discussed above, it is the correspondence of both physical and market characteristics between the study and policy sites that confers accuracy to the benefit transfers.

It goes without saying that even the best studies will contain some error. After all, even when all the assumptions are met, estimates of value will be subject to estimation errors that are likely to be magnified through the lens of transfer. However, estimation errors can be managed. Original studies that use larger, more representative, samples and meta-analyses that have larger variations in research design reduce estimation error.

Publication selection bias

Publication selection bias means that the empirical literature is not an unbiased sample of empirical evidence. With publication selection, there is a preference for statistically significant results or for results that conform to theoretical expectations (Florax, 2002; Stanley, 2005, 2006). For example, price elasticities of water demand have been found to be exaggerated four-fold through publication selection bias (Dalhuisen et al., 2003; Stanley, 2005). Many economists, including meta-analysts, use the negative sign of an own price elasticity as a specification criterion, re-specifying the demand relation and/or re-estimating it when a positive elasticity is found. Everyone knows that own price elasticity must be negative; the ‘law’ of demand demands it. Yet ironically, when researchers use negative price elasticity as a model selection criterion, the average estimate of elasticity reported in the literature will be much too large, much too elastic. When price elasticity is over-estimated by a factor of four, the water conservation implications from a given pricing policy will be a disappointing 25% of its intended target. Publication selection bias can greatly distort a reasoned assessment of key environmental parameters.

Although publication selection bias reduces the validity and reliability of meta-regression analyses for benefit transfer, these biases are equally problematic to any summary of empirical research (Laird and Mosteller, 1988; Sutton et al., 2000; Stanley, 2001, 2005). Thus, it is not the process of meta-analysis that is the source of these biases, but rather the research publication-dissemination system itself. In fact, meta-analysis provides the only defensible methods for detecting and correcting these biases (Stanley, 2005, 2006).

Medical researchers and many areas of social science have long recognized the seriousness of publication selection (Sterling, 1959; Rosenthal, 1979; Begg and Berlin, 1988), and more recently, economists have uncovered publication bias in many areas of economic research with the help of meta-regression analysis (Card and Krueger, 1995; Ashenfelter et al., 1999; Gorg and Strobl, 2001; Doucouliagos and Laroche, 2003; Abreu et al., 2005; Doucouliagos, 2005; Nijkamp and Poot, 2005; Rose and Stanley, 2005; Stanley, 2005) . Card and Kreuger (1995: 239) identify three sources of publication selection in economics: (1) reviewers and editors may be predisposed to accept papers consistent with the conventional view; (2) researchers may use the presence of conventionally expected results as a model selection test; and (3) everyone may possess a predisposition to treat ‘statistically significant’ results more favorably.

In the area of non-market valuation, publication selection is more a matter of methodological innovation than statistical significance. Most journals in the environmental economics field are not interested in new estimates of benefits for their own sake (Smith and Pattanayak, 2002: 273). Thus, the accuracy of the reported estimates may be less than ideal. When measurement error and publication selection bias are working in the same direction, an empirical literature can become quite skewed. And, the additional layer of approximation and error required for benefit transfer may easily cause noise to dominate signal.

Several recent economic meta-regression analyses explicitly model the publication venue of the environmental research studies (Table 3). Smith and Huang (1995) (air quality), Woodward and Wui (2001) (wetland values), Dalhuisen et al. (2003) (residential water demand elasticities), Zelmer (2003) (voluntary contributions for public goods) and van Kooten et al. (2004) (costs of carbon sequestration in forests) have all employed a dummy variable that identifies the publication source (i.e., journal article or peer-reviewed) as a moderator variable in their meta-regression models and as a proxy for publication bias. Woodward and Wui (2001) and Zelmer (2003) did not find a significant effect from publication source, but Smith and Huang (1995) did. van Kooten et al. (2004) found peer-reviewed studies reported higher cost estimates than non-peer-reviewed studies.

Many peer-reviewed journals and dissertations have the explicit objective of making a methodological contribution, not provide a new estimate of value. When improved methods are the objectives of research, their success will be judged less on the magnitude or the statistical significance of their reported estimates of value. For example, Gallett and List (2003) (elasticities of cigarette demand) included a dummy variable identifying publication in the top 36 economics journals. This measure of journal prestige was significant and negative in the price elasticity model and significant and positive in the income elasticity model. Both of these directional effects imply that reported demand elasticities are larger (more elastic) in the most prestigious economics journals than other outlets for publishing environmental. This is precisely the sort of effects that are the expected result of publication bias. Publication pressures cause researchers and/or reviewers to use theoretical expectations to select among submitted results. The higher the prestige of the journal, the greater this selection bias will often be. However, conventional claims to the contrary, Dalhuisen et al. (2003) found larger elasticity estimates in the unpublished literature of residential water demand.

Preliminary indicators of publication selection bias are found in an existing database of recreation use values (Rosenberger and Loomis, 2001). They find a significant and negative effect on use value estimates when a dummy variable identifying those estimates published in peer-reviewed journals is added to the meta-regression model. Split-sample t-tests also show that not only do journal publications have a smaller aggregate mean estimate than non-journal publications, but there is also greater variation in estimates provided across published studies. Furthermore, split-sample t-tests show documents that make methodological contributions have a smaller aggregate mean estimate than documents providing new estimates of value. Thus, it appears that a concern about publication selection is justified. Thus far, researchers have found consistent directional effects in the environmental valuation literature: benefit estimates are smaller and cost estimates are larger in the peer-reviewed, published literature (see Table 3).

Measurement error and publication selection need not be mutually exclusive sources of error in benefit transfers. Measurement error may masquerade as publication bias. For example, researchers' choices regarding methodology can be influenced through the peer-review process. When the objective is the publication of a paper compromises will be made in model specifications and estimation techniques. Likewise, statistical issues with a database may also result in publication bias. In fact, selection from these 'random' misspecification biases is the primary source of publication bias.

Conclusions

Evidence of generalization error, measurement error, and publication selection bias supports the current trends and emerging discussions regarding accessibility to the valuation literature. In particular, one means of making primary research more amenable to benefit transfer is to improve reporting of research design and value estimation. Researchers, whose primary goal is the estimation of value, should consistently apply methods of valuation that minimize measurement error, or that transparently treat sources of known bias. Advances in the state-of-the-art of valuation research should continue to be goal of scientists, but in those cases where primary research targets new estimates of value, accepted methods should be applied.

Generalization error and publication selection bias might be reduced through the development of protocol, restructuring existing publication outlets, and introducing new publication outlets, especially an e-journal. As we gain a better understanding from meta-regression analyses of how research design, estimation methods, and other factors systematically affect values, a set of protocols could be established to help guide the application of benefit transfers. Ultimately, however, benefit transfers are only as good as the data on which they are based. It is the accumulation of knowledge through empirical research that forms the basis for conducting benefit transfers and meta-analyses. Without complete and consistent recording of empirical research outcomes, our published body of knowledge may be little more than a biased collection of case studies. Therefore, incentives for fully recording and reporting on valuation studies should be integrated into the review process for existing publication outlets. This would increase the worth of these publications for benefit transfer practitioners and the ability of meta-regression analyses to control for observable forms of bias.

In addition, the field should consider developing an e-journal whose sole purpose is the accurate and complete recording of studies that estimate values, including studies that replicate previous research designs (Sutton et al. 2000). There need be no page limits with an e-journal, so full recording of study details is not only possible, but desired. As a condition of posting in this e-journal, the researcher could be required to fill out a survey that specifies the values of potential moderator variables that might be used by others for meta-analysis or for estimating benefit transfer functions. Benefit transfer practitioners would be the primary beneficiaries of such a journal, especially if it is linked to an active database such as the Environmental Valuation Reference Inventory (www.evri.ca).

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Table 1 Summary of benefit transfer validity tests

Reference	**Resource/Activity**	**Value Transfer Percent Error**[a]	**Function Transfer Percent Error**[a]
Loomis (1992)	Recreation	4 – 39	1 – 18
Parsons and Kealy (1994)	Water\recreation	4 – 34	1 – 75
Loomis et al. (1995)	Recreation		
Nonlinear least squares model		---	1 – 475
Heckman model		---	1 – 113
Bergland et al. (1995)	Water quality	25 – 45	18 – 41
Downing and Ozuna (1996)	Fishing	0 – 577	---
Kirchhoff et al. (1997)	Whitewater rafting	36 – 56	87 – 210
	Birdwatching	35 – 69	2 – 35
Bowker et al. (1997)	Whitewater rafting		
Pooled data (n-1)		---	14 – 160
Pooled data (all)		---	16 – 57
Kirchhoff (1998)	Recreation/habitat		
Benefit function transfer		---	2 – 475
Meta-analysis transfer		---	3 – 7028
Brouwer and Spaninks (1999)	Biodiversity	27 – 36	22 – 40
Morrison and Bennett (2000)	Wetlands	4 – 191	---
Rosenberger and Loomis (2000a)	Recreation	---	0 – 319
Piper and Martin (2001)	Rural water supply		
Individual sites (similar)		---	6 – 20
Individual sites (dissimilar)		---	89 – 149
Pooled data		---	3 – 23
VandenBerg et al. (2001)	Water quality		
Individual sites		1 – 239	0 – 298
Pooled data (multi-state)		0 – 105	1 – 56
Pooled data (state-level)		3 – 57	0 – 39
Pooled data (contaminated sites)		3 – 100	2 – 50
Shrestha and Loomis (2001)	International recreation	---	1 – 81
Chattopadhyay (2003)	Air quality		
N = 304 (similar subgroups)		106 – 429	104 – 486
N = 609 (similar subgroups)		57 – 150	57 – 153
N = 913 (similar subgroups)		42 – 82	42 – 82
N = 1218 (similar subgroups)		36 – 67	36 – 67
N = 1522 (similar subgroups)		32 – 58	32 – 58
N = 913 (dissimilar subgroups)		89 – 128	65 – 110
Ready et al. (2004)	International air and water quality (health benefits)	20 – 81	20 – 83
Jeong and Haab (2004)	Marine recreational fishing		
Access per trip		---	4 – 230
Per one fish increase		---	2 – 457
Rozan (2004)	International air quality (health benefits)	---	19 – 44
Jiang et al. (2005)	Coastal land protection	---	53 – 85

[a]All percent errors are reported as absolute values. Adapted from and expanded on Brouwer (2000).

Table 2 Non-reporting of study characteristics in a recreation use values database (131 independent studies reporting 682 estimates)

Attribute	**Percent Reporting**
Sample average income	2.5%
Sample average education level	0.5%
Sample average age	3.3%
Sample gender composition	16.0%
Sample size	61.0%

Table 3 Regression-based dummy variable tests of publication bias

Source	**Resource**	**Unit of Analysis**	**Significance**	**Direction of published to unpublished**[a]
Smith & Huang (1995)	Air quality	WTP via hedonic property method	Significant	<
Woodward & Wui (2001)	Wetlands	WTP via various methods	Insignificant	<
Zelmer (2003)	Public goods	Voluntary contributions	Insignificant	<
Rosenberger (2005)[b]	Recreation	WTP via various methods	Significant	<
Van Kooten (2004)	Carbon sequestration costs in forests	Cost	Significant	>
Dalhuisen et al. (2003)	Residential water demand	Price elasticity	Significant	<[c]
		Income elasticity	Significant	<[d]
Gallet & List (2003)	Cigarette demand	Price elasticity	Significant	>[e]
		Income elasticity	Significant	>[f]

[a]Gallet & List (2003) created a dummy variable identifying estimates published in the top 36 premier journal. Dalhuisen et al. (2003) created a dummy variable identifying unpublished estimates. Van Kooten (2004) created a dummy variable identifying estimates published in peer-reviewed sources.
[b]Publication dummy variables were tested in the Rosenberger and Loomis (2001) meta-analysis for this paper
[c]Smaller absolute values for price elasticities in unpublished studies.
[d]Greater absolute values for income elasticities in unpublished studies.
[e]Greater absolute values for price elasticities in top journal publications.
[f]Greater absolute values for income elasticities in top journal publications.

Selection Effects in the Meta-Analytic Transfer of Ecosystem Values

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Abstract:

The analysis develops an approach for dealing with selection effects in the meta regression of ecological values. The approach is based on Heckman's (1979) two stage procedure and is adaptable to cross section and unbalanced panel data. The approach identifies both a method of testing for selection effects and for consistent estimation if selection effects are shown to be statistically significant. The approach is illustrated with a meta regression of wetland ecosystem values. The application shows that selection is statistically and economically significant. Selection effects lead to generic wetland values that are almost 4 times larger than values computed using the selection corrected parameters. Value adjustment factors for wetland services and methodological variables appear less prone to selection effects. The uncorrected value adjustment factors for wetland services and research methods are, on average, within 12 percent of the selection corrected value adjustment factors.

Key Words: benefit transfer, ecosystem valuation, meta analysis, selection

Selection Effects in the Meta-Analytic Transfer of Ecosystem Values

Meta regression is a meta analytic method that uses linear statistical models to summarize and evaluate previous research results (Stanley and Jarrell 2005). Smith (1990) and Walsh (1992) were among the first to use meta regression techniques in non-market valuation. In benefit transfer, meta regression results may be used qualitatively, to corroborate new primary results, or to transfer values (Deck and Chestnut 1992). Meta regression in benefit transfer summarizes the weight of the evidence and characterizes the degree of uncertainty about quality-adjusted ecosystem values. Meta regression also extends the range of primary valuation studies by allowing the estimation of marginal values for services and functions that are constant within each primary valuation study, but vary across different valuation studies (Johnston et al. 2005).

In meta regression the value estimates from primary valuation studies are treated as the individual observations (Smith and Pattanayak 2002; Walsh, Johnson, and McKean 1992). While such data have advantages, meta data also pose special difficulties. A key difficulty is obtaining a representative sample of ecosystem values. For benefit transfer, unbiased estimation is desirable and unbiasedness is facilitated by a sample that approximates a random draw from a defined population. Meta analytic data are unlikely to represent a random draw since meta data are usually subject to various forms of selection that may bias the estimated results. Publication bias is a widely recognized form of selection bias (Egger and Smith 1998) and standardized methods exist for detecting and evaluating the effects of such publication bias (Stanley 2005). Rosenberger (in review) shows that such publication bias can affect both the mean and variance of benefit transfer results.

Selection decisions prior to publication may also truncate the sample of available studies. Valuation research is costly and such costs limit the feasibility of proposed studies (Brookshire and Neill 1992; McConnell 1992). Decisions to fund and do research are not random, but are linked to the human awareness of the resource, whether stakeholders view the resource as important, and the magnitude of the policy decisions made in response to conflicts over resources. Ecosystems that are considered valuable *a priori* by some segment of the public seem more likely to be researched and valued (Woodward and Wui 2001). Administrative rules are often quite specific in requiring primary valuation studies only when a policy conflict reaches a threshold level of severity, as in the cases of U.S. natural resource damage assessment (USC 2005) and regulatory analysis (USP 1981; USP 1993). Awareness, importance, and policy all seem to be possible factors in determining whether valuation studies are proposed, funded, and completed. The effect of such selection is denoted as research priority selection. Research priority selection shifts the set of available valuation studies toward resources and ecosystems that have been of primary policy concern.

The present analysis develops methods for testing for and evaluating the effects of selection, particularly research priority selection. The methods are based on the Heckman model of incidental truncation (Heckman 1979). In the Heckman model, there are two stochastic equations, one is the main equation of interest and the other is a selection equation. Selection effects arise in estimating the coefficients of the main equation when the stochastic term in the main equation is correlated with the stochastic term in the selection equation. In the present analysis, the main equation is the meta regression and the selection equation specifies the research priority of a valuation study. The selection equation is defined over a cross section of political jurisdictions, such as states or counties. Within a jurisdiction, valuation studies either have or have not been conducted. The selection equation specifies the probability that a valuation study is completed in a particular jurisdiction. The selection equation is used to create an inverse Mills ratio that may be entered in the meta regression equation to test for and remove the effects of research priority selection on the estimated coefficients of the main equation.

The developed methods are illustrated with a meta regression of wetland ecosystem values. The wetland data were originally collected and analyzed by Woodward and Wui (2001). The estimated selection equation confirms that wetland valuation studies are not random events, but are systematically related to variables that influence research priority. Moreover, the test using the inverse Mills ratio indicates that research priority selection is statistically significant in the meta regression of wetland ecosystem values. Research priority selection results in generic wetland values that are almost four times larger than the corrected value estimates.

Meta Regression and Selection Effects

This section derives econometric methods for testing and correcting for effects of research priority selection. The analysis begins by specifying both a benefit transfer meta regression and a research priority selection equation. The conditions leading to research priority selection are identified and are shown to result from a non-zero covariance between the stochastic elements of the meta regression and research priority equation. Heckman's two-stage procedure is outlined and used to derive an ordinary least squares test for research priority selection effects as well as a method of correcting for selection effects, should they be confirmed by the test.

Estimated ecosystem values are functions of the services and functions of a particular type of ecosystem (de Groot, Wilson, and Boumans 2002; Faber, Costanza, and Wilson 2002). Estimated values are also influenced by the research choices and methods (McConnell 1992; Woodward and Wui 2001). Hence, the dependent variable in meta regression are the values estimated in primary studies. The independent variables include variables that measure ecosystem services, ecosystem functions, and methodological characteristics of the individual studies. The model for the meta regression equation is

(1) $$v_i = x_i\beta + u_i$$

where v_i is a mathematical transformation (e.g., a linear, logarithmic, or other transformation) of the wetland value estimated by the *i*th valuation study, $i = \{1,...,I\}$, x_i is a 1xK vector of variables measuring ecosystem services, ecosystem functions, and research methods, β is a Kx1 vector of coefficients, and u_i is a stochastic error term with $E[u_i] = 0$ and $E[u_i^2] = \sigma_u^2$. The first element of x_i is a one and the first element of β is the intercept constant.

The decision to do a valuation study depends on its research priority. In the present case, the research priority of the *i*th valuation study may be thought of as an unobserved or latent variable, h_i^*, that is a function of independent variables. The variable h_i^* increases as research priority increases. The independent variables that influence h_i^* may include variables such as measures of (a) a human population's awareness of a particular type of ecosystem, (b) the local scarcity of an ecosystem, (c) the degree of human pressure on local ecosystem resources, and (d) the income and wealth of the local human population. The model for the research priority equation is

(2) $$h_i^* = z_i \pi + e_i$$

where z_i is a 1xQ vector of the independent variables, π is a Qx1 vector of coefficients, and e_i is a stochastic error term with $E[e_i] = 0$ and $E[e_i^2] = \sigma_e^2$. The first element of z_i is one and the first element of π is the intercept constant.

Research priority identifies whether a study is funded and completed or not. Without loss of generality, the research priority variable is normalized so that when $h_i^* > 0$ a valuation proposal is funded and completed, and a v_i is observed. When $h_i^* \leq 0$, a study proposal is shelved and no v_i is produced.

To derive the effect of research priority selection on meta regression, let b be the ordinary least squares (OLS) estimator of β. Given the meta regression and research priority equations, the OLS estimator of β is conditional on the expectation of u given e,

(3) $$E(b) = \beta + \delta E(u \mid e)$$

where $\delta = (X'X)^{-1} X'x$, X is a matrix of the stacked x_i, and $E(u \mid e)$ is the conditional expectation of the stacked vector of stochastic terms, $u = (u_i)$ and $e = (e_i)$. When the stochastic terms have a zero covariance, the conditional expectation in equation (3) is equal to zero and the expectation of the OLS estimator b is equal to β. In the latter case of zero covariance, the OLS estimator b is an consistent estimator of the meta regression coefficients, β. However, when the stochastic terms are correlated and have a non-zero covariance, the OLS estimator is inconsistent (Heckman 1979) since its expectation equals β plus a vector of constants that depend on δ and the distribution of the error terms.

The derivation of a consistent estimator of β in the presence of selection begins with the expected value of v_i,

$$\begin{aligned} E[v_i \mid v_i\, observed] &= E[v_i \mid u_i^* > 0] \\ &= E[v_i \mid e_i > -z_i\pi] \\ &= x_i\beta + E[u_i \mid e_i > -z_i\pi] \end{aligned} \tag{4}$$

As in equation (3), the conditional expectation of the meta regression stochastic term is not zero. Heckman (1979) shows that when the stochastic terms are jointly normal with covariance σ_{ue} and correlation coefficient $\rho = \dfrac{\sigma_{ue}}{\sigma_u \sigma_e}$, the last line of equation (3) can be rewritten as

$$E[v_i \mid v_i\, observed] = x_i\beta + \rho\sigma_u\lambda_i \tag{5}$$

where $\rho\sigma_u\lambda_i = E[u_i \mid e_i > -z_i\delta]$ and λ_i is the inverse Mills ratio. As with equation (3), the conditional expectation with jointly normal variates, $\rho\sigma_u\lambda_i]$, is zero when the covariance of the stochastic terms is zero. The inverse Mills ratio is a function of the research priority coefficients and data, $\lambda_i = \phi(z_i\pi)/\Phi(z_i\pi)$ where ϕ is the normal density function, and Φ is the cumulative normal density function.

Equation (5) provides a model for a two-stage procedure to test and correct for selection effects (Heckman 1979). The terms ρ and σ_u are simply constants while λ_i is a variable determined by the independent variables that influence research priority. Hence, λ_i may be estimated and treated as an independent variable in a revised meta regression model in the form of equation (5). The correlation and standard deviation terms may be treated as a single coefficient, $\beta_\lambda = \rho\sigma_u$, to be estimated as the other coefficients β are estimated.

The two-stage procedure is applied to the meta regression problem by first specifying an observable research priority selection equation. It is supposed that there are N jurisdictional units that have the potential to fund and complete valuation studies. Jurisdictional units may be states, counties, or administrative units such as national forest regions. A selection variable s_i is defined for each jurisdictional unit. The variable $s_i = 1$ if a v_i is recorded within a jurisdiction and $s_i = 0$ otherwise, $i = \{1,...,N\}$. The research priority selection model is the probability of observing v_i,

(6)
$$\begin{aligned} \Pr ob[s_i = 1] &= \Pr ob[h_i^* > 0] \\ &= \Phi(z_i \pi) \end{aligned}$$

Equation (6) is a probit model defined on the selection indicator, s_i, and independent variables, z_i. The parameters π are estimated with maximum likelihood using data on the selection indicator and independent variables, $\{(s_i, z_i) \mid i = 1,...,N\}$. Estimation requires that some jurisdictional units have no observed valuation studies. The estimates, $\hat{\pi}$, and data, z_i, are then used to compute estimates of the inverse Mills ratio for each observation using $\hat{\lambda}_i = \phi(z_i\hat{\pi}) / \Phi(z_i\hat{\pi})$.

The second stage of the Heckman procedure reformulates the meta regression, equation (1), to include the inverse Mills ratio as an independent variable,

(7)
$$v_i \mid h_i^* > 0 = x_i\beta + \beta_\gamma \hat{\lambda}_i + \varepsilon_i$$

where $\beta_\lambda = \rho\sigma_u$ is a coefficient to be estimated and ε_i is a randomly distributed error term with $E[\varepsilon_i] = 0$. Equation (7) is estimated using only the observed value data and corresponding independent variables, $\{(v_i, x_i, \hat{\lambda}_i) \mid i = 1,...,M < N\}$ where the strict inequality $M < N$ holds since values, v_i, are only observed in a proper subset of jurisdictions.

Consistent estimates of the coefficients in equation (7) are obtained by applying OLS to the data. The estimated OLS coefficients and standard variance matrix can also be used to test the statistical significance of the hypothesized research priority bias. The null hypothesis is one of no statistically significant selection while the alternative hypothesis is that selection is statistically significant. The OLS variance matrix is valid under the null hypothesis and may be used to test the null (Wooldridge 1995).

The test for selection is simply a t-test of whether the estimated coefficient of the inverse mills ratio, $\hat{\beta}_\lambda$, is statistically different from zero. If a t-test indicates that $\hat{\beta}_\lambda$ is not statistically different from zero, the test fails to reject the null hypothesis of no statistically significant incidental truncation. When the test fails to reject the null hypothesis, selection may be ignored and OLS estimates of equation (1) are consistent under the standard conditions (Wooldridge 1995). If the t-test rejects the null hypothesis, the OLS coefficient estimates are consistent but the variance matrix is invalid since it does not account for estimated rather than true inverse Mills ratio. Standard statistical programs compute the variance matrix derived by Heckman (1979).

When jurisdictional units are large, there may be more than one valuation study per jurisdictional unit. In this case, the meta regression equation (7) becomes

$$v_{mn} \mid h_m^* > 0 = x_{mn}\beta + \beta_\gamma \hat{\lambda}_m + \varepsilon_{mn} \tag{8}$$

where $m = \{1,...,M\}$ denotes a jurisdictional unit with one or more observed valuations, $n = \{1,...,N\}$ denotes the nth valuation study, and ε_{mn} is an independently and identically distributed stochastic term with $E[\varepsilon_{mn}] = 0$ and a variance $E[\varepsilon_{mn}^2] = \sigma_{mn}^2$. Independent variables are uncorrelated with the stochastic term ε_{mn}. The N observations in the mth jurisdiction share the same inverse Mills ratio. This leads to a panel data meta regression model,

$$\begin{aligned} v_{mn} \mid h_{mn}^* > 0 &= x_{mn}\beta + \beta_\gamma \hat{\lambda}_m + \beta_\gamma(\lambda_m - \hat{\lambda}_m) + \varepsilon_{mn} \\ &= x_{mn}\beta + \beta_\gamma \hat{\lambda}_m + \eta_{mn} \end{aligned} \tag{9}$$

where $\eta_{mn} = \beta_{\lambda}(\lambda_m - \hat{\lambda}_m) + \varepsilon_{mn}$. The stochastic term, η_{st}, includes both the stochastic term ε_{mn} and an error component due to the estimated inverse Mills ratio, $\beta_{\lambda}(\lambda_m - \hat{\lambda}_m)$. Though ε_{mn} is independent within and across jurisdictions, the error term stemming from the estimated Mills ratio is the same for each valuation within the *m*th jurisdictional unit, so the stochastic terms η_{mn} have a non-zero correlation within a jurisdiction. The standard Heckman variance matrix cannot be applied since it fails to account for the panel structure of the error in equation (9). To account for the panel structure, standard Heckman variance is replaced with a robust variance matrix developed for balanced and unbalanced panel data applications (Wooldridge 1995).

The analysis concludes, then, with a test for research priority selection and a method of correcting the estimation process to obtain both consistent coefficient estimates and valid variance estimates. The first step is to specify both a meta regression and a research priority selection equation. The second step is to estimate the selection equation (6) using maximum likelihood. The inverse Mills ratio is then computed for each observation in the meta regression data set. OLS is used to estimate the coefficients and variance matrix for equation (7). A t-test used to evaluate whether the coefficient of the inverse Mills ratio is statistically different from zero. If the coefficient is not statistically different from zero, selection is rejected and the meta regression is estimated without dealing with selection. If the inverse Mills ratio is statistically different from zero, the OLS coefficient estimates of equation (7) and (8) are consistent, but the OLS variance matrix is invalid. The valid cross section variance matrix is given by Heckman (1979) and the panel variance matrix is derived by Wooldridge (1995).

Data

The two stage approach for testing and correcting research priority selection is illustrated using data wetland ecosystem values compiled by Woodward and Wui (2001). Wetland ecosystems are particularly suitable for the meta regression and benefits transfer. Primary studies that value wetland ecosystems value a bundle of ecosystem services and functions that comprise an ecosystem (Turner, van den Bergh, and Brouwer 2003). Meta regressions of primary wetland values have the potential to result in both quality-adjusted value.

Woodward and Wui (WW) used 65 primary wetland values in a meta regression. The data were composed of 53 observations from within the United States (U.S.) and 12 observations from countries other than the U.S. WW normalized the valuation data so that the dependent variable for each observation was measured in dollars (1990 price level) per wetland acre. WW sought to determine whether different valuation methods influenced the estimated values so WW value data included producer surplus values estimated using net factor income, consumer surplus values estimated using contingent valuation and travel cost methods, and market value approaches based on hedonic analysis and net factor income.

The WW data included three types of independent variables: variables to measure the presence or absence in the primary ecosystem of specific wetland services and functions; dummy variables to indicate normative characteristics of the study such as whether good econometric practices were evident in a primary study; and dummy variables to indicate the type of valuation method used in a primary study. Altogether, WW included 22 independent variables in their final analysis of ecosystem values.

WW were concerned that selection effects might be present in their data, but offered no procedure to quantify the possibility (Woodward and Wui 2001). Interestingly, the spatial distribution of the data also suggests a lack of randomness in the jurisdictions with observed valuations. Only 14 of 50 U.S. states are represented in the meta data. Almost 50 percent of the observations come from five states with relatively large areas of wetlands: Florida, Louisiana, Massachusetts, Michigan, and North Dakota. The fact that states with extensive wetlands are more evident in the data suggests that human awareness and development pressures on wetlands may be more important than physical ecosystem scarcity as factors that influence research priorities. In addition, 63 of the 65 observations are from U.S., Canada, and the European Union, areas that are relatively wealthy on a global basis. Hence, a jurisdiction's income or wealth may be important as an additional selection variable.

The WW meta regression data were supplemented with variables that might influence the research priority of valuation studies within jurisdictions. Unfortunately, consistently measured estimates of wetlands areas and land use were not available for nations outside the U.S., so the selection data was limited to the 48 states within the contiguous borders of the continental U.S. The dependent variable in the selection equation was a dummy variable having a value of one if a primary valuation study for that state appeared in the WW data set, and zero otherwise. The first independent variable in the selection equation measured that the extent of wetlands in a state. This wetlands variable *Wetland/open space* was measured as a ratio of (a) wetland area (NRC, 2005) to (b) the open space area present in a state. Open space was measured as undeveloped, non-agricultural land area (Demographia 2000a). Increases in *Wetland/open space* were thought to be related to increasing general awareness of wetlands as an ecosystem as well as the increasing possibility that development might noticeably infringe on wetland ecosystems. The second selection variable was population density (Demographia 2000b), denoted *Density*, and was entered as a measure of the relative development pressure on wetlands. The third independent variable was per capita income (BEA 2005) and was denoted *Income*.

The selection data reduce the effective size of the WW meta data in two ways. First, the data for the wetlands to open space ratio are only available for U.S. states. This limits the meta analysis with selection to the 53 U.S. observations in the WW data set. Second, from the point of view of jurisdictions, the WW data are an unbalanced panel (Wooldridge 2002). Each of the 14 states in the WW data set is a cross section unit for which there are one or more observations. Statistical analysis with the panel is limited by the cross section with 14 distinct units and requires that the number of independent variables be limited to less than 14 in order to preserve degrees of freedom for the statistical tests (Rogers 1994).

To meet this latter constraint, the independent variables in the meta regression are limited to the nine independent variables for which WW estimated coefficients that were statistically different from zero. These statistically significant variables includes five variables that measured wetland ecosystem characteristics: *Lna* denotes the natural logarithm of wetland size measured in acres; *Birdhunt* equals one for a wetland open to bird hunting and zero otherwise; *Birdwatch* equals one for a wetland open to bird watching and zero otherwise; *Amenity* equals one for a wetland with some amenity services and zero otherwise; and *Quality* equals one for a wetland provided water quality control and zero otherwise. The variables with statistically significant coefficients also included four methodological variables: *PS* equals one for a producer surplus value estimate and zero otherwise; *HP* equals one for a hedonic price estimate and zero otherwise; *RC* equals one for a resource cost valuation estimate and zero otherwise; and *WMetric* equals one for a study using poor quality econometric practices as evaluated by WW and zero otherwise. Contingent valuation is the default method when *PS*, *HP*, and *RC* are equal to zero.

The above considerations led to two meta regression data sets. The first was the WW meta regression data set with 65 observations, average wetland value as the dependent variable, and nine ecological and methodological independent variables. The first data set provided a point of comparison with the WW analysis. The second meta regression data set contained 53 observations from 14 states within the continental U.S. The second data set was paired with the selection data and was used to implement the test and correction for research priority selection.

Results

This section describes the empirical results for the meta regression and selection analyses. The descriptive statistics for the meta regression data, the selection data, selection analysis, and meta regressions are described. The test for research priority selection effects rejects the null hypothesis of no selection. Final results are presented for the meta regression corrected for selection using the Heckman two-stage procedure.

Table 1 lists descriptive statistics for the WW data and two subsets of the WW data, the International and U.S. data groups. The International data contains 12 observations and the U.S. data contains 53 observations, adding up to the 65 observations in the WW data set. The International data are drawn from six countries: Austria, Canada, Mexico, Nigeria, Scotland, and Sweden. The U.S. data come from 14 states: California, Florida, Illinois, Iowa, Kentucky, Louisiana, Massachusetts, Michigan, North Dakota, Nebraska, South Carolina, South Dakota, Virginia, and Wisconsin.

The mean value per acre of wetlands is \$868 in the WW data, \$752 in the International data, and \$894 per acre in the U.S. Data. The mean Inverse Mills ratio estimated in the selection analysis (described below) for the U.S. data was .992. As described in the previous section, it was not possible to estimate the Inverse Mills ratio variable for the International and combined WW data sets. The International data included wetlands with slightly more bird hunting and bird watching than those for the U.S. data, but the U.S. data encompassed wetlands with a greater incidence of environmental amenities and water quality control. The mean sizes of the wetlands in the International and U.S. data were very similar. Producer surplus and resource cost approaches comprised a smaller share of the international data than they did of the U.S. data. Hedonic pricing values were entirely absent from the International data and constituted only four percent (2 observations) in the U.S. data. WW identified weak econometric procedures in 15 percent of the U.S. data, but none in the International data.

Table 2 lists descriptive statistics and probit coefficient estimates for the research priority selection analysis. The units of observation are the 48 states in the continental U.S. The dependent variable is the *Valued*. *Valued* equals one if the WW data set for a state that has at least one wetland valuation in the WW data, and zero otherwise. Fourteen states of 48 continental states have at least one wetland valuation study so the mean of *Valued* is 29 percent. The mean ratio of wetland area to open space area, *Wetland/open space*, is 23.7 percent. The mean population density for the 48 states is 169 people per square mile and mean per capita income by state $16.9 thousand (1990 price level).

Predictions from the probit equation are correct in 79.2 percent of the cases. The coefficient for the *Wetland/open space* ratio is statistically different from zero at the 99 percent level. An increase in *Wetland/open space* increases the probability that a wetland valuation study has been conducted in a given state. Smaller wetland to open space ratios indicate a scarcity of wetlands relative to overall open space, so the *Wetland/open space* result suggests that physical resource scarcity does not appear to drive research priorities. Larger wetland to open space ratios are likely to mean that the public is more aware of wetland and that development is more likely to affect wetlands. Thus, the results for *Wetland/open space* indicates that awareness of wetlands and the likelihood of development are the factors that influence research priorities. Table 2 also indicates that increases in *Density* and *Income* increase the likelihood of valuation research, but the coefficients for these variables are not significantly different from zero at the 90 percent level.

Table 3 lists the meta regression results. As in the WW analysis, the dependent variable in each equation is the natural logarithm of the estimated average wetland value per acre. Column 1 lists the independent variables used in the meta regressions and Columns 2 to 5 list estimated coefficients for four different equations. Standard errors are listed in parentheses. Standard errors for the OLS estimates in columns 2 to 4 were computed using a robust variance matrix for panel data (Rogers 1994). Standard errors for the Heckman estimates in column 5 were computed using the robust procedures developed by Wooldridge (1995).

Columns 2 and 3 list the OLS coefficients estimated using, respectively, the 65 observations from WW data and the 53 observations from the WW subset of U.S. data. The results indicate that excluding the International data from the sample has little impact on the estimated coefficients and their statistical significance. At the 95 percent level, the same coefficients are statistically different from zero in each of the two equations and the same two variables, Birdhunt and Quality, have coefficients that are not statistically different from zero. The coefficients for the U.S. data are within seven percent of those estimated with the WW data for the *Intercept*, *Birdhunt*, *Amenity*, *Lna, HP*, and *WMetric*. The WW and U.S. OLS coefficients for *Birdwatch*, *Quality*, *PS*, and *RC* differ by 12 to 23 percent. Average wetland values increase with bird watching opportunities, but decrease with the presence of environmental amenities and wetland size. Producer surplus estimates and estimates based on weak econometrics appear be smaller than the baseline contingent values estimated with good econometrics. Hedonic price and resource cost value estimates are larger than the baseline contingent values.

Column 4 in Table 3 reports the OLS test for research priority selection effects. The test is based on the OLS equation using the nine ecosystem and methodological variables plus the *Inverse Mills ratio* estimated from the probit results. The results show that the *Inverse Mills ratio* coefficient is statistically different from zero at the 95 percent level, so the test confirms research priority selection. The results imply that the OLS coefficient estimates from Columns 3 are inconsistent and the OLS variance estimate is invalid. Hence, the results of Columns 3 are misleading in terms of both the size and statistical significance of the estimated coefficients.

The OLS coefficients in Column 4 are consistent given selection, but the OLS variance matrix and standard errors are invalid. Column 5 labeled Heckman reports the OLS coefficient estimates of Column 4 paired with consistent estimates of the standard errors computed using Wooldridge's (1995) robust procedures. With the robust procedures, there is a small increase in the size of the standard error of the *Inverse Mills ratio*, but it remains statistically different from zero at the 90 percent level. Standard errors for Birdhunt and Quality are smaller with the robust procedures than with the OLS procedures, so that the Heckman estimates of wetland service and methodological coefficients are all statistically different from zero at at least the 95 percent level.

The effect of research priority selection on the size of the estimated coefficients is mixed. The Heckman coefficient estimates for the Intercept, *Birdwatch*, *Quality*, *Lna*, and *WMetric* differ from the uncorrected OLS coefficients in Column 3 by more than 24 percent, while the Heckman coefficient estimates for *Birdhunt*, *PS*, and *RC* are within a few percent of the uncorrected OLS coefficients in Column 3.

Implications for Benefit Transfer

This section examines how the differences in the uncorrected OLS and Heckman coefficients described in the previous section affect benefit transfer. The semi-logarithmic valuation function estimated by WW and used above is based on a log-normal distribution (LND). Given the skewness of the LND, median values are often used to represent central tendency in place of mean values which are influenced of the skewed tail.[1] WW estimated median values and the are also used here. Given the meta regression coefficients, the median total value of a wetland with characteristics $x_s = \left(x_{s1},...,x_{sK}\right)$ is

(10) $$\kappa^s = \exp(\hat{\beta}_0 + x_{1s}\hat{\beta}_1 + x_{2s}\hat{\beta}_2 + ... + x_{Ks}\hat{\beta}_K)$$

where $\hat{\beta} = \left(\hat{\beta}_0,...,\hat{\beta}_K\right)$ are estimated coefficients.

A baseline generic wetland value is derived from equation (10) by setting the independent variables for special ecological services and selected methodological variables equal to appropriate values. For instance, given the variables using in the empirical analysis, the contingent value of a generic wetland with no special services and estimated with good econometrics is

(11) $$\kappa^{s0} = \exp(\hat{\beta}_0 + \overline{x}_{1s}\hat{\beta}_1)$$

The generic value is a function of the estimated intercept, $\hat{\beta}_0$, the mean of the log of average wetland size, $\overline{x}_{1s}$, and the estimated coefficient for wetland size, $\hat{\beta}_1$. The coefficients of special ecological services such as water quality control do not enter the generic wetland value since special services are not present in a generic wetland. Contingent valuation with good econometrics is the methodological condition in equation (11), so the methodological variables are also set equal to zero.

Table 4 lists the generic wetland values, κ^{s0}, estimated using the using the uncorrected coefficients for OLS (Column 3, Table 3) and the Heckman selection procedure (Column 5, Table 3). The κ^{s0} estimated from the OLS results is \$360 dollars per acre and appears to be significantly different from zero at the 99 percent level. However, the uncorrected OLS coefficients are inconsistent and the OLS variance matrix is invalid, so the OLS benefit results may be misleading. The κ^{s0} estimated using the Heckman coefficients from Column 4 in Table 3 indicates that the generic wetland value is \$96 per acre, less than 1/3 of the OLS value. The standard error for the Heckman generic value is 18 percent larger than the OLS standard error. The source the large difference between the OLS and Heckman generic wetland values is the large difference between the OLS and Heckman intercept coefficients in Table 3. The OLS coefficient for *Lna* is smaller than the Heckman estimate and would lead to a generic OLS value than the Heckman value. The large OLS intercept estimates overwhelms the effect of Lna and results in the OLS generic value that is almost 4 times larger than the Heckman estimate.

Conclusions

The analysis developed an approach for dealing with research priority selection in the meta regression of ecological values. The approach was based on Heckman's (1979) two stage estimation method. The approach identified methods for testing the statistical significance of research priority selection and for consistent estimation when selection effects are shown to be present. An empirical application to the meta regression and benefit transfer of ecosystem values shows that research priority selection is both statistically and economically significant.

The empirical analysis examined the implications of research priority selection for benefit transfer. The meta regression intercept was shown to be central to transferring point estimates of total and marginal wetland values, but the uncorrected OLS intercept estimate was sensitive to the selection effects. The generic wetland value based on uncorrected OLS estimates was about 4 times larger than the generic value based on statistically consistent estimates. Hence, testing and correcting for selection appears to be an important step in meta regression for benefit transfer when the objective is to transfer consistent point estimates of ecological values.

The approach and empirical results reported here appear to be a first application of an econometric approach to selection effects in meta regression. As such, the empirical results are tentative. Further empirical work needs to be completed to evaluate the effects of selection with other data sets. For instance, it may be that as the number of studies grows large, such as in the data set used by Rosenberger and Loomis (2001), the effects of research priority are diminished. There is also an opportunity to apply maximum likelihood (ML) to the joint estimation of the selection and meta regression equations since existing ML procedures do not address the case where the selection data are cross sectional and the meta regression data are an unbalanced panel.

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Footnotes

1. The mean value is proportional to the median value with a log-normal distribution. The factor of proportionality is greater than one and depends on the variance of the distribution (Stynes et al 1986).

Table 1. Mean Values for Analytical Variables

Variable	Data Group		
	WW	International	U.S.
Value per Acre	868 (1803)	752 (1177)	894 (1295)
Inverse Mills ratio	--	--	.992 (.54)
Birdhunt	.400 (.49)	.750 (.45)	.321 (.47)
Birdwatch	.277 (.45)	.500 (.52)	.226 (.42)
Amenity	.154 (.36)	.083 (.29)	.170 (.38)
Quality	.200 (.40)	.083 (.29)	.226 (.42)
Lna	9.28 (3.3)	9.69 (3.1)	9.19 (3.4)
PS	.277 (.45)	.083 (.29)	.321 (.47)
HP	.031 (.17)	0 --	.038 (.19)
RC	.277 (.45)	.167 (.39)	.301 (.46)
WMetric	.123 (.33)	0 --	.151 (.36)
Number of observations	65	12	53

a. Standard deviations are listed in parentheses.

Table 2. Selection Equation Variable Means and Coefficient Estimates

Variable	Mean[a]	Coefficient Estimates[b]
Valued	.292 (.46)	--
Intercept	--	-2.12 (1.6)
Wetland/open space	.237 (.30)	3.37*** (1.7)
Density	169 (238)	-.000414 (.0011)
Income ($1,000)	16.9 (2.6)	.0498 (.095)
Log likelihood	--	-22.7
Prob>chi^2	--	.006
Percent correct predictions	--	79.2
Number of observations	48	48

a. Standard deviations are given in parentheses.

b. Coefficient standard errors are given in parentheses. A "***" indicates that a coefficient is statistically different from zero at the 99 percent level.

Table 3. Valuation Transfer Equation Estimates

Variable	Estimated Coefficients[a,b]			
	WW Data	U.S. Data		
	OLS	OLS	OLS Test	Heckman
Table Column:	2	3	4	5
Intercept	7.67***	7.87***	5.97***	5.97***
	(.65)	(.93)	(1.3)	(1.5)
Inverse Mills ratio	--	--	1.15**	1.15*
			(.41)	(.63)
Birdhunt	-.861	-.888	-.834	-.834**
	(.51)	(.77)	(.64)	(.38)
Birdwatch	1.87***	1.54***	1.02**	1.02***
	(.41)	(.33)	(.36)	(.31)
Amenity	-3.38***	-3.63***	-2.98***	-2.98***
	(.89)	(.86)	(.87)	(.75)
Quality	.890	.683	1.07*	1.07**
	(.55)	(.58)	(.53)	(.41)
Lna	-.229***	-.217**	-.153	-.153**
	(.058)	(.093)	(.09)	(.06)
PS	-2.26***	-2.66***	-2.70***	-2.70***
	(.38)	(.34)	(.38)	(.41)
HP	3.91***	3.94***	2.87***	2.87***
	(.81)	(.77)	(.93)	(.92)
RC	1.73***	1.94***	2.00***	2.00***
	(.38)	(.32)	(.41)	(.37)
WMetric	-3.24***	-3.43**	-2.68**	-2.68***
	(1.0)	(1.1)	(1.8)	(.71)
R^2	.51	.56	.59	.59
Prob F > 0	0.00	0.00	0.00	0.00
Number of observations	65	53	53	53
Number of States/Countries	20	14	14	14

a. Standard errors for the coefficients are given in parentheses

b. Significance levels were evaluated with 13 degrees of freedom due to clustering (Rogers 1994). A "***" indicates that a coefficient is statistically different from zero at the 99% level. A "**" indicates that a coefficient is statistically different from zero at the 95% level. A "*" indicates that a coefficient is statistically different from zero at the 90% level.

Table 4. Value Estimates for Baseline and Single Service Wetlands

Service	Values for U.S. Data[a]	
	OLS	Heckman
Generic wetland value, κ^{s0}	360*** (94)	96.4 (111)

a. Standard errors estimated using the delta method are given in parentheses. A "***" indicates that a coefficient is statistically different from zero at the 99% level.

Combining attitudinal and choice data to improve estimates of preferences and preference heterogeneity: a FIML, discrete-choice, latent-class model

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Abstract:

This paper shows how attitudinal data can be combined with choice data to more efficiently estimate preference parameters and preference heterogeneity. Most surveys collect both types of data, but the majority of econometric models of preferences rely solely on choice data. Two types of data, answers to choice questions and likert-scale attitudinal questions, are used to simultaneously estimate (1) the probability that an individual belongs to a particular preference class, (2) the parameters in each classes' conditional, indirect-utility function, and (3) for each attitudinal question, the probability that an individual in a particular class will give a particular response. Estimation is with the expectation-maximization (E-M) algorithm. FIML (full-information maximum likelihood) estimates are obtained by finding those values of the parameters in the model that maximize the likelihood of simultaneously observing both the attitudinal and choice data. The parameter estimates from FIML estimation are compared with those from sequential estimation, which are not asymptotically efficient.

Key words: Latent class, attitudinal data, choice data, FIML, preference heterogeneity

JEL code: C51,Q51,D12

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Surveys often include a significant number of attitudinal questions. Attitudinal questions often assess the relative importance the individual places on different attributes of a good and indicate how the individual "feels" about those attributes. In other words, attitudinal questions can reveal how much an individual likes or dislikes a particular attribute.[1]

Consider an example Likert-scale attitudinal question from a survey of Green Bay anglers:

> On a scale from 1 to 5 where 1 means ”Not at all Bothersome” and 5 means ”Very Bothersome”, answer the following question. For the fish you would like to fish for in the waters of Green Bay, how much would it bother you, if at all, if PCBs resulted in the following fish consumption advisory: "Do not eat".

Or, from a survey of depressed individuals about the possible side effects of treatment alternatives:

> How much would little or no interest in sex bother you? ("Not at all, slightly, some, a fair bit, a lot")

While many economists do not view answers to attitudinal questions as data one uses in an econometric model to estimate preferences, we believe that attitudinal data can provide significant information about the existence of different preference classes and how preferences vary across those classes.[2] Here we assume that the answers to attitudinal questions are expressions of exogenous well-behaved preferences: individuals can rank states of the world. Preferences are latent (unobservable), and both choices (actual and hypothetical) and answers to attitudinal questions are manifestations of those unobserved preferences. Given these assumptions, it would seem derelict to estimate preferences and preference heterogeneity without using attitudinal data, if it is available. Including both attitudinal and chocie data in estimation results in more efficient estimation.

The intent of this paper is to identify and estimate preference heterogeneity for environmental amenities in terms of a small number of preference classes. Class membership and the preferences of each class are latent. What is observed are the choices made, the attributes of the alternatives in the choice sets, and the answers to attitudinal questions about those attributes.

Using the E-M (expectation-maximization) algorithm (Dempster et al., 1977), we implement full-information maximum likelihood (FIML) estimation to find those values of the parameters in the model that maximize the likelihood of observing both the attitudinal and choice data. Deriving this joint likelihood function, developing an algorithm to find the values of the parameters that maximize it, implementing that algorithm, and comparing the FIML results to those obtained from sequential estimation are the main accomplishments of this paper.

Section 2 provides a brief background on latent-class models. Section 3 presents a model with both attitudinal and choice data and explains two alternate methods for estimating this model: FIML and seqential estimation. In section 4, we show how this model can be implemented by applying it to fishing preferences.

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[1]Attitudinal questions differ from questions that ask the individual to indicate his or her perceived level of an attribute.

[2]Economists who have viewed answers to attitudinal answers as data include Ben-Akiva et al. (2002), McFadden (1986), and Boxall and Adamowicz (2002).

2 Background: the LC_A, LC_C, and LC_{AC} models

The raison d'être of latent-class models is to model preference heterogeneity among discrete groups without assuming some observable deterministic explanation for that heterogeneity. We build on previous work by us and others in this area. Morey et al. (2005), ignoring the available choice data, estimate a latent-class model of preferences using only the answers to attitudinal questions - a LC_A model.[3] Examples of LC_A models outside of economics include Clogg and Goodman (1984), McCutcheon (1987), McCutcheon and Nawojcyzk (1995), De Menezes and Bartholomew (1996), Yamaguchi (2000).

While an LC_A model can provide a lot of information about preferences, many economists equate estimating preferences with estimating the values of preference parameters in utility functions. In a latent class context, once can estimate an LC_C model, a discrete choice, latent-class model estimated with only choice data. One assumes some number of classes and specifies a conditional, indirect-utility function that allows the preferences parameters to vary by class. Choice data is used to estimate the number of classes, the probability of class membership, and the preference parameters in each class's conditional, indirect-utility function. No attitudinal data is used. Examples of LC_C models include: Gupta and Chintagunta (1994), Kamakura and Russell (1989), Greene and Hensher (2002), Provencher et al. (2002), Hu et al. (2004), and Scarpa and Thiene (2005).

In this paper, we estimate a combined LC_A model and LC_C model: the LC_{AC} model. We estimate both the probability of class membership and the parameters in an indirect utility function using both the choice and attitudinal data.[4] We would not be surprised if others have already estimated a LC_{AC} model, but know of no example.[5]

3 A latent-class model of choice and attitudinal data: the LC_{AC} model

Assume the population consists of C different preference classes. An individual's preference class is latent. The researcher observes, for each individual, the data $(\boldsymbol{x}_i, \boldsymbol{y}_i)$; $\boldsymbol{x}_i$ is the set of individual i's answers to the attitudinal questions (the individual's attitudinal response pattern) and $\boldsymbol{y}_i$ represents individual i's answers to a set of stated preference (SP) choice pairs. An example of a SP question is included in the appendix.

[3] LC denotes "latent-class" and the subscript(s) denote what type or types of data are used to estimate the model.

[4] Also related to, but different from the LC_A, LC_C, and LC_{AC} models are discrete-choice models where some of the attributes of alternatives in the choice sets are latent variables. Ben-Akiva et al. (2002) provides empirical examples. These models deal with varying perceptions with respect to attribute levels; our LC_{CA} model does not. Most of these models are not latent-class models.

[5] Our LC_{AC} model is similar in appearance to but fundamentally different from the models of Boxall and Adamowicz (2002) and Swait (1994). Boxall and Adamowicz (2002) assumes class membership is a function of latent psychological variables, and that the answers to the attitudinal questions are indicators for these unobserved psychological variables. Put simply, Boxall and Adamowicz (2002) make the probability of class membership a function of the answers to attitudinal questions, whereas the LC_{AC} model assumes class membership is exogenous and determines how one will answer attitudinal questions.

If one observes $\mathbf{x}_i$, $\mathbf{y}_i$, and class membership, the likelihood function for the sample can be written as:

$$L = \prod_{i}^{N} \Pr(\mathbf{x}_i, \mathbf{y}_i, c_i). \qquad (1)$$

But, since class membership is unobserved, the best one can do is to model:

$$L = \prod_{i} \left[\Pr(\mathbf{x}_i, \mathbf{y}_i)\right] = \prod_{i} \left\lfloor \sum_{c=1}^{C} \Pr(c) \Pr(\mathbf{x}_i, \mathbf{y}_i \mid c) \right\rfloor, \qquad (2)$$

where $\Pr(c)$ is the unconditional probability of belonging to class c. $\Pr(\mathbf{x}_i, \mathbf{y}_i \mid c)$ is a conditional probability and represents the probability of observing the individual's attitudinal and stated preference responses, conditional on belonging to class c.[6]

Because individuals in the same class respond and behave similarly to one another, the response patterns of individuals from the same class are more correlated with each other than with individuals in other classes. Latent-class models assume that once one has conditioned on class, an individual's answers to all of the stated-choice and attitudinal questions are independent of one another. Accepting this assumption, the likelihood function can then be written:

$$L = \prod_{i} \left\lfloor \sum_{c=1}^{C} \Pr(c) \Pr(\mathbf{x}_i \mid c) \Pr(\mathbf{y}_i \mid c) \right\rfloor, \qquad (3)$$

where

$$\Pr(\mathbf{x}_i \mid c) = \prod_{q=1}^{Q} \prod_{s=1}^{S} (\pi_{qs|c})^{x_{iqs}} \qquad (4)$$

and

$$\Pr(\mathbf{y}_i \mid c) = \prod_{k=1}^{K} \prod_{j=1}^{J} (P_{jk|c})^{y_{ijk}}. \qquad (5)$$

$\pi_{qs|c}$ is the probability that an individual in class c answers level s to attitudinal question q; x_{iqs}=1 if individual i's answer to attitudinal question q is level s and 0 otherwise. $P_{jk|c}$ is the probability of choosing alternative j in SP-choice pair k, conditional on being a member of class c; y_{ijk}=1 if individual i choose alternative j in choice pair k and 0 otherwise.

$P_{jk|c}$ are functions of the parameters in the class-specific conditional-indirect utility functions, the β_c parameters. That is, $P_{jk|c}$ is the probit or logit probability of choosing alternative j from

[6]One could also model the probability of belonging to a class as a function of a number of observable characteristics of the respondents, such as gender and age.

SP-choice set *k*, conditional on being a member of class *c* and takes the typical form. For example, if a logit model is assumed, the probability of choosing alternative *j* is,

$$P_{jk|c} = \frac{\exp(\beta_c' \mathbf{z}_{jk})}{\sum_{j=1}^{J} \exp(\beta_c' \mathbf{z}_{jk})} \quad c = 1,2,\ldots,C, \tag{6}$$

where $\mathbf{z}_{jk}$ is the vector of characteristics of the good in alternative *j* of choice-pair *k*.

The goal of estimation is to find the β_c, the $\pi_{qs|c}$, and the Pr(*c*) that maximize Equation 2. Two conditional class membership probabilities will be useful for estimating these parameters. The first is the probability that an individual is a member of class *c* conditional on her answers to the attitudinal questions. By Bayes Theorem, this probability is:

$$\Pr(c|\mathbf{x}_i) = \frac{\Pr(c)\prod_{q=1}^{Q}\prod_{s=1}^{S}(\pi_{qs|c})^{x_{iqs}}}{\Pr(\mathbf{x}_i)}, \tag{7}$$

where

$$\Pr(\mathbf{x}_i) = \sum_{c=1}^{C} \Pr(c)\Pr(\mathbf{x}_i \mid c) = \sum_{c=1}^{C} \Pr(c)\prod_{q=1}^{Q}\prod_{s=1}^{S}(\pi_{qs|c})^{x_{iqs}}. \tag{8}$$

The second useful probability, the probability that an individual is a member of class *c* conditional on **both** her answers to the attitudinal questions and her answers to the SP choice questions, is

$$\Pr(c \mid \mathbf{x}_i, \mathbf{y}_i) = \frac{\Pr(c)\prod_{q=1}^{Q}\prod_{s=1}^{S}(\pi_{qs|c})^{x_{iqs}}\prod_{k=1}^{K}\prod_{j=1}^{J} P_{jkc}^{y_{ijk}}}{\Pr(\mathbf{x}_i, \mathbf{y}_i)} \tag{9}$$

.

where Pr($\boldsymbol{x}_i$,$\boldsymbol{y}_i$) is individual *i*'s contribution to the likelihood function (the bracketed term in Equation 3).

We next lay out two alternative methods for estimating the LC_{AC} model: FIML and sequential estimation. The FIML estimates are consistent and asymtotically efficient; the sequential estimates are only consistent.

With both types of estimation, we utilize a variant of the E-M algorithm, a technique to do maximum-likelihood estimation with incomplete information (Dempster et al. (1977), Arcidiacono and Jones (2003)). The missing pieces of information in the LC_{AC} model is class membership and the preference parameters for each class.

Put simply, the E-M algorithm replaces unobserved information with its expected value and then conducts maximum likelihood estimation as if these expectations were correct. The maximum likelihood estimates can be then used to update the original expectations. The E-M algorithm consists of two steps: an expectation step and a maximization step. In the expectation step, one calculates the expected value of the unobserved information. In the maximization step, one conducts maximum likelihood estimation as if the true value of the unobserved information was the expected value of the unobserved information. Based on the results of the maximization step, one then updates the expected value of the unobserved information. The process continues until the change in the log-likelihood function becomes very small.

3.1 FIML estimation of the LC$_{AC}$ model

In this section, we describe in more detail how one can use the E-M algorithm to do FIML estimation. Estimates obtained from this approach will be efficient as they use all of the data.

1. Guess or estimate the $N\acute{}C$ initial values of $\Pr(c\,|\,\mathbf{x}_i,\mathbf{y}_i)$, denoted $\Pr(c\,|\,\mathbf{x}_i,\mathbf{y}_i)^{\{0\}}$ where $\{d\}$ refers to iteration d. These initial guesses at the conditional probabilities, Equation 9, could be from a sequential estimation of the parameters in the model. Sequential estimation is discussed below.

2. Use the $\Pr(c|\mathbf{x}_i,\mathbf{y}_i)^{\{0\}}$ to calculate the unconditional membership probabilities, $\Pr(c)^{\{1\}}$. They are obtained by maximizing the likelihood function and solving the first order conditions:

$$\Pr(c)=\frac{1}{N}\sum_{i=1}^{N}\Pr(c\,|\boldsymbol{x}_i,\boldsymbol{y}_i|. \qquad (10)$$

Equation 10 is simply the average of the conditional class-membership probabilities for all individuals in class c. So, at this point one has calculated $\Pr(c_i)^{\{1\}}$ for each respondent in the sample, conditional on the $\Pr(c_i\,|\boldsymbol{x}_i,\boldsymbol{y}_i\,)^{\{0\}}$.

3. Then use the $\Pr(c)^{\{1\}}$, the $\Pr(c_i\,|\,\mathbf{x}_i,\mathbf{y}_i)^{\{0\}}$, and the attitudinal data to calculate the $\pi_{qs|c}^{\{1\}}$. The formula, obtained by maximizing the likelihood function and solving the first order conditions, is:

$$\pi_{qs|c}=\frac{\sum_{i=1}^{N}\Pr(c_i\,|\,\mathbf{x}_i,\mathbf{y}_i)x_{iqs}}{\Pr(c)N}. \qquad (11)$$

The denominator in Equation 11 is an estimate of the number of people in class c. The numerator, $\sum_{i=1}^{N}\Pr(c_i\,|\,\mathbf{x}_i,\mathbf{y}_i)x_{iqs}$, is the number of times individuals in the sample answered level s to question q, each weighted by the conditional probability that the individual is in c. That is, the numerator is an estimate of the number of times individuals in class c answer level s to question q. The ratio is therefore an estimate of the proportion of times individuals in class c answer level s to question q.

4. Now use the $\pi_{qs|c}^{\{1\}}$ and Equation 4 to calculate the $\Pr(\mathbf{x}_i \mid c)^{\{1\}}$.

Summarizing to here, based on our initial "guesses" for the $\Pr(c \mid \mathbf{x}_i, \mathbf{y}_i)$ and the data, we have come up with the estimates of the $\Pr(c)^{\{1\}}$ and the $\Pr(\mathbf{x}_i \mid c)^{\{1\}}$. Steps 2-3 are an application of the E-M algorithm. One is finding the values of the Pr(c) and the $\pi_{qs|c}$ that maximize the expectation of the joint likelihood function. It is an "expected" likelihood function because one is using the expected values of the conditional membership probabilities, the $\Pr(c \mid \mathbf{x}_i, \mathbf{y}_i)^{\{d\}}$, as if they were the true values.

5. Plugging in the Pr(c) and $\pi_{qs|c}$, the likelihood function, conditional on these estimates, is:

$$L_r^{\{1\}} = \prod_i \left[\sum_{c=1}^{C} \Pr(c)^{\{1\}} \Pr(\mathbf{x}_i \mid c)^{\{1\}} \Pr(\mathbf{y}_i \mid c) \right] \tag{12}$$

$$L_r^{\{1\}} = \prod_i \left[\sum_{c=1}^{C} \Pr(c)^{\{1\}} \Pr(c) \prod_{q=1}^{Q} \prod_{s=1}^{S} (\pi_{qs|c})^{x_{iqs}} \prod_{k=1}^{K} \prod_{j=1}^{J} P_{jkc}^{y_{ijk}} \right] \tag{13}$$

Use a maximization algorithm (such as Optimum or Maxlik in Gauss) to maximize lnL_r in terms of the β_c Denote these parameter estimates $\beta_c^{\{1\}}$. The subscript r indicates that the likelihood function is conditioned/restricted.

6. Now plug the $\beta_c^{\{1\}}$, the $\Pr(c)^{\{1\}}$, the $\pi_{qs|c}^{\{1\}}$, along with the attitudinal and choice data into Equation 9 to calculate $\Pr(c_i \mid \mathbf{x}_i, \mathbf{y}_i)^{\{1\}}$. $\Pr(c_i \mid \mathbf{x}_i, \mathbf{y}_i)^{\{1\}}$ is an expected value. This is the end of iteration 1.

Return to step 1 but start with new best estimate of the $\Pr(c|\mathbf{x}_i, \mathbf{y}_i)$, the $\Pr(c|\mathbf{x}_i, \mathbf{y}_i)^{\{1\}}$. Continue iterating until the conditional likelihood function, Equation 12, increases by less than some predetermined amount.

3.2 Sequential estimation of the LC$_{AC}$ model

Sequential estimation can be viewed as an alternative to FIML estimation or a way to get good initial estimates of the conditional class membership probabilities, the $\Pr(c|\mathbf{x}_i, \mathbf{y}_i)$, for the start of FIML estimation.

Sequential estimation, as defined here, first obtains maximum likelihood estimates of the Pr(c) and the $\pi_{qs|c}$ using only the attitudinal data. These estimates are not as efficient as the FIML estimates because not all of the information/data is used in their estimation. Denote these estimates $\Pr(c)^{s1a}$ and $\pi_{qs|c}^{s1a}$ where the superscript "$s1a$" denotes stage 1 sequential estimates based solely on the attitudinal data. These can then be plugged into Equation 7 to obtain stage 1 maximum likelihood estimates of the conditional class-membership probabilities, $\Pr(c \mid \mathbf{x}_i)$. Denote these $\Pr(c \mid \mathbf{x}_i)^{s1a}$.

At stage 2 one obtains the maximum likelihood estimates of the β_c, taking as given the $\Pr(c \mid \mathbf{x}_i)^{s1a}$, and using only the SP-choice data. At the end of sequential estimation one has estimates of all of the parameters in the joint model (the Pr(c), $\pi_{qs|c}$, and β_c) but these estimate are only consistent. They are not asymptotically efficient because none were estimated using all of the data. Put simply, they are not the parameter estimates that maximize the joint likelihood function, Equation 3.

In more detail, consider first the likelihood function for the attitudinal data:

$$L_a(\Pr(c), \pi_{qs|c}) = \prod_i \sum_{c=1}^{C} \left[\Pr(c) \prod_{q=1}^{Q} \prod_{s=1}^{S} (\pi_{qs|c})^{x_{iqs}} \right]. \qquad (14)$$

This likelihood function is developed, estimated, and explained in Morey et al. (2005). One maximizes Equation 14 to obtain the $\Pr(c)^{s1a}$ and $\pi_{qs|c}^{s1a}$. These estimates are then used to calculate conditional class-membership probabilities using Equation 7. Note these conditional probabilities are conditional on only the attitudinal data.

The likelihood function for the b$_c$ parameters, taking as given the $\Pr(c)^{s1a}$, and using only the SP-choice data, is

$$L_{sp}(\beta_c) = \prod_i i\left[\sum_{c=1}^{C} \Pr(c \mid \mathbf{x}_i)^{s1a} \prod_{k=1}^{K} \prod_{j=1}^{J} P_{jk|c}^{y_{ijk}} \right] \qquad (15)$$

That is, each individual's probability of choosing alternative *j* in choice-pair *k*, conditional on being a member of class *c*, is weighted by the stage 1 best estimate of the probability that the individual is in class *c*. The estimated stage 2 estimates of the β_c, β_c^{s2sp} are obtained by using Gauss to maximize $\ln(L_{sp}(\beta_c))$.

Note that one can use the sequentially estimated $\Pr(c)^{s1}$, $\pi_{qs|c}^{s1}$, and β_c^{s2} along with all of the data in Equation 9 to obtain initial estimated values for the conditional membership probabilities, $\Pr(c|\mathbf{x}_i,\mathbf{y}_i)$. Denote these $\Pr(c|\mathbf{x}_i,\mathbf{y}_i)^{\{0\}}$ because they can be used to as initial estimates for $\Pr(c|\mathbf{x}_i,\mathbf{y}_i)$ in the first step in the first iteration of FIML estimation.[7]

4 Application: preferences of Green Bay anglers

To show how the LC$_{AC}$ model can be implemented in practice, we apply the model to estimate preferences over the fishing characteristics of Green Bay, a large bay on Lake Michigan that is contaminated by PCBs. The goal is to characterize the preferences, and heterogeneity in those preferences, of anglers for the fishing characteristics of Green Bay. The site characteristics

[7]The sequential model could also be estimated in the opposite order with the indirect utility parameters estimated first.

examined are launch fees, catch rates by species (yellow perch, walleye, salmon, bass), and fish consumption advisory (FCA) levels. Anglers answered 15 likert-scale attitudinal questions and eight SP questions of the type: Would you rather fish Green Bay under conditions *A* or *B*? The attitudinal questions and an example choice question are included in the appendix. The target population is active Green Bay anglers who purchase Wisconsin fishing licenses in eight Wisconsin counties near Green Bay; most Green Bay fishing days are by these anglers. The sample consists of 640 anglers.

We first estimate the model using sequential estimation and then compare these results to those obtained using FIML.

4.1 Sequential estimation

4.1.1 Stage one

We start with sequential estimation because we use the sequential estimates to calculate starting values for the FIML estimation. To keep things simple, we assume only two classes; therefore, Pr(*class* 2)=1-Pr(*class* 1). The first stage estimates of the $\Pr(1)^{s1a}$ and $\pi_{qs|c}^{s1a}$ are obtained by maximizing Equation 14.

Table 1: Average Response to Attitudinal Questions by Class: Sequential vs FIML

	Sequential		**FIML**	
	FCA	**Perch/Walleye**	**FCA**	**Perch/Walleye**
Attribute Importance (5=Very Important)				
Catch:Bass	3.42	2.96	3.36	2.92
Catch:Perch	3.59	3.52	3.64	3.48
Catch:Trout/Salmon	3.17	2.59	3.13	2.52
Catch:Walleye	3.75	3.40	3.75	3.34
FCA:Bass	4.33	2.35	4.05	2.19
FCA:Perch	4.58	3.35	4.47	3.20
FCA:Trout/Salmon	4.34	2.60	4.08	2.46
FCA:Walleye	4.74	3.25	4.56	3.10
Fee	3.13	3.05	3.05	3.09
Amount Bothered (5=Very Bothersome)				
FCA: 1/week	3.94	2.61	3.81	2.46
FCA: 1/month	4.34	3.37	4.34	3.19
FCA: Don't eat	4.68	4.13	4.72	4.00
WTP Higher Fees: Higher Catch	2.84	2.83	2.92	2.78
WTP Higher Fees: No PCBs	3.69	3.17	3.75	3.04
Green Bay Quality	3.63	3.83	3.68	3.84

Columns 1 and 2 in Table 1 reports the average responses to the attitudinal questions for the anglers most likely to belong to each class. These average responses indicate an FCA class and a Perch/Walleye class. Table 1 shows that those in the FCA class are "more bothered" by FCA levels than those in the Perch/Walleye class. In addition, those in the FCA class stated that the FCA levels for all species were the most important factors in their choices; those in the Perch/Walleye class stated that the most important factors for them were the catch and FCA levels for Perch and Walleye. The probability of class membership for the FCA class using only the attitudinal data, $\Pr(1)^{s1a}$, is 0.296; that is, 29.6% of Green Bay anglers are predicted to be in the FCA class. This estimate is imposed as an assumed fixed value at the second stage of sequential estimation.

The 124 estimated response probabilities from stage 1 ($\pi_{qs|c}^{s1a}$) can be combined with the unconditional class membership probabilities and each individual's attitudinal data to estimate each angler's class membership probability conditional on his answers to the attitudinal questions, the $\Pr(1|\mathbf{x}_i)$ (Equation 7).[8] Most of these conditional estimates of membership put each individual into one of the classes with high probability; the maximum of the probabilities for the two classes is 90% or greater for 89% of the sample and effectively 100% for 69% of the sample.

4.1.2 Stage two

To estimate the second stage and obtain estimates for parameters in the indirect utility function, we assumed that the functional form of the deterministic part of the conditional-indirect utility function for a Green Bay fishing day is a linear function of the catch times for the different species, FCA levels, and the cost of a trip to Green Bay. Cost simplifies to only the launch fee because travel cost, for an angler, is always the same constant. In the Green Bay SP-choice pairs there were nine possible configurations of FCA levels. Each specified the level ("do not eat", "once a month", "once a week", no advisory") for each of the four species. Level one indicates PCB levels for which there is no health risk from consumption. Level nine is the most restrictive. Level four corresponds to current conditions on Green Bay. FCAs were considered the important policy variables and thus were allowed to vary among the classes. The perch and walleye species are also more important from a policy perspective and thus catch rates for these two species were allowed to vary between classes; catch parameters on bass and salmon were assumed to not be different. We assumed a logistic probability.

The second-stage estimates of the indirect utility parameter estimates, **b**, are reported in columns 2 and 6 in Table 2. These estimates are consistent with the first stage results; this provides evidence, and supports our assumption, that both the choice and attitudinal responses are manifestations of the same underlying preferences. In explanation, those in the Perch/Walleye class care more about the Perch and Walleye catch rates than do those in the FCA class; they get more disutility from increased catch-times. And, at every FCA level, the FCA class is more concerned about that FCA level than is the Perch/Walleye class.

At this point, we could stop: we have consistent estimates of all of the parameters and they could be used, for example, to obtain expected compensating variations for changes in FCA levels. However these estimates are not asymptotically efficient: all of the information/data was not used to simultaneously estimate all of the parameters.

One can also use the sequential estimates to calculate for each angler the probability of being in class 1 conditional on answers to both the attitudinal and choice data ($\Pr(1|\mathbf{x}_i,\mathbf{y}_i)$) using Equation 9. For 411 of the anglers, $\Pr(1|\mathbf{x}_i,\mathbf{y}_i)$ puts them in one of the classes with at least 90% certainty - a high degree of separation. It is of interest to compare the membership probabilities conditional on full information with those based on only the attitudinal data. Summarizing, for 629 anglers (98% of the sample), both of the conditional probabilities predict the same class (181 in the PCB class and 448 in the Perch/Walleye class). For these 629 anglers, the probability of class membership increases for 276 anglers when all of the data and preference parameters are

[8]For a Q level likert scale question, if Q-1 levels are estimated, the last level is implicitly known. Therefore, since there are 14 questions with four estimated levels and one question with six estimated levels, and two classes, there are 124 response probabilities total.

used to estimate class membership. Many of those that did not improve had $\Pr(1|\mathbf{x}_i,\mathbf{y}_i)$ of effectively zero or one, so there was no room for improvement. There are no examples where $\Pr(1|\mathbf{x}_i,\mathbf{y}_i)$ predicted class membership with high certainty and $\Pr(1|\mathbf{x}_i,\mathbf{y}_i)$ predicted membership in the other class with high certainty.

Table 2: Sequential and FIML Parameter Estimates (Est/SE)

Parameters	Sequential	FIML		Sequential		FIML		
		Iter 1	Iter 10	Converged		Iter 1	Iter 10	Converged
		FCA Class			**Perch/Walleye Class**			
Pr(class)	0.296	0.301	0.346	0.404				
Perch	-0.291	-0.311	-0.363	-0.378	-0.630	-0.632	-0.623	-0.623
	(-3.245)	(-3.646)	(-4.459)	(-5.029)	(-11.805)	(-11.883)	(-11.356)	(-10.884)
Walleye	-0.032	-0.033	-0.032	-0.031	-0.038	-0.038	-0.039	-0.04
	(-4.466)	(-4.785)	(-4.849)	(-5.261)	(-9.516)	(-9.449)	(-9.424)	(-9.401)
FCA2	-0.523	-0.513	-0.494	-0.484	-0.060	-0.057	-0.055	-0.03
	(-4.395)	(-4.540)	(-4.605)	(-4.802)	(-0.874)	(-0.841)	(-0.778)	(-0.404)
FCA3	-0.603	-0.572	-0.603	-0.602	-0.143	-0.141	-0.111	-0.084
	(-4.947)	(-4.970)	(-5.552)	(-6.008)	(-2.159)	(-2.125)	(-1.605)	(-1.168)
FCA4	-1.046	-1.02	-1.038	-1.051	-0.283	-0.281	-0.247	-0.178
	(-8.578)	(-8.972)	(-9.636)	(-10.448)	(-4.251)	(-4.209)	(-3.578)	(-2.468)
FCA5	-1.373	-1.367	-1.392	-1.325	-0.435	-0.422	-0.379	-0.345
	(-10.719)	(-11.313)	(-12.080)	(-12.424)	(-6.299)	(-6.127)	(-5.355)	(-4.687)
FCA6	-1.031	-1.028	-1.07	-1.035	-0.275	-0.268	-0.225	-0.178
	(-8.469)	(-8.901)	(-9.721)	(-10.128)	(-4.233)	(-4.127)	(-3.358)	(-2.545)
FCA7	-1.485	-1.467	-1.509	-1.456	-0.558	-0.539	-0.485	-0.451
	(-11.802)	(-12.615)	(-13.579)	(-14.124)	(-8.570)	(-8.303)	(-7.271)	(-6.463)
FCA8	-1.951	-1.956	-1.976	-1.949	-0.806	-0.78	-0.722	-0.639
	(-13.846)	(-15.031)	(-16.167)	(-17.439)	(-11.699)	(-11.419)	(-10.239)	(-8.706)
FCA9	-2.098	-2.088	-2.171	-2.205	-0.9	-0.889	-0.812	-0.699
	(-15.144)	(-16.063)	(-17.305)	(-18.891)	(-12.906)	(-12.777)	(-11.387)	(-9.428)
	Same Parameters for Both Classes							
Fee	-0.477	-0.478	-0.481	-0.48				
	(-15.324)	(-15.362)	(-15.396)	(-15.366)				
Salmon/Trout	-0.028	-0.028	-0.028	-0.028				
	(-7.913)	(-7.853)	(-7.759)	(-7.891)				
Bass	-0.032	-0.032	-0.032	-0.032				
	(-9.415)	(-9.341)	(-9.300)	(-9.341)				

4.2 FIML estimation

The $\Pr(1\,|\,\mathbf{x}_i, \mathbf{y}_i)$ from sequential estimation were used as starting values for FIML estimation using the E-M algorithm (Section 3.1). The six steps involved in each iteration were programmed in Gauss (step 5 using MaxLik).[9] . Convergence was achieved at 30 iterations.[10] Assuming the assumptions of our model are correct, the parameters from FIML estimation are asymptotically efficient, whereas those from sequential estimation are not. Two questions arise. (1) How does the characterization and sizes of the two classes estimated jointly with the attitudinal and choice data differ from the two classes estimated with only the attitudinal data? And (2), how, if at all, have the estimated preference parameters changed?

As can be seen by comparing columns 1 and 2 with columns 3 and 4 in Table 1, the qualitative characterizations of the two classes in terms of the average predicted responses to the attitudinal questions are the same whether those responses are estimated with the FIML or sequential approach: there is a FCA class and a Perch/Walleye class.

Table 2 reports estimates of the unconditional probability of belonging to the FCA class and the preference parameters from both sequential and FIML estimation. For FIML estimation, the results are reported after one iteration, after 10 iterations, and at convergence.

The first and most important thing to notice is that in going from sequential estimation to FIML estimation the probability of belonging to class 1, the FCA class, has increased from 29.6% to 40.4%. This is a 37% increase and quite substantial.

The second major observation is that the separation between the two classes increases in terms of the preference parameters: the FCA class remains effectively the same in terms of the preference parameters and the Perch/Walleye class's disutility from FCAs declines.[11] In explanation, the Perch/Walleye class has effectively the same catch-time parameters as in sequential estimation, but, for every FCA level, the estimated disutility had declined; in other words, the FIML Perch/Walleye class cares less about FCA levels than does the sequential Perch/Walleye class. Comparing the sequential and FIML estimates for the FCA class, FIML estimates indicate more disutility associated with increased perch catch-times (still much less than for those in the Perch/Walleye class) and no change in the estimate of the preference parameter for Walleye catch-times. The disutility associated with the different FCA levels change little, except for level nine where the estimated disutility is marginally higher.

The FIML parameters on fee and catch times for Trout/Salmon and Bass are the same as the sequential estimates.

Thinking in terms of willingness-to-pay for the absence of PCBs, and speaking loosely, for the FCA class, the FIML and sequential parameter estimates would generate almost the same willingness to pay per Green Bay fishing day for eliminating the need for the current FCA levels, but FIML estimates predicts a much larger FCA class. For the Perch/Walleye class both FIML

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[9]The code and data are available at xxxx

[10]Convergence was assumed when the probability of membership in Class 1 remained the same, rounded to the nearest percentage, for three iterations, and simaltaneously the estimates of the β parameters all changed by less than 1% of their value.

[11]Since the fee parameter essentially is the same in all models, this is equivalent to an analysis in terms of MRS.

estimated damages and the size of the class are smaller than those based on the sequential estimates.

Interestingly, the estimated asymptotic *t* statistics for all of the FCA class-specific preference parameters have all increased in absolute value, appearing to demonstrate that FIML estimation with all of the data has made these parameter estimates more efficient. However, the opposite has happened with all of the Perch/Walleye class-specific preference parameters, suggesting that the accuracy of the parameter estimates for this class have decreased. Perhaps the increased *t* statistics for the FCA class have more to do with its increased size than with FIML versus sequential estimation.

4.3 Discussion and possible extensions

This paper has two objectives. The first objective is to combine standard choice data with the answers to attitudinal survey questions to estimate preferences and preference heterogeneity. While both types of data are standard in surveys, they are not both typically used to estimate preferences. Our contribution is to jointly model answers to attitudinal and choice questions in the context of a discrete-choice, latent-class, random-utility model of preference heterogeneity. The results in our application indicate that answers to both the choice and attitudinal questions are coming from the same data generating process, supporting the underlying assumption of our proposed model.

The second objective is to compare a FIML model of attitudinal and choice data with a sequential model. In our application, the FIML and sequential estimation approaches give similar qualitative results. There are significant quantitative differences between the results of the two methods, however. The class sizes, the parameters in the indirect utility functions, and the significance levels all change, which impact elasticities, predicted choices, and willingness to pay measures. While sequential estimation is certainly much easier to perform, a joint FIML model is preferred on efficiency grounds. In addition, if one only oestimates a sequential model, one doesn't konw if the FIML estimates differ.

What lessons can we draw from this analysis? First, if you have two types of data generated by the same process, use both sets of data to get the best estimates. Second, modeling both types of data simultaneously will result in more efficient estimates than modeling both types of data sequentially.

There are a number of possible extensions that can be done with the presented model. It can be modified to use revealed-preference choice data or combined stated and revealed preference choice data. The number of classes can be estimated rather than assumed. The probability of class membership can be modeled as a function of observable covariates such as age or gender. Finally, the results from a LC_{AC} can be compared with those of a L_C model in order to examine the added value of attitudinal data.

Code and data to replicate, or extend, our results is available at xxxxx.

5 Attitudinal questions

1. On a scale from 1 to 7 where 1 means ”Much Worse” and 7 means ”Much Better”, how do you rate the quality of fishing on the water of Green Bay compared to other places you fish?

2. On a scale from 1 to 5 where 1 means ”Not at all Bothersome” and 5 means ”Very Bothersome”, answer the following question. For the fish you would like to fish for in the waters of Green Bay, how much would it bother you, if at all, if PCBs resulted in the following fish consumption advisories:
   (a) Eat not more than one meal a week.
   (b) Eat not more than one meal a month.
   (c) Do not eat.
3. On a scale from 1 to 5 where 1 means ”Strongly Disagree” and 5 means ”Strongly Agree”, how do you feel about each of the following statements about boat launch fees? If you don’t fish from a boat, please think of the daily boat launch fee as a fee you would have to pay to fish the waters of Green Bay.
   (a) I would be willing to pay higher boat launch fees if catch rates were higher on the waters of Green Bay.
   (b) I would be willing to pay higher boat launch fees if the fish had no PCB contamination.
4. On a scale from 1 to 5 where 1 is ”Not at all important” and 5 is ”Very Important”, when you were making your choices in $Q15$ through $Q34$, how important were each of the following?
   (a) The average catch rate for yellow perch
   (b) The fish consumption advisory for yellow perch
   (c) The average catch rate for trout/salmon
   (d) The fish consumption advisory for trout/salmon
   (e) The average catch rate for walleye
   (f) The fish consumption advisory for walleye
   (g) The average catch rate for smallmouth bass
   (h) The fish consumption advisory for smallmouth bass
   (i) Your share of the boat launch fee (or daily access fee if not fishing from a boat)

Figure 1: Example choice question

Figure 5-1
Example Choice Question

If you were going to fish the waters of Green Bay, would you prefer to fish the waters of Green Bay under Alternative A or Alternative B? *Check one box in the last row*

	Alternative A ▽	Alternative B ▽
Yellow Perch		
Average catch rate for a typical angler......................	40 minutes per perch	30 minutes per perch
Fish consumption advisory.......	No more than one meal per week	No more than one meal per week
Trout and Salmon		
Average catch rate for a typical angler......................	2 hours per trout/salmon	2 hours per trout/salmon
Fish consumption advisory.......	Do not eat	No more than one meal per month
Walleye		
Average catch rate for a typical angler......................	8 hours per walleye	4 hours per walleye
Fish consumption advisory.......	Do not eat	No more than one meal per month
Smallmouth bass		
Average catch rate for a typical angler......................	2 hours per bass	2 hours per bass
Fish consumption advisory.......	No more than one meal per month	Unlimited consumption
Your share of the daily launch fee............................	Free	$3
Check the box for the alternative you prefer.......................................	❑	❑

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A Hybrid Choice Model of Stated Preferences and Cognitive Perceptions about GM Food

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Abstract

Cognitive processes have been identified as important determinants of choices, but have not been explicitly and systematically integrated into economic models of non-market valuation and choice. We analyze Indian consumers' stated buying behavior of GM potato using an approach that integrates aspects of psychometric and econometric modeling. We measure two latent variables representing cognitive perceptions of gene technology in food, and integrate them into a Random Utility Model of product choice. We interpret the latent variables to represent two distinct cognitive/attitudinal orientations towards GM foods which we tentatively identify as risk/concern and benefit/progress. We estimate the model parameters using a simultaneous latent variable method. Interaction between intended purchase of GM potatoes, perceived benefits and risks of genetically modified food are examined. This modeling approach appears promising for exploring integrated behavioral models and for policy applications.

JEL Classification: **C12**, **C35**, **D12**

Keywords: Genetically Modified food, perceptions and economic choice, hybrid choice models, latent variable models

Introduction

Most previous empirical research on GM food can be broadly divided into two categories based on methods and conceptual framework: psychometric and econometric analysis. Psychometric studies primarily use correlation analysis to examine associations among variables believed to reflect and influence attitudes and cognitive perceptions towards GM food. "*Perceived risks*," which loosely represent potential losses and "*perceived benefits*," which loosely represent potential gains are often found to be main determinants of attitudes towards GM food (Bredahl). Attitudes towards nature (Frewer), attitudes towards technology (Sparks), attitudes towards science (Hoban), alienation from market place (Frewer, 96), trust in government and food industry (Gaskell) and other factors are linked to the perception of risks and benefits relevant to GM Food (see Verdurme and Viane, 2002 for a review of the literature on GM foods). In short, the psychometric analysis focuses on relationships among stated attitudes and cognitive perceptions and generally does not model the effect of these cognitive states and processes on economic behavior.

Econometric research on GM food usually applies econometric models to analyze the determinants of demand for GM food. This approach inquires directly into consumer choice and includes studies of revealed preferences (observed behavior) and studies of stated preferences (hypothetical behavior). In contrast to psychometric research, the cognitive underpinning of demand -- such as perceptions of risks and benefits of GM food -- are usually not explicit in the econometric models. Econometric models tend to reflect the economic point of view that takes the preference structure as fixed and given and assumes a rational choice process. Attitudes and perceptions are seen as internal factors within the cognitive black box. Formation of the contents of the cognitive black box is prior to economic theory and econometric models. While, some econometric studies (e.g., Li et al.) have included various cognitive variables as proxy variables, the cognitive variables are generally ad hoc or the underlying concepts or theories are not articulated explicitly.

Our objective is to integrate the psychometric and econometric approaches into a systematic empirical model. We believe this approach may be useful for a wide variety of inquiries into agent behavior. Here, we use the model to address the nature of food preferences in India over Genetically Modified (GM) versus non-GM foods. We propose a specific empirical approach which incorporates elements of both the cognitive "black box" and the demand-utility-choice approach of micro-economics. We model peoples' (cognitive) perceptions related to gene technology (psychometric approach) in food as one or more latent variables (factors) and incorporate these factors into a Random Utility Model (econometric approach). We infer latent factors representing these perceptions in part from responses to a series of questions regarding general cognitive perceptions about GM foods. Hence, we integrate a psychological-cognitive model of beliefs and attitudes into a utility-consumer model of rational choice to form a Hybrid Choice Model (Ben-Akiva et al. 2002). This hybrid choice model facilitates an examination of how cognitive perceptions affect stated demand for GM food and, in turn, are affected by individual personal characteristics. The key feature of the model is that it not only includes features of both psychometric and econometric models, it allows the two kinds of features to interact. We suggest that this approach opens a new research window on behavior.

Turning to the empirical topic of our paper, attitudes towards, and demands for, GM foods have been the subject of a great deal of recent research over many countries (Bredahl, Hoban 97, 99, Euro-barometer Studies, Quan Li et al., *inter alia*). India contains the world's second largest human populace, and is among the most culturally diverse countries. However, to the best of our knowledge, no published research has systematically examined Indian consumer perceptions and potential demand for GM Food. We apply our hybrid choice model to help understand Indian consumer attitudes potential demand for GM food.

This article is organized as follows. In section 2, we provide a brief statement about the purpose and place of a hybrid model of behavior. In section 3, we present a hybrid choice model of stated willingness to buy and latent perceptions. Section 4 summarizes the data used in the analysis, section 5 develops the estimation method and presents results, and section 6 concludes.

2. The place and purpose of a hybrid choice model

While methods for marketing, economic and policy surveys continue to improve, many scholars, especially economists, are troubled about the relationship between verbal responses and the "true" beliefs, values and preferences of the respondents. Thus, the Exxon Valdez oil spill in Alaska engendered a vigorous debate over whether stated preferences from surveys could be used to estimate meaningful economic values. Can stated preference studies such as those using the contingent valuation method (CVM) reveal "true preferences?" Preferences, values and attitudes lie buried in the subjective "mind;" they are ultimately not observable. Hence, one's view of whether and how "true preferences" can be revealed tends to reflect one of several fundamental points of view about the nature of mental states and choice (given that the ultimate entities are not sensible, the question of their existence is one of inference and even metaphysics). Opinions range from a belief that continued research into survey methods will lead to ever improved estimates of true preferences to the belief that verbal answers are inherently flawed reflectors of true values, to the belief by some that the very concept of a stable preference structure is misguided. We believe our empirical model provides a useful tool to explore questions about the nature of preferences and attitudes and the relationships between the categories, if any. To understand the place of the model in this discussion it is useful to consider different approaches to discovery of “true preferences” based on different conceptual models of choice, values, and attitudes.

First, consider the traditional micro-economic paradigm that takes preferences as real and given; agents possess an ex ante ordering (complete and transitive) of all potential choices (Preferences Theory). From this perspective, the task for the analyst is to reveal or discover these true preferences. While most economists probably hold this view, they may differ on the practicality of discovering true preferences. Some practitioners of stated preference approaches, such as the contingent valuation (CV) method, believe that continued research on survey tools will bring increasingly accurate representations of the utility function and associated demand equations. For examples, see the classic CV methods book by Mitchell and Carson (1989) or the Hanemann (1994) defense of the CV method.

Another group of scholars, including many economists, believes that, while stable and true preferences exist, methods which use stated preferences are unlikely to reveal them. To these scholars, an agent’s verbal responses are so entangled in noise, biases and strategic maneuvering that they are essentially useless for probing true preferences . Only actual behavior can be trusted to reveal “true” preferences. See, for example, Diamond and Hausman (1994), and articles in the anthology edited by Hausman (1993). Perhaps most economists fall somewhere along a continuum between the belief that noise, bias, and strategy make recovery of true preference very difficult to the hope that improved methods will bring us closer to uncovering truth (NOAA, 1993, Cummings et al, 1997).

A second fundamental paradigm holds that “true preferences” do not exist; the mental states and processes are too inchoate to be represented by orderly preferences. In this view, stated preferences, mental states and physical behavior are all constructed (label this Constructionist Theory). Thus, a paradigm that can be labeled "strong social constructionist" posits that all perceptions, attitudes and beliefs are constructed in the social world and reflect the agent's role as an actor in that world. In this view there is no grounding for preferences in the internal state of agents. Verbal responses on surveys and behavior in most social situations are situational. Survey responses merely reflect survey formulation and the social context of the interview. Change the survey parameters or the social setting, and one gets an entirely new set of responses. Agents have no internally coherent cognitive structure to ground their behavior. Interestingly one can see this paradigm emerging from two rather disparate schools of social scientists – the social constructionists cited above and the behaviorists. The behaviorist psychologists of the first half of the 20th century treated the mind as a black box. Agents have a few innate drives (for survival or reproduction) and these drives ground responses to external stimuli which then become established stimulus-response behavioral patterns. This simplistic view admits no systematic evaluation, coordination, or forethought in determining choices and actions. We presume this nihilistic theory is invalid, but it serves as a useful alternative hypothesis.

A third general paradigm (label this Beliefs Theory) supposes that mental states and processes are real and behaviorally effectual, but that they are messier than the buried, but exact and stable, preference structure imagined in micro theory. Suppose that agents possess a set of judgments over objects, attributes, and categories of objects (where objects include actions and processes) rather than a set of distinct ordered values over every discrete potential choice. Label these assessments or judgments *attitudes*. Attitudes may have affective (emotional, e.g., feared or pleasing) or cognitive-normative (i.e., good or bad) components[1]. Also, the agent's cognitive structure includes perceptions; where here we refer to cognitive perceptions rather than simple, direct sensory experience. Cognitive perceptions are mental constructs grounded in sensory data about the real world but which are interpreted through a cognitive lens. Cognitive perceptions are intertwined with attitudes and also reflect learned and innate human cognitive constraints and heuristics. We include both a priori perceptual frameworks and current perceptions under the cognitive perception rubric. Roughly, attitudes and perceptions correspond to the epistemological categories of normative and empirical knowledge; however, practical human knowledge does not cleanly separate normative knowledge from positive knowledge. We will refer to attitudes and perceptions jointly as beliefs. (For examples of ideas like those outlined in this paragraph, see the survey by Chaiken and Stangor, 1987.)

In the attitudinal world, agents have beliefs over individual objects as well as domains and classes of objects and characteristics. These attitudes and perceptions are arranged hierarchically around some very stable core beliefs. In fact, cognitive dissonance/consistency theory suggests that new information that contradicts a core belief (perception or attitude) will be rejected or modified. While core beliefs may be very stable, attitudes towards specific objects in a single choice event may be extremely labile. Hence, preferences as understood by economic theory are tentative and context dependent. Thus, Agent Betty may have a stable liking for chocolate over strawberry, low fat over high fat, rich creamy taste over watery taste. However, which ice cream she will buy at the grocery store on a given shopping event may be contingent on many small factors and will be somewhat "noisy" and unstable. It will be an instantaneous

[1] While attitudes ground behavior, they may also be modified by behavior. Hence, the relationship between attitudes and behavior varies among theories of attitudes and attitude change.

judgment as to the agent's best choice. We would not expect the choice to be stable and invariant as minor circumstances vary.

Our purpose is not to decide in favor of one of these paradigms, though we consider the constructionist theory too simplistic to hold except for comparison tests. Rather, we observe that the existence of these three (plus variations) paradigms raises questions and provides insight into a number of interesting theoretical and practical issues about preferences, values and beliefs. We think consideration of these questions and insights motivates development of an empirical model that will facilitate exploration of the issues. Such a model should allow both an unobserved deep cognitive structure of attitudes and perceptions and an unobserved specific set of preferences over commodities at a specific time and place. Our hybrid model does not choose between the Beliefs Theory or the Preferences Theory. While the inclusion of a latent beliefs cognitive structure is obviously compatible with the Beliefs Model, it may also be consistent with a "weaker" Preferences Theory. For example, if "real preferences" exist, but are difficult to detect, attitudes may serve as proxies for a partially known class of preferences. In this interpretation preference orderings would be knowable in theory but difficult to detect because of the noise and heuristics which frame the choice. In this interpretation "attitudes" are not real, but are inexact proxies of the deep real structure that we cannot directly observe and may never exactly know. In contrast, under the Beliefs Theory, measures of attitudes and perceptions are measures of real, though latent, cognitive entities. We will discuss briefly the implications and uses of the framework in the conclusions.

3. Model development: Simultaneous Latent Variable framework

The hybrid choice model takes the form of a simultaneous latent variable model (SLVM) with two basic components: a structural component which corresponds roughly to the traditional econometric approach, and a measurement component more like those common in the psychometric literature. The measurement component identifies and measures the latent variables of our model. The structural model estimates how the latent and observable factors influence choice of GM versus non-GM foods.

We hypothesize three latent variables in our model. One latent variable represents the conditional relative ***utility*** received from otherwise similar GM and non-GM foods. This is a variable which is implicit or explicit in econometric models. Here it will be defined as part of a random utility model (RUM), an approach commonly used for problems of discrete choice. More specifically, this latent variable is the *utility difference* (η_u). This utility difference is an integral component of RUM models generally, and is reflected through buying or not buying GM food. We can interpret this utility difference in either the strict rational choice story of stable ex ante preferences, or as a transitory judgment of the relative values of the alternatives under current circumstances (survey wording, interview setting, etc.) under the Beliefs Theory.

The other two variables are the latent cognitive perception variables that were discussed above. For expository purposes these can be labeled generalized *perceived risks* (η_r) and generalized *perceived benefits* (η_b) of GM foods. The reader should be advised that the terms risk and benefits have specific meanings in the context of the model, meanings more consistent with usage in the psychometric than the econometric literature. Provisionally, the term "perceived risks" denotes the agent's ex ante cognitive perception (belief) over a class of objects (GM foods) comprising an agent's beliefs about potential losses (including general social losses as well as agent specific losses) resulting from the existence and consumption of GM foods, the probability that such losses will occur, and the subjective significance of those losses. The term "subjective significance" reflects features of belief found in the psychological and social literatures whereby the value of a characteristic depends on some features other that the mean and variance of an expected event matter to individuals. Thus, some events evoke "dread" and some circumstances create subjective over and under estimates of the probability or the extent of expected damage from a loss. The so-called "irrational fear" that many people have of nuclear power illustrates. Similarly, (and provisionally) the term "perceived benefits" denotes the agent's ex ante cognitive perceptions of beneficial attributes over a class of objects (GM foods) comprising an agents beliefs about expected gains from the existence and consumption of GM foods (including social gains as well as agent specific gains) and their associated subjective attributes and probabilities. Conceptually, these beliefs comprise generalized "priors" over the object class based on (limited) ex ante information. Since these variables are not directly observable, the interpretation given above is necessarily provisional. Ultimately no interpretation can reach the status of demonstrable truth, as we are trying to peer inside the inaccessible "black box" that is the brain. Our interpretation of the latent variables will hinge on how well the empirical findings "fit" the conceptual model. In summary, (η_r) and (η_b) are latent variables that underlie and inform responses to questions about attitudes towards the characteristics of GM foods.

In conjunction with the measurement component, the structural component of the model estimates the relationship between the observed individual characteristics and the endogenous latent perception factors.

2.1 Determinants of latent factors

Following the modified standard RUM (Random Utility Model) framework (see, for example, Haab & Mcconnell 2002), let an individual's utility associated with GM food and its regular alternative be characterized as

$$U_{GM} = \gamma_{GM}\mathbf{X_u} + \beta_b\eta_{b,GM} + \beta_r\eta_{r,GM} + \theta_{GM}(M - P_{GM}) + \xi_{GM} \quad (2.1.1)$$

$$U_R = \gamma_R\mathbf{X_u} + \theta_R(M - P_R) + \xi_R \quad (2.1.2)$$

where **X** is a (KX1) vector of explanatory variables representing demographic and psychological characteristics of individual consumers and γ_j is the (1XK) dimensional vectors of associated parameters. η_b and η_r are continuous latent variables (factors), representing perceived benefits and perceived risks associated with GM potatoes, and β_b and β_r are associated parameters. M is the individual's income, θ_j is the parameter associated with income. $\eta_{b,R}$ and $\eta_{r,R}$ are standardized to zero. Equivalently, the perceived risks and benefits of regular potatoes are common with GM potatoes. ξ_{GM} and ξ_R are disturbances. The usual RUM assumption is that an individual knows her own utility completely, and the indeterminacy is due to researchers' inability to observe an individual's utility function fully.

Let $\eta_u = U_{GM} - U_R$ be the indirect utility differential between GM potato and its regular alternative. We assume that marginal utility of income remains the same over the two alternative situations, so $\theta_j = \theta_{GM} = \theta$. Furthermore, the survey design data for this study assumes $P_{GM} = P_R$. Based on 2.1.1 and 2.1.2 the utility differential is

$$\eta_u = \boldsymbol{\gamma_u}\mathbf{X_u} + \beta_{ub}\eta_b + \beta_{ur}\eta_r + \zeta_u \,, \quad (2.1.3)$$

where $\boldsymbol{\gamma_u} = (\gamma_{GM} - \gamma_R)$ and $\zeta_u = \xi_{GM} - \xi_R$.

The structural equation part hypothesizes that η_b and η_r are functions of observable personal characteristics $\mathbf{X}_b$ and $\mathbf{X}_r$ respectively, and may influence each other:

$$\eta_b = \beta_{br}\eta_r + \boldsymbol{\gamma}_b\mathbf{X}_b + \zeta_b \tag{2.1. 4}$$

$$\eta_r = \beta_{rb}\eta_b + \boldsymbol{\gamma}_r\mathbf{X}_r + \zeta_r \tag{2.1. 5}$$

where ζ's are assumed to follow a (possibly bivariate) standard normal distribution and the vectors $\mathbf{X}_u$, $\mathbf{X}_b$ and $\mathbf{X}_r$ may share some common elements. An exact detail of **X** is given in the next section. . Combining 2.1.3 to 2.1.5 in matrix notation provides

$$\boldsymbol{\eta} = \mathbf{B}\boldsymbol{\eta} + \boldsymbol{\Gamma}\mathbf{X} + \boldsymbol{\zeta}, \tag{2.1.6}$$

where

$$\boldsymbol{\eta} = \begin{pmatrix} \eta_u \\ \eta_b \\ \eta_r \end{pmatrix}, \quad \mathbf{B} = \begin{pmatrix} 0 & \beta_{ub} & \beta_{ur} \\ 0 & 0 & \beta_{br} \\ 0 & \beta_{rb} & 0 \end{pmatrix}, \quad \mathbf{X} = \begin{pmatrix} \mathbf{X}_u \\ \mathbf{X}_b \\ \mathbf{X}_r \end{pmatrix}, \quad \boldsymbol{\Gamma} = \begin{pmatrix} \boldsymbol{\gamma}_u \\ \boldsymbol{\gamma}_b \\ \boldsymbol{\gamma}_r \end{pmatrix}, \quad \boldsymbol{\zeta} = \begin{pmatrix} \zeta_u \\ \zeta_b \\ \zeta_r \end{pmatrix}.$$

To the extent that the perceived risk factor accurately represents perceptions of the likelihood and scale of a possible bad outcomes from the existence and consumption of GM foods relative to the non-GM foods, the sign of its effect η_u, is expected to be negative ($\beta_{ur} < 0$). Similarly, to the extent that the perceived benefit factor represents the likelihood and scale of positive outcomes from consuming GM rather than non-GM food, β_{ub} is expected to be positive. The expected sign of β_{br} and β_{rb} is more ambiguous. Past research on perceived risk and benefit suggests an inverse association between these two perceptions (Alhakami & Slovic, 1994; Lloyd, Hayes, Bell, & Naylor, 2001; Siegrist, 1999; Zajonc, 1980; Slovic et al. 2002). One interpretation of this research comprises the common sense notion that risks and benefits are simply negative and positive values along a single dimensional metric. Under this interpretation, there should only be one latent variable measuring risk versus benefit along a single scale from negative to positive. However, in principle, there are any number of dimensions along which respondents might perceive and assess the qualities of foods, each with different affective or normative values. In our study we can estimate as many independent latent variables as we have empirical indicators – in our example up to six. Hence, we can infer the number of assessment/perceptional dimensions from the data. We hypothesize two latent variables representing two perceptual categories (discussion below).

2.2 Effects of latent factors on stated perceptions and preferences

Variables η_b, η_r and η_u are unobserved, and in this modeling context are inferred from a larger set of stated preferences and perceptions. To generate an index representing these underlying latent factors, we use confirmatory factor analytic (CFA). Following a standard RUM framework, an individual's stated intention of buying GM food instead of a non-GM counterpart for a given price ratio is used as the indicator for $\eta_{u,}$ such that Y_u=1 when η_u >0 and Y_u = 0 otherwise. For η_b and η_r, we have survey responses relating to six possible characteristics of GM foods, which we will use to infer the latent perceptions. Responses to the survey questions were measured on a Likert scale with 5 categories. The latent variables η_b and η_r are hypothesized to be common factors that underlie the responses to a series of questions about the characteristics of GM foods. In our survey, the responses $\mathbf{Y_b}$ and $\mathbf{Y_r}$, characterizing η_b and η_r respectively, are ordered categorical variables.

Because the survey responses Y_u, $\mathbf{Y_b}$ and $\mathbf{Y_r}$ are categorical in nature, we proceed by hypothesizing corresponding continuous latent variables $\mathbf{Y}^* = [Y_u^*, \mathbf{Y}_b^*, \mathbf{Y}_r^*]$, which in turn are related to the latent common factors $\boldsymbol{\eta}$ and individual specific noise $\boldsymbol{\varepsilon}$ such that

$$\mathbf{Y^* = \Lambda\eta + \varepsilon} \tag{2.1.7}$$

where,

$$\mathbf{Y}^* = \begin{pmatrix} Y_u^* \\ \mathbf{Y}_b^* \\ \mathbf{Y}_r^* \end{pmatrix},\ \boldsymbol{\Lambda} = \begin{pmatrix} \Lambda_u & 0 & 0 \\ 0 & \boldsymbol{\Lambda}_b & 0 \\ 0 & 0 & \boldsymbol{\Lambda}_r \end{pmatrix},\ \boldsymbol{\eta} = \begin{pmatrix} \eta_u \\ \eta_b \\ \eta_r \end{pmatrix},\ \text{and}\ \boldsymbol{\varepsilon} = \begin{pmatrix} \varepsilon_u \\ \boldsymbol{\varepsilon}_b \\ \boldsymbol{\varepsilon}_b \end{pmatrix},$$

where $\boldsymbol{\varepsilon} \sim N(\mathbf{0},\boldsymbol{\Theta})$, and $\boldsymbol{\Theta}$ is a covariance matrix of dimension equal to the number of indicator variables in $\mathbf{Y}^*$. $\boldsymbol{\Lambda}$ is the loading vector representing regression coefficients between the independent latent factors and the dependent indicator variables. Rearranging 2.1.6 leads to

$$\mathbf{(I - B)\eta = \Gamma X + \zeta} \tag{2.1.8}$$

$$\mathbf{\eta = (I - B)^{-1}(\Gamma X + \zeta)} \tag{2.1.9}$$

Substituting $\boldsymbol{\eta}$ in 2.1.11 provides the reduced form

$$\mathbf{Y}^* = \mathbf{\Lambda((I - B)^{-1}(\Gamma X + \zeta)) + \varepsilon} \tag{2.1.10}$$

$$= \mathbf{\Lambda(I - B)^{-1}\Gamma X + \Lambda(I - B)^{-1}\zeta + \varepsilon} \tag{2.1.11}$$

$$= \mathbf{\Pi(q)X + \delta} \tag{2.1.12}$$

where $\mathbf{\Pi(q) = \Lambda(I - B)^{-1} \Gamma}$ are reduced form parameters, and $\boldsymbol{\delta} = (\mathbf{\Lambda(I - B)^{-1}} \boldsymbol{\zeta} + \boldsymbol{\varepsilon})$ is the reduced form disturbance matrix distributed $\mathbf{N(0, \Sigma^*)}$ with

$$\mathbf{\Sigma^* = \Sigma^*(q) = \Lambda(I - B)^{-1} \psi (I - B)'^{-1} \Lambda' + \Theta_\varepsilon} \qquad (2.1.13)$$

where $\mathbf{\Psi}$ is the factor covariance matrix and $\mathbf{\Theta_\varepsilon}$ is error covariance matrix.

With Categorical indicator variables **Y**, the scale of the latent variables $\mathbf{Y}^*$ is indeterminate (Madalla and Lee 1976), and a standardization is therefore required. let $\mathbf{Y}_s^* = \nabla \mathbf{Y}$ where ∇ is a diagonal matrix with diag(∇) = [diag(V($\mathbf{Y}^*$|$\mathbf{X}$))]$^{-1/2}$. Therefore,

$$\mathbf{E(Y_S^* \mid X) = \mu_S^*(X) = \nabla \mu^*(X) = \nabla \Pi(q) = \nabla E(Y^* \mid X)} \qquad (2.1.14)$$

$$\mathbf{V(Y_S^* \mid X) = \Sigma_S^* = \nabla \Sigma^* \nabla = \nabla V(Y^* \mid X) \nabla} \qquad (2.1.15)$$

$\mathbf{Y}^*$ contains variable in their original metric while $\mathbf{Y_S^*}$ is standardized to unit variance. The model requires the estimation of the elements of **q** and ∇ . These scaling parameters (∇) can be estimated in two ways (appendix 2).

3. Survey methods and data description

A survey was conducted with face-to-face interviews at food markets in Calcutta (now Kolkata) and Bangalore during August and September of 2004 by employees of a Non Governmental Organization from Bangalore and the Communication Department of Jadavpur University, Kolkata. Whereas it is now standard practice to offer incentives to respondents, in India, paying survey respondents for survey participation is not customary and no remuneration or gifts of any kind was offered to the respondents. The survey provided 240 usable observations, each representing one individual.

The survey focused on two GM commodities. In this paper we deal with the responses related to GM potatoes. The GM potato is offered as an alternative to a non-GM potato with otherwise similar tangible characteristics. The survey contains six risk and benefit characterizing questions [generating indicators **Y**], where three concerned hypothesized risk/concern [indicators Y_1, Y_2, Y_3]', and three concerned potential benefits [indicators Y_4, Y_5, Y_6]'. Another question asks whether the respondent would buy a GM food or a similar non-GM food given the same price. These variables are described more completely in Tables 1a, with summary statistics in table 1b.

The risk-related indicator questions [Y_1, Y_2 and Y_3] relate to the likelihood of exacerbated food allergies from GM potatoes, other unforeseen long-term health hazards from GM foods, and environmental damage from GM food production or consumption. The benefit related indicator questions [Y_4, Y_5 and Y_6] relate to the possibility of higher nutrition value from GM foods, improved storage characteristics, and less pesticide usage for GM foods.

Explanatory variables $\mathbf{X_u}$, $\mathbf{X_b}$, and $\mathbf{X_r}$ are exogenous personal characteristics that are hypothesized to affect the utility differential, benefit perceptions, and risk perceptions, respectively. These variables are described in table 2a, with summary statistics presented in table 2b. These variables include standard demographic, household, and shopping characteristics of individuals as well as variables that characterize their general perceptions of science and technology. Specifically, the vectors of explanatory variables that we arrived at through preliminary regressions are

$\mathbf{X_u}$ = *income, employment, BioSci, GMheard, BuyNewFood, NoNewTrust.*

$\mathbf{X_b}$ = *Gender, Mainshopper, income, employment, education, BioSci, age, FoodShopFreq, HuRight, Techprog, RedFooProb.*

$\mathbf{X_r}$ = *Gender, Mainshopper, income, employment, education, BioSci, age, FoodShopFreq, ModEnv, Techprog, RedFooProb.*

4. Estimation

For estimation of the parameters of this hybrid choice model we fit the Mean and Covariance adjusted Weighted Least Square Estimator (WLSMV) developed in a series of papers by Muthen (1983, 1984, 1995, 1997)[2]. WLSMV is a minimum distance estimator (MD) based on minimizing the weighted squared distance between the unrestricted reduced form parameters and the (restricted) structural parameters in an over-identified system (Cameron and Trivedi, 2005). Estimation was carried out using MPlus, Version 3.13, and performed in three steps.

Step 1. The first step estimates the reduced form quantile regressions using an iterative quasi-maximum likelihood approach. Recall that the dependent variables $\mathbf{Y}$ in the reduced form regression are categorical; binary in the case of $\mathbf{Y}_u$ and ordinal with with 5 categories in the case of $\mathbf{Y}_b$ and $\mathbf{Y}_r$. Let $\hat{\boldsymbol{\kappa}} = \left(\hat{\boldsymbol{\Pi}} \quad \hat{\boldsymbol{\Sigma}}\right)'$, where $\hat{\boldsymbol{\Pi}}$ is the reduced form parameter vector obtained by maximizing a univariate conditional likelihood probit regression for Y_u and ordered probit regressions for each element of $\mathbf{Y_r}$ and $\mathbf{Y_b}$), on every $\mathbf{X}$. The resulting parameter estimate vector $\hat{\boldsymbol{\Pi}}$ contains a parameter for each explanatory variable in $\mathbf{X}$ as well as intercept (threshold) parameters (one for the binary probit regression and 4 each for the six ordered probit regressions). The variances of the disturbance of the categorical variables are standardized to one. The correlation estimates in $\hat{\boldsymbol{\Sigma}}$ are then computed by maximizing a bivariate likelihood

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[2] Similar approach could be found in Brown & Arminger (1995), Joreskog on Structural equation modeling with ordinal indicators.

function for each pair of regressions conditional on $\hat{\mathbf{\Pi}}$, resulting in 7(7-1)/2 sets of bivariate probit regressions. The assumption of conditional multivariate normality gives us univariate as well as bivariate conditional normality for the $\mathbf{Y}^*$ variables. Additional details are provided in Bhattacharjee (2005).

Step 2. A weighting matrix $\mathbf{W}$ is defined as an estimate of asymptotic covariance matrix of $\hat{\boldsymbol{\kappa}}$, and is estimated as the block diagonal matrix,

$$\hat{\mathbf{W}} = \begin{pmatrix} \tilde{\mathbf{V}}\left(\hat{\mathbf{\Pi}}\right) & \mathbf{0} \\ \mathbf{0} & \tilde{\mathbf{V}}\left(\hat{\mathbf{\Sigma}}\right) \end{pmatrix},$$

which is generated from estimates from step 1.

Step 3. Parameter estimates $\hat{\boldsymbol{\kappa}}$ from step 1 are the unconstrained reduced form parameter estimates of $\boldsymbol{\kappa}(\mathbf{q})$. The structural parameters $\mathbf{q}$ are obtained by minimizing a function of the discrepancy between the vector of reduced form estimates $\hat{\boldsymbol{\kappa}}$ and the vector of constrained estimate $\boldsymbol{\kappa}(\mathbf{q})$:

$$\underset{\mathbf{q}}{\text{Min}}\; F_{WLS} = \left(\hat{\boldsymbol{\kappa}} - \boldsymbol{\kappa}(\mathbf{q})\right)' \mathbf{W}^{-1}\left(\hat{\boldsymbol{\kappa}} - \boldsymbol{\kappa}(\mathbf{q})\right) \qquad (4.5)$$

Optimization is carried out through the iterative quasi-Newton method. A robust covariance matrix for the estimated parameter vector $(\hat{\mathbf{q}})$ is

$$\mathbf{AsyV}(\hat{\mathbf{q}}) = \mathbf{n}^{-1}(\boldsymbol{\Delta}'\mathbf{W}^{-1}\boldsymbol{\Delta})^{-1}\boldsymbol{\Delta}'\mathbf{W}^{-1}\boldsymbol{\Gamma}\mathbf{W}^{-1}\boldsymbol{\Delta}(\boldsymbol{\Delta}'\mathbf{W}^{-1}\boldsymbol{\Delta}), \qquad (4.6)$$

where $\boldsymbol{\Delta} = \partial\boldsymbol{\kappa}(\mathbf{q})/\partial\mathbf{q}$. We report four commonly used goodness-of-fit indices, and with categorical outcomes, rule-of-thumb indications of good fit are, TLI (Tucker Lewis Index) > 0.95, CFI (Comparative Fit Index)>0.95, RMSEA (Root Mean Square Error of Approximation) <0.05 (Browne and Cudeck, 1993) and WRMR (weighted Root Mean Square residual)<0.90.

5. Results

Using theta parameterization (appendix 2 for details) fit indices of our primary (*unrestricted*) model are TLI = 0.853, CFI = 0.882, RMSEA= 0.045 and WRMR = 0.838. Three of four fit-indices suggest a relatively poor fit.

The coefficients of the structural model are reported in table-4A. As hypothesized, the factors representing perceived benefit and perceived risk have significant positive (.801) and negative (-1.264) impact respectively on willingness to buy GM potatoes. The coefficient on *BuyNewFood* is positive (0.33) and significant, suggesting that people with a tendency to try new foods also tend to be more inclined to buy GM. Lack of trust in new food *(NoNewTrust*) has a negative (-0.321) and significant impact on stated demand for GM potato. The effect of income on the propensity to buy GM potatoes is positive but weak, a result consistent with previous studies (Li, 2002). Individuals who think that humans have the right to use nature to serve their own purpose (*HuRight*) tend to perceive more benefit in gene technology in food as well. Also those who think that for India, greater technological progress should be pursued even at the cost of the environment (*TechProg*) tend to perceive more benefit in GM food. *Gender*, *age*, the number of times an individual consumes potato (*PotEatFreq*), and the number of times an individual shops for food (*FoodShopFreq*) have insignificant effects on perceived benefit.

Table 4A also shows that main grocery shopper of the household (*MainShopper*) tends to perceive that GM foods are riskier than non-GM food. Individuals who think modifying the environment for human may alter the balance of nature (*ModEnv*) tend to perceive GM potato as riskier. The more often a person shops for food (*FoodShopFreq*), the less is her perceived risk. None of *gender*, *education*, *BioSci*, *employment*, *age* or *PotEatFreq* influences perceived risk significantly. In earlier works (Gaskell 2002), found “gender” to be significant.

Overall, Table-4A results indicate that variables which have significant effects on one perception factor do not influence the other perception factor significantly. This finding supports the idea that risk and benefit perceptions are not themselves determined by a common underlying factor – simply opposite directions in a common metric.

Recall that we had built both our survey and our model around a presumption of two cognitive perceptual entities which we label perceived risks and perceived benefits. However, in the empirical work we treated the existence of two latent variables as an hypothesis and the relationship between the indicators and the latent variables as hypothesis as well. Hence, we checked for the number of latent variables and the influence of indicator variables. We performed an exploratory factor analysis (EFA). We ran two EFA models, one assuming only one factor and the other one assuming two factors. In both cases we used all six possible indicator variables [**Y**]. In the one factor model we do see that the three risk indicators have opposite signs from the three benefit indicators, lending some support to the one metric, two poles hypothesis. However, from the respective fit indices, we can see that the two factors model fits our data significantly better (table 3A and 3B). However, table-3B also shows some possibility of cross or multiple loading such that any particular indicator (**Y**) significantly represents more than one common factor (latent variable). As a result of this analysis, we found that Y_1, Y_2, Y_4 and Y_6 each are significant indicators of the factors we have labeled perceived risk and perceived benefit. In the unrestricted model Y_3 is omitted from the benefits indicators and Y_5, is omitted from the risks indicators, referring to reductions in pesticide for GM crops and general health issues respectively. In order to confirm the possibility of multiple loading we check the results of measurement models.

Coefficients of the measurement model are reported in the third column of table 4B. The loading parameter is the regression coefficient between the independent latent continuous factor, i.e. our construct variables η's and its associated dependent latent indicators (Y^*).[3] Table 4B shows that as the value of (*GMPallergy*), (*GMPother*) and (*GMPeco*) increases, the perceived risk factor increases. Similarly, when (*GMPvita*), (*GMPstor*) and (*GMPpesti*) increases, the perceived benefit factor increases. Point estimates in Table 4A also shows that risk perception is negatively influenced by benefit perception ($\beta_{rb} = -0.286$), and benefit perception is positively influenced by risk perception ($\beta_{br} = 0.235$). However, neither estimate is statistically significant, suggesting again that risk perceptions and benefit perceptions are largely independent (though at this stage largely by construction).

To further illustrate the relationship between the two perception variables and willingness to buiy GM foods, we calculate factor scores (estimated value of η_r and η_b)[4] and plot them along with willingness to buy responses. In Figure1 we sort observations such that benefit factor scores are in descending order, and plot benefit, risk, and utility difference factors. The graph shows a positive relationship between perceived benefit and willingness to buy GM food, but no clear relationship between the benefit and risk factors – as one expects by construction. Similarly, observations were sorted by estimated risk factor scores. Figure 2 shows a clear negative

[3] When we fix a factor loading equal to 1 for one of the indicator then the estimate results are "un-standardized", the scale (and thus variance) of the factor is similar to that of the variable it was identified with, i.e. a one-unit increase in the factor, results in a one-unit increase in the observed variable. Results of "un-standardized" are presented in third column of table 4B. However, irrespective of which observed variable has loading fixed to 1, the "standardized estimates" (standardized using the variances of the η's,) created from the un-standardized estimates will be the same, though the un-standardized estimates will change depending on which loading is fixed to 1 (see appendix 6 for exact formulation of standardized coefficient). Standardized coefficients of "measurement model" are reported in column 4 of table 4B. (For exact derivation of standardized estimates, see Chapter 3, MPlus Technical Appendix, http://statmodel.com/download/techappen.pdf)

[4] Calculation of factor score:
MPlus Technical Appendix, Chapter 11, http://statmodel.com/download/techappen.pdf

relationship between perceived risk and willingness to buy GM food, but no clear relationship between the benefit and risk factors – as one expects by construction.

Many of the parameters associated with the explanatory variables in Table 4 are insignificant. In search of a more parsimonious model, we employ chi-square difference testing. Table 5(A-C) reports the results of a *restricted* model, after having dropped the weak explanatory variables from estimation. Under the *restricted* model β_{rb} = -0.406 and β_{br} = 0.597. In contrast to the unrestricted model, the values of β_{br} and β_{br} are larger in absolute value and significant under model restriction, whereas they were both insignificant in the previous model. This result suggests that there might be some common psychological antecedent influencing both risk and benefit factors as modeled in this paper.

Conclusions

Ben-Akiva et al (2002) discuss the development of predictive choice models that go beyond the archetypal random utility model. They incorporate several elements of cognitive process that have been identified as important to the choice process, including strong dependence on history and context, perception formation, and latent constraints. This study contributes to this ongoing strain of research that attempts to systematically integrate psychometric and econometric methods, although here the effort is at the empirical rather than theoretic level. The simple brute fact that we were able to make this model "work" quite successfully on a set of empirical data is suggestive for theory. One must suppose that either the weaker "Preference Theory" or the "Beliefs Theory" better fit reality than the nihilistic "Constructionist Theory." (Recall that in the Preference Theory, the estimated latent variables are proxies for a "gestalt" of the underlying preferences, whereas in the Beliefs Theory, the estimated latent variables are estimates of real, cognitive entities.)

More generally, we believe that the model developed here offers several practical advantages and research opportunities. In some respects this model just adds an intervening layer between the usual exogenous variables and the dependent variable(s) in the empirical models used to analyze stated preferences from contingent valuation (CV) method surveys. In this study, we analyzed the choice between two substitute goods (GM versus non-GM versions of the same food). With data on price differentials, the model can be extended to numerical estimation of willingness to pay. Hence the model can be used in all the ways estimates of value from CV studies are customarily used; for example one could calculate demand elasticities and estimate welfare measures.

Where the hybrid model is used for estimation of WTP values, one might ask what it offers that existing models do not already do; the hybrid model just adds more structure. In one sense then, the hybrid model is more restrictive than usual empirical models. We argue that, in return for accepting this restriction, the hybrid model provides potential benefits. Of course, if the restriction accurately represents the real world, estimates with the restricted model are more efficient. Another benefit is that the hybrid model provides a systematic framework to suggest some additional explanatory variables which could enrich WTP models. It provides a logical structure to enter additional informational variables and additional affective and normative variables bearing on perceptions and attitudes. In short, hybrid models give us a systematic logic for attitude and perception variables that otherwise tend to be ignored in the theoretic development and then appear as ad hoc entries to the empirical model.

By providing a structure for simultaneously exploring attitudes, perceptions, and economic choices, the hybrid choice model also presents a venue for developing and testing theories of human behavior. The award of the 2002 Nobel Memorial Prize in Economics to Daniel Kahneman and Vern Smith illustrates a recent blossoming in cross-fertilization between social psychology and experimental economics and the hybrid choice model seems a potentially valuable tool to research in this area.

Finally, the hybrid choice model offers a practical, policy relevant advantage over current econometric models of stated preference data. The unit WTP values and aggregate welfare estimates from current studies are often used to inform public decisions, sometimes formally through inclusion in benefit cost studies. However, many times it is only the qualitative results that matter. Many policy decisions can be informed as well by information on attitudes and beliefs as by numerical estimates of economic value. In fact, formal benefit-cost analysis is the exception rather than the rule. Moreover, many policy and marketing actions target beliefs and attitudes rather than implement specific projects or programs. Information on beliefs and attitudes is therefore more practical for many uses than numerical estimates of willingness to pay.

Turning to the specific results of this empirical study, the integration of psychometric and econometric aspects of consumer decision-making reveals some interesting features of Indian consumer thinking about GM foods. We found that individual can have both concerned and optimistic perceptions towards gene technology in food. While past research on perceived risk and benefit suggests an inverse association between these two latent factors, we found that the relationship between risk and benefit perception is not as straightforward as these studies suggested. GM food is quite a new concept to the people of India. Consumer acceptance is among the most important factors for the survival of a newly introduced technology in any market. Consumer acceptance in turn depends on the way they weigh the perceived benefits and risks associated with the technology.

Table 1a: Data description, Dependent variables (Y)

Name of the variables used in our model	[*corresponding LABEL used in our result section*] Questions being asked to generate variables	Possible answers to the survey questions (no. in parenthesis indicates value coding)
Y_u	*[WTBpotato]* Both GM and regular potato are available at the same price (Rs. 7.00 / Kilo), which one would you buy?	1 = if respondent wants to buy GM potato, 0 = otherwise
Y_1	[*GMPallergy*] GM potato may cause allergy to humans	On a 5-category ordinal scale, where 4 = strongly agree and 0 = strongly disagree
Y_2	[*GMPother*] GM potato, in general may have some other unknown long-term health effects	”
Y_3	[*GMPeco*] GM potato production may causes damages to the ecosystem	”
Y_4	[*GMPvita*] In comparison to Regular (Non-GM) Potato, which one of the following do you think characterizes the Vitamin contents of GM potato?	On a 5-category ordinal scale, where 4 = very high and 0 = very low
Y_5	[*GMPstor*] Regular (Non-GM) Potato has 5 days shelf life/ storage life. Which one of the following you expect is the closest to the storage time for GM potato	On a 5-category ordinal scale, where 0= lower than, 1=same storage time and 4=highest
Y_6	[*GMPpesti*] Given that yields are the same, in comparison to Regular (Non-GM) potato production, what do you think is the percentage of pesticide use for GM potato production?	On a 5-category ordinal scale, where 4 = very low and 0 = very high

Table 1b: Data summary statistics, Dependent variables (Y’s)

Name of the variables used in our model	Corresponding LABEL used in our result section	Min	Max	Mean	Std.Dev
Y_u	WTBpotato	0	1	0.325	0.469
Y_1	GMPallergy	1	5	2.254	0.742
Y_2	GMPother	1	5	2.342	0.732
Y_3	GMPeco	1	5	2.208	0.802
Y_4	GMPvita	1	5	2.375	0.721
Y_5	GMPstor	1	5	2.908	1.039
Y_6	GMPpesti	1	5	2.138	0.934

Table 2a: Data description, Explanatory variables (X's)

LABEL used in our result section	Data description
Sex	Male=1, Female=0
MainShopper	Whether the respondent buys most of the groceries for household, Yes =1, No=0
Income	Respondents' monthly income, on a six scale category spaced by Rs. 5000 difference, where 1=below 5000 Rs. and 6=above 50,000 Rs.
GMheard	How many times does the respondent heard anything about GM food, on a five scale category, 1=almost never, 5= many a times
Employment	1=employed, 0=otherwise
Education	Below bachelors=0, bachelors=1, above=2
BioSci	Have ever taken bioscience during college level, 1 = yes, 0 = no
Age	On a 9 scale category spaced by 5 years age difference, where 1= below 25 and 9= above 60
PotEatFreq	How often does the consumer eat potato, on a scale of 4, 4=daily and 1= less than once a month
FoodShopFreq	How often does respondent shop for food, on a scale of 5, 5= daily, 1=once a month
[HuRight] Humans have the right to use nature (plants & animals) to serve their own purposes	On a 5-category ordinal scale, where 5=strongly agree and 1=strongly disagree
[ModEnv] Modifying the environment for human use does not change the balance of nature significantly	"
[TechProg] For India, Greater technological progress should be pursued even at the cost to the environment	"
[RedFooProb] Serious technological advancement in food production is required to reduce India's food problem	"
[BuyNewFood] Respondent always buys new and different foods, provided their prices are affordable	"
[NoNewTrust] Respondent doesn't trust new foods	"

Table 2b: Data summary statistics, Explanatory variables (X's)

Corresponding LABEL used in our result section	Min	Max	Mean	Std. Dev
Sex	0	1	0.546	0.499
MainShopper	0	1	0.463	0.500
Child16	0	3	0.342	0.640
Income	1	6	2.627	1.221
GMheard	1	5	2.875	1.409
Employment	0	1	0.779	0.416
Education	0	2	1.271	0.701
BioSci	0	1	0.225	0.418
Age	1	9	2.463	2.538
PotEatFreq	1	5	4.354	0.789
GMread	1	5	2.533	1.267
FoodShopFreq	1	5	2.192	1.216
HuRight	1	5	2.346	1.183
ModEnv	1	5	2.413	1.031
TechProg	1	5	2.200	1.024
RedFooProb	1	5	2.983	0.928
BuyNewFood	1	5	2.675	0.999
NoNewTrust	1	5	2.888	0.977

Table 3a: Results for Exploratory Factor Analysis (1 Factor)

Estimated Factor Loadings:

Y_1^*	Y_2^*	Y_3^*	Y_4^*	Y_5^*	Y_6^*
0.586	0.548	0.597	-0.006	-0.173	-0.4

RMSEA: 0.161, RMSR: 0.1087

Table 3b: Results for Exploratory Factor Analysis (2 Factors)

Promax rotated loadings:

	Y_1^*	Y_2^*	Y_3^*	Y_4^*	Y_5^*	Y_6^*
Factor1	0.525	0.62	0.638	0.136	0.00	-0.324
Factor 2	0.223	-0.146	-0.088	-0.445	-0.63	-0.276

RMSEA: 0.078, RMSR: 0.0346[5]

Promax Factor Correlations: 0.161

[5] RMSEA is Root Mean Square Error of Approximation and RMSR is Root Mean Square Residual

Table 4A: Structural Model

	Utility/ WTB		Perceived Benefit		Perceived Risk	
	Estimate (St. Error)	Standardized Estimate	Estimate (St. Error)	Standardized Estimate	Estimate (St. Error)	Standardized Estimate
Perceived Risk	-1.264** (0.369)	-0.671	0.235 (0.361)	0.171	-	
Perceived Benefit	0.801** (0.31)	0.585	-		-0.286 (0.231)	-0.394
Sex	-		0.157 (0.147)	0.215	0.14 (0.122)	0.264
MainShopper	-		0.192 (0.164)	0.263	0.269** (0.114)	0.506
Income	0.111 (0.131)	0.111	0.134** (0.064)	0.183	0.026 (0.05)	0.05
Employment	-0.931** (0.337)	-0.931	-0.31 (0.19)	-0.424	0.118 (0.125)	0.222
Education	-		0.244** (0.12)	0.334	-0.074 (0.081)	-0.139
BioSci	0.934** (0.357)	0.934	0.356** (0.171)	0.487	0.031 (0.13)	0.059
Age	-		-0.007 (0.027)	-0.01	-0.026 (0.019)	-0.049
PotEatFreq	-		-0.107 (0.08)	-0.146	-0.082 (0.065)	-0.155
GMheard	-0.361** (0.107)	-0.361	-		-	
FoodShopFreq	-		-0.098 (0.07)	-0.134	-0.098** (0.052)	-0.185
HuRight	-		0.193** (0.064)	0.264	-	
ModEnv	-		-		-0.109** (0.051)	-0.205
TechProg	-		-0.149** (0.082)	-0.204	-0.091 (0.06)	-0.172
RedFooProb	-		-0.011 (0.076)	-0.015	-0.092 (0.057)	-0.172
BuyNewFood	0.33** (0.159)	0.33	-		-	
NoNewTrust	-0.321** (0.162)	-0.321	-		-	

** indicates significance at 5% level, * indicates significance at 10% level
Standardized Estimates are being standardized with respect to the continuous latent factors.

Table 4B: Measurement Model[6]

		Factor loading of Perceived Risk (η_r)		Factor loading of Perceived Benefit (η_b)	
		Est (St. Error)	Standardized Estimate	Est (St. Error)	Standardized Estimate
Y_1^*	GMPallergy	1 0	0.531	-0.404** (0.143)	-0.295
Y_2^*	GMPother	1.672** (0.386)	0.888	0.188 (0.191)	0.137
Y_3^*	GMPeco	1.938** (0.496)	1.029	-	-
Y_4^*	GMPvita	0.475** (0.222)	0.252	0.91** (0.25)	0.665
Y_5^*	GMPstor	-	-	1 0	0.73
Y_6^*	GMPpesti	-0.809** (0.203)	-0.43	0.65** (0.198)	0.475

** indicates significance at 5% level,
Standardized Estimate are being standardized with respect to the continuous latent factors η

Table 4C: R-Square

Observed Variable	Scale Factor	R-Square
Y_1	0.87	0.285
Y_2	0.784	0.44
Y_3	0.733	0.514
Y_4	0.881	0.322
Y_5	0.866	0.348
Y_6	0.865	0.309
Y_u	0.774	0.646
η_r		0.178
η_b		0.314

[6] With 2 factors we need 4 restrictions on $\mathbf{\Lambda}$ and $\boldsymbol{\psi}$. Two unit loadings are the two restrictions. For two other restrictions, we put 0 weights on those indicators for which we get the least value in table 3B. (further details on restriction, see Joreskog, 1979)

Table 4D Threshold values
(st. error)

$\tau_{Risk1,\,1}$	-2.711**	$\tau_{Risk2,\,1}$	-6.42**	$\tau_{Risk3,\,1}$	-5.466**
	(0.972)		(1.278)		(1.419)
$\tau_{Risk1,\,2}$	-1.33	$\tau_{Risk2,\,2}$	-4.411**	$\tau_{Risk3,\,2}$	-3.581**
	(0.967)		(1.088)		(1.3)
$\tau_{Risk1,\,3}$	0.792	$\tau_{Risk2,\,3}$	-2.269**	$\tau_{Risk3,\,3}$	-1.434
	(0.971)		(1.045)		(1.271)
$\tau_{Risk1,\,4}$	2.09**	$\tau_{Risk2,\,4}$	-0.32	$\tau_{Risk3,\,4}$	0.382
	(0.968)		(1.046)		(1.266)

$\tau_{Benefit1,\,1}$	-2.27*	$\tau_{Benefit2,\,1}$	-1.691*	$\tau_{Benefit3,\,1}$	-2.376**
	(1.182)		(0.932)		(1.111)
$\tau_{Benefit1,\,2}$	-1.889*	$\tau_{Benefit2,\,2}$	0.468	$\tau_{Benefit3,\,2}$	-1.531
	(1.165)		(0.9)		(1.165)
$\tau_{Benefit1,\,3}$	0.618	$\tau_{Benefit2,\,3}$	1.537*	$\tau_{Benefit3,\,3}$	0.457
	(1.121)		(0.902)		(1.162)
$\tau_{Benefit1,\,4}$	2.394**	$\tau_{Benefit2,\,4}$	2.208**	$\tau_{Benefit3,\,4}$	1.211
	-(.194)		(0.91)		(1.191)

τ_{Bid}	1.425
	(1.391)

** indicates significance at 5% level, * indicates significance at 10% level

Table 5A: Structural Model (*Restricted*)
0 in cell represents restriction on parameter

	Utility/ WTB		Perceived Benefit		Perceived Risk	
	Estimate (St. Error)	Standardized Estimate	Estimate (St. Error)	Standardized Estimate	Estimate (St. Error)	Standardized Estimate
Perceived Risk	-1.304** (0.38)	-0.673	0.597* (0.333)	0.427	-	
Perceived Benefit	0.878** (0.337)	0.635	-		-0.406** (0.183)	-0.569
Sex	-		0		0	
MainShopper	-		0		0.259** (0.12)	0.501
Income	0		0.133** (0.068)	0.184	0	
Employment	-0.937** (0.341)	-0.937	-0.411** (0.2)	-0.568	0	
Education	-		0.313** (0.116)	0.433	0	
BioSci	0.921** (0.35)	0.921	0.338* (0.184)	0.467	0	
Age	-		0		0	
PotEatFreq	-		0		0	
GMheard	-0.365** (0.11)	-0.365	-		-	
FoodShopFreq	-		-		-0.095* (0.05)	-0.184
HuRight	-		0.193** (0.071)	0.267	-	
ModEnv	-		-		-0.137** (0.062)	-0.265
TechProg	-		-0.123 (0.082)	-0.171	-0.107 (0.066)	-0.207
RedFooProb	-		0.014 (0.073)	0.019	-0.099 (0.063)	-0.193
BuyNewFood	0.334** (0.161)	0.334	-		-	
NoNewTrust	-0.324** (0.163)	-0.324	-		-	

** indicates significance at 5% level, * indicates significance at 10% level
Standardized Estimates are being standardized with respect to the continuous latent factors.

Table 5B: Measurement Model (*Restricted*)
0 in cell represents restriction on parameter

		Factor loading of Perceived Risk (η_r)		Factor loading of Perceived Benefit (η_b)	
		Est (St. Error)	Standardized Estimate	Est (St. Error)	Standardized Estimate
Y_1^*	GMPallergy	1	0.516	-0.431**	-0.312
		0		(0.144)	
Y_2^*	GMPother	1.544**	0.797	0	
		(0.343)			
Y_3^*	GMPeco	2.158**	1.114	-	-
		(0.586)		-	
Y_4^*	GMPvita	0.502**	0.259	0.926**	0.669
		(0.244)		(0.309)	
Y_5^*	GMPstor	-	-	1	0.723
		-		0	
Y_6^*	GMPpesti	-0.849**	-0.438	0.611**	0.441
		(0.204)		(0.199)	

** indicates significance at 5% level,
Standardized Estimate are being standardized with respect to the continuous latent factors η

Table 4C: R-Square

Observed Variable	Scale Factors	R-Square
Y_1	0.875	0.28
Y_2	0.812	0.389
Y_3	0.705	0.554
Y_4	0.863	0.328
Y_5	0.856	0.343
Y_6	0.871	0.295
Y_u	0.766	0.651
η_r	-	0.034
η_b	-	0.1

Figure 1: Individuals sorted by Benefit factor score in descending order

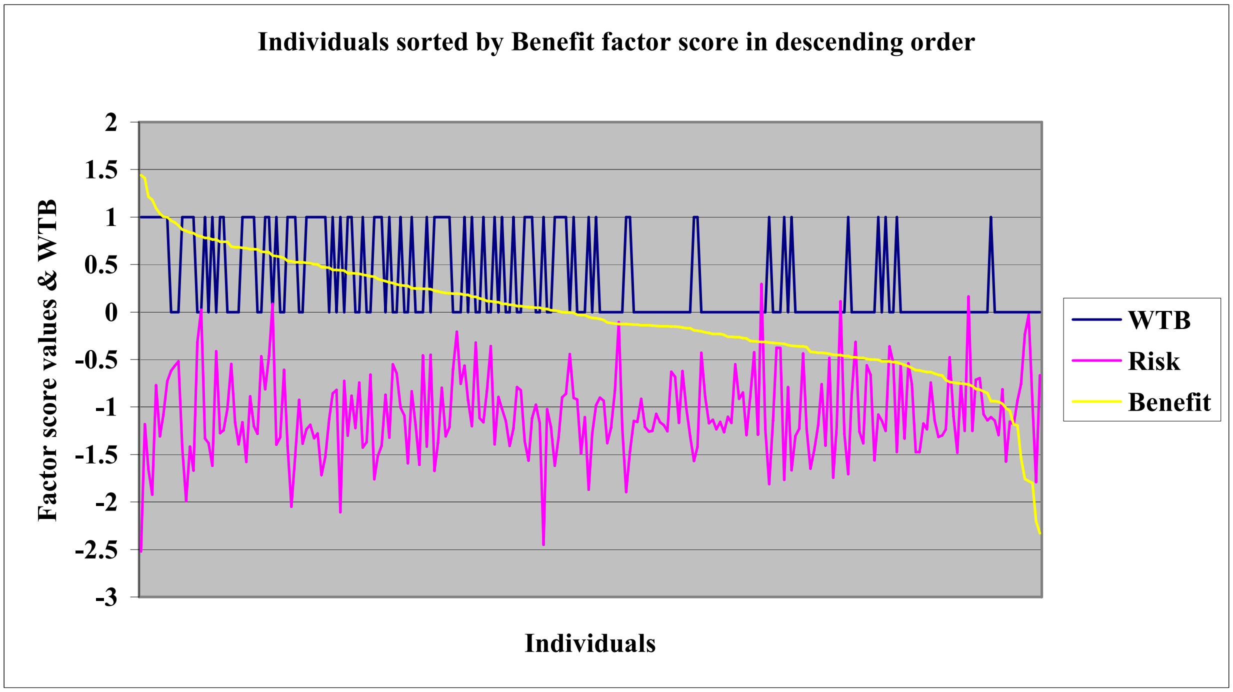
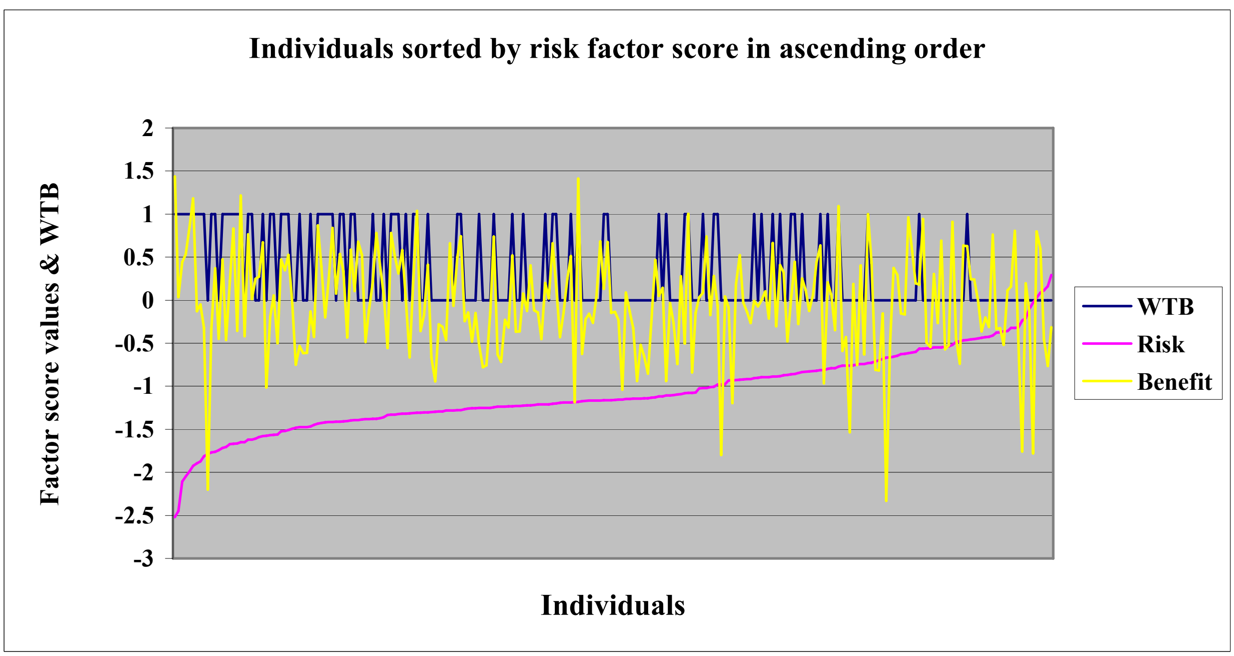


Figure 2: Individuals sorted by risk factor score in ascending order



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ESTIMATING THE VALUE OF WATER USE PERMITS: A HEDONIC APPROACH APPLIED TO FARMLAND IN THE SOUTHEASTERN US

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Abstract:

In the State of Georgia, any agricultural producer who wishes to pump more than 100,000 gallons of water a day for crop irrigation is required to have an irrigation permit. The permit stays with the land and in the event of sale the permit is transferred with the property. Until recently, permits were essentially granted freely to all applicants in the Flint River water basin, without limit. In 1999, however, with increasing demand for water from growing urban Atlanta and several years of drought in the Southeast, the state of Georgia placed a moratorium on the issuance of agricultural water permits in the Flint River basin. This research exploits this policy change within a hedonic pricing framework to estimate the value of irrigation rights in the Southeast US. While the value of irrigation rights has been studied extensively in the western US, differences in property rights and legal regimes, as well as a lack of established water-rights markets in the East, leave us with little information regarding the value of irrigation rights in this setting.

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I. INTRODUCTION

In 1999, during the first year of what became a four-year drought, and amid growing demands for water from Atlanta and the agricultural sector in Southeast Georgia, as well as litigation with Florida and Alabama over waters in the Flint River (as well as other rivers), the State of Georgia initiated a moratorium on the issuance of agricultural water permits in the Flint River Basin. Any land owner with an existing water permit at the time of the moratorium still has the legal right to irrigate. The permit, required for any water use in excess of 100,000 gallons of water a day, is attached to the land, and in the event of a sale of the land, the permit is transferred to the new owner. Land owners without permits can dryland crop or pump-irrigate less than 100,000 gallons a day (approximately one-third of an acre-foot of water a day).[1]

Although the purpose of the moratorium was to contain the amount of water pumped from the Flint River, it is likely to have consequences for agricultural property markets. Permits and the irrigation they allow, while essentially a free resource prior the moratorium, are now restricted to specific parcels.[2] This constraint on a vital input into agricultural production would most likely raise the value of existing permits and, by extension, affect property values.

While there is much evidence of the value of water rights in the Western U.S., very little

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[1]Roughly, 100,000 gallons/day could be sufficient to irrigate 10-20 acres of most crops grown in Georgia (e.g., cotton, corn, wheat, peanuts, soybeans) in a dry season if applied judiciously.

[2]Permits were routinely granted and the cost to apply for a permit was negligible prior to the moratorium. The land owner merely had to fill out an application.

is known about the value of water use permits in the East.[3] Important differences exist between the western half of the US and the east such that applying derived values from the west to the east may not be appropriate. Under western water-law, a right to use water is established by putting water to beneficial use. This water right is a property right which can be sold or leased; such rights are not tied to the land, nor is a state permit involved. The value of water rights in these situations will reflect local supply and demand conditions for water use for agriculture, industry, and municipal use.[4] However, in Georgia, and many other states in the eastern U.S., permits for water use must be obtained from the state, and these permits can only be used for irrigation of the land for which the permit was given and may not be traded or leased. Thus, the estimated value will only reflect the value of irrigation on-site, and it is not clear how markets for agricultural land will react to a constraint on permit issuances.

In this research, hedonic analysis is applied to agricultural property sales data from 1993 to 2003 for Dooly county, Georgia, to estimate the impacts of the water-use permit moratorium on property values. While there have been hedonic applications to farmland for the purposes of valuing water rights in the west (e.g., Hartmand and Anderson, 1962, and Faux and Perry, 2000), no analyses have been conducted that estimate the value of water-use permits in agriculture in the Southeast. Dooly county is an ideal setting to conduct the analysis as roughly half of the county is in the Flint River Basin and the other half is in basins that are unaffected by the

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[3]Water use permits in Georgia, or any Eastern state, confer a usufructuary right (the right to use) to water use. In Georgia there is no time limit or expiration to this usufructuary right for agricultural permits, and the right stays with the land. We will use the term “irrigation rights” interchangeably with “irrigation permits” for ease of exposition, although we note here that the correct terminology would be to qualify “rights” as “usufructuary rights.”

[4] Brown (2004), in his review of water valuation studies in the West, states that roughly half of water transactions are for municipal purposes. Only 23% of transactions are for irrigation. For another review of valuation studies, see Frederick, VandenBerg, and Hanson (1996).

moratorium. This environment allows us to separate the effects of the moratorium on property values and control for any spurious correlation by observing how property values changed in the unaffected basins during the same time period.

With increasing population in metropolitan areas and associated urban sprawl, the conflict between urban and rural economies grows. Water allocation among these competing demands continues to play a more central role. Currently, there are conflicts between each southeastern state and at least one neighbor over interstate streams. For example, rivers such as the Potomac (Maryland/Virginia)[5], Roanoke (Virginia/North Carolina)[6], Pee Dee (North Carolina/South Carolina)[7], Savannah (South Carolina/Georgia)[8], and the Chattahoochee and Flint (Georgia/Alabama/Florida)[9] have been the source of inter-state allocation conflicts and/or lawsuits. Intrastate disputes among localities mirror this relatively new, contentious, climate in the southeast. For example, an ongoing, sometimes heated, debate has developed regarding the potential development of water permit trading markets in Georgia as a method to meet growing water-demands in urban areas (primarily Atlanta). An ex-ante estimate of the value of irrigation rights for agricultural purposes in the Southeast is an important input into these debates.

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[5] *Virginia* v. *Maryland*, 540 U.S. 56 (2003).

[6] For a chronology of events, see Virginia Beach Public Utilities, “Department of Public Utilities — Lake Gaston Pipeline Project Information,” http://www.vbgov.com/common/printable/0,1359,11728,00.html.

[7] See Franck and Pompe, 2005

[8] Georgia Water Planning and Policy Center, "Water Scarcity Battles Heat up in Fast-Growing Coastal Georgia," *Water Talk*, June 2004. http://www.h2opolicycenter.org/pdf_documents/June%202004.pdf

[9] See Moore (1999).

II. Model

The hedonic model recognizes that the sale price of an agricultural parcel reflects the net-present value of the future rents expected from the parcel (Palmquist, 1989). The land is a considered a differentiated factor of production, with the n characteristics of the land, $\{z_1, \ldots z_n\}$, affecting productivity and thus affecting sales price, P, or $P = P(z_1, \ldots z_n)$. While it is convenient to consider sales prices from an empirical standpoint, a time-consistent theoretical model of profit-maximization for the farmer is more conveniently represented in a per-period context. Thus, let us assume that rental rates, are a simple transformation of the sales price $R = R(P(z_1, \ldots z_n))$.[10] In other words, rental rates are equivalent to the annuitized sales price with all farmers having access to the same market-clearing interest rate.

Let a farmer seeking to purchase a property produce a single output, q, sold in a competitive market. His production function, $q = q(X, Z, \alpha)$, depends on a vector of non-land inputs, X, such as irrigation equipment, and a vector of property inputs (characteristics), Z, and a set of farmer-specific skills, α.

Initially, consider the farmer's maximization of "variable profits", Π^V, which is defined here as the difference between the value of output and the value of non-land inputs.[11] The farmer's maximization problem is given by:

$$\begin{aligned} \max_x \ \Pi^V &= mq - \sum_j c_j x_j \\ s.t. \ q &= q(X, Z, \alpha) \end{aligned} \qquad \textbf{(1)}$$

[10] Either the owner rents land to a farmer, or can be considered renting the land to him/herself for the purposes of agricultural production.

[11] We are not using the term "variable profits" in the usual way, unless all property characteristics are considered fixed and all other inputs are variable.

where m is the market price of output, c is the cost of variable input x, and all other variables are as defined above.[12] First order conditions imply the following input demand for the j^{th} non-property input:

$$x^j = x_j(r, Z, C, \alpha) . \qquad \textbf{(2)}$$

Substituting x^j back into Π^V to obtain Π^{V*}, we can compute total profit, Π, as the difference between variable profit and property costs (assuming all non-property inputs are variable):

$$\Pi = \Pi^{V*} - R(Z) = m \cdot q(X, Z, \alpha) - \sum_j c_j x^j(r, Z, C, \alpha) - R(Z) . \qquad \textbf{(3)}$$

Profit maximization thus requires that the choice of land characteristics be such that the marginal rent paid for the characteristic equals its incremental contribution to variable profits, evaluated at the optimum level of non-property inputs, or:

$$\partial R(z_i)/\partial z_i = \partial \Pi^{V*}/\partial z_i = m \cdot \partial q/\partial zi - \sum_j c_j \partial x^j/\partial z_i . \qquad \textbf{(4)}$$

The alternative formulation of the farmer's problem is to determine the optimal amount the farmer will bid on a property with characteristics Z. The optimal bid, θ, is defined by $\Pi = \Pi^{V*} - \theta$. It is the difference between variable profits and the price paid for land, such that a desired level of profit is achieved. Thus, we have:

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[12]This specification does not explicitly model the presence of housing on an agricultural parcel. Housing can be an important component of total parcel value, and approximately 35% of the parcels used in our analysis have some form of improvement located on the property. To reflect this, the model may be made extended by allowing the cost function to be separable in non-land inputs — specifically to allow the value of housing stock to be separable from other non-land inputs. This formulation would recognize that the farmer may either rent the housing or be thought of as renting the housing from him/herself. In this formulation, the variable profit function would become $\Pi^V = mq - \sum_j c_j x_j - a(H)$, where $a(H)$ is the time-consistent rental value of the housing on-site.

$$\theta(Z, m, C, \Pi, \alpha) = m \cdot q(X, Z, \alpha) - \sum_j c_j x^j(r, Z, C, \alpha) - \Pi . \tag{5}$$

From equation (5), the marginal bid for any specific characteristic of land is:

$$\theta_{z_i} = \partial\theta/\partial z_i = m \cdot \partial q/\partial zi - \sum_j c_j \partial x^j/\partial z_i . \tag{6}$$

Consider a land characteristic that increases agricultural productivity, such as improved soil quality. These characteristics will have positive marginal bids ($d\theta/dz_i \geq 0$) since $\partial x^j/\partial z_i \leq 0$ and/or $\partial q/\partial z_i \geq 0$. For instance, the demand for fertilizer might decrease if land has better quality soils, and/or plant yields (i.e., output) might increase with increased soil quality.

The above defines the optimal rental bid. For farmers who own their land, they can be considered renting the land from themselves, and in general the optimal rental bid will have a direct corresponding optimal bid for land purchases. However, there are deviations possible. For farmers who are not landowners, the optimal rental bid will only consider factors which influence cropland productivity during the course of the rental contract. Future productivity, or alternative future uses of the land (such as conversion to residential property) will not be considered in a rental bid. However, these factors would be considered in an optimal bid to purchase the land. As discussed below, one of our empirical models directly considers the possibility that sales prices, and the influence irrigation rights have on sales prices, may be affected by expectations about future productivity of the land (i.e., expected productivity of land that is not currently cropland, but could be converted to cropland).

The model makes clear how to incorporate irrigation rights in the empirical specification of the hedonic price function. In the case of irrigation permits in Georgia, we can consider the presence of a permit as a site-specific characteristic of the land. Unlike western states where rights can be traded and thus have value which is separable from the land, an irrigation permit in Georgia cannot be traded separately from the parcel to which it was issued, and thus has no value other than the increased productivity it allows through crop-land irrigation. Thus, the value of a permit in Georgia will be either through $\partial q/\partial z_i$ (i.e., increased output associated with the ability to irrigate sufficiently) or due to reductions in demand for other inputs, such as fertilizer, when sufficient irrigation is possible (i.e., through $\partial x^j/\partial z_i$ in equation (6)). An empirical specification must thus be chosen which is consistent with the value of a permit being expressed only through the acreage to which irrigation would be applied — i.e., to cropland. For example, woodlands would not benefit from having an irrigation right (assuming no future conversion of the land to cropland is possible), and thus irrigation rights would have no value to acreage in this landuse.[13]

III. Study Area

Agricultural property sales in Dooly county, Georgia will be the subject of our analysis. Dooly county is an ideal geographical location to study the effects of the agricultural water permit moratorium on property values for two important reasons. First, we can exploit its unique geographic location straddling several river basins, including the Flint River basin, the Suwannee, and the Ocmulgee. The moratorium applied only to the issuance of water permits in the Flint River Basin. Thus, by examining land sales not subject to the moratorium, but in similar agricultural productive areas, we can isolate the effects of the policy change on property values. Prior to the moratorium on the issuance of water permits, we would expect sales prices to

[13]Note, woodlands are distinct from orchards.

be unaffected by the possession of a water permit, for land both within and outside of the Flint River Basin. The cost of applying for a permit was small, and all permit applications were approved. After the moratorium was implemented, however, we would expect the value of irrigation rights to be capitalized into the value of the land. We would expect this to be the case for parcels in the Flint River Basin but not necessarily for parcels outside of the basin. Our analysis will investigate these hypotheses.

Second, the county tax assessors office in Dooly county, from which sales information is collected, has developed electronic databases describing properties and sales information. This makes study of Dooly county possible. Many other rural counties in Georgia still keep property and sales records on paper only, making the collection of transactions data for all sales in the county so labor intensive as to be prohibitive.

Dooly county is located on the Eastern most edge of the Flint River Basin in Southwestern Georgia. The population of the county is 11,552 (2000 Census) and covers 393 square miles. The county is the largest single producer of cotton in the state (140,000 bales valued at approximately $41 million in 2003[14]) and is also a leading producer of wheat. Dooly also has a substantial production of peanuts and soybeans.

Before describing the data we use to estimate the value of irrigation rights, we first describe a few key aspects of Dooly County agricultural production that must be considered in our analysis as well. While much of the non-residential land in Dooly county is used for some type of agricultural production, leasing land for recreational hunting is a common and profitable activity. Since 1993, Dooly County has implemented regulations protecting young antlered

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[14]Value estimate derived from *Georgia Agricultural Facts* (2004).

whitetail bucks from being hunted, and as a result, the number of mature bucks available to the hunting public has increased by 300%.[15] As a result, since 1995, the county has experienced a surge in the purchase and subsequent rental of land for recreational hunting. Land suited for this purpose typically has forest cover and a water source located on the land and is not necessarily in competition with agricultural production. Crop and recreational land are geographically distinct, and we will control for the type of land in each sale in order to isolate the effects of irrigation rights on cropland values.

Another factor to be considered is whether or not peanut poundage quotas were transferred in an agricultural sale. Up until the enactment of the 2002 Farm Bill, peanut farmers holding a peanut poundage quota received a subsidized per pound payment from the government for the allowable pounds of peanuts on the quota. With the 2002 Farm Bill, those subsidized payments were eliminated, and peanut farmers only received the prevailing market price. Prior to 2002, peanut poundage quotas could be transferred, and they were sometimes sold as part of a real estate transaction. We have gathered information from Dooly county on whether or not a peanut poundage quota was transferred with the land at the time of sale. In addition, whether there is saleable timber on a parcel was recorded by the Dooly County assessors. We also include this feature in our analysis of property sales in the county.

Lastly, the state of Georgia has two programs that provide tax savings to agricultural property owners. In 1992, the state implemented the Conservation Use Valuation (CUV) program. If enrolled in this program, agricultural land owners are taxed at the current use value of the land rather than the fair market value. In exchange, the land owner agrees to keep the land

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[15] Dooly County Chamber of Commerce, http://www.doolychamber.com/outdoor.html, provided this information.

in agricultural or forest use for a period of ten years. The second program, Agricultural Preferential Assessment (Ag. Pref.) is for agricultural and forest land. Owners receive an average tax savings of 25%. Enrollment in these programs could have important effects on sales prices, and thus we also include information on whether or not the land is enrolled in these programs at the time of sale.

IV. DATA

Three databases are combined for the hedonic analysis: (1) data on land sales, characteristics and improvements, (2) data on additional assets included in the sale including timber or peanut poundage quotas, and (3) data on water permit locations. Each of these data are described in turn below.

Sales Prices, Land Characteristics and Improvements

Data on sales prices and characteristics of the land were collected from the Dooly County tax assessors office. The data include information on all digitally recorded property sales in Dooly County (some 16,000 individual sales from 1930 to the present). Each observation is a legally-defined parcel of land. Because we are only interested in sales of agricultural land, we will limit the data to only those sales of agricultural land, including sales of land in the Conservation Use Value or Agricultural Preferential programs, from 1993 to 2003. If a property was sold multiple times over the 10-year period, the most recent sale was chosen. The database thus contains information describing 341 sales of agricultural land in Dooly County for the period 1993 to 2003.[16] The 341 sales represent a total of over 50,000 acres of agricultural land in Dooly County, which is a substantial portion of the total land area of Dooly County, and are widely dispersed throughout the county. There were fourteen observations missing key data and three sales which were not considered reliable, thus leaving 324 observations available for analysis.[17]

Table 1 summarizes variables that have been developed to describe the agricultural sales. The mean sales price (unadjusted for inflation) in our data was $175,800.[18] There were generally between 25 and 40 sales in each year and sales prices varied substantially, from under $20,000 to well over $1 million. Sixteen properties were enrolled in a conservation use program at the time of sale. Total acreage included in each sale also varied substantially. The mean parcel size was 142 acres, and acreage included in the sale varied from less than 10 acres to just over 1,100

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[16]Note, there were 399 parcels involved in 341 sales. Some parcels were sold as a group, and one sale price was recorded. In these instances, we aggregated the information for each parcel to represent the characteristics associated with the sale. For instance, the number of acres of cropland for each parcel was added together to represent the total number of acres of cropland purchased in a particular sale.

[17]By reliable, we mean that the recorded sales price does not reflect an arms-length transaction generated by local supply and demand conditions. For instance, one removed sale was for the largest tract that sold in Dooly during our study period (2,538 acres, more than twice the size of the next largest parcel), all in the recreation/residential category. The property sold for more than 50% more than the next highest sale (its sale price was over $3 million), however, the sale was from a private landowner to the State of Georgia. This property had a permit associated with it, and the sale occurred pre-moratorium. Coefficients estimates for pre-moratorium variables are affected by the inclusion of this sale. A second sale included 692 acres of cropland (95th percentile in size), and sold for a price of approximately $400/acre, which is the bottom 5th percentile for sales prices in our study. The grantee and grantor in this sale had the same first and last name, thus indicating that this was likely not an arms-length transaction. This sale occurred post-moratorium, and coefficient estimates for post-moratorium variables are affected by the inclusion of this sale. A last sale also occurred at a low acre-price given the land characteristics. Results are qualitatively unchanged whether or not this sale is included in the models. Note, the tax assessor data did include a field indicating if the sale was arms-length. This field was not used to determine whether a sale price was reliable. The coding of this field was erratic and missing for a number of parcels. Nonetheless, we have tested the robustness of our results to inclusion of 16 sales that the tax assessor data originally coded as not arms-length. Coefficient estimates (and significance) remain remarkably stable, especially for those related to the value of an irrigation permit.

[18]If one adjusts for inflation in agricultural land prices over the study period, the mean price is $249,300 (2003 dollars, using a price index created from average agricultural land prices in Georgia as reported by *Georgia Agricultural Facts* (various years)).

acres.[19] Dooly County tax assessors record the land-use of each parcel (parcels may have acreage in more than one land-use). We developed three major land-use categories to describe each parcel: cropland, recreation/residential, and a general category that combines woodland, ponds and orchards (see Table 1). There are approximately 14,000 acres of land that sold in Dooly County during our study period that are recorded as cropland. The largest category of land is recreation/residential with over 24,000 acres, and the smallest category includes land that is recorded as woodland, orchards or ponds with approximately 8,000 acres. Note, of the acres in this latter category, over 7,000 are categorized as woodland. As such, for ease of exposition, we refer to this aggregate category as just "woodlands."

Dooly County also records the quality of the soil of each acre of land. We aggregate the tax-assessor's six-category index of soil quality into three categories: above-average, average, and below-average soils.[20] Table 1 also reports the total number of acres recorded in each of our three soil quality categories. As indicated in Table 1, most of the acreage in Dooly County (62%) is categorized as having above-average soil, and only a small proportion (14%) is categorized as having average soil. The remaining 24% of land is categorized as having below-average soil.

In addition to information on the land-use and soil quality, other characteristics of the land which we include in our analysis are whether or not the lot is considered level, whether or not there is access to municipal or well-water, and whether or not the land is generally considered to be of average or above-average "desirability" by the Dooly County tax assessor

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[19]Results remain unchanged if transactions of less than 20 acres are excluded from the analysis.

[20]Dooly county's classification system aggregates a 1-9 quality index developed by the state of Georgia for parcels in conservation-use programs. The state's quality index is based on the 107 soil types found in the state.

(see Table 1 for summary statistics of each of these property descriptors).[21]

Dooly County is active in peanut production. Because the peanut poundage quotas were quite valuable and could be transferred in a land sale prior to 2002, it is important to include in the analysis whether or not a property sale included the transfer of quotas in the sales price. Dooly County tax assessor maintains a list of agricultural properties sold every year and if the sale included other assets not captured in the existing data on land improvements. The list includes information on whether or not a peanut poundage quota was transferred with the sale (and thus included in the sale price). Because the exact number of pounds of peanut quota sold with the land is not recorded for each sale (i.e., some sales simply record that a peanut quota was transferred, but do not indicate the poundage of the quota), we can only include in our analysis a categorical variable equal to one if the sale included a peanut poundage quota, and equal to zero otherwise (see Table 1).

Also included in the Dooly County records are whether or not marketable timber was present on the land at the time of sale. Again, the records only indicate the number of acres of timber for a few properties, and most properties are simply recorded as having some (unknown) amount of timber. Thus, included in our analysis a categorical variable equal to one if the land had marketable timber on it at the time of sale, and equal to zero otherwise (see Table 1).

Information about improvements associated with the land (housing) is also recorded by the assessor. As reported in Table 1, 114 or 35 percent of the properties had some improvement

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[21]Other characteristics of the land which we had available for analysis were whether or not the road leading to the property was paved, the drainage quality of the land (above-average, average or below-average drainage), and whether or not there is sewage treatment available. These factors were not included in our analysis because of correlation with factors already included (for example, almost all properties that are coded as having no water on site also have no sewage removal capabilities) or little variation in the data (for example, only 3 properties were coded as having below average drainage).

on them at the time of sale. Dooly County records a number of characteristics of these improvements including whether it is a mobile-home, house, or multi-family dwelling, the square footage of each improvement that is heated, whether or not the improvement has central heat or air-conditioning, and the observed condition of the improvement (as determined by looking at the improvement from the outside only). We include total heated area of improvements in our models, and interact total heated area with a variable which captures the quality of the improvements on site (see "Above Average Quality" in Table 1). Because twenty percent of sales included more than one improvement (i.e., a single parcel might have both a single-family home and a mobile home), we also include in our models a variable which weights the total heated area by the number of improvements on the property. Table 1 reports that the mean heated area of improvements included in a sale is 1,838 square feet. Note, the average square feet of heated area pertains to all improvements included in a sale. Thus, the average square feet of heated area for any single improvement will be less than 1,838 square feet.

Lastly, we compute the value of accessories included in each sale. Irrigation equipment such as a center-pivot or subsurface drip irrigation systems are expensive, and likely to remain with the land in the event of a property transfer. While we do not have information on the type of irrigation systems included in a sale explicitly, we can compute a proxy measure for the value of all accessories included in a sale. The Dooly County assessors data include a field for the current assessed value of the property. This value includes the estimated value of land, improvements and all accessories associated with the sale. The assessor data also includes separate fields for the assessed value of land only, and the assessed value of improvements only. To compute the value of accessories, we simply subtract the assessed value of land and improvements from the total assessed value of the property (see "Accessory Value" in Table 1). In addition to irrigation equipment, this value would include additional features such as tractors or other heavy equipment included in the sale. In many cases, however, the value of irrigation equipment would be the primary component of accessories sold in a transaction.

Irrigation Permits

The Environmental Protection Division (EPD) of the Georgia Department of Natural Resources provided data on whether or not each property had been issued an irrigation permit (surface or groundwater) at the time of sale. The data are contained in a Geographic Information System (GIS) map of the location of surface and groundwater permits in Dooly county. Included in the GIS map is the exact location of the permitted pump, the basin in which the pump is located, the year the permit was issued, and the unique permit identification number. To match permits with parcels, it was necessary to accurately geo-locate each parcel in the sales database on a GIS map consistent with the permit map. Dooly County tax assessors office maintains paper copies of parcel boundaries for properties that transfer ownership. These boundaries are hand-drawn onto a satellite image of the county. The satellite image used by Dooly County is identical to that available for permits. Thus, we could digitize the boundaries of each parcel on an electronic map and overlay the EPD permit data.[22] A spatial join was then performed using ArcView GIS to determine which parcels had an irrigation permit located within its border.

Overall, in Dooly County there are 151 surface-water permits. Of those, 101 are in the

[22]The property boundary match with the EPD map is quite good. The EPD map includes outlines of the irrigated acreage associated with each of the permits. The outlines of the irrigated acreage matched the parcel boundaries consistently for the entire county.

Flint River Basin, 22 are in the Ocmulgee Basin and 28 are in the Suwannee Basin. There were 351 ground-water permits issued in Dooly County, of which 245 are in the Flint River Basin, and 102 are in the Ocmulgee and 4 are in the Suwannee Basins. In addition to determining which properties have a permit, it was necessary to determine if the permit was issued prior to the sale of the parcel. The EPD permit data contains information on the date of issuance of a permit, so we could incorporate this aspect into our analysis. In our sales data, there are 46 sales which included parcels with a permit located within their boundaries at the time of sale. Of these, 31 sales included permits that lie in the Flint river basin, and 15 sales included permits that lie in either the Suwannee or Ocmulgee basin. As indicated in Table 1, of the 46 properties that sold with a permit, 33 sales occurred prior to the moratorium, and 13 sales occurred post-moratorium.

Table 2 reports summary statistics regarding parcels that had a permit issued to them at the time of sale. Of note in Table 2 is the difference in the average number of acres included in a sale. As Table 2 indicates, for all three land-use types, the mean number of acres included in a sale was larger if a permit was attached to the land at the time of sale. This is true for each of the major land-use categories as well (cropland, recreation/residential, and woodlands). While the mean number of acres is larger for permitted properties, the range is similar across categories (i.e., there are very large parcels that do not have a permit).

Lastly, the digitized parcel-boundary map was used to determine in which basin all parcels lie, not just those with a permit. In the sales data, 230 sales (71%) had parcels lying within the Flint River Basin, and 94 sales (29%) had parcels lying within either the Ocmulgee (91 sales) or the Suwannee (3 sales) basin.

V. EMPIRICAL MODEL AND RESULTS

We estimate two types of hedonic price functions. The first is naive and assumes that the value of irrigation rights are priced into all acres of a parcel equally. This assumption implies that all acres can be used or converted to productive agricultural land (at a small cost) within each parcel. Under this assumption, the hedonic price function that we estimate is given in equation (7):

$$\ln(P_{it}) = \alpha_0 + \sum_{t=1}^{T} \alpha_t D_t + \sum_{j=1}^{J} \alpha_j L_{jit} + \tag{7}$$

$$\beta_1 \, Totalacres + \beta_2 \, Totalacres * permit^{pre} + \beta_3 \, Totalacres * permit^{post} + e_{it},$$

where $\ln(P_{it})$ is the natural log of sales price of property i in time t; α and β are coefficients to be estimated; D_t are dummy variables indicating the year of the sale; L_{jit} are J characteristics other than acreage of the land which are hypothesized to influence sales price, and e_{it} is the error term. The variables included in L_{jit} are Conservation Use, Level Lot, No Water, Overall Desirability, Peanut1, Timber1, Totalheat, Totalheat interacted with Above Average Quality, Totalheat divided by the Number of Improvements, and Accessory Value (see Table 1 for a description of each variable). Also included in Equation (7) are the total acres included in the sale, and the total acres interacted with two dummy variables, $permit^{pre}$ and $permit^{post}$. The variable $permit^{pre}$ is equal to one if a the property had a permit at the time of sale and the sale occurred either pre-moratorium if the property was located in the Flint basin or in any year if the property was located outside of the Flint basin. Thus, in this category, we are capturing all sales which occurred when permits were essentially freely obtainable. The moratorium took effect on December 1, 1999, which is the cut-off date for determining whether a sale was pre-moratorium or post-moratorium. Because there was some prior warning of the moratorium, we examine how prior information may have been incorporated into the land market. The Georgia Environmental Protection Division (EPD) officially announced the moratorium only one month prior to its effective date. However, in the spring of 1999, the Director of the Environmental Protection Division had informally mentioned that permits could not be granted in the basin "indefinitely". So, while no direct discussion of a moratorium had taken place, the mention had been made. Uncertainty regarding the future availability of permits may have been capitalized into the land prices prior to the moratorium effective date of December 1, 1999. We test the robustness of our models to the date chosen to represent the beginning of the moratorium.

There has been no discussion among policy makers in Georgia to date about restricting permits in the Ocumulgee or Swannee basins. Thus, it is reasonable to assume that these permits continue to be viewed as freely attainable both pre- and post-moratorium.

The empirical model given in equation (7) indicates that the value of irrigation rights, per acre, post-moratorium can be simply computed as:

$$\textit{Permit Value per Acre} = \beta_3^e \times \textit{Saleprice}, \qquad \textbf{(8)}$$

where β_3^e is the estimate of β_3.[23] An analogous computation would be made to compute the value of irrigation rights pre-moratorium, should they be found to significantly affect sales price.

As stated earlier, equation (7) presents a model in which it assumed that the value of

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[23]More correctly, the value of the permit post-moratorium should be computed using the Halvorsen-Palmquist adjustment to our coefficient estimate (Halvorsen and Palmquist, 1980). However, our coefficient estimates are so small that the adjustments only affect our coefficients at the fifth decimal place, and indicate no practical change in our permit value estimates. Furthermore, Kennedy's (1981) extension of the Halvorsen-Palmquist adjustment indicates the Halvorsen-Palmquist adjustment would be an over-adjustment in our case.

permits are captured equally in all acreage associated with a property. This may not be true if parcels are not uniform in terms of potential land-use. For instance, if acreage classified as recreation/residential cannot be converted to crop land, or if it is costly to convert, the value of irrigation rights should be capitalized into the land-prices differentially, depending on the land-use of the acreage. To test this hypothesis, we also estimate models which follow the basic structure given in equation (7), but disaggregates land use as follows:

$$\ln(P_{it}) = \alpha_0 + \sum_{t=1}^{10} \alpha_t D_t + \sum_{j=11}^{17} \alpha_j L_{jit} + \beta_1 \, crop + \beta_2 \, crop*permit^{pre} + \beta_3 \, crop*permit^{post} + \beta_4 \, recres + \beta_5 \, recres*permit^{pre} + \beta_6 \, recres*permit^{post} + \beta_7 \, woods + \beta_8 \, woods*permit^{pre} + \beta_9 \, woods*permit^{post} + e_{it}, \qquad \textbf{(9)}$$

where crop, recres, and woods indicate the number of acres under each land-use (see Table 1 for a description), and all variables other than land-use are as described for equation (7). This model allows us to test for differences in the capitalization of irrigation rights across land-use types.

Results

Equations (7) and (9) are estimated using a linear regression model with robust standard errors.[24] Tests for spatial autocorrelation were conducted, and we could not reject the null hypothesis of no spatial dependence in the error term.[25] This is not surprising as the study area is

[24]We can reject the null hypothesis of homoskedastic errors at the 1% level using a Cook-Weisberg (1983) test. Thus, we correct for an unknown form of heteroskedasticity using a Huber/White/sandwich estimator for the variance/covariance matrix.

[25]Tests for spatial correlation are based on Moran's I test statistic. Moran I test statistics are estimated under the assumption of homoskedastic innovations in the error term, as well as under the assumption of heteroskedastic innovations in the error term (see Kelejian and Prucha, 2001). Moran I statistics are not greater than 0.92 (in absolute value) for any of our models, regardless of whether homoskedastic or heteroskedastic errors are assumed. The test statistics are not significant at any conventional level, indicating that we cannot reject the null of no spatial correlation of the error terms.

relatively small and homogenous. The county is roughly rectangular in shape and approximately 20x20 miles in dimension. There are no major urban areas, or particularly important agricultural marketplaces, in or near the county (the county seat has a population of less than 3,000). Thus, we expect the primary factors that influence agricultural land prices would be the suitability of the acreage contained in the parcel for agricultural purposes. While soil quality and topography may be spatially related across parcels, we control for these factors directly in our hedonic regression.

The dependent variable is the natural log of sales price. Changes in prices due to inflation are controlled for by including a series of dummy variables indicating the year in which the sale occurred (given by ΣD_t in equations 7 and 9). Before discussing the results of the variables directly related to permits, a brief description of the results for variables describing the parcels given by L_{jit} in equations (7) and (9) is warranted. The results for these variables are stable across all models in Table 3 and, although not reported, also in Table 4. Across all models, the presence of marketable timber or a peanut quota has a large and statistically significant effect on sales price. In addition, sales prices are significantly higher if a parcel is enrolled in a conservation use program. Parcels that are characterized by the tax assessors office as having average or above average desirability have significantly higher sales prices as well (see "Overall Desirability" in Table 3). Whether or not the parcel has some segments that are considered "level lots" or a potable water supply installed are generally not statistically significant.[26]

The variables describing the total heated size of housing improvements on the property

[26]The variable for "level lot" is statistically significant in two of the four models reported in Table 4.

and their quality are significant in all models. Accessory value is also an important determinant of sales price. The models generally indicate that a $1 increase in the value of accessories increases sales price by approximately the same amount.

Of primary interest are the results regarding our hypothesis that the value of an irrigation permit will be capitalized into the sales price of the land post-moratorium. Both models in Table 3 are naive models in the sense that they assume the value of irrigation rights are capitalized into all acres equally. Model 1 is a parsimonious model, and Model 2 includes variables which control for the quality of the soils present in the parcel (percent of total acres included in the sale that are characterized as above average or percent that are characterized as average), and interaction terms that test for general basin-wide effects. The basin-wide effects are captured by four interaction terms: *flintbasin*pre*, *flintbasin*post*, *otherbasin*pre*, *otherbasin*post*. The variables *flintbasin* and *otherbasin* are dummy variables indicating the basin in which the parcel is located (see Table 1 for a description) and the variables *pre* and *post* are dummy variables equal to 1 if the property sold pre-moratorium or post-moratorium, respectively. The category left out of the model is *flintbasin*pre*.

As indicated by the coefficient estimate for *totalacres* in both models presented in Table 3, the value of an acre of land without a permit is approximately $630, when the model is evaluated at the mean sales price. The models also indicate that having a permit present on the land, pre- or post-moratorium is not associated with an increase in the value of the land. The coefficients for $totalacres*permit^{pre}$ and $totalacres*permit^{post}$ are not statistically significant at any standard level of confidence in model 1 or 2. In addition, model 2 indicates that there are no basin-wide fixed effects associated with the moratorium (coefficient estimates for *flintbasin*post*, *otherbasin*pre*, and *otherbasin*post* are not statistically significant). Lastly, model 2 also indicates that measures of the soil quality associated with a parcel (*percent above average*, and *percent average*) are not statistically significant.

The above discussion is based on models that assume all acreage benefits equally from irrigation. This may not be the case. Some portions of a parcel may not be suitable for agricultural production (either current or future). While measures of the potential productivity of each acre of land contained in a parcel do not exist, we do have crude measures that might be related. The tax assessor categorizes the land use of each acre in a parcel as either crop, recreation/residential or woodlands. The first two models presented in Table 4 allow that the value of a permit might be capitalized differently into each of these three land-uses. The acreage of each land use type is interacted with a dummy variable indicating the sale occurred pre-moratorium (or outside the Flint River basin) or post-moratorium. Model 1 is a parsimonious model, and model 2 includes the measures of soil quality and basin-wide fixed-effects as described in Table 3.

As indicated in model 1, there is no significant difference between the value of land with a permit or without a permit pre-moratorium (or outside the Flint basin), regardless of its land-use classification. However, post-moratorium, there are significant differences in the value of land with and without a permit. Land characterized as cropland or recreation/residential have significantly higher prices per-acre post-moratorium if a permit is attached to the land at the time of sale. Land classified as either cropland or recreation/residential is estimated to sell for approximately $500 more per acre post-moratorium if a permit is associated with the sale. Model 2, which includes controls for soil quality and basin-wide fixed-effects indicates a somewhat larger value of a permit post-moratorium for crop and recreation/residential land (approximately $550 to $600), although these estimates are not significantly different than those from model 1.

Similar to the results presented in Table 3, there are no statistically significant basin-wide effects associated with the moratorium, however, in contrast to the results in Table 3, the measures of soil quality for each type of land is statistically significant.[27] For all three land-use types (crop, recreation/residential, and woods), the percent of a tract that has above average soils is associated with a higher sales price. These results are particularly strong for cropland. Indeed, when the soil quality associated with cropland is included in the model, the coefficient estimate for the number of acres of cropland is not significantly different from zero. This is not surprising given the correlation coefficient between *crop* and *percent crop above average* is over 0.7 and soil quality would be expected to be a particularly important determinant of the value of crop land.

The results for woodlands are somewhat puzzling. The coefficient estimate for *woods* indicates land classified as woodlands (without a permit) sells for approximately the same amount, per acre, as land classified as recreation/residential. This perhaps reflects the value of this type of land for hunting purposes as discussed earlier. However, the models in Table 4 also indicate that parcels with more acreage classified as woodland and have a permit sell for less post-moratorium as compared to parcels with woodlands, without a permit. The negative, significant coefficient for $woods*permit^{post}$ could indicate that properties with significant woods are less agriculturally productive than parcels without woodlands if the woods reflect natural

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[27]Note, there were no acres of woodlands categorized as having average soils, only above average and below average.

barriers to large-plot farming (i.e., reflect something about topography that precludes the development of optimally sized fields).

Two additional models are reported in the last two columns of Table 4. Model 3 is identical to model 2, but aggregates crop and recreation/residential acres. F-tests indicate that the coefficient estimates for *crop*permit*^{post} is not statistically different than the coefficient estimate for *recres*permit*^{post} in either model 1 or 2 (p-values are greater than 0.8 in each test). This may indicate that the recreation/residential land, which the tax assessors office indicated was "desirable" land with broad potential, might be suitable for converting to agricultural production at minimum cost. Thus, we aggregate these two land uses (call them "productive acres" for ease of exposition) and interact the number of productive acres with the pre- and post-moratorium dummy variables. The estimated value of a permit from this model is somewhat lower than the first two models ($420), but again, is not significantly different than the estimates from model 1 or 2.

Model 4 considers the possibility that agricultural producers in the area may have had some prior warning of the moratorium. Recall, the moratorium was officially announced one month prior to its effective date. However, an informal statement suggesting that permits may not be granted indefinitely in the basin had been made by the Director of the agency earlier in the year, which may have suggested that a moratorium was coming at some point. Thus, uncertainty regarding the future availability of permits may have been capitalized into land prices prior to December 1999. Model 4 is identical to model 2, except it uses a cut-off date for post-moratorium sales of May 1, 1999. Results are qualitatively unchanged, although the coefficient estimates indicating the value of a permit post-moratorium are smaller and have increased standard errors. This result is consistent with our original presumption that there was no effective prior warning of a moratorium. If we erroneously attribute sales prior to the moratorium as being part of post-moratorium sales, then we would expect the coefficient estimates indicating the value of permits post-moratorium to biased towards zero.[28]

V. CONCLUSIONS

While water *rights* have been extensively studied in the west, we know very little regarding the value of water *permits* for agricultural purposes in the eastern U.S. Permits for water use in the eastern half of the U.S. may not be traded or leased and thus market values for water use in agricultural production are not directly observable. We exploit a policy change by the state of Georgia, which placed a moratorium on the issuance of new water-use permits in 1999, to estimate the value of water use permits as capitalized into agricultural land values post-moratorium.

Overall, our results indicate that permits confer substantial value to agricultural land once access to permits is restricted. For productive agricultural acreage, we find that post-moratorium, land with a permit sells for approximately $500/acre more than land without a permit.[29] The median sales price of an acre of land during our study period is $1,500 (2003 dollars), indicating approximately a 30% increase in property values if a permit for irrigation has

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[28]We do not know the exact date at which the Director made an informal comment about the potential restriction on permits, only that it occurred in the spring of 1999. Thus, we estimated models which assume a variety of cut-off dates for what is considered to be a post-moratorium sale. The earlier the assumed cut-off date, the lower the estimated coefficient for the value of a permit post-moratorium.

[29]We recognize that our estimates are generated from a relatively small number of parcels that sold post-moratorium with a permit. However, they do represent all parcels that sold within our study area post-moratorium. While caution should be used to extrapolate our results to other counties and settings, we do note that our estimates are within the range of "conventional wisdom" regarding the value of permits in the agricultural region we study. Real estate agents in the area loosely estimate the value per acre of a permit to be between $500 and $700.

been granted to the parcel.

We have no evidence from the eastern US to which we can compare these values, and so we compare our estimates to those found from western water markets. Of course, the observed market values for water rights in the west are not strictly comparable to those we estimate. Our estimate is based on revealed preference techniques and indicate the value of a permit that is tied to the land. The market forces at play are very different than those observed in the west where water rights are separable from the land and may be traded for use in agriculture, industry or municipal purposes. Further complicating our comparison is that water prices in the western-half of the U.S. are denominated in dollars per acre-foot of water per year.[30] Our estimate of the value of a permit is denominated in dollars per acre of land to which the permit is attached. Because agricultural water permits in Georgia allow the permit holder to irrigate as much as desired, we must estimate the expected average irrigation needs in Dooly County to convert our estimate of dollars per acre of land to dollars per acre-foot of water.

During the years 1989 to 2004, drought conditions existed 25% of the time, very wet conditions were experienced 20% of the time, and "normal" rainfall was experienced the remaining 55% of the years. A rough estimate of water use during normal and wet years is 7 and 4 inches of water applied per acre during the growing season, respectively. Estimates of water use during dry years are approximately 1 to 1.5 feet of water applied per acre during the growing season. Given these estimates, and assuming farmers consider the past 16 years representative of the future, an expected average annual irrigation need would be approximately eight and a half inches, or 0.7 acre-feet per acre. Thus, assuming a 3% interest rate and a 30 year time

[30]One acre-foot of water is enough water to cover one acre of land one foot deep in water, which is 325,851 gallons.

horizon, our estimated value per acre of land of $500, translates to an implied annuity value $24.77 for 0.7 acre-feet of water, or $35.38 per acre-foot of water per year.

Brown (2004) collected data on water market transactions in fourteen western states from 1990-2003, and reports a median price of an acre-foot of water for the purposes of irrigation to be $28 (2003 dollars) — somewhat smaller than our estimate for Georgia.[31] However, median prices for the purpose of irrigation varied substantially across western states, from $72/acre-foot in Colorado to $4/acre-foot in Idaho (Brown, 2004). If one excludes transactions in the state of Colorado, the median annualized price for water transfers was $16/acre-foot. Our estimate of the value of a permit to irrigate is in the upper range of these western values (e.g., annualized median prices are $24/acre-foot in Texas and $45/acre-foot in Arizona and California).

As conflict over water allocation between urban and rural economies becomes more common in the eastern U.S., it is important to understand the value of water-use to each of the stakeholders. An *ex-ante* estimate of the value of water use in agricultural production is one important input into debates about water allocation among competing demands. This research provides the first estimate of the value of water in agricultural irrigation in the South Eastern United States.

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[31]Brown emphasizes the use of median prices, rather than mean prices in his analysis because he finds that the price distributions are heavily skewed, and so median prices better reflect the price of a typical water sale.

Table 1. Property Sales Description*

Variable Name	Variable Description	Summary Statistics
Sales Price	Sales price of property	$175,800[a] (238,000)
	Acreage and Land-Types	
Total Acres	Total acres included in the sale.	141.7[a] (169.1)
Crop	Number of acres included in the sale that are designated as open and/or crop-land by Dooly County tax assessors office.	13,805 acres total (30% of total acreage); mean=43 acres per sale
Recres	Number of acres included in the sale that are designated as recreation/residential by Dooly County tax assessors office. This land is characterized as having some water available on the land, some wooded coverage, and is considered suitable for wildlife habitat and hunting.	24,115 acres total (52% of total acreage); mean=74 acres per sale
Woods	Number of acres included in the sale that are designated as woodland, orchards, or ponds by Dooly County tax assessors office. Woodlands are the dominate land-use in this category, with over 7,000 acres recorded as woodland.	7,993 acres (17% of total acreage); mean=17 acres per sale
Bestsoil	Categorical variable equal to one if the acreage is considered to have above average soil quality (a code of 1 or 2 in the Dooly County soil classification).[b]	28,294 acres (62% of total)
Avgsoil	Categorical variable equal to one if the acreage is considered to have above average soil quality (a code of 3 in the Dooly County soil classification).[b]	6,365 acres (14% of total)
Worstsoil	Categorical variable equal to one if the acreage is considered to have above average soil quality (a code of 4, 5, or 6 in the Dooly County soil classification).[b]	11,254 acres (24% of total)
	Other characteristics of the land and its improvements	
Conservation Use	=1 if the property was in a conservation use program at the time of sale, =0 otherwise.	16 properties[c] (5%)
Level Lot	=1 if lot is considered level by the tax assessors office, =0 otherwise	53 properties[c] (16%)

Variable Name	Variable Description	Summary Statistics
No Water	=1 if the parcel does not have municipal or well water, =0 otherwise	25 properties[c] (8%)
Overall Desirability	=1 if property is designated as being of average, or above-average desirability, =0 otherwise. This variable is coded from the tax assessor's categorization of desirability, on a 5 point scale (with 3 being average).	315 properties[c] (97%)
Peanut	=1 if the sale included a transfer of a peanut quota, =0 otherwise.	33 properties[c] (10%)
Timber	=1 if the land included marketable timber, =0 otherwise.	40 properties[c] (12%)
Improvement	=1 if property has an improvement on site (e.g., home or mobile home), =0 otherwise.	114 properties[c] (35%)
Totalheat	Total heated area of all housing improvements included in the sale.	1,838[d] (1,747)
Above Average Quality	= 1 if the weighted average construction quality of all improvements on-site is one standard deviation above the mean construction quality in the sample, =0 otherwise.	33 properties (of 114 with improvements)
Accessory Value	= value of all accessories included in the sale. Computed by subtracting the assessed value of land and improvements from the total assessed value.	11,569 (49,041)[a]
	Permits and Basin Information (see also Table 2)	
flintbasin	=1 if a property is located in the Flint basin, =0 otherwise.	230 properties (71%)
otherbasin	=1 if a property is located in the Ocmulgee (98 properties) or Suwannee (3 properties) basin, =0 otherwise.	94 properties (29%)
$Permit^{Pre}$	=1 if property had an irrigation permit at time of sale and the sale occurred <u>either</u> pre-moratorium (for properties located in the Flint River basin) or during any year if the property is located outside the Flint River basin (i.e., in either the Swannee or Ocumulgee basins).	33 properties (10%)
$Permit^{Post}$	=1 if property had an irrigation permit at time of sale and the sale occurred in the Flint basin post-moratorium	13 properties (4%)

[*] Source: Dooly county tax assessors office.
[a] Mean (standard deviation).
[b] Dooly County tax assessors aggregate a 1-9 quality index assigned by the state of Georgia (originally developed for parcels in conservation-use programs, but every property in Dooly county is given soil quality codes). The state's quality index is based on the 107 soil types found in the state. The codes that Dooly County developed are =1 if best soil, =2 if second-best soil, =3 if average soil, =4 if fourth-best soil, =5 if fifth-best soil, and =6 if wetland/swamp.

[c] The number of properties with specific characteristic present (in parentheses is the percentage of properties with the specific characteristic present).
[d] Mean (standard deviation) is reported for just the properties which have a value of this variable that is greater than zero.

Table 2. Summary of major land-use types and permit holdings in Dooly County sales data.

	Land-Use Category			
	Cropland	Recreation/ Residential	Woods, etc.	Total
Mean acres [range] for all parcels.				
Land having a Flint permit at time of sale.	122 [0 - 543]	77 [0 - 482]	37 [0 - 120]	236 [3 - 657]
Land having a Suwannee or Ocmulgee permit at time of sale.	176 [0 - 814]	25 [0 - 176]	52 [0 - 183]	253 [24 - 91400]
Land having no permit at time of sale.	26 [0 - 765]	77 [0 - 1,118]	22 [0 - 780]	1267 [2 - 1,118]
Mean acres [range], not including parcels with 0-acres in a specific land-use.				
Land having a Flint permit at time of sale.	223 [51 - 543]	150 [42 - 482]	44 [1 - 120]	—
Land having a Suwannee or Ocmulgee permit at time of sale.	241 [12 - 814]	93 [33 - 176]	64 [3 - 183]	—
Land having no permit at time of sale.	128 [1 - 765]	115 [9 - 1,118]	48 [1 - 779]	—

Table 3. Model results with aggregated land use.*

	Model 1	Model 2
Conservation Use	0.3990[b] (0.1943)	0.4055[b] (0.1898)
Level Lot	-0.2759 (0.1796)	-0.2779 (0.1800)
No Water	0.1985 (0.1991)	0.2072 (0.1989)
Overall Desirability	1.319[a] (0.2347)	1.214[a] (0.2435)
Peanut 1	0.4210[a] (0.1186)	0.4657[a] (0.1252)
Timber1	0.3416[b] (0.1383)	0.3171[b] (0.1436)
Total Heat	0.00005[b] (0.00002)	0.00005[b] (0.00002)
Total Heat * Above Average Quality	0.00026[a] (0.00008)	0.00026[a] (0.00009)
Total Heat/Number of Improvements on Site	-0.0002[b] (0.00009)	-0.0002[b] (0.0000)
Accessory Value[d]	0.00006[a] (0.000007)	0.000006[a] (0.00001)
totalacres	0.0036[a] (0.0005)	0.0035[a] (0.0005)
totalacres* permit ^{pre}	0.0003 (0.0006)	0.0005 (0.0006)
totalacres* permit ^{post}	-0.00002 (0.0006)	-0.00002 (0.0006)
percent above average		-0.2346 (0.1844)
percent average		0.0189 (0.3193)
flintbasin*post		0.1023 (0.1877)
otherbasin*pre		0.0566 (0.1225)
otherbasin*post		0.0930 (0.2705)
Number Obs.	324	324
F (p-value)	15.3 (0.000)	13.3 (0.000)

* Dependent variables is ln(salesprice) for all models. Robust standard errors are reported in parentheses. A series of year-specific dummy variables are also included in the models, but not reported here for succinctness.

[a] Indicates significance at the 1% level.

[b] Indicates significance at the 5% level.

[c] Indicates significance at the 10% level.

[d] Accessory Value is in thousands of dollars.

Table 4. Model results with dis-aggregated land use.[*]

	Model 1	Model 2	Model 3	Model 4[**]
crop	0.0026[b] (0.0011)	-0.00008 (0.0009)		-0.0001 (0.0009)
recres	0.0041[a] (0.0005)	0.0044[a] (0.0006)		0.0044[a] (0.0006)
woods	0.0041[a] (0.0009)	0.0049[a] (0.0012)	0.0041[a] (0.0008)	0.0049[a] (0.0010)
crop/recres			-0.0034[a] (0.0005)	
crop*permit^{pre}	0.0007 (0.0015)	0.0014 (0.0011)		0.0016 (0.0010)
crop*permit^{post}	0.0026[b] (0.0013)	0.0029[a] (0.0011)		0.0022[c] (0.0013)
recres*permit^{pre}	0.0003 (0.0007)	-0.00006 (0.0007)		-0.0002 (0.0008)
recres*permit^{post}	0.0029[a] (0.0010)	0.0034[a] (0.0011)		0.0026[a] (0.0008)
woods*permit^{pre}	0.0022 (0.0030)	-0.0006 (0.0022)	-0.0011 (0.0021)	-0.0016 (0.0023)
woods*permit^{post}	-0.0098[a] (0.0027)	-0.0125[a] (0.0028)	-0.0131[a] (0.0032)	-0.0074[c] (0.0043)
crop/recres*permit^{pre}			-0.00001 (0.0008)	
crop/recres*permit^{post}			0.0024[a] (0.0007)	
percent crop above average		1.666[a] (0.2899)		1.7165[a] (0.2909)
percent crop average		2.412[a] (0.6636)		2.0317[a] (0.6588)
percent recres above average		0.3646[c] (0.1973)		0.3531[c] (0.1964)
percent recres average		0.4163 (0.3315)		0.3970 (0.3349)
percent woods above average		1.152[a] (0.3195)	1.1858[a] (0.3992)	1.1543[a] (0.3281)

continued, next page

	Model 1	Model 2	Model 3	Model 4**
percent crop/recres above average			0.4869[b] (0.1959)	
percent crop/recres average			0.3845 (0.3204)	
flintbasin*post		0.0818 (0.1796)	0.0895 (0.1827)	-0.1227 (0.2581)
otherbasin*pre		-0.0025 (0.1244)	0.0024 (0.1294)	0.0495 (0.1331)
otherbasin*post		0.1649 (0.2680)	0.1216 (0.2622)	-0.2611 (0.2710)
Number Obs.	324	324	324	324
F (p-value)	19.6 (0.000)	17.5 (0.000)	15.4 (0.000)	17.0 (0.000)
Permit Value post-moratorium for Crop Land[d]	453 [10 - 897]	513 [117 - 910]		387 [-54 - 828]
Permit Value post-moratorium for Rec./Res. Land[d]	522 [175 - 869]	603 [229 - 978]		463 [180 - 745]
Permit Value post-moratroium for Crop/Rec./Res. Land[d]			413 [14 - 686]	

* Dependent variables is ln(salesprice) for all models. Robust standard errors are reported in parentheses. Each model reported also contains the first ten variables reported in Table 3, as well as a series of year-specific dummy variables. Results for these variables are not reported here for succinctness, but are available from the authors upon request.

** Model 4 is identical to Model 2, but uses a beginning date of May 1, 1999 to signify the beginning of the moratorium.

[a] Indicates significance at the 1% level.

[b] Indicates significance at the 5% level.

[c] Indicates significance at the 10% level.

[d] Values are $/acre, evaluated at the mean sale price for all sales. The 95% confidence interval is in brackets.

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Welfare Implications of the Policy Process: Estimating Context-Sensitive Willingness to Pay for Agricultural and Open Space Conservation

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Abstract:

Economists frequently assess willingness to pay (WTP) for land preservation outcomes independent of information regarding policy implementation. The public, however, may not only be concerned with the consequences of land management, but also may have systematic preferences for policy procedures applied to achieve management goals. This paper examines relationships between preferences for land preservation outcomes and attributes of the policy process, considering preferences for farm and forest preservation in two Northeastern states. The approach departs from traditional welfare assessments in that it does not constrain attributes of the policy process to be utility-neutral. Results indicate that utility is influenced by policy process attributes, even after controlling for the influence of land use outcomes often correlated with specific policy techniques. Results further suggest that even comprehensive specification of land use outcomes by stated preference instruments may be insufficient to prevent systematic shifts in WTP related to unspecified, yet assumed, policy process attributes.

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Introduction

Farm and forest preservation may be accomplished using a variety of policy techniques, and implemented by a range of public and private agents (American Farmland Trust 1997).[1] Economists, however, frequently assess willingness to pay (WTP) for preservation outcomes independent of information regarding the policy process or with little or vague reference to techniques of policy implementation. This follows the standard neoclassical purchase model, in which utility and WTP are assumed to be determined solely by policy outcomes, independent of the policy process leading to those outcomes (Bulte et al. 2005; Kahneman et al. 1993). Following this implicit framework, stated preference (SP) analyses of land preservation typically suppress most information regarding policy implementation, or assesses welfare contingent upon a single, often vaguely described policy process (Johnston et al. 2003; e.g., Halstead 1984; Beasley et al. 1986; Ready et al. 1997; Bowker and Didychuk 1994; Duke and Ilvento 2004).

This common practice notwithstanding, recent evidence suggests that individuals may have systematic preferences for methods used to achieve policy outcomes in general (e.g., Bosworth et al. 2006; Bulte et al. 2005; Mansfield and Smith 2002), and land use outcomes more specifically (e.g., Inman and McLeod 2002; McLeod et al. 1998; McLeod et al. 1999; Johnston et al. 2003; Rosenberger et al. 1996). For example, Bosworth and Cameron (2006) show that WTP for mortality reductions vary according to whether those reductions are achieved using prevention or treatment mechanisms. Bulte et al. (2005) show that WTP to decrease wildlife reductions depends on whether reductions result from man-made or natural events. Regarding land use policy, Johnston et al. (2003) find that positive values for particular land use outcomes do not guarantee support for policies necessary to obtain those outcomes, while Inman and McLeod (2002) report preferences for public versus private land management. Focus groups of McLeod et al. (1998), moreover, suggest that residents' preferences can extend into such areas as fairness in enforcement of zoning regulations and the number of zoning variances granted. Despite such evidence, however, the published literature thus far provides no systematic, quantitative information on how WTP for farm and forest preservation may be influenced by the

[1] The land preservation *policy process* is a choice of preservation *technique* and implementing *agent*. Duke and Lynch (2006) identify 28 techniques used to preserve farm and forest land. The survey described in this paper focused on three of the most common, and thus familiar, techniques: conservation easements (described to respondents as "preservation contracts"); fee simple purchase ("outright purchase"); and enhanced zoning protections ("conservation zoning"). The survey also distinguished two types of agents implementing preservation techniques: state and local governments; and nongovernmental organizations, such as the Nature Conservancy or land trusts.

attributes of the policy process or details of policy implementation.

The omission of policy details from SP analysis of land use preferences is related to a fundamental and perhaps mistaken assumption that attributes of the policy process are utility-neutral. This assumption, if incorrect, can have significant implications both for the validity of welfare estimates and for the use of these estimates for policy guidance. For example, SP surveys that partially or completely omit information on policy implementation may cause respondents to assume that certain unanticipated policy techniques are applied. If these techniques are not utility-neutral, methodological misspecification (Mitchell and Carson 1989) may occur, leading to bias in resulting welfare estimates. Alternatively, if welfare estimates are contingent upon a single, non-utility-neutral policy technique, their application to policy would be limited to instances in which very similar or identical policy techniques are applied. Neither possibility is reflected in the current literature, which often compares WTP for outcomes irrespective of the attributes of the associated policy process.

This paper examines relationships between stated preferences for land preservation outcomes and attributes of the policy process, with regard to farm and forest preservation in two Northeastern states. The approach departs from traditional applied welfare assessments in that it does not constrain the attributes of the policy process to be utility-neutral. The model is constructed upon a more flexible representation of utility, designed to capture systematic changes in welfare related to the policy techniques used to obtain environmental outcomes. The associated choice experiment survey allows estimation of the systematic effects of policy implementation on utility, thereby providing welfare measures that reflect policy process information and avoiding potential bias associated with the omission of such details.

A Conceptual and Theoretical Model of Land Use Policy Preference

Systematic preferences for land preservation policy process attributes may emerge for at least two reasons. First, process attributes may *appear* to influence utility if they serve as proxies for unobserved land use outcomes. Such patterns may occur in both stated preference research and in actual processes used to create policy. For example, in the absence of information regarding public access, respondents might assume—correctly in many cases—that conservation easements are less likely to provide access than the fee simple technique (American Farmland Trust 1997). Individuals might also associate particular policy processes with increased or decreased probability of long-term preservation success. Still others might associate certain policy techniques with an increased realization of rents or personal benefits associated with environmental policies (Mansfield and Smith 2002). Such patterns lend themselves to a more traditional interpretation of utility, in which policy process attributes are not truly valued, but rather proxy for omitted yet nonetheless utility-relevant land use outcomes.

A second possibility, however, is that respondents might indeed maintain systematic preferences for particular policy tools apart from any measurable land use outcome. For example, some respondents might maintain a systematic preference for government involvement in land preservation—apart from any observable outcome of that intervention (Inman and McLeod 2002; Johnston et al. 2003). Residents might also believe that certain policy actions represent an inappropriate use of public (or private) authority or funds. Such preferences might manifest in a change in utility associated with government-implemented policies, apart from any land use outcome of those policies (cf. Inman and McLeod 2002; McLeod et al. 1999). Beyond preferences for public versus private involvement, individuals might maintain altruistic preferences for consumption bundles realized by others (McConnell 1997), leading to varying support for land use policies anticipated to generate particular distributions of costs and benefits.[2] In other instances respondents may show clear preferences for the distribution of program costs across different groups, aside from any effects related to their personal household costs (Mansfield and Smith 2002). To the extent that such preferences (e.g., altruism) are of the type that should legitimately be incorporated in benefit cost analysis (Freeman 2003, p. 150), associated WTP measures represent a legitimate component of welfare analysis that is not associated with traditionally measured land use outcomes.

The former case—in which policy process attributes proxy for missing land use outcome attributes (e.g., public access provisions)—is most appropriately addressed though more complete specification of the vector of relevant land use outcome attributes, based on evidence from appropriate survey design methods (Kaplowicz et al. 2004; Johnston et al. 1995). That is,

[2] This paper distinguishes *land use* outcomes typically represented in survey instruments from other attributes that may or may not be appropriately characterized as policy "outcomes". For example, as a semantic matter, one might define altruistic preferences as related to a measure of policy "outcome," in this case related to benefit distributions associated with particular policy tools. However, even if such attributes are defined as outcomes, they are nonetheless independent of the typical *land use* outcomes typically represented in stated preference research. Moreover, if certain distributions are unique features of specific policy techniques, it may be difficult to distinguish preferences for the policy from preferences for the distributional outcome. In either case, such preferences are not appropriately captured by stated preference instruments that omit details of the policy process.

the apparent utility effects of policy process attributes would indicate that utility-relevant outcomes have not been sufficiently specified in survey scenarios—a potential source of bias in SP welfare estimation. The latter case, however, represents a situation in which utility is systematically influenced by policy process attributes, even after accounting for the full set of land use outcomes that enter the utility function. Such effects are denoted 'pure' policy preferences. In such cases, WTP estimates associated with land use outcomes alone (i.e., in the absence of information regarding policy implementation) will at best provide misleading or partial welfare guidance. Moreover, the omission of utility-relevant policy process attributes may generate statistical biases in WTP related to the methodological misspecification of valuation contexts (i.e., respondents' unobserved yet potentially systematic assumptions regarding applied policy process attributes).

The Theoretical Model

The theoretical model is derived from the standard random utility specification in which utility is divided into observable and unobservable components (Hanemann 1984). Given the emphasis on pure policy preferences, a critical element of the model is the experimental control of utility-relevant land use attributes often omitted from SP analyses, yet potentially associated with particular policy techniques (e.g., public access attributes). Without this control, that which appears to be a systematic preference for process attributes may instead be a preference for omitted land use outcomes. The theoretical model hence distinguishes between land use outcomes assumed to be independent of the policy process attributes in question and those assumed to be related to process attributes.

To model individual *i*'s choices among preservation programs, we define a utility function including outcomes and policy process attributes of preservation plan *j* and the net cost of the plan to the respondent (Hanemann 1984; McConnell 1990),

$$U_{ij}(\cdot) = U_{ij}(\mathbf{X_{ij}},\mathbf{W_{ij}}, Y_i\text{-}F_{ij}) = v(\mathbf{X_{ij}},\mathbf{W_{ij}}, Y_i\text{-}F_{ij}) + \varepsilon_{ij} \qquad (1)$$

where

$\mathbf{X_{ij}}$ = a vector of variables describing land use outcomes of preservation program *j*;

$\mathbf{W_{ij}}$ = a vector of variables describing the policy process of preservation program *j*;

Y_i	=	disposable income of respondent *i*;
F_{ij}	=	the cost to the respondent of preservation plan *j*, through a mandatory payment vehicle;
$v_{ij}(\cdot)$	=	a function representing the empirically measurable component of utility;
ε_{ij}	=	the unobservable or random component of utility, modeled as econometric error.

The vector, $\mathbf{X_{ij}} = [\mathbf{X_{ij1}} \mid \mathbf{X_{ij2}}]$, is further partitioned such that $\mathbf{X_{ij1}}$ is a sub-vector representing land use outcomes assumed independent of $\mathbf{W_{ij}}$, or delivered equally regardless of the details of policy implementation. Examples of attributes in $\mathbf{X_{ij1}}$, depending on the policy context, might include the number of acres and type of land conserved.[3] In contrast, $\mathbf{X_{ij2}}$ represents land use outcomes assumed to be related to at least one element of $\mathbf{W_{ij}}$, or whose delivery depends on the specific attributes of policy implementation. Attributes in $\mathbf{X_{ij2}}$ might characterize such amenities as public access, which is likely to vary depending on preservation techniques used. The elements in $\mathbf{X_{ij1}}$ and $\mathbf{X_{ij2}}$ are likely to vary according to the policy context.

Given the above specification, individual *i* chooses among three policy plans, (j=A,B,N). The individual may choose option *A*, option *B*, or may reject both options and choose the status quo (neither plan, j=N). A choice of neither plan would result in zero preservation and no preservation policy, $\mathbf{X_{ij}}= \mathbf{W_{ij}}=0$, and zero household cost, $F_{ij}=0$. The model assumes that individual *i* assesses the utility that would result from available choice options (j=A,B,N) and chooses that which offers the greatest utility. Given (1), individual *i* will choose plan A if

$$U_{iA}(\mathbf{X_{iA}},\mathbf{W_{iA}}, Y_i\text{-}F_{iA}) \;\geq\; U_{ik}(\mathbf{X_{ik}},\mathbf{W_{ik}}, Y_i\text{-}F_{ik}) \quad \text{for } k=B,N, \tag{2}$$

such that

$$v_{iA}(\mathbf{X_{iA}},\mathbf{W_{iA}}, Y_i\text{-}F_{iA}) + \varepsilon_{iA} \;\geq\; v_{ik}(\mathbf{X_{ik}},\mathbf{W_{ik}}, Y_i\text{-}F_{ik}) + \varepsilon_{ik.} \tag{3}$$

If the ε_{ij} are assumed independently and identically drawn from a type I extreme value distribution, the model may be estimated as a conditional logit model (Maddala 1983; Greene 2003).

The partitioning of $\mathbf{X_{ij}} = [\mathbf{X_{ij1}} \mid \mathbf{X_{ij2}}]$ is not necessary for model estimation. However, the specification is useful to understand potential ramifications of omitting $\mathbf{X_{ij2}}$ or $\mathbf{W_{ij}}$ from an SP scenario. First, assume that a valuation scenario includes $\mathbf{X_{ij1}}$ and $\mathbf{X_{ij2}}$, but omits $\mathbf{W_{ij}}$. If

[3] Although one could easily think of counter-examples in which the number of acres that could be conserved would depend on the techniques used for preservation.

$\partial v(\cdot) / \partial \mathbf{W_{ij}} \neq 0$ and respondents make systematic assumptions concerning elements of $\mathbf{W_{ij}}$ based on the correlated elements of $\mathbf{X_{ij2}}$ present in the survey scenario, the result will be biased, inconsistent estimates for *all model parameters*, including those associated with $\mathbf{X_{ij1}}$ (Greene 2003).[4] If the analyst includes $\mathbf{W_{ij}}$ in survey scenarios but omits $\mathbf{X_{ij2}}$, the results are analogous. The problem lies in the assumption by the analyst that choices depend *only* on attributes incorporated in the SP scenario. However, the assumed correlation of $\mathbf{X_{ij2}}$ and $\mathbf{W_{ij}}$ may lead to choices that depend on values for elements of $\mathbf{X_{ij2}}$ and $\mathbf{W_{ij}}$ that are *assumed* by respondents, despite their omission from the SP scenario and associated statistical model. The result is a combination of methodological misspecification (the behavioral implication) and bias in associated parameter and welfare estimates (the statistical implication).

The implication of this model is that appropriate estimation of WTP—including marginal WTP for specific land use attributes that may not be correlated with omitted policy process attributes ($\mathbf{X_{ij1}}$)— may depends on an appropriate specification of utility-relevant policy process attributes, $\mathbf{W_{ij}}$, in SP scenarios. Omission of these attributes will bias estimated model parameters. The crucial hypotheses, then, is whether $\partial v(\cdot) / \partial \mathbf{W_{ij}} = 0$, i.e., whether attributes of the policy process are utility-neutral.

The Data

To test this hypothesis, a stated preference model is estimated for land preservation preferences using choice experiment data. The resulting random utility model provides a systematic assessment of the impacts of policy attributes on WTP, holding associated land use outcomes constant. The data are drawn from the *Mansfield* and *Preston Land Preservation Surveys* in CT and the *Georgetown* and *Smyrna Land Preservation Surveys* in DE. Surveyed communities were selected based on a number of factors, including the presence of similar and increasing development pressures, the lack of a major urban center in close proximity, and the existence of substantial areas of undeveloped (farm and forest) land. The combination of data from two non-adjacent states allows a least preliminary assessment of the robustness of results

[4] The result is omitted variables bias in a discrete choice model, in which observed choices depend on an assumed set of values for policy process variables that are nonetheless omitted from the statistical model. Yatchew and Griliches (1984) demonstrate that such omitted variables result in bias and inconsistency across the full range of estimated parameters in discrete choice models, regardless of the orthogonality of included and omitted variables.

across regions.

Survey development required over 18 months of background research, interviews with land use experts and stakeholders, and 14 focus groups (Johnston et al. 1995) including cognitive interview sessions (Kaplowicz et al. 2004). Extensive pretests were conducted in focus groups and interviews to ensure that the survey language and format could be easily understood by respondents and that respondents shared interpretations of survey terminology and scenarios. Focus groups led to a self-administered, mail survey design, following the choice experiment framework (Opaluch et al. 1993; Adamowicz et al. 1998).

Prior to the administration of choice experiment questions, the survey provided extensive background information on such features as the details of land use and land change in respondents' local area, tradeoffs implicit in land conservation and reminders of budget constraint, and techniques used to preserve farm and forest land. The survey also provided instructions and information on the subsequent choice experiments, including details of certain attributes. This included potential attribute levels that might occur in choice questions, following guidance in the literature to provide visible choice sets (Bateman et al. 2004).

The choice experiment asked respondents to consider alternative preservation options for hypothetical parcels of farm or forest land located in their community. Respondents were provided with two preservation options that would each preserve a single parcel of land of varying attributes, "Option A" and "Option B," as well two status quo options that would result in no policy change. The first status quo option stated simply, "I would not vote for either program," following standard language in choice experiment surveys (Bennett and Blamey 2001). The second option stated, "I support these programs in general, but my household would/could not pay for either Option A or B." This latter option was included based on focus group results and findings of prior research (e.g., Loomis et al. 1999; Brown et al. 1996) as an outlet for those who might wish to express symbolic support for land preservation, yet nonetheless would not pay for either of the provided options. Specifically, it was designed to ameliorate the potential quandary facing "individuals who would not pay the bid amount, but nevertheless want to register [symbolic] support for provision of the public good" (Loomis et al. 1999). For purposes of estimation the two status quo options—both indicating a choice of no preservation—were combined into a single choice category.[5]

[5] Fundamental model results are unchanged by this treatment of the responses.

Each respondent was provided with three choice experiment questions and was instructed to consider each as an independent, non-additive choice. Attributes characterized land use outcomes identified by focus groups, interviews, and background research as significant to choices among land preservation options. These included land use outcome attributes assumed to be in vector $\mathbf{X}_{ij1}$ (attributes provided approximately equally by a wide range of policy processes) and those in vector $\mathbf{X}_{ij2}$ (attributes whose provision often varies according to the specific preservation method applied). Attributes characterized such features as the type of land preserved, the number of acres, the provision and type of public access, the likelihood of development of unpreserved parcels, and the cost of preservation to the respondent's household.

Choice questions also specified elements of $\mathbf{W}_{ij}$, including the specific *method* that would be used to preserve each parcel in question, as well as the *agent* that would be responsible for implementing the technique. Techniques included fee simple purchase, conservation easements, and conservation zoning. Implementing agents for the easement and fee simple techniques included the state government or local land trusts.[6] The survey provided detailed information on each of these policy attributes prior to administration of choice questions. Table 1 describes the attributes distinguishing hypothetical preservation options.

The experimental design was constructed by the University of Delaware STATLAB using a fractionated D-optimal design tailored to choice experiment data (cf. Kuhfeld and Tobias 2005). The design is significantly larger than typical main effects plans designed for linear regression (500 unique sets of three choice questions), and allows for estimation of a wide range of main effects and interactions with relatively high efficiency. The survey was implemented from October 2005 to January 2006. Surveys were mailed to 3000 randomly selected residents of the four CT and DE communities (750 surveys per community), following Dillman's (2000) survey design method. Of the 2763 deliverable surveys, 1136 were returned, for an average response rate of 41.1%. Returned surveys provide 3309 complete and usable choice responses.

The Empirical Model

Results are based on a pooled discrete choice model combining observations from both states. To allow for preference heterogeneity across the two states, however, two models are

[6] Given that zoning in Connecticut and Delaware is implemented at the local level, this method was always associated with local government.

estimated—one that constrains parameter estimates to be equal across the two states, and another that allows for systematic differences in parameter estimates using fixed effects. In addition, the Swait-Louviere procedure (Swait and Louviere 1993) is applied to account for potential differences in the scale parameter across the CT and DE data.

As the final data are comprised of three responses per survey, there is a possibility that responses provided by individual respondents may be correlated even though responses across different respondents are considered *iid*. Moreover, conditional logit (CL) models are subject to the restrictive independence from irrelevant alternatives (IIA) property. For both reasons, researchers are increasingly considering mixed logit (ML) models for SP applications (Greene 2003; McFadden and Train 2000; Hensher and Greene 2003). ML models allow for coefficients on attributes to be distributed across sampled individuals, according to a set of estimated parameters and researcher-imposed restrictions (Hu et al. 2005). While ML requires a greater number of researcher choices regarding model specification (e.g., the specification of fixed versus random parameters, the assumed distribution and correlation of random parameters, etc.), they have much greater flexibility and can indeed approximate any random utility model (Greene 2003; McFadden and Train 2000; Hensher and Greene 2003). For comparison, both CL and ML specifications of the final model are presented.

Although the most flexible ML specifications allow for a random distribution of the entire parameter vector, in practice one may experience difficulties in convergence when large numbers of random parameters are incorporated (e.g., Layton 2000; Johnston et al. 2003). Here, the inclusion of large numbers of random parameters led to repeated convergence failure, despite various specifications of the model and simulation procedure. Accordingly, the ML model is estimated following Layton (2000) with only the parameter on program cost random across respondents. Following common practice, the parameter is estimated with a lognormal distribution and sign-reversal on the cost variable (Hensher and Greene 2003).[7] The model is estimated using maximum likelihood for mixed logit with Halton draws applied in the likelihood simulation.

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[7] Preliminary models with *cost* distributed normally and with a triangular distribution underperformed (in terms of model fit, log likelihood, and variable significance) the model including lognormally distributed cost. An advantage of the lognormal distribution is that it constrains the parameter on program cost to be negative (or positive for sign-reversed cost), implying a positive marginal utility of income. Hence, this distribution is often used for the payment vehicle in stated preference models. However, a disadvantage of the lognormal distribution is the characteristically "fat" right tail, which tends to lead to unrealistic mean WTP values calculated over the full distribution (Hensher and Greene 2003).

Results

Results for three models are illustrated in table 2, including two CL specifications and one ML specification. Model one is an unrestricted CL model, with dummy variables allowing for systematic variation in estimated parameters across the CT and DE samples.[8] Model two is a restricted CL model in which a single set of parameters is estimated across the pooled data. Both models are statistically significant at better than $p<0.01$. A Swait-Louviere test (Swait and Louviere 1993) fails to reject the null hypothesis of equal variances (or scale) across the CT and DE data (χ^2=0.95, p=0.33), while a likelihood ratio test of the restricted versus unrestricted model fails to reject the null hypothesis of equivalent parameter estimates across CT and DE samples (χ^2=8.13, p=0.36). The combination of these two tests provides no evidence of statistically significant preference heterogeneity or difference in scale between responses from the two states. Hence, subsequent discussion emphasizes results of the simpler pooled model.

Model three is an ML specification of the pooled model. As noted above, *cost* is specified random with a lognormal distribution and sign-reversal (i.e., cost data enters the model as a negative variable). A likelihood ratio test of the ML versus CL model (model three compared to model two) rejects the null hypothesis of a fixed coefficient vector (χ^2=1176.19, $p<0.01$), with the standard deviation on cost significant at $p<0.01$. Beyond statistical significance, however, the relatively large standard deviation on *cost* suggests substantial heterogeneity in the marginal utility of this attribute. Given the superior performance of the ML model relative to the CL model, subsequent discussion emphasizes ML results (model three).

Of 22 estimated parameters in the ML model, 17 are statistically significant at $p<0.10$ or better, with signs of significant parameters conforming to prior expectations, where expectations exist (table 2). Respondents prefer options that preserve a greater number of acres (*acres*>0), provide public access (*walking* and *hunting* >0) and target parcels at higher risk of development (*dev_not_30* and *dev_10_30* <0). Moreover, public access for walking and biking is preferred to public access for hunting (*walking* > *hunting*), supporting prior findings preference for public access differs according to the type of access provided, particularly in cases where certain types of access (e.g., hunting) may be assumed by respondents to have at least some negative consequences (e.g., McGonagle and Swallow 2005; Johnston et al. 2005).

[8] Slopes are permitted to vary systematically for all linear (non-interaction) variables.

Interestingly, while policy process attributes are statistically significant (see discussion below)—along with attributes characterizing public access, parcel size, development risk, and cost—land *type* attributes are not significant (*nursery*, *forest*, *idle*). This pattern is robust across a wide range of preliminary and final model specifications. These results suggest that while respondents value the preservation of farm and forest land, and distinguish between attributes of preservation programs, the type of farm or forest preserved is not a statistically significant determinant of program preferences. While these results may be subject to the specific land types considered, they support some prior work showing, for example, that WTP changes with the agricultural productivity and/or type of farmland (Duke and Ilvento 2004) but contradict other work (Kline and Wichelns 1996; Ozdemir et al. 2004).

Policy Process Attributes and Preservation Preferences

The model specification was designed such that the effects of both preservation techniques and preservation agents could be estimated and distinguished. Policy process attributes are incorporated as four binary (dummy) variables allowing for systematic variation in utility, relative to the default of preservation accomplished using state-implemented conservation easements (tables 1, 2). The associated parameters capture the potential (marginal) influence of these attributes on utility. Alternatives include conservation easements implemented by local land trusts using government block grants (*tr_contract*), fee simple purchase by the state (*st_purch*), fee simple purchase by land trusts using government block grants (*tr_purch*), and conservation zoning (*zoning*).[9] Associated parameter estimates indicate influence on utility, holding constant other attributes such as public access and household cost.

All four parameter estimates are statistically significant at $p<0.10$, indicating that attributes of the policy process have a statistically significant influence on the utility of land preservation options. Variation in both preservation *techniques* and *agents* can have a significant influence on marginal utility. For example, compared to the default of state-implemented conservation easements, there is a statistically significant decline in marginal utility associated with otherwise identical contracts implemented by land trusts. As noted in table 1, both techniques were described as generating preservation that was "contractually and permanently"

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[9] Land trust activities were described as involving government block grants to motivate the payment vehicle (taxes) in choice questions.

guaranteed, regardless of the agency administering the programs. Nonetheless, choice model results show that estimated marginal utility for preservation conducted using land trust easements (*tr_contract*, $p<0.02$) is lower than that associated with otherwise identical state easements.

In contrast, preferences for fee simple purchase policies are virtually identical for land trust and state agents. Both *st_purch* ($p<0.07$) and *tr_purch* ($p<0.05$) are statistically significant, with parameter estimates that are statistically indistinguishable (-0.318 vs. -0.295). Interestingly, this suggests that respondents prefer state agencies to implement conservation easements but display *equal* preferences for fee simple purchase of farm and forest by public and private agents. These results indicate a fair degree of subtlety in respondents' preferences for—and ability to distinguish between—different types of preservation policies. That is, preference for publicly versus privately implemented preservation varies according to the type of preservation technique (fee simple versus easements) applied. Again, these results hold preservation likelihood and duration constant—in both cases preservation is described as being permanent and guaranteed.

Holding program cost and other attributes constant, the least preferred preservation technique is conservation zoning (*zoning*, $p<0.01$)—a result consistent with prior focus group findings.[10] Not only did focus group respondents associate zoning with the potential for additional restrictions on land use community-wide, but the survey noted that "[w]hile zoning can guarantee preservation in the short term, there is no guarantee that regulations will not be changed in the future so that land may be developed." Given the combination of the zoning impermanence and the potential for additional restrictions on personal land use, it is not surprising that marginal utility is lowest for programs including conservation zoning, *ceteris paribus*. As zoning is universally (at least in CT and DE) implemented at the local level, this technique was not allowed to vary according to implementing agency.

The statistical significance of the four policy process attributes is an important finding which—if applicable to a wide range of preservation contexts—calls into question the validity of both the utility specifications assumed by and the associated results of prior valuation research that suppresses information regarding policy implementation. Consequences could include omitted variables, associated bias in estimated parameters, and the possibility of inappropriate

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[10] Of course, in many instances zoning techniques may be less expensive than alternative means of land preservation. Hence, considering the combined utility associated with the variables *zoning* and *cost*, a cheaper zoning policy might be preferred to a more expensive purchase or easement policy.

welfare and policy guidance to land preservation agencies. Results also suggest a fair degree of subtlety in respondents' policy preferences—with, for example, preferences for particular policy techniques depending on the entity implementing those techniques. This is another result not well reflected in the SP literature, yet of potential relevance for welfare estimation and policy guidance.

Implications and Discussion

As noted by Inman and McLeod (2002, p. 93), "governments are unlikely to succeed in implementing land protection programs without support of their constituents." They, and other authors (e.g., Johnston et al. 2003, McLeod et al. 1999; Rosenberger et al. 1996) emphasize the relationships between policy techniques that are applied and constituents' support for land preservation. Further supporting the importance of the policy process to public preferences is evidence from the hedonic literature that property value impacts of open space depend on policy techniques used (e.g., conservation easements, fee simple purchase) to prevent development (Irwin 2002; Ready and Abdalla 2005). Such evidence notwithstanding, the dominance of the neoclassical purchase model has led researchers to suppress details of policy implementation in stated preference research, or at most to specify preservation as subject to a single, invariant policy process. The associated argument is that utility should be measured over "outcomes, not over what induced [those] outcome[s]" (Bulte et al. 2005).

Results from the current model highlight possible limitations of the standard approach. Choice model results illustrate the potential welfare implications associated with attributes of policy process, after controlling for possibly confounding factors such as public access, cost, land type, and likelihood of (preservation) permanency.[11] Results indicate that preferences can extend both to the type of preservation techniques applied as well as the agents that implement those techniques.

There are at least three potential explanations for the statistical significance of policy process attributes, each of which, at a minimum, implies some adjustment in SP methods for farm and forest valuation. First, despite extensive efforts to ensure that potentially confounding land use outcomes were controlled by the survey and experimental design, it is nonetheless possible that respondents may have viewed policy process attributes as proxies for heretofore

[11] The exception, of course, is zoning, which cannot be reasonably divorced from the possibility of future change.

unsuspected land use outcome attributes—a variant of methodological misspecification (Mitchell and Carson 1989; Johnston et al. 1995). If such is the case, despite our explicit efforts to eliminate such possibilities in survey and experimental design, this suggests that current specifications of land use outcomes in SP surveys are likely inadequate and that additional research is required to better identify welfare-relevant outcomes of land use policy.

Second, it is possible that respondents' choices reflect a genuine individual preference for policy process attributes, yet one that is not appropriate for inclusion in neoclassical welfare evaluation based on Pareto optimum allocations (cf. Freeman 2003, p. 150). For example, if certain policy process attributes are preferred due to *nonpaternalistic altruism*[12] or related concerns, the associated WTP—while measurable—would be irrelevant for welfare analysis (Lazo et al. 1997; McConnell 1997). In such cases—despite the welfare irrelevance of WTP measures—it is nonetheless critical to account for such factors in SP analysis to avoid statistical biases in discrete choice models and associated WTP estimates. Moreover, given that WTP for policy process attributes would be irrelevant for social welfare estimation in this case, it is critical to ensure that welfare estimates for land use preservation do not incorporate inappropriate WTP associated with assumed policy techniques.

A final possibility is that model results reflect a genuine and welfare-relevant preference for policy process attributes. For example, individuals might have systematic preferences for public versus private control of undeveloped lands related to strongly-held views regarding the appropriateness of certain types of public or private intervention (cf. Inman and McLeod 2002). Individuals might also prefer certain types of policy techniques (e.g., easements over fee simple purchase) due to a *paternalistic* concern for the consumption bundles of others (McConnell 1997)—for example a desire for landowners to retain the right to use their land for private purposes. If such preference patterns hold, then policy process attributes are indeed welfare-relevant, and should either be incorporated or controlled for in welfare analysis.

Like many research efforts, the present analysis perhaps raises more questions than it answers. Statistical results of the present analysis cannot unambiguously establish which of the above patterns apply here, nor which are more likely to influence farm and forest valuation more broadly. Nor do results indicate the extent to which similar results hold for other policy contexts,

[12] Nonpaternalistic altruism is defined by Freeman (2003, p. 150) as a case "where one individual cares about the general well-being of others but does not have any preferences regarding the composition of consumption bundles of others."

or for other potential case studies. However, results clearly reveal statistically significant preference and WTP patterns associated with both land use preservation policies and agents who implement those policies, *ceteris paribus*. Results also suggest caution in the comparison of welfare results across different policy contexts—a critical issue for benefits transfer—as WTP may not be directly comparable where different policy processes are applied to land preservation. Finally, model results suggest the potential benefit of additional research into the implications of the policy process for welfare estimation and benefit cost analysis. While our case study applies solely to farm and forest preservation, it is possible that such effects may apply more broadly, with implications for benefit cost analysis in a wide range of policy contexts.

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Table 1. Variables and Descriptive Statistics

Variable	Description	Mean (Std. Dev.)[a]
environmental	Binary (dummy) variable identifying respondents who report membership in environmental organizations.	0.189 (0.391)
age	Age of respondent, in years.	54.332 (15.637)
protest	Binary (dummy) variable identifying responses that show clear evidence of being protests.	0.004 (0.062)
college	Binary (dummy) variable identifying respondents with at least a four-year college degree.	0.400 (0.490)
CT	Binary (dummy) variable identifying respondents from Connecticut (omitted default is respondents from Delaware).	0.567 (0.496)
neither	Alternative specific constant (binary) identifying the status quo option (omitted default is Option B).	0.333 (0.471)
age_neither	Multiplicative interaction between *age* and *neither*.	18.111 (27.158)
env_neither	Multiplicative interaction between *environmental* and *neither*.	0.063 (0.243)
prot_neither	Multiplicative interaction between *protest* and *neither*.	0.001 (0.036)
coll_neither	Multiplicative interaction between *college* and *neither*.	0.133 (0.340)
option_A	Alternative specific constant (binary) identifying Option A (omitted default is Option B).	0.333 (0.496)
acres	Number of acres preserved (single parcel).	62.893 (70.337)
nursery	Binary (dummy) variable indicating that the parcel is an active nursery (omitted default is a food or dairy farm).	0.132 (0.338)
forest	Binary (dummy) variable indicating that the parcel is forest (omitted default is a food or dairy farm).	0.132 (0.339)
idle	Binary (dummy) variable indicating that the parcel is idle (non-active) farmland (omitted default is a food or dairy farm).	0.137 (0.343)
st_purch	Binary (dummy) variable indicating that preservation is accomplished through fee simple purchase of the parcel, implemented by the state (omitted default is preservation by state-implemented conservation easements).	0.219 (0.413)
tr_purch	Binary (dummy) variable indicating that preservation is accomplished through fee simple purchase of the parcel, implemented by the land trusts, using block grant funds from the state (omitted default is preservation by state-implemented conservation easements).	0.223 (0.416)
tr_con	Binary (dummy) variable indicating that preservation is accomplished through conservation easements, implemented by land trusts, using block grant funds from the state (omitted default is preservation by state-implemented conservation easements).	0.072 (0.257)

zoning	Binary (dummy) variable indicating that preservation is accomplished using conservation zoning (omitted default is preservation by state-implemented conservation easements).	0.079 (0.270)
walking	Binary (dummy) variable indicating that the preserved parcel would offer public access for walking and biking (omitted default is no public access).	0.154 (0.361)
hunting	Binary (dummy) variable indicating that the preserved parcel would offer public access for hunting (omitted default is no public access).	0.139 (0.346)
dev_not_30	Binary (dummy) variable indicating that the parcel, if not preserved, would likely remain undeveloped for at least 30 years (omitted default is development likely in less than 10 years).	0.226 (0.418)
dev_10_30	Binary (dummy) variable indicating that the parcel, if not preserved, would likely be developed in 10 to 30 years (omitted default is development likely in less than 10 years).	0.217 (0.412)
age_not30	Multiplicative interaction between *age* and *dev_not_30*.	12.256 (29.391)
age_dev10_30	Multiplicative interaction between *age* and *dev_10_30*.	11.740 (23.526)
cost	Unavoidable household cost of preservation (state/town taxes and fees), with sign reversal.	-43.921 (62.521)

[a] Includes zeros for the ‘status quo’ option.

Table 2. Conditional and Mixed Logit Results

Variable	**Model One** Conditional Logit, Unrestricted Parameter Estimate (Std. Error)	**Model Two** Conditional Logit, Restricted Parameter Estimate (Std. Error)	**Model Three** Mixed Logit, Restricted Parameter Estimate (Std. Error)
neither	-0.5969 (0.2553)	0.1012 (0.2193)	-1.1886 (0.3437)***
env_neither	-0.5770 (0.1018)***	-0.5876 (0.1009)***	-0.5975 (0.2168)***
age_neither	0.0130 (0.0034)*	0.0130 (0.0033)*	0.6492 (0.0057)
prot_neither	2.9498 (1.048)***	2.9359 (1.048)***	4.0113 (0.9563)***
coll_neither	-0.6731 (0.0791)***	-0.6815 (0.0779)***	-1.0343 (0.1666)***
option_A	0.1761 (0.0764)**	0.1054 (0.0486)**	0.1149 (0.0399)***
acres	0.0014 (0.0006)**	0.0019 (0.0004)***	0.0023 (0.0005)***
nursery	-0.0905 (0.1192)	-0.1172 (0.0766)	-0.0800 (0.0881)
forest	-0.1594 (0.1215)	-0.0559 (0.0776)	-0.0039 (0.0904)
idle	-0.0024 (0.1170)	-0.0130 (0.0760)	0.1181 (0.0906)
st_purch	-0.2732 (0.1803)	-0.1972 (0.1185)*	-0.2954 (0.1604)*
tr_purch	-0.3779 (0.1807)**	-0.2009 (0.1187)*	-0.3176 (0.1580)**
tr_con	-0.3092 (0.2048)	-0.1628 (0.1315)	-0.3912 (0.1662)**
zoning	-0.5843 (0.2028)***	-0.4099 (0.1314)***	-0.5017 (0.1653)***
walking	0.4876 (0.1402)***	0.5666 (0.0922)***	0.8672 (0.1292)***
hunting	0.2052 (0.1444)	0.2530 (0.0944)***	0.3399 (0.1273)***
dev_not_30	-0.2700 (0.2443)	-0.3921 (0.2339)*	-0.5126 (0.2421)**
dev_10_30	-0.2087 (0.2502)	-0.2405 (0.2413)	-0.4839 (0.2891)*
age_not30	0.0066	0.0067	0.0090

	(0.0042)	(0.0042)	(0.0043)**
age_dev10_30	0.0045 (0.0043)	0.0043 (0.0043)	0.0091 (0.0052)*
cost (sign-reversal)	0.0051 (0.0008)***	0.0038 (0.0005)***	--
cost (lognormal, sign-reversal)	--	--	-4.5099 (0.3679)***
cost (standard deviation)	--	--	7.3350 (0.7600)***
CT × neither	0.2566 (0.2408)	--	--
CT × option_A	-0.1212 (0.0992)	--	--
CT × acres	0.0007 (0.0008)	--	--
CT × nursery	-0.0477 (0.1558)	--	--
CT × forest	0.1771 (0.1583)	--	--
CT × idle	-0.0142 (0.1542)	--	--
CT × st_purch	0.1251 (0.2394)	--	--
CT × tr_purch	0.3020 (0.2399)	--	--
CT × tr_con	0.2547 (0.2673)	--	--
CT × zoning	0.2993 (0.2663)	--	--
CT × walking	0.1459 (0.1862)	--	--
CT × hunting	0.0910 (0.1910)	--	--
CT × dev_not_30	-0.1983 (0.1317)	--	--
CT × dev_10_30	-0.0733 (0.1355)	--	--
CT × cost	0.0021 (0.9752)**	--	--
-2 Log Likelihood χ^2	552.098***	535.829***	1712.016***
Pseudo-R^2	0.078	0.075	0.241
Chow Test: Equal Scale Parameter (CT and DE)	χ^2=0.95, p=0.33	--	--
Likelihood Ratio Test: Restricted vs. Unrestricted Model	--	χ^2 = 16.628, p = 0.36	--

Likelihood Ratio Test: Mixed vs. Conditional Logit Model	--	--	$\chi^2 = 1176.188$, $p < 0.001$
Observations (N)	3309	3309	3309

* p<0.10
** p<0.05
*** p<0.01

Willingness to Pay for a Conservation Project: A Comparison across Populations

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Abstract:

National Parks in Taiwan focus on conservation for future generations. However, pressure from increased visitor usage is compromising the survivability of rare plant and animal species. A contingent valuation study allows for the measurement of conservation benefits to three distinct populations: individuals who live in a major metropolitan area distant from the park, those that live in a rural area proximate to the park and visitors to the park. We find these three populations all express concern about the protection of rare plant and animal species in the park. However, as expected, the populations with higher income levels provide higher willingness to pay values.

Conservation of diverse ecosystems is an issue worldwide. In Taiwan, National Parks are established to protect natural scenery, historic relics and wildlife, to conserve natural resources, facilitate scientific research and promote environmental education. Taiwan's newest national park, Taroko, was established in 1986. Located in the north-eastern part of Taiwan, it covers more than 227,332 acres. Taroko National Park includes high mountains and steep gorges. The elevation in the park ranges from sea level to 12,000 feet above sea level. This variation in elevation gives rise to diverse habitat for a number of rare plant and animal species that are native to Taiwan. Most areas of Taroko National Park are relatively undisturbed. However, visitors and the associated recreation in the park can thwart conservation efforts. Scarce resources must be allocated to provide a balance between conservation efforts and recreation management. In an effort to shed some light on the benefits of a conservation program within Taroko National Park, a nonmarket valuation study was conducted. Three diverse samples are compared: individuals who live in a major metropolitan area distant from Taroko National Park, those who live proximate to the park, and visitors to the park.

Study design

Survey

The study was designed to provide estimates of the value of a conservation program within Taroko National Park. A contingent valuation (CV) survey was implemented. The survey was carefully designed to provide respondents with a baseline description of National Parks in Taiwan (e.g., number, locations, purpose) prior to providing detailed information about Taroko National Park and the "Taroko National Park Species Conservation Program." The program description mentioned that the habitat for rare plant and animal species were disturbed by visitors going off trail and leaving garbage. The program would allow the park to hire rangers to protect and monitor the habitats of rare plant and animal species. The CV question was posed as donation to the program. While a donation mechanism may not be incentive compatible (Carson, Groves and Machina 2000; Champ et al. 2002), an incentive compatible tax mechanism was not considered feasible in Taiwan as it would likely elicit an excessive number of protest responses. A payment card response format allowed survey respondents to circle the maximum they would be willing to donate. The survey included many other items such as measures of visitation to National Parks, attitudes toward the environment, environmental behaviors and demographic characteristics.

Sample

The sample was developed to include visitors, potential visitors and non-visitors. Three distinct areas were sampled: the city of Taipei, a large metropolitan area that is a substantial distance from Taroko National Park; Hualien county, a rural county proximate to Taroko National Park; and visitors to Taroko National Park. Respondents were recruited in-person between July 20 and August 1, 2005. The survey was self-administered. A total of 930 surveys were completed, 307 in Taipei, 323 in Haulien, and 300 in Taroko National Park.

Results

Not surprisingly, the demographic characteristics of the three samples varied (Table 1). In Taipei, a larger percent of the sample was male (58%) relative to the visitor and the Hualien samples. Visitors to Taroko had the highest education levels and income, while residents of Hualien had the lowest.[1]

Attitudes toward National Parks in general were elicited via responses to six Likert scale items (Table 2). In general, respondents weakly agree that recreation is an important use of National Parks. However, they feel more strongly that recreation should be limited to protect threatened native plants and animals. Likewise, they tended to disagree with the statement that National Parks should be easily accessible by automobiles and that the government spends too much money on National Parks. The results were similar across all three samples. Based on these results, we characterize the study population as being very supportive of National Parks and sensitive to the need to protect plants and animals at the expense of recreational opportunities.

We also asked about environmental behaviors. In particular, we asked about donations to environmental organizations, volunteering for environmental organizations, watching nature

[1] The response format for the income question was a bit unusual in that it included a "fluctuates" category. As shown in Table 1, a substantial number of respondents chose this response category. For the regression analyses, we recode the income variable to midpoint of each income category and recode the "fluctuates" responses to the mean income level.

based programming, and reading outdoor or environmental magazines (Table 3). Interestingly, few respondents in all three samples had donated or volunteered for an environmental organization. However, almost everyone surveyed had watched an environmental or nature based program on television. Likewise, the majority in all three samples had read an outdoor or environmental magazine.

The survey included questions about experience with Taroko National Park and the Species Conservation Program. Most of the survey respondents had visited Taroko in the past 5 years (95% of the Taipei sample and 99% of the Hualien sample). For all three samples, those who had visited Taroko rated the quality of their visit very positively (Table 4). Residents of Hualien were most likely to say they plan to visit Taroko in the next 12 months (27%) compared to the Taipai (16%) and visitor (18%) samples. The conservation program described in the survey was received very favorably. Respondents from all three samples stated high levels of concern about the protection of rare plant and animal species in Taroko National Park and almost all the survey respondents said that the Taroko National Park Species Conservation Program was a worthwhile program.

Response to the Willingness to Pay Question

The willingness to pay question was posed as a donation to the described "Taroko National Park Species Conservation Program." Respondents chose their maximum willingness to pay from a payment card. The visitor sample was most likely to respond positively to the willingness to pay question with 84% circling some positive amount on the payment card (Table 5). The Taipei sample had the next highest percent responding positively to the willingness to pay question (78%). The Hualien sample had the lowest percent responding positively to the willingness to pay question (66%). However, the respondents in the Taipei sample who were willing to pay something were willing to pay more relative to the visitor sample. This result is reflected in the ordering of the mean and median willingness to pay estimates (Table 6). The estimates of mean willingness to pay are higher for the Taipei sample relative to the visitor sample but the difference is not statistically significant. The estimates of mean willingness to pay for the Hualien sample are significantly lower than the other two samples. This result is not surprising when one considers the difference in the demographic characteristics of the Haulien sample.

Multivariate models

The multivariate models provide more insight into differences and similarities across the three samples (Table 7). The models are estimated using the maximum likelihood interval approach (Welsh and Poe 1998; Cameron and Huppert 1989). Age has a negative and significant effect on willingness to pay in all three samples. Income only comes up significant ($\alpha = .10$ level) in the Hualien model. The expressed level of concern about protection of rare plant and animal species in Taroko National Park was significantly related to willingness to pay in all three samples. Those who expressed higher levels of concern were willing to pay more. The measures of "environmental behaviors" (donating time or money to environmental causes, watching or reading nature media), did not have much influence on willingness to pay. Having made a previous donation to an environmental cause was significantly related ($\alpha = .10$ level) to willingness to pay in the visitor model. Likewise, volunteering for an environmental organization was significantly related to willingness to pay in the Taipei sample. Some of the attitude items were also related to willingness to pay in a statistically significant manner. In the visitor sample, those who were more likely to agree with the statement "Protecting animal and plant species that are native to Taiwan is a very important reason for having National Parks" were willing to pay more for the Taroko National Park Species Conservation Program. In the Taipei and the Hualien samples, agreeing with the statement "Recreation in Taiwan's National Parks should be limited to protect threatened native plants and animals" was positively related to willingness to pay. In the Taipei and visitor samples, disagreeing with the statement "National Parks should be easily accessible by automobiles" was associated with higher willingness to pay. Finally, in the Hualien sample, disagreeing with the statement "The government spends too much money on National Parks" was associated with higher willingness to pay values. The multivariate analysis highlights the differences across the three samples. Only the effects of age and concern about protection of rare plant and animal species were consistent across all three samples.

Conclusion

This applied study provided useful information about the value of a program in Taroko National Park to conserve rare plant and animal species. Surveying three distinct populations provided some additional insights. Those that live proximate to the park are more likely to visit the park in the next twelve months and are very concerned about protection of rare plant and animal species in the park. They were most likely to have volunteered for an environmental organization. However, the area proximate to the park is rural with lower income and education levels relative to the other two samples. Therefore this group of respondents is willing to pay less for the Taroko National Park Species Conservation Program. This result is likely similar for many rural areas near National Forests in the United States. Those who live near the land are very concerned about conservation but are more income constrained than distance populations in metropolitan areas. The study participants in the Taipei sample were least likely to think they would visit Taroko in the next 12 months. However, they revealed positive attitudes toward conservation efforts in National Parks. Even though this sample expressed the least concern about protection of rare plant and animal species in Taroko National Park, they provided the highest willingness to pay values. Those who said they would pay something have the income to make sizeable donations to the Taroko National Park Species Conservation Program. The visitor sample was interesting in that 95% of the study participants rated the quality of their visit to Taroko as high or very high. They expressed the highest level of concern relative to the two other samples about the protection of rare plant and animal species in Taroko National Park. Likewise, 100% of the study participants said they thought the Taroko National Park Species Conservation Program was a worthwhile program. Perhaps completing the survey while in Taroko National Park, influenced their sense of the various threats to the fragile plant and animal species.

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Table 1: Socio-demographic Characteristics of the Samples				
		Taipei	Hualien	Visitors
Gender:	Male	58%	42%	53%
	Female	42%	58%	47%
Age:	< 21	12%	12%	10%
	21-30	37%	25%	39%
	31-40	27%	26%	26%
	41-50	16%	21%	19%
	51-60	6%	13%	5%
	61-70	2%	3%	1%
	> 70	1%	1%	-
Education:	Junior highschool	3%	18%	3%
	Senior highschool or vocational school	25%	47%	14%
	College	22%	13%	22%
	University	39%	18%	48%
	Graduate School	11%	3%	13%
Monthly income (NT$):	Fluctuates	39%	40%	28%
	< 20,000	10%	14%	10%
	20,000-29,999	8%	18%	7%
	30,000-39,999	17%	11%	13%
	40,000-49,999	11%	7%	14%
	50,000-69,999	9%	7%	17%
	70,000-99,999	5%	1%	9%
	> 100,000	2%	2%	3%

Table 2: Attitudes toward National Parks			
	Taipei	Hualien	Visitors
Mean rating (1=strongly disagree; 5=strongly agree)			
Recreation is a very important use of National Parks	3.67	3.96	3.89
Protecting animal and plant species that are native to Taiwan is a very important reason for having National Parks	4.24	4.43	4.35
It is important to have National Parks for future generations to enjoy	4.03	4.08	4.00
Recreation in Taiwan's National Parks should be limited to protect threatened native plants and animals	4.41	4.44	4.45
National Parks should be easily accessible by automobiles	2.63	3.03	2.74
The government spends too much money on National Parks	2.25	2.59	2.24

Table 3: Environmental Behaviors			
	Taipei	Hualien	Visitors
	Percent Yes		
Ever made a donation to an environmental organization	13%	15%	17%
Ever volunteered for an environmental organization	4%	12%	7%
Ever watch environmental or nature based programs	97%	96%	99%
Ever read outdoor or environmental magazines	62%	70%	76%

Table 4: Summary of Responses to Questions about Taroko National Park and the Species Conservation Program			
	Taipei	Hualien	Visitors
Quality of Visit to Taroko:			
Very Low	0%	1%	0%
Low	0%	1%	0%
Average	17%	16%	4%
High	55%	46%	51%
Very High	28%	36%	44%
Percent that plan to visit Taroko in next 12 months	16%	27%	18%
Average level of concern about the protection of rare plant and animal species in Taroko National Park (1=not at all concerned; 10=very concerned)	6.65	7.15	7.23
Percent that think Taroko National Park Species Conservation Program is a worthwhile program	98%	97%	100%

Table 5: Percent Responding Positively to Willingness to Pay Question by Sample	
Taipei	78%
Hualien	66%
Visitors	84%

Table 6: Willingness to pay Estimates in Taiwanese (NT$) and U.S. dollars by Sample				
	Mean (95% Confidence Interval)		Median	
Taipei (n=291)	1746 NT$ (1246, 1896)	$52 (37, 57)	377 NT$	$11
Hualien (n=320)	635 NT$ (515, 755)	$19 (15, 22)	160 NT$	$5
Visitors (n=299)	1364 NT$ (1258, 1470)	$41 (38, 44)	391 NT$	$10

Table 7: Multivariate Models			
	Coefficient Estimate (Std. Error)		
Variable	Taipei (n= 303)	Hualien (n=322)	Visitor (n=300)
Intercept	4.204[1] (1.15)	3.951[1] (1.003)	3.563[1] (1.294)
Age	-0.024[1] (0.010)	-0.037[1] (0.009)	-0.021[1] (0.011)
Monthly income (in thousands of NT$)	-0.002 (0.006)	0.012[2] (0.007)	0.008 (0.006)
Gender (0=male; 1=female)	-0.243 (0.211)	-0.158 (0.208)	0.140 (0.198)
Plan to visit Taroko in next 12 months (1=yes; 0=no)	0.238 (0.274)	0.350 (0.225)	-0.230 (0.248)
Level of concern about protection of rare plant and animal species in Taroko National Park (1=not at all concerned to 10=very concerned)	0.118[1] (0.047)	0.209[1] (0.050)	0.219[1] (0.048)
Donation to environmental or conservation organization (1=yes; 0=no)	0.343 (0.317)	0.407 (0.286)	0.467[2] (0.266)
Volunteer for environmental or conservation organization (1=yes; 0=no)	0.940[1] (0.475)	-0.161 (0.322)	-0.291 (0.380)
Watch nature based programs (1=yes; 0=no)	0.519 (0.695)	-0.122 (0.554)	0.898 (0.967)
Read outdoor or environmental magazines (1=yes; 0=no)	0.140 (0.220)	0.059 (0.230)	0.083 (0.225
Recreation is a very important use of National Parks (1=strongly disagree; 5=strongly agree)	-0.040 (0.100)	0.050 (0.101)	-0.013 (0.098)
Protecting animal and plant species that are native to Taiwan is a very important reason for having National Parks (1=strongly disagree; 5=strongly agree)	0.218 (0.133)	0.205 (0.128)	0.267[1] (0.142)
It is important to have National Parks for future generations to enjoy (1=strongly disagree; 5=strongly agree)	-0.361 (0.116)	-0.131 (0.109)	-0.223[2] (0.103)
Recreation in Taiwan's National Parks should be limited to protect threatened native plants and animals (1=strongly disagree; 5=strongly agree)	0.378[1] (0.152)	0.213[2] (0.128)	-0.027 (0.133)
National Parks should be easily accessible by automobiles (1=strongly disagree; 5=strongly agree)	-0.224[1] (0.094)	-0.121 (0.078)	-0.148[2] (0.083)
The government spends too much money on National Parks (1=strongly disagree; 5=strongly agree)	-0.026 (0.104)	-0.249[1] (0.088)	0.088 (0.089)
Log Likelihood	-730	-699	-741

Models of Soil Conservation Benefits: The Sum of Some of the Benefits

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Abstract:
Reductions in soil erosion affect consumer and producer surplus in many ways. The goal of this paper is to document the most important features of the models that are used to assess the soil conservation benefits of federal programs. The paper lists the available models, specifies the consumer and producer surplus impacts each model measures, and briefly overviews the economic reasoning, analytic approach, and data supporting each model. Fifteen soil conservation benefit models used to value agri-environmental policies' are presented. The models likely provide conservative estimates because not all environmental impacts of erosion have been accounted for. The models provide benefit estimates by USDA Farm Production Region (FPR)—there are ten FPRs within the contiguous 48 states. Some of the models were estimated more than 20 years ago. The more-recent models have taken advantage of increases in the availability and quality of data and thus are able to offer more geo-specific measures of conservation benefits.

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Introduction

In response to the public's interest in agriculture's contribution to and burden on environmental quality, USDA conservation program expenditures more than tripled since the early 1980's to over $5.1 billion in 2004 (Amber Waves, 2006). The greater portion of this spending has gone to support land retirement programs such as the Conservation Reserve Program (CRP) and the Wetland Reserve Program (WRP). However, funding for conservation on working lands, through programs such as the Environmental Quality Incentive Program (EQIP) and the Conservation Security Program (CSP), has also been increasing.

There is little question that agri-environmental policies improve environmental quality (Claassen et al., 2001). However, when program funding decisions arise, there is always the question of how improvements compare to costs and thus the need for reliable estimates of policies' benefits. Furthermore, when designing conservation programs, measures of benefits can be used to target conservation expenditures to areas where benefits are maximized relative to costs. A variety of models have been developed to estimate many of the benefits of agri-environmental policies.

When valuing environmental improvements from national programs, analysts can, in some cases, take advantage of physical process models. The physical process models—developed by agronomists, soil scientists, hydrologists, biologists, and others—help link agricultural practices to environmental quality. There are several physical process models (i.e., SWAT (Soil & Water Assessment Tool), CREAMS (Chemicals, Runoff, and Erosion from Agricultural Management Systems), GLEAMS (Groundwater Loading Effects on Agricultural Management Systems), EPIC (Erosion-Productivity Impact Calculator), and SPARROW (SPAtially Referenced Regressions On Watershed attributes) model (Alexander et al., 2000; Neitsch et al., 2000: Smith et al., 2000; Smith et al., 1997)). But the available models do not quantify the full range of environmental impacts of farming practices. Furthermore, few models capture all steps in the fate and transport process so that changes in farming practices cannot generally be linked to changes in environmental amenities.

Another factor that makes it difficult to quantify the environmental impacts of farming practices is that a single practice can have multiple impacts. For example, conservation tillage decreases field runoff and, hence, reduces sediment and nutrient loadings in lakes and rivers and, subsequently, improves the quality of municipal water intake, improves fisheries, reduces the rate of sediment accumulation in reservoirs, etc. but increases chemical percolation to groundwater and increases pesticide use. Thus, to calculate the benefits of conservation tillage, one must be able to value changes in each of these amenities.

Each of the 15 models provides a means of estimating one of agricultural soil conservation's environmental benefits (see box). The set of models is not comprehensive-—models that value impacts on endangered species, wetlands, etc. have not been estimated. Some models do not include relevant impacts—the reservoir benefit model does not include impacts on all reservoirs, the soil conservation benefit model does not include estimates of the welfare effects of changes in crop output and consumer prices, etc.

Most of the models are not able to provide benefit estimates for areas that are smaller than multi-state regions thus limiting the geo-precision of benefit estimates and extent to which the models can be used to improve environmental targeting. But the models do provide estimates—probably conservative estimates—of regional and national benefits of soil conservation policies and programs.

Each benefit model relies on at least one physical process model. However, the models do not capture all steps in the fate and transport process. Instead, the benefit models take a 'reduced-form' approach where benefits are modeled as a function of conditions at one step in the fate and transport process. As a result, the benefit functions embody physical effects on amenities and the effects of amenities on consumer and producer surplus.

The goal of this paper is to describe the major features of 15 models that can be used to assess the soil conservation benefits of federal programs. To do so, the paper describes the social costs that each of the soil conservation benefits models attempts to measure and overviews the economic reasoning, analytic approach, and data supporting each model. The paper does not provide a full description of the technical features of the benefit models and thus should not be viewed as a single source for a full documentation of each model. Citations are provided for those interested in more in-depth documentation. For demonstration purposes, the benefit models are used to estimate the soil conservation benefits of the CRP.

Methods

Five theoretical frameworks underlie the benefit models discussed here. The contingent valuation (CV) framework directly solicits individuals' willingness to pay. The other four frameworks—travel cost, damage function, restoration cost, and averting expenditure—are indirect means of valuing environmental benefits. Thus a key element in these frameworks is the way in which environmental quality is assumed to affect firms' or individuals' behavior.

Two of the frameworks—the CV and travel cost (TC) models—are commonly used to estimate individuals' willingness-to-pay (WTP) for changes in environmental quality.

- The CV method directly solicits individuals to reveal their willingness to pay for a change in environmental quality. By surveying the relevant population for its willingness to pay for varying levels of change in environmental quality, the demand for environmental quality is directly estimated.
- The TC method uses expenditure and trip data to estimate the demand for a good or activity where environmental quality is one of the determinants of demand. Changes in consumer surplus associated with changes in environmental quality are derived from the estimate demand function. The approach requires data on respondents' recreational activities and travel costs (including the cost of time), the environmental quality of available sites, etc.

The other three frameworks (damage function, restoration cost, and averting expenditures) are means of estimating changes in producer surplus associated with changes in environmental quality. These frameworks are viewed as naive approaches because they do not account for all behavioral or market responses.

- The damage function (DF) approach assumes that the loss in welfare due to a change in environmental quality is approximately equal to the corresponding loss in revenues or increase in operating costs. The DF approach is thought to provide conservative approximations because, first, it implicitly assumes that no remedial actions are taken and, second, there is no consideration of market effects (Freeman, 1993). However, in the case of a single product firm (s), the DF can provide an accurate measure of the change in producer surplus as long as the change in environmental quality does not change the quality or quantity of the firm's output (Ribaudo and Hellerstein, 1992).

- The restoration cost (RC) method assumes that the welfare loss due to in decrease in environmental quality is approximately equal to the corresponding increase in the costs of restoring, replacing, or repairing goods and capital assets. The RC approach is also believed to provide conservative benefit estimates. As with the DF approach, the RC method assumes that no remedial actions are taken. Furthermore, the RC approach ignores the cost of reduced performance before the good is replaced (Winpenny, 1991; McNeely, 1988).

- The averting expenditures (AE) approach assumes that the loss in welfare due to a decrease in environmental quality is approximately equal to the increase in expenditures made to prevent the loss or degradation of goods or assets. The AE approach assumes that marginal changes in defensive expenditures leave the quality of the environmental good(s) unchanged. The AE approach does not address errors and uncertainties in predictions of future damages. Also, the AE approach assumes that changes in expenditures are a perfect substitute for changes in environmental quality (Freeman, 1993; Ribaudo, 1989). The AE approach is thought to provide conservative estimates because, first, expenditures are made with respect to future impacts, hence embodying a discounting of impacts. The AE approach does not account for discounting. And second, AE can be viewed as a tax on production (in that erosion imposes an additional production cost) that shifts supply upward and, as with a tax, introduces deadweight loss.

The quality of the benefit models is also constrained by the strength of the supporting data. The most desirable data are disaggregated enough to capture all the significant variations in resource quality, bio-physical impacts, and the subsequent impacts on consumer and producer surplus. Of course, building models from such data is not without technical challenges. Furthermore, the most desirable data are often not available. The data used to generate the benefit models, along with their strengths and weaknesses, are discussed below.

Benefit models	**Consumer/producer surplus**
Reservoir services	The public's willingness to pay for less sediment and thus more services from reservoirs due to a reduction in soil erosion.
Navigation	The navigation industry's willingness to pay to have less sediment affecting shipping channels and harbors.
Water-based recreation	People's willingness to pay to view and recreate in cleaner fresh water.
Municipal water treatment	Municipalities' willingness to pay to have less sediment in water processed for public consumption.
Dust cleaning	Households' willingness to pay to have less cleaning due to a reduction in wind erosion and wind-borne particulates.
Irrigated agriculture	Farmers' willingness to pay to reduce the adverse yield impacts of the salts and minerals in irrigation waters that were dissolved from sediment.
Irrigation ditches and cannels	Agriculture's willingness to pay to reduce the buildup of sediment and aquatic plants in irrigation ditches and canals.
Soil productivity	Farmers' willingness to pay to reduce losses in soil productivity.
Marine fisheries	The marine fisheries industry's willingness to pay to reduce sediment's impact on fish catch.
Freshwater fisheries	The freshwater fisheries industry's willingness to pay to reduce sediment's impact on fish catch.
Marine recreational fishing	The public's willingness to pay for an improvement in fish catch-rates due to reductions in erosion.
Municipal and industrial water use	Municipalities' and industries' willingness to pay to reduce damages caused by the salts and minerals in sediment.
Steam electric power plants	Power producers' willingness to pay to reduce plant growth on heat exchangers caused by nutrients in suspended sediment.
Flood damages	The public's willingness to pay to reduce damages associated with flooding.
Road drainage ditches	State governments' willingness to pay for a reduction in sediment accumulation in ditches along rural roads and highways

Models and Data

Over time, data availability and quality have increased. As a result, the newer benefit models are able to provide estimates that have greater geographic resolution. The three most recent models, estimated since 1997, provide benefit estimates for areas as small as the United States Geological Survey (USGS) 2,111 8-diget hydrologic unit code (HUC) watersheds of the contiguous states (figure 1). The other benefit models, estimated in the 1980s, provide benefit estimates by state, by USGS 4-diget aggregated sub-area (ASA) watersheds, or by USDA's multi-state Farm Production Regions (figure 2). The differences in the geographic resolution of the benefit models reflect the nature of the data available when the models were estimated.

Of the three most-recent models, one values the soil conservation impacts on reservoir services; another values impacts on the navigation industry[1]; and the third values impacts on water-based recreation. Each is designed to link erosion changes within a HUC to the affected population.

Reservoir services

As sediment accumulates in reservoirs, the quantity and quality of reservoir services are reduced. Slowing or preventing sediment from settling in a reservoir leaves reservoir service levels higher in subsequent years than what had been expected (Hansen and Hellerstein, 2006). Thus one benefit of soil conservation is the greater level of future reservoir services. The reservoir benefits model values the increase in (present and future) reservoir services due to a marginal reduction in reservoir sedimentation.

The parameters of the benefit function are estimated by applying the replacement cost method and assuming that reservoir owners/managers dredge reservoirs when optimal. At the optimal time to dredge, marginal benefits equal marginal costs. This efficiency condition allows parameters of the benefit function to be calculated using observations on cost (Hansen and Hellerstein, 2006). A sedimentation model, linking changes in erosion to reduction in sedimentation, is coupled with the benefit models so that changes in benefits due to marginal reductions in erosion can be estimated.

The reservoir benefit model uses the National Inventory of Dams data on more than 70,000 reservoirs in the 48 contiguous states. Erosion data are from the National Resources Inventory (NRI). The NRI contains 800,000 statistically-based sample points on U.S. nonfederal range, crop, pasture, and forest lands (USDA, SCS, 1984). Dredging cost data come from various federal, municipal, local, and private sources.

Navigation industry

Sediment buildup in shipping channels and harbors delays and damages ships and barges that run aground. To avert these damages and delays, the navigation industry, through the support of the Army Corps of Engineers, dredges harbors and shipping channels. Thus dredging expenditures are assumed to represent averting expenditures.

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[1] The navigation industry embodies all commercial traffic on freshwater and saltwater.

The navigation industry model provides HUC-level estimates of the expected reduction in averting expenditures due to a 1-ton reduction in erosion. The model is estimated in two steps. First, an average dollar-per-ton cost of erosion is estimated for each site dredged by dividing total site-level dredging costs—where sites are harbors and segments of shipping channels—by total upstream erosion. Data on erosion and a hydrologic model are used to estimate the total tons of erosion upstream of each site. And second, HUC-level per-ton benefit estimates are estimated for each HUC by summing the dollar-per-ton estimates across all relevant downstream sites (Hansen et al., 2002).

The hydrologic data are from the Environmental Protection Agency's River Reach File, which interconnects 3.2 million miles of streams. Estimates of agricultural erosion by HUC are based on data from the 1997 NRI. Dredging-cost data are from the U.S. Army Corps of Engineers (1999a; 1999b).

Water-based recreation
Suspended sediment in lakes, rivers, and streams tends to decrease the water's asthetic appeal and can decrease the quality of fishing, swimming, and other water-based activities. Thus decreases in suspended sediment loadings can increase consumer surplus. A travel cost model is used to calculate sediment's impact on consumer surplus. The travel cost is estimated in a two-step process (Feather et al., 1997; Feather et al., 1999). First, the site-selection process is characterized by a random utility model (RUM). The RUM (estimated using data on individuals and site characteristics) predicts the probability that an individual would select a given site and thus is used to generate an expected price (travel cost) and environmental quality (where erosion and the size of water bodies serve as proxies for environmental quality). And second, the demand for water-based recreation is estimated by specifying the quantity demanded (trips) as a function of the expected price and environmental quality measures (generated by the RUM) and other demand determinants.

The behavioral data used to estimate the RUM and the demand for water-based recreation are taken from the 1992 National Survey of Recreation and the Environment (NSRE). The environmental quality data are from the 1992 NRI.

The sums of these HUC-level estimates of *reservoir services*, *navigation*, and *water-based recreation* indicate that, in 251 of the 2,111 HUCs, the dollar-per-ton soil conservation benefit estimates equal zero. These are regions where there is no significant agricultural erosion. Most of the non-zero impacts are less than $2.50 per ton, although in one HUC, the estimated soil conservation benefits are $11.70 per ton (figure 1).

Non-linear state- and ASA-level models
Two models, supported by data with enough geographic resolution to generate state- or ASA-level estimates of marginal impacts, embody non-linear relationships between erosion and restoration costs. The models value the effect of marginal changes in erosion on municipal water treatment and household cleaning costs.

Municipal water treatment

A water-treatment cost model, developed by Holmes (1988), estimates per-gallon treatment costs. In the Holmes model, treatment cost is a function of water turbidity. To determine the effect of erosion on treatment costs, the Holmes model was coupled with a water turbidity model (estimated by Helvey, Tiedmann, and Anderson (1985)) where turbidity is a function of erosion. With these models, per-gallon municipal water treatment costs are estimated and, by applying municipal water use estimates from Solley and others (1983), total costs are estimated for each of the 99 USGS Aggregated Sub-area (ASA) watersheds. Changes in treatment costs are estimated by differencing the treatment cost estimates at two levels of erosion and summing across relevant regions (Ribaudo, 1989; Ribaudo and Hellerstein, 1992).

Dust cleaning

Cost estimates were derived from a household cleaning-cost model, where costs are a function of household characteristics and wind erosion within the county. This cost model is estimated using contingent valuation techniques and data from a survey of households in New Mexico (Huszar and Piper, 1986; Huszar, 1989). Using the cost model, Census data on households, and wind-erosion data from the 1982 NRI, household cleaning costs are estimated by state for all states in and west of the Northern and Southern Plains FPRs. The household cleaning-cost model is non-linear with respect to wind-erosion. Thus, as with the municipal water treatment model, costs estimates are generated at two levels of erosion—the level observed in 1982 and the level that was estimated to exist after the implementation of the CRP. The differences in these cost estimates are summed across states within each FPR to generate regional estimates. The per-ton benefit variables are derived by dividing the changes in cleaning costs by the associated changes in erosion due to the CRP (Ribaudo et al., 1989).

Linear state-level models

All of the remaining nine models assume a linear relationship between erosion and cost (e.g., marginal cost is assumed to be constant and equal to average cost). However, three of these models are supported by data with enough geographic resolution to generate state-level dollar-per-ton estimates, though, because of limits on the availability of data, the models may not capture all cross-state variations in the value of erosion. The models estimate soil erosion's impact on irrigated agriculture, irrigation ditches and canals, and soil productivity. Each model was developed by, first, deriving state-level estimates of the relevant cost and, second, dividing cost by the agricultural erosion within the state.

Irrigated agriculture

Erosion is assumed to increase the salinity of irrigation water and thus reduce the productivity of (and hence damage) irrigated lands. An estimate of the per-acre value of the loss in yields due to salinity from agricultural erosion was taken from a study of the lower Colorado River basin by the U.S. Department of the Interior, Water and Power Resources Services (1980) (Clark et al., 1986). Total cost within a state was estimated by multiplying the per-acre cost estimate by the number of irrigated acres, as reported in the 1978 Census of Agriculture (US Bureau of the Census, 1981). Total costs are then divided by total erosion within the state (Ribaudo, 1986).

Irrigation ditches and canals
Agricultural erosion is estimated to account for approximately one-fourth of all maintenance costs of irrigation ditches and canals (Clark et al., 1986). State-level data on total maintenance costs of irrigation ditches and canals were obtained from the 1978 Census of Agriculture (U.S. Bureau of Census, 1981). Thus, by taking one fourth of the reported costs, state-level estimates of agriculture's impacts on maintenance (or restoration) costs are generated (Ribaudo, 1986).

Soil productivity
The Erosion Productivity Impact Calculator (EPIC) model (Williams et al., 1985) was used to estimate yield losses and increases in input use due to erosion across relevant crops, soil types, crop rotations, and tillage practices. The model assumes that farmers, as profit maximizers, increase nutrient use to offset some of the productivity impacts of soil loss. Thus the loss in yields represent damages due to erosion and the increases in input use represents averting expenditures. Data on crop and nutrient prices are used to value erosion's impact. Summing the EPIC results across the relevant lands generates the state-level estimates of erosion impact on productivity (USDA, ERS, 1986; Ribaudo et al., 1989).

Farm Production Region models
Six models generated benefit estimates for multi-State regions—the U.S. Department of Agriculture's 10 Farm Production Regions (FPR). Each of the models was developed by, first, parceling out national estimates of relevant erosion costs to FPRs and, second, by dividing the costs by total agricultural erosion within the relevant FPR. Thus FPR-level costs (*Cost_FPR*) is a function of national cost (*Cost_US*) and a distribution factor or:

$$\text{Cost\_FPR}_i = \text{Cost\_US} * (\text{w\_FPR}_i/\text{W\_US}) \qquad (1)$$

where w_FPR_i/W_US is a ratio that is assumed to appropriately distribute costs across regions. For example, in estimating soil erosion damages to marine fisheries, *Cost_US* is the reported total value of the loss in fish harvests due to agricultural erosion, w_FPR_i is the number of estuaries in FPR_i that are impaired by agricultural erosion, and *W_US* is the total number of US estuaries impaired by agricultural erosion. Information on the variables in each application of equation 1 can be found in table 1.

Marine fisheries
The estimate of the total costs—the value in the losses in fish catch—to marine fisheries due to sediment's impact on fish populations is taken from Bell and Canterberry (1975). Agriculture's share of the costs is based on estimates of Vaughn and Russell (1982) and Clark and others (1986). Agriculture's share of total costs is allocated across FPRs based on a FPR's share of the total number of erosion-impaired estuaries. Data on the location of 180 estuaries are from the National Oceanic and Atmospheric Administration (NOAA) (Ribaudo, 1986).

Freshwater fisheries

The national costs of sediment's impact on freshwater fisheries—estimated by Clark and others (1986) using estimates by Freeman (1982) and Vaughan and Russell (1982)—are allocated across FPRs based on the FPR's share of the total river-miles having impaired water quality. Estimates of national and regional water-quality impaired river miles are based on USGS National Stream Quality Monitoring Network (NASQAN) data, National Water Discharge Inventories, data of Resources for the Future (RFF), and Environmental Protection Agency's (EPA's) River-reach file (Ribaudo, 1986).

Marine recreational fishing
The national cost of sediment to marine recreational fishing (estimate from Clark and others (1986)) is allocated across FPRs based on a FPR's share of the total number of water-quality impaired fishing days. The number of water-quality impaired fishing days within a FPR is estimated by multiplying the number of days spent marine fishing within a FPR by the percent of the FPR's estuaries that are water-quality impaired (Ribaudo, 1986).

Municipal and industrial water use
The national cost of damages to industrial equipment due to dissolved materials (from Clark and others (1986)) is allocated across FPRs based on a FPR's share of the total quantity of water withdrawn by both industry and households (from Solley, et al., 1983) (Ribaudo, 1986).

Steam-electric power plants
The national cost to thermal-electric power plants and other facilities that rely on water-cooling include the cost of in-take water chlorination for bio-fouling control and removal of algae growth—induced by sediment-borne agricultural nutrients—from condensers (from Clark and others (1986)). The national costs are allocated across FPRs based on a region's share of the total quantity of water withdrawn for thermal-electric power production (Ribaudo, 1986).

Flood damages
The total costs of agricultural sediment-related flood damages (estimate from Clark and others (1986) using various regional studies[2]) are allocated across FPRs based on a FPR's share of the total (agricultural and non-agricultural) flood damages reported by the U.S. Water Resources Council (1978) (Ribaudo, 1986).

A national model
Road drainage ditches
The *road drainage ditches* model is a national model in that it generates a dollar-per-ton benefit estimate that is assumed to be constant across the 48 contiguous states. A sediment removal cost model was estimated with data from 33 states. Total state removal costs were specified as a linear function of gross erosion, rural road mileage, and cubic yards removal costs. The estimated coefficient on the erosion variable is the marginal impact on cost from a reduction in erosion (Ribaudo, 1989).

[2] Some studies used by Clark and others, such as the one by Brown (1947), were published several decades ago.

Figure 2 provides a perspective of the magnitude and variation in the sum of the per-ton benefit estimates of 12 models at the FPR-level. Note that, before model estimates were summed, the state-level and ASA-level estimates were converted to erosion-weighted FPR-level averages. Soil conservation benefits range from $2.19 to $8.13 per ton, a much smaller range than the HUC-level estimates (figure 2).

Application of the benefit models

In a recent application, the available benefit models were used to estimate the soil conservation benefits of the CRP (Sullivan et al., 2004). The application involved 4 steps.

1. The expected post-CRP erosion rates (e.g., the rates expected if all CRP contracts were terminated) are calculated for each NRI point (based on observed practices on non-CRP farmland and assuming that practices on CRP lands would be the same as those on surrounding lands),
2. The benefits models are estimated based on the 1997 erosion rates and the expected post-CRP rates,
3. The changes in benefits are calculated by subtracting the 1997-based from the post-CRP benefit estimates,
4. Regional/national benefit estimates are generated by summing all benefits across relevant NRI points

Unfortunately, the *water-based recreation* model could not be directly applied because the independent variables needed to calculate benefits are not available. Instead, what are available, and have been applied, are HUC-level estimates of the average benefit of a one-ton reduction in erosion. The per-ton benefits were generated from the *water-based recreation* model by:

- estimating the cost of erosion to water-based recreation in 1992,
- estimating the cost of erosion to water-based recreation if erosion was ten percent below the 1992 level,
- subtracting the second estimate from the first, and
- dividing the difference by the change in erosion.

These dollar-per-ton estimates are then applied to changes in erosion (steps 1 and 2 above) to estimate changes in benefits.

The independent variables needed to solve the six state-level models were also unavailable. What are available are FPR-level estimates of the average benefit of a one-ton reduction in erosion. The FPR-level estimates are weighted averages of the state-level estimates where the level of agricultural erosion serves as the weight. Three of the state-level benefit models assume that the marginal cost of erosion is constant and equal to average cost—*irrigated agriculture*, *irrigation ditches and canals*, and *soil productivity*––thus calculating the FPR-level dollar-per-ton cost estimate was straightforward. But the *road drainage ditches*, *municipal water treatment*, and *dust cleaning* benefit models are non-linear. Thus estimates of the 'average' marginal benefit of a one-ton reduction in erosion were calculated for each state using a procedure similar to the one applied to the *water-based recreation* model. Once the state-level estimates were generated, calculating the FPR-level dollar-per-ton cost estimate was straightforward.

The estimates of the expected erosion rates are built on the assumption that the mix of agricultural outputs and production practices on highly erodible lands (HEL) and non-HEL lands within regions would remain unchanged if the CRP were eliminated. Sullivan and others (2004) justify this assumption by arguing that farmers will expand their operations, but not change their production specialization (i.e., a grain farmer will not start a cattle operation in order to keep land leaving the CRP in permanent cover for hay or pasture). Thus erosion rates on HEL and non-HEL lands that leave the CRP will equal the rates observed on similar lands within the same region, where regions are defined as HUCs.

Data on erosion rates and non-CRP lands are from the 1997 NRI (USDA, NRCS, 2000). The NRI identifies the county and HUC where each sample point lies, the number of acres represented by each point, the farming practices being used, and the wind and water erosion rates. With these data, average erosion rates are estimated for HEL and non-HEL non-CRP farmlands within each HUC. The average HEL (non-HEL) rates are the expected post-CRP erosion rates and thus are tied to CRP observations on HEL (non-HEL) lands.

To estimate the CRP's effect on erosion, the expected erosion rates are multiplied by the number of acres represented by each CRP observation and summed. Sullivan et al. (2004) estimate that the CRP reduces annual erosion by approximately 224 million tons, ranging from 0.6 million tons in the Northeast to nearly 68 million tons in the Southern Plains (table 2).

Applying the CRP's estimated impacts on erosion, Sullivan et al. (2004) estimate that annual soil conservation benefits of the CRP are approximately $500 million, ranging from $8 million in the Northeast to $175 in the Corn Belt (table 2).

These results indicate that the soil conservation benefits of the CRP in the Corn Belt region are more than double that of any other FPR. This is due, in part, to the region's large reduction in erosion. Three regions have larger reductions in erosion, but in these regions the per-ton benefit estimates are, on average, lower than the Corn Belt's (table 2).

Expanding and improving soil conservation benefit models

This review of soil conservation benefit models suggests that additional research may generate more and better models. With more models, benefits that are not yet accounted for can be estimated. With better models, the accuracy and geo-resolution of benefit estimates can be improved.

Future analyses of soil conservation benefits will be improved if additional benefit models were available. Additional models will provide a more complete picture of national and regional benefits.

Replacing available models with models built on stronger theoretical foundations and supported by more extensive and detailed data will also improve analyses of soil conservation benefits. Such replacements will improve the accuracy and geo-resolution of benefit estimates.

In cases where past models cannot be replaced, re-estimating the models using on more recent data will improve conservation program benefit analyses. Such updating of the models is likely to provide more accurate estimates, though not improve the geo-resolution of estimates.

Finally, if models cannot be replaced or re-estimated, the available models would be improved by incorporating the effects of changes in income, population, technology, and other relevant factors. The benefit estimates presented here have been adjusted for inflation but not other factors. Adjustments for other factors need to be designed on a variable-by-variable basis and should be supported by some analysis, if only a case study. Such changes will improve benefit estimates, if done correctly, but will not provide a more comprehensive set of measures nor improve the geo-resolution of the model estimates.

In prioritizing research, another factor to be considered is the level of uncertainty behind model estimates. Ribaudo (1986; 1989) provides high, low, and most-likely estimates in applications of eight of the models discussed here. To provide a perspective of the level of uncertainty in the estimates, the high, low, and most-likely estimates of each model are divided by the model's most-likely estimate and plotted in figure 3. Five models generate per-ton benefit estimates that fall near the center of their ranges. However, based on figure 3, the municipal water treatment model may generate estimates that are one-fifth the actual level of benefits. (Note that these ranges reflect analysts' uncertainties in the data and do not reflect to the models' shortcomings discussed here.)

Summary

This paper discusses 15 benefit models, estimated since 1985, that can be used to value 15 soil conservation benefits. The economic frameworks, assumptions, and primary data behind each model and guidelines for future model development are also discussed.

Fourteen of the 15 models do, to varying degrees, account for geographical variations in the benefits of a marginal change in erosion. Three models—those with the best resolution—provide marginal benefit estimates by USGS Hydrologic Unit Code watersheds. Eleven models provide estimates by state or multi-state regions.

The models' estimates may understate the actual benefits for two reasons. First, 8 of the models are based on the average variable—not marginal—cost of erosion and marginal cost tends to be greater than average variable cost. Second, the economic frameworks supporting fourteen of the models are expected to provide conservative benefit estimates. These frameworks rely on rather restrictive assumptions on individuals' and firms' behavior and thus are not likely to capture all welfare effects.

Although the benefit estimates are conservative, they do provide insights into total program benefits and the distribution of these benefits. More accurate assessments of soil conservation benefits will be possible in the future if additional research improves the accuracy and geo-resolution of the models that are now available and increases the set of benefit models now available.

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Table 1. National benefit models

Benefit model	Cost _US	w_FPR	W_US
Marine fisheries	Value of reduction in marine fish harvests due to agricultural erosion	Number of estuaries, by FPR, with fish habitat impaired by sediment	Total number of estuaries with fish habitat impaired by sediment
Freshwater fisheries	Value of reduction in Great Lakes' fish harvests due to agricultural erosion	Number of Great Lakes estuaries, in FPR i, with fish habitat impaired by sediment	Total number of Great Lakes estuaries with fish habitat impaired by sediment
Marine rec. fishing	Value of reduction in marine recreational fishing (function of total days and consumer surplus per-day of fishing)	Number of marine estuaries in FPR_i with habitat impaired by sediment	Total number of marine estuaries with fish habitat impaired sediment
Municipal and industrial water use	Replacement and damage costs of salts and minerals associated with soil erosion	Gallons of water withdrawn by municipalities and households by FPR	Gallons of water withdrawn by US municipalities and households
Steam electric power plants	Cost of removing aquatic plants from cooling systems (portion of plant growth attributable to nutrients attached to sediment)	Gallons of water withdrawn for thermoelectric power generation by FPR	US total gallons of water withdrawn for thermoelectric power generation
Flood damages	Total damages from flooding due to agricultural sediment	US Water Resources Council's estimates of total flood damages	US Water Resources Council's estimates of regional flood damages

*DF=damage function; RC=restoration cost

Table 2. CRP reductions in erosion and soil conservation benefits

Farm Production Region	CRP acres* (10^6 acres)	Soil erosion reduction* (10^6 tons/year)	Soil conservation benefits* (10^6 \$/year)	Per-acre benefits (\$/acre/year)	Average per-ton benefit (\$/ton)
Northeast	0.19	0.6	8	44	13.33
Lake States	2.53	16.1	51	20	3.17
Corn Belt	4.77	38.6	175	37	4.54
Northern Plains	8.58	30.4	41	5	1.35
Appalachia	0.91	6.9	33	36	4.78
Southeast	1.5	6.1	26	17	4.26
Delta	1.17	9.2	44	37	4.78
Southern Plains	5.04	67.7	71	14	1.05
Mountain	6.33	40.6	36	6	0.89
Pacific	1.67	7.3	15	9	2.05
U.S.	32.89	223.5	500	15	2.24

*Source: Sullivan et al., 2004

Figure 1. Sum of the dollar-per-ton benefit variables estimated by HUC

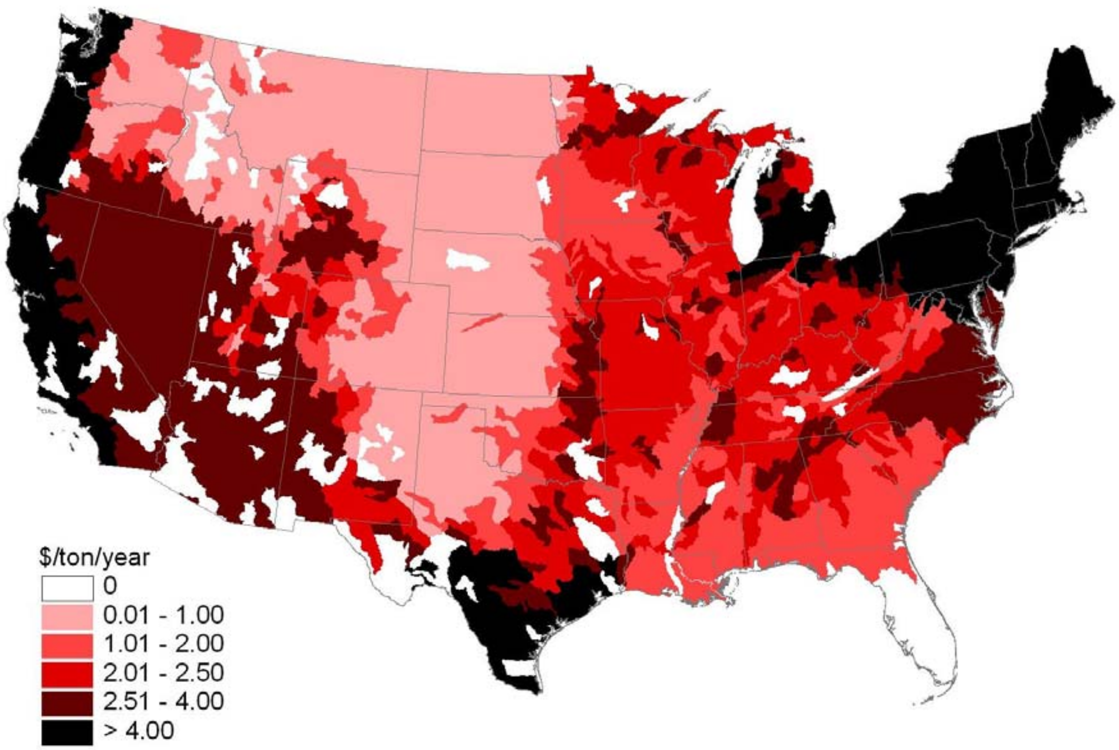
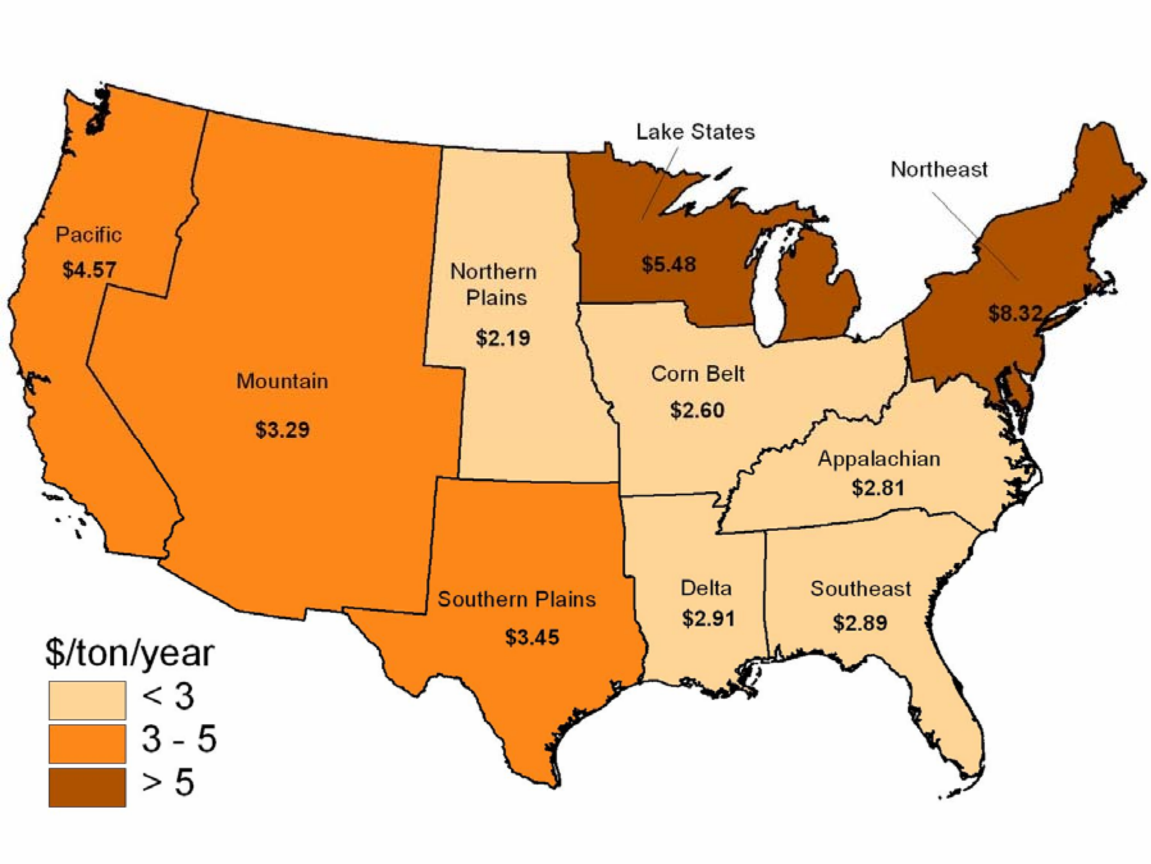
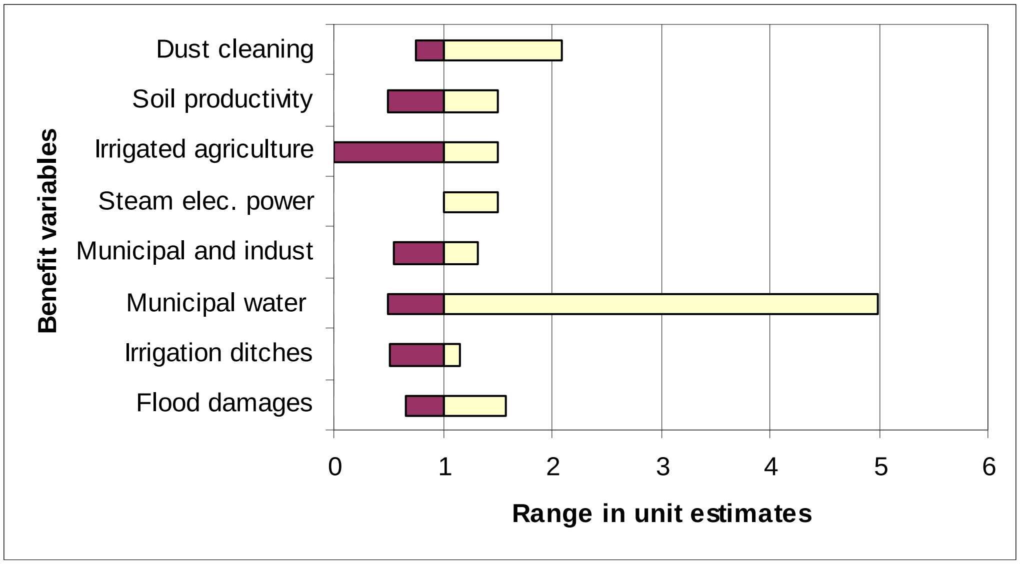


Figure 2. Sum of the dollar-per-ton benefit variables estimated by FPR



The reported values are based on the sum of estimates from 12 of the benefit models: *irrigated agriculture, irrigation ditches and canals, soil productivity, road drainage ditches, municipal water treatment, dust cleaning, marine fisheries, freshwater fisheries, marine recreational fishing, municipal and industrial water use, steam-electric power plants*, and *flood damages*.

Figure 3. Ranges in unit-mean values of the estimated benefit variables



Source: Based on estimates reported by Ribaudo (1986) and Ribaudo (1989).

The Distributional Impacts of Recreational Fees: A Discrete Choice Model with Incomplete Data

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Abstract:
In this manuscript we explore the consequences to different income groups of various policies to reduce overfishing in the Gulf marine recreational fishery. To do so we estimate a discrete choice model of marine recreational fishing demand allowing for incomplete data and a nonconstant marginal utility of income. By allowing the marginal utility of income to vary across three categorical income groups, our model allows us to explore the distributional consequences of various fee schemes and other policy programs that would change catch in the fishery. We find that flat fees have a strong proportional effect on the participation of shore fishermen, where lowincome anglers predominate, and a relatively small impact on the expensive charter fishing mode. We also find that the welfare consequences of declining catch rates would fall disproportionately on low income anglers. (JEL Q51, Q22)

* Kim is a PhD student, Woodward is Associate Professor, Shaw is a Professor in the Department of Agricultural Economics, and Shaw has a 25% appointment with A&M's Department of Recreation, Parks, and Tourism Sciences. The authors thank anonymous reviewers, George Davis, Kerry Smith, Don Waldman, John Whitehead, and particularly Edward Morey for helpful comments on an earlier version of the paper or model. Partial support for this research was provided by the National Oceanic and Atmospheric Administration's Marine Fisheries Initiative and by the Texas Agricultural Experiment Station.

The Distributional Impacts of Recreational Fees: A Discrete Choice Model with Incomplete Data

I. Introduction

Congestion and overfishing are serious issues in marine fisheries across the globe, and while the commercial fishery is often blamed, there is increasing recognition that recreational fisheries are contributing to these problems but may also play a role in their solution (Coleman et al. 2004). Hence, in addition to longstanding regulations of the commercial sector, recreational fisheries are increasingly being controlled by government actions. For example, in the Gulf of Mexico, several existing recreational fisheries are closed periodically as a way to ensure that total harvests do not exceed the total allowable catch, which is set at a level to ensure the sustainable growth of the fish stock (Gulf of Mexico Fishery Management Council 2004). Such regulations are equivalent to quantity rationing schemes, which have a negative stigma among economists because they may lead to inefficient resource allocation and encourage wasteful rent-seeking behavior. A fee-based approach might be an alternative way to reduce recreational effort that would avoid the inefficiencies that arise because of closures, promoting use by the highest-value anglers. Partly in response to budgetary pressures in the United States, managers of public recreational resources are turning more and more to park entrance fees and fishing license fees to supplement their endowments and government allowances (More 2002).

However, in contrast to quantity rationing, fees have a negative stigma among many recreational users, government officials, and other non-economists. In the recreation and leisure literature and amongst many policy makers, the use of fees is criticized on the grounds of equity because they may exclude the poorest user groups from use of resources (More and Stevens

2000). Though economists who estimate welfare impacts (gains or losses in benefits) rarely do so, it is sometimes important to examine the distributional impact of a policy (see Arrow et al. 1996). We examine the various consequences of such a fee approach on three income groups in this manuscript: we explore whether the burden of fishing fees falls disproportionately hard on lower income people. We also examine the anglers' maximum willingness to pay (WTP) to avoid a decline in catch rates.

To examine income effects requires departure from the usual recreation modeling approaches. Revealed preference models of recreation demand are often estimated using discrete choice approaches (or random utility models/RUMs), and unfortunately since the beginning of the use of these, the marginal utility of income is typically assumed to be constant [e.g., Caulkins et al.(1986) and Bockstael et al.(1987)]. There are very few exceptions.[1] In contrast, in our variant on the random utility model income effects are allowed so that distributional consequences can be explored.

The remainder of the paper is organized as follows. In the next section we provide some additional background and a review of some of the relevant literature.[2] We then present the model, a discussion of the 1997 Marine Recreational Fishery Statistics Survey (MRFSS) data set that we use, and finally, our empirical results. To preview these: we find that a flat fee imposed on all modes of fishing can be quite effective in reducing recreation demand for recreational fishing in the Gulf of Mexico. When we look at the impacts across different income groups, we find that in the Gulf a fee on fishing would tend to affect the behavior of low-income anglers more than that of high income anglers. The welfare cost of such fees to high income anglers is greater in absolute terms, but smaller relative to their income. We also look at anglers' WTP to prevent a decline in catch rates and find that, as with the welfare costs of a fee, these impacts also vary by income groups. Finally, there is evidence here that because mode choice varies across income groups, policies that seek to take into account the equity impacts can be targeted at specific modes of fishing.

II. Additional Background and Literature

To begin, we first briefly consider the RUM and the role income plays and second, some literature on imposing fees in the recreational setting. The use of the RUM in recreation demand or travel cost models is now quite well documented in the literature and we will not provide an extensive review of such literature here, as that has been done in numerous other papers (see for example, the introductory chapter in Hanley et al. 2003 and references therein). The RUM has a few distinct advantages over some other types of models applied to recreation demand (specifically the single-site count data approach) in that it handles substitution among sites rather well. However, in virtually all existing recreation demand models that have been estimated using the RUM-based approach, income effects are assumed to be absent, or at least assumed not to matter when choosing among various sites to visit. We are aware of very few estimated RUM models that appear in published or unpublished papers that allow for a non-constant marginal utility of income.[3] This is probably not a matter of carelessness or an oversight on the part of modelers: incorporating income effects generally leads to some very difficult technical issues (for discussion see Herriges and Kling 1999; McFadden 1999; Shaw and Ozog 1999). To incorporate income effects, our econometric model below draws on recent work by Morey et al. (2003a and 2003b) that incorporates income effects in a simple fashion. Morey and his colleagues assume that utility is "a piece-wise linear spline function" of expenditures. In this case, the change in the marginal utility of money is assumed to be a step function of the amount of the individual's income. This piece-wise spline approach is used to introduce an income effect below. The approach is well suited for our data set, where income is available categorically. We use this approach within the context of a repeated discrete choice version of the RUM that is geared to the data that we have.

Fee Impacts

Distributional consequences of environmental or resource programs have been considered in a variety of settings, including tradable pollution permits, the share of water shortages, and in situations where “grandfathering” allocation schemes are allowed (see Rutström and Williams 2005). The distributional impacts of recreation fees, in the context of well-developed utility-theoretic recreation demand models, have not been frequently addressed in the mainstream literature on non-market valuation. One notable exception is the contingent valuation study by Adams et al. (1989): their study of hunting and fees illustrates that lower income groups have higher losses than higher income hunters when a flat “per-head” fee is imposed on them.

Several authors of leisure studies (Reiling et al. 1996; Bowker et. al 1999; More and Stevens 2000) have concluded that implementation of a fee or an increase in a fee would lower recreational participation by low-income people. More and Stevens (2000) found that a $5 daily fee to access public lands would affect almost half of the low-income people as compared to a smaller portion (33%) of high-income people. Reiling et al. (1996) estimated that recreational demand for public lands on the part of low-income groups is more elastic than that of middle or high income groups, which implies that low income people would be more responsive to a price increase. These studies support the notion that income inequity is problematic in recreational activities. In contrast to these studies, Kyle et al. (2002) find no significant correlation between household income and willingness to pay for fees, and Winter et al. (1999) found that income was less helpful in understanding public response to fees than a measure of social trust. Because the RUM we develop is almost entirely driven by the data we have for this analysis, we next describe key features of it before we describe the model.

III. Key Data Features/Model

To estimate recreational demand, one would ideally like to know the destination, the frequency, and what mode is chosen for each and every trip an individual takes, for as long a time period as is possible. Such accurate diary data are rarely available to those interested in recreation, for the simple reasons that collecting it can be complicated, there are limits to respondent recall, and attrition among recreational users that remain in the sample for the entire length of the period is common; for all these reasons such data collection efforts are likely cost-prohibitive to most researchers. Hence, it is often the case that data are gathered by making trade-offs between study and survey cost, accuracy for the information that is collected, and focus on one or more important policy issues.

The key policy issues of interest to us here ultimately relate to management of the Gulf-marine fishery, which is suffering from overfishing. The data used here come from the 1997 Marine Recreational Fishery Statistics Survey (MRFSS) questionnaire. We use this data set as the only data currently available to examine several policies of interest in the Gulf marine fishery. Here are the key features of the data that come from this survey:

(i) Anglers were intercepted at some site, in some mode of fishing.[4]

(ii) The destination of, and mode of fishing for this intercept trip is known.

(iii) Anglers are asked the total number of trips they took over a two month period.

(iv) Anglers are asked how many of these total trips are also to the intercept destination.

(v) Anglers are not asked the specific destinations and modes of the total remaining trips, aside from those that were taken to the intercept site.

Because of the lack of knowledge about the specific destinations on all trips, this is far from ideal data. We nevertheless use it to analyze various policies, in lieu of doing no research that may shed light on them, or undertaking an expensive new data collection effort, which would entail a brand new survey and sample. Morey et al. (1991) developed a statistical and theoretical model that takes advantage of data of exactly this type because it was also data from the exact same type of questionnaire, and we therefore follow very closely the discrete choice/RUM they developed. The history of the MRFSS data set is discussed in Hicks et al. (2000) and the data used here are in fact from the 1997 study (also discussed in detail in Whitehead and Haab 1999), which uses add-on questions to the standard intercept data. Other specific details about the data used here are given below, and next we lay out the model.

Random Utility Model of Fishing Participation, Site and Mode Choice

In the Morey et al. (1991) model, the assumption was made that anglers engage in a pattern corresponding to a repeated decision, leading to a "repeated" discrete choice or random utility model of recreation demand. The repeated choice model framework is adopted by Morey et al. (1993), Parsons et al. (1999), Shaw and Ozog (1999), and a host of others (see Morey 1999 for discussion). Within the repeated choice framework the season is divided up into choice occasions so that not more than one trip can be taken during a single choice occasion. Note that even with a sample of anglers who were intercepted on site, most anglers will not participate in fishing on every choice occasion. However there were a few anglers that took as many as sixty-one fishing trips during a two-month interval; so a choice occasion is equivalent to a "trip" in our analysis.

In our context, an individual making repeated choices confronts two simultaneous decisions: whether to go recreational fishing at all during some choice occasion, and if she (reader: freely substitute "he" below) does so, choose the site and mode that will be used for fishing. The mode choices include whether to fish from shore, from a private-rental boat (private boat here after), or a from charter boat. In principle, anglers could choose to travel to one of 38 possible counties along the Gulf, from Louisiana to Florida.[5] However, because only day trips are included in our analysis, anglers tended to fish as sites near to their homes. Of all intercept trips, 84% were to one of the three nearest counties, the nearest six counties accounted for 95% of all trips and no one ventured beyond the tenth nearest site. To capture the diversity of sites while maintaining a fairly small set of choices, each angler is treated as choosing from one of seven destinations corresponding to the six nearest sites or some other site. The characteristics of the seventh site for each angler (travel cost and catch rate) are set using a weighted average for the 7th to 10th closest sites (Table 1). Of course, for each angler in the sample the set of sites considered is different.[6]

The econometric model estimated below is based on one presented in the appendix to Morey, Shaw and Rowe (hereafter, MSR - 1991), which takes full advantage of the partial data available here. To our knowledge this model has not been estimated before. It essentially reduces to estimating two conditional probabilities. First, individual i has a probability of not going fishing on any given choice occasion, equal to π_i^{nf} . On any choice occasion she can alternatively take a trip to a destination we observe, or to a destination we do not observe. The maximum number of trips an individual can take over the period is T. There are J total destinations and M possible modes that an angler might use. Hence, if K_i is the number of trips individual i takes over the period for which the destination and mode are observed (the intercept destination) and y_{jmi} is the number of trips to observed site j using mode m taken by individual i, then:

$$\sum_{j=1}^{J} \sum_{m=1}^{M} y_{jmi} = K_i \, . \tag{1}$$

We assume that the vector of random variables for these observed trips is randomly drawn from a Type I Extreme Value distribution. We estimate the probability that individual angler i chooses to fish at site j, using mode m for her intercept trip, π^{f}_{jmi}. This, in absence of other features, would lead to the conventional conditional multinomial logit model. However, we also have information about trips to "some site" of unknown destination. Ignoring these data, all we would know is the destination on trips to the intercept site. Let Q_i be the number of trips taken to destinations we do not observe (all other destinations and modes). The marginal distribution of Q_i is assumed to follow the binomial. The probability of observing individual i's choices, y_{jmi} can be written

$$P_i\left(\alpha,\beta,T\right)=\left\{\left[\frac{(T-K_i)!}{Q_i!(T-K_i-Q_i)!}\right]\left(\pi_i^{nf}\right)^{(T-K_i-Q_i)}\right\}\left\{\left(1-\pi_i^{nf}\right)^{Q_i}\right\}\left\{\left[\frac{K_i!}{\prod_{j=1}^{J}\prod_{m=1}^{M} y_{jmi}!}\right]\prod_{j=1}^{J}\prod_{m=1}^{M}\left(\pi_{jmi}^{f}\right)^{y_{jmi}}\right\} \tag{2}$$

The probability has three main parts: the first part relates to the probability of not fishing on a given choice occasion; the middle part pertains to the probability of fishing, but at some other unobserved destination; and the last part is the usual K trial multinomial on observed destination trips (McFadden 1976). This specification takes full advantage of the data that we have from this survey. Those data that we have preclude estimation using some other, more recently developed choice modeling approaches.[7]

The use of the probability in equation (2) in the likelihood function would suffer from intercept bias since those that fish more often are more likely to be interviewed. Hence, in the likelihood function estimated, we introduce a correction for potential intercept bias, replacing the distribution of unobserved trips with a sampling distribution that assumes being in the sample is proportional to the total number of trips one takes.[8] With this assumption, the modified likelihood function becomes:

$$L_k = \prod_{i=1}^{N} \left[\frac{(K_i + Q_i + 1)}{(1-\pi_i^{nf})(T-K_i)+1} \right] P_i(\alpha, \beta, T).$$

In order to estimate the probabilities of making the mode/site and participation choices, a functional form for the indirect utility function must be specified. Applying the typical linear specification of a RUM model to the problem of mode choice, the utility of an angler on choice occasion t is a function of the individual's fishing budget in period t, B_{ti}, and whether or not a particular site j and mode m has been chosen for the intercept trip at a personal cost of P_{jmi}, with catch rates CR_{jm}. That is, we write $U_{0ti} = \alpha_0 + \beta(B_{ti})$ if the angler does not fish, and $U_{jmti} = \alpha_m + \beta(B_{ti} - P_{jmi}) + \gamma CR_{jm} + \varepsilon_{jmti}$ if the angler chooses site j, mode m, where ε_{jmti} is the error term, capturing unexplained variation in the utility when the angler chooses to fish, presuming we know the destination/mode. The coefficients α_0 and α_m can be functions of variables describing the angler, the mode, or the season.

An angler will not fish if the reservation utility, U_{0ti}, is greater than the utility enjoyed in all of the modes. Hence the probability that an angler does not fish, π_i^{nf}, is the probability that $U_{0ti} > U_{jmti}$ for all other modes, so π_i^{nf} is a decreasing function of the difference $U_{jmti} - U_{0ti}$. This difference can be simplified to

(3) $$U_{jmti} - U_{0ti} = (\alpha_m - \alpha_0) - \beta P_{jmi} + \gamma CR_{jm} + \varepsilon_{jmti}.$$

As in MSR (1991) it is assumed that the non-fishing utility is deterministic so that the error in this equation is captured in the single error term, ε_{jmti}. There is a straight forward interpretation of equation 3. The identifiable difference between the α's in the parentheses can be thought of as the utility gain achieved by fishing in site *j* and using mode *m*. The $-\beta P_{jmi}$ term reflects the cost in terms of decreased utility that the angler must pay in order to gain the benefits of the fishing trip.

The usual assumption in the applied literature is that the marginal utility of income is constant so that β is the same for all possible uses of income or income levels. This specification implies, therefore, that if an angler's fishing costs increase by one dollar, his or her utility declines by a fixed amount that does not vary across incomes or for any other reason. Because the marginal utility of income may actually vary over incomes, we relax this assumption.

To allow for some variation in the marginal utility of income, we adopt Morey et al.'s (2003a, 2003b) linear spline function approach in which the marginal utility of income varies for different income brackets. If this approach is adopted, then an angler's utility (temporarily suppressing the coefficient on and the catch rate variable) taking a trip to site *j*, mode *m* would be written

$$
(4)\qquad
\begin{aligned}
U_{jmti} &= \alpha_0 + \beta_0\left(B_{ti} - P_{jmi}\right) + \varepsilon_{mti} && \text{if } (B_{ti} - P_{jmi}) \le M_0 \\
&= \alpha_0 + \beta_0 M_0 + \beta_1\left(B_{ti} - M_0 - P_{jmi}\right) + \varepsilon_{jmti} && \text{if } M_0 < (B_{ti} - P_{jmi}) \le M_1 \\
&= \alpha_0 + \beta_0 M_0 + \beta_1\left(M_1\right) + \beta_2\left(B_{ti} - M_1 - P_{jmi}\right) + \varepsilon_{jmti} && \text{if } M_1 < (B_{ti} - P_{jmi})
\end{aligned}
$$

where M_0 and M_1 are threshold points where it is assumed that the marginal utility of income changes. Using this approach and assuming that travel costs do not change an individual's income bracket, the usual utility difference equation becomes

$$
(5)\qquad U_{jmti} - U_{0ti} = \left(\alpha_m - \alpha_0\right) - \beta_k P_{jmi} + \varepsilon_{jmti}
$$

where k=0,1,2 for the three different income categories.

IV. Details on the Data, Estimation, and Empirical Results

Data Details

Anglers in 1997 MRFSS intercept survey were contacted at a variety of locations including docks, marinas, and other sites along the Atlantic and Gulf Coasts (except along the Texas coast). Interviews were spread unevenly throughout the year with a greater proportion conducted in the Sep-Oct and May-Jun waves (19.79% and 19.71%, respectively) and the fewest in the coldest and hottest months, Jan-Feb and Jul-Aug (12.91% and 14.04%, respectively). However, during any given month we assume that interviewers were told to spend the same amounts of time at each type of mode, as was true in earlier MRFSS survey efforts (see MSR's related footnote 10, 1991, p. 189). The follow-up economic survey was conducted over the telephone. The data are divided into six waves, each lasting two months each.

The questions in the survey include those about general characteristics of respondents, their number of fishing days within the previous two months, the specific information on intercept trips, i.e., what mode of fishing they engaged in, when they went fishing, and the number of fish that they caught.[9] Here we focus on single-day trips for a sample of anglers living in four states along the Gulf of Mexico coast. After eliminating incomplete observations there are 3232 observations on anglers remaining (see Table 1 for some summary statistics). As such, this is one of the larger samples of anglers we have ever worked with, which certainly adds some computational burdens, but also adds the benefit of reduced sampling error, as compared to working with only hundreds of anglers, as researchers often must do.

Anglers that were interviewed reported fishing an average of 7.3 days during a two month fishing period. As is standard procedure in the repeated framework, we divide the overall period into choice occasions, such that no angler can take more than one fishing "trip" on a choice occasion, resulting in 61 choice occasions. The most commonly chosen mode of fishing for anglers in our sample on the observed intercept trip was using a private boat (73.8%), which is not surprising since about 63% of anglers in the overall sample owned a boat. The other mode is charter boat (4.3%), with the remainder fishing from the shore (21.9%).

The focus here is on income and the survey questionnaire identified income in 11 categories, which we aggregate into three broad categories: low (less than $35,000), middle ($35,001 to $75,000), and high (greater than $75,001). These income levels correspond roughly to the 50% and 80% thresholds for U.S. households reported in the U.S. Census Bureau's Current Population Survey.[10] Because 34% of respondents in the sample do not reveal their income, the log linear ordinary least squares regression model suggested and estimated by Haab et al. (2000) is used to impute missing income values. After using imputed income, those in the lowest income category constitute 47.7% of the total sample. The middle income category contains 42.6% of the respondents, and the remainder of those in our sample (9.7%) falls into the highest income category. These income levels are identified by the dummy variables: DM_0=1 if household income is less than $35,000, DM_1=1 if household income is $35,000 to $75,000 and DM_2=1 if household income is greater than $75,001.[11]

Construction of Travel Costs/Catch Rate Variables

Travel costs to the three modes for seven destinations near the angler's home (the intercept destinations) are constructed using distances calculated using the Zipfip program.[12] Other expenses and boat fees varying by mode are computed simply as the sample average. In addition, the opportunity cost of an individual's time in travel to and from the site is factored in using assumed travel speeds and reported wage rates as the opportunity cost of time per hour, if these are available in the individuals' responses. For individuals not reporting wage rates but reporting annual income we used average hourly income instead, and for those reporting neither wage nor income, we used a hedonic regression to predict their wage rate per hour. Retirees are assumed to have an opportunity cost of time equal to the minimum wage rate.[13]. It is noteworthy that the average cost of fishing from a charter vessel is considerably more than all other modes, and sometimes an order of magnitude more costly than the cost of shore fishing.

As mentioned above, the mode-site catch rates used are the average of reported catch rates for each site and mode. When, for a given mode-site combination, only a few individuals report catch, the average reported catch could be problematic. As our sample is rather large, this was not a major problem, but when less than 20 observations were available, observations from adjacent site(s) were included until at least 20 observations were obtained. In this way, a catch rate was available for each of the 38 counties and for each of the three modes.

Estimation and Empirical Results

The final empirical specification of the probability of not fishing and the probability of site/mode choice can be written as:

$$(6) \qquad \pi^{f}_{mji} = 1 \Big/ \sum_{s=1}^{M} \sum_{l=1}^{J} \exp\begin{bmatrix} \alpha_{0s} - \alpha_{0m} + \gamma \cdot (CatchRate_{sl} - CatchRate_{mj}) \\ -(\beta_0 DM_0 + \beta_1 DM_1 + \beta_2 DM_2)(P_{sli} - P_{mji}) \end{bmatrix}.$$

and

$$(7) \qquad \pi^{nf}_{i} = \exp\left\{ -\sum_{m=1}^{M} \sum_{j=1}^{J} \exp\left[\alpha_{0m} + \gamma \cdot CatchRate_{mj} - \left(\beta_0 DM_0 + \beta_1 DM_1 + \beta_2 DM_2 \right) P_{mji} \right] \right\}$$

Note, as discussed above, that the intercept term, α_{0m} in equation (6) captures the difference between intercept in the non-fishing and mode-*m* utility function, i.e. $\alpha_{0m} = \alpha_m - \alpha_0$.

Estimation results for the participation and site/mode choice model are presented in Table 2. Table 2 indicates the explanatory variables that are significantly different from zero with expected signs: the constant terms for modes and the catch rate are positive, and the travel cost parameters are negative because of the fact that the likelihood function equation specifies the negative of the parameter in estimation. Because of the presence of 21 site/mode choice alternatives and the additional choice of not fishing for over 3,000 anglers, the model is not trivial to estimate, and most attempts to include other variables were unsuccessful. Note that the largest mode-specific constant term is for charter boat trips, which might indicate some unobserved, but non-random influence of such trips is attributable to the experience of being taken out fishing by a knowledgeable boat captain.

Of particular interest are the varying coefficients on the marginal utility of income. Comparing these across income levels, e.g., β_0, β_1, and β_2, we find some difference, with the smallest MU of income for the highest income group, consistent with economic theory: the value of a fishing dollar declines as income increases. As we discuss in more detail below, these differences across incomes indicate that anglers in the low income group, who largely participate in shore fishing, are more responsive to fee changes than other fishermen.

In Table 3 we examine predicted trips, also breaking these up by income groups and modes. The expected number of trips over the period are equal to $61\times(1-\pi_i^{nf})$, and the expected trips for each mode are calculated by $61\times(1-\pi_i^{nf})\times(\pi_{mi}^{f})$. First, it is interesting to note that after correcting for selection bias, the average number of predicted trips is significantly reduced as compared to a model with no such correction. With no selectivity correction, the reported and average predicted trips are well over six trips in the period. The corrected average in Table 3, is just over half that number: about 3.4 trips predicted for the high income groups. It can easily be seen that the private boat mode is preferred for all income categories. There is important variation across incomes for the other modes. First, only 0.5 charter trips per two-month period are predicted to be taken by low income anglers, while high income anglers are predicted to take 0.26 trips using that mode. Shore fishing is enjoyed almost equally by all income groups with high income anglers predicted to take only 5% more shore trips than low income anglers. Overall, the model finds that fishing is a normal good with high income group taking 50% more trips on average than the low income group.

V. Fee Effects on Trips and Welfare of Anglers in Different Income Groups for Fees and Catch Rate Declines

Next, we explore the impacts of various fee policies that might be used to reduce fishing pressure in the Gulf. We can easily simulate the consequences of these on the low, middle, and high income groups because of our allowance for income effects. Specifically, we first examine the impact on trips of several flat daily fees ($5, $10, $20) imposed on all modes, and on selected modes. A $5 per day fee is likely within the realm of any policy change that might accompany a program to recover revenue today, or rising costs of managing facilities. A $20 per day fee, on the other hand, would represent a substantial increase in the daily cost of fishing, particularly in the low cost mode of shore fishing. Second, we report the WTP to prevent a 20% decline in catch. Though we were unable to uncover catch rates that vary by species, we hypothesize that there may well be quite different welfare impacts from catch rate declines on income groups that use different modes.

Trip Impacts

Table 4 reports the impact on trips of the fees equally imposed on all modes across the income groups, as well as a weighted average of these (far right hand column). Note that the percentage loss in trips over the period is highest among low income groups, as one might expect. In fact, a $20 daily fee imposed on all modes is predicted to reduce by three-fourths the number of trips taken by low income anglers. High income anglers also reduce their trips substantially, (about 65%), though their response is not as pronounced as for the low income group.

Imposing a fee across all modes of fishing is likely not an effective way to reduce overfishing, particularly when the species targeted by different modes varies greatly. The boating anglers (private and charter boat) are able to catch species in deep water that are not accessible to shore anglers. If these types of fish are of more concern than those caught from shore, then it may make the most sense to impose the fee on these modes, together or separately. Table 5 considers the loss in offshore boat trips and the corresponding total number of trips by imposing the same daily fees as considered in Table 4, but only on the offshore private and charter boat modes. Notice that when the fee is imposed on only these modes, the percentage reduction in total number of trips is much smaller than it is when the fee is on all modes. Differences across groups are small at low fees, but rise as the fee increases to $20 per day. As a percent of the base number of trips, a $20 fee causes a 30% reduction in charter and private trips taken by the high income anglers, but a much greater reduction, 48%, by the low income group. It appears that high income anglers are more likely to stick with expensive offshore boat modes, while low income anglers who might sometimes use a boat (offshore) mode tend to shift their fishing to inexpensive shore fishing.

For the purpose of contrast, Table 5 also considers the impact of a flat fee imposed only on shore fishing. While probably not plausible in any political sense, it is interesting to note the much higher loss in all shore trips that would accompany a $20 fee on shore trips. Nearly every shore trip (92%) that would be taken over the period is lost to low income groups. This policy will also decrease the total number of trips taken by the low income group more than it would affect the behavior of higher income groups. Even though anglers can substitute away from the shore fishing into the other modes, our empirical results indicate that a fee on shore fishing increases the probability of not fishing so much that even fishing in other modes is predicted to decline.

Welfare Estimates

Welfare losses with income effects can be computed by examining the usual log-sum formula in the repeated choice version of the random utility model (see Morey 1999). We consider the per-period compensating variation (PPCV), which can be interpreted as the maximum willingness to pay on each choice occasion (or per day, in our case) to avoid the fee increase. Expected CV's for a \$5 fee imposed on two separate modes (offshore boats only and shore only), and for a catch rate decline of 20% are reported in Table 6. We also calculate CV as percentage of per period income to yield a better picture of the relative impact on the various income groups.

When a \$5 fee is imposed for each day of offshore fishing, the daily impact on the high income group is predicted to average about a \$3.84, with a smaller loss of \$3.34 on the low income anglers, as might be expected. However, the welfare impact on low income anglers as a percentage of daily income (0.88%) is bigger than that on high income group (0.22%). In contrast, if a \$5 fee were levied for each day of shore fishing, the impact on low income anglers would be greatest in both absolute and relative terms. Clearly, fee can have markedly different impacts on anglers of differing incomes and low income anglers appear to always be affected more by a fee in terms of welfare loss relative to their income.

If nothing is done to solve over-fishing in the Gulf marine fishery, catch rates may fall. We do not know by how much they would fall, so we consider the case of a 20% decline and examine the accompanying welfare measures. In the bottom of Table 6 the per-day WTP to prevent this decline is reported. We estimate WTP values ranging from about $0.6 to $0.85, with higher income anglers benefiting the most. As percentage of daily income, however, the CV of low income anglers is the greatest, 0.16% versus 0.05% for high income anglers. This relative distribution of impacts across income groups is the same as the distribution of costs if a $5 fee is imposed on off-shore fishing. In contrast, if a fee is imposed only on shore fishing, the low income anglers would pay the highest cost in both absolute and relative terms.

VI. Summary and Conclusions

There is increasing recognition that recreational fisheries as well as commercial fishing must be involved in solutions to overfishing. Standard economics dictates that limiting catch by using a price mechanism would be more efficient than seasonal closures or other forms of quantity rationing. However, a flat pricing policy, such as an access fee for all anglers may have more significance to those on the lower end of the income distribution than to those on the upper end. Certainly if a fee is imposed on inexpensive onshore trips, the percentage burden on low income anglers will be higher than for other anglers because they use this mode of fishing.

To obtain our results we used a model geared to the type of data we have. It is based on the approach taken by MSR (1991), which is appropriate when complete trip data are not available. Our model here extends that estimated by MSR (1991) in that it allows for a correction to intercept bias, as well as letting the price coefficient/marginal utility of income to vary across three income groups. We note, as a caveat, that many modern versions of the random utility model relax the assumption of the independence of irrelevant alternatives (IIA), while our model here does not. We therefore caution against any reading of the welfare impacts as being exact in our analysis, while noting the importance of relative orders of magnitude across the income groups, which is our focus.[14]

Although economists regularly wash their hands of equity-based analysis, there are two reasons why equity implications of fishery policies should be considered. First, the political viability of a policy is affected by its fairness or the perception of fairness. Some policies are just going to be dead on arrival, if they hit some groups too hard. Second, equity remains one of the fundamental normative principles accepted by most economists and, although we may not be able to provide definitive recommendations based on this principle, it is informative to all if an analyst is able to present the distributional consequences of a policy (Arrow et al. 1996). The analytical tools that are used must be capable of providing information on distributional consequences. We cannot think of a way to do this unless income effects are allowed in the model.

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TABLE 1
Distributions and Variable Summary Statistics

		Frequency	Percentage
Mode Distribution (Intercept Trips)		(trips)	
Charter		139	4.3%
Private-Rental		2384	73.8%
Shore		709	21.9%
Site Distribution (Intercept Trip)		(trips)	
1st Closest Site		1886	58.4%
2nd Closest Site		560	17.3%
3rd Closest Site		276	8.5%
4th Closest Site		182	5.6%
5th Closest Site		106	3.3%
6th Closest Site		46	1.4%
Rest of 7th to 10th Closest Site		176	5.4%
Income Distribution		(anglers)	
less than \$35,000	(DM_0)	1541	47.7%
\$35,001 to \$75,000	(DM_1)	1378	42.6%
Greater than \$75,001	(DM_2)	313	9.7%

Note: Variable names in parentheses where appropriate.

TABLE 2
Estimation Results of Participation-Mode-Site Choice Model

Parameter		Estimate	St. Error	P-value
Constant for charter	($\alpha_{0charter}$)	14.491	0.729	.000
Constant for Private	(α_{0shore})	5.410	0.092	.000
Constant for Shore	($\alpha_{0private}$)	3.089	0.106	.000
Coefficient for Catch Rate	(γ)	0.073	0.027	.006
MU of Income for Low Income Group	(- β_0)	-0.071	0.004	.000
MU of Income for Middle Income Group	(- β_1)	-0.063	0.003	.000
MU of Income for High Income Group	(- β_2)	-0.054	0.004	.000

Note: Standard Errors computed from analytic first and second derivatives (Eicker-White).

TABLE 3

Average Predicted Trips for Two-month Period by Income groups and modes

Predicted Trips		Total	Charter	Private	Shore
Low Income	less than $35,000	2.267 (100%)	0.005 (0.2%)	1.595 (70.4%)	0.667 (29.4%)
Middle Income	$35,001 to $75,000	2.700 (100%)	0.032 (1.2%)	1.970 (73.0%)	0.699 (25.9%)
High Income	greater than $75,001	3.381 (100%)	0.256 (7.6%)	2.422 (71.6%)	0.703 (20.8%)

Note: Percentages of the number of trips in the Parentheses

TABLE 4

Predicted Total Number of Trips When Flat Fee is imposed for all modes

	Total Trips per two-month period by income			
Daily Fee	Low	Middle	High	Weighted Average
$0	2.267	2.700	3.381	2.524
	(0.0%)	(0.0%)	(0.0%)	(0.0%)
$5	1.604	1.984	2.600	1.830
	(-29.2%)	(-26.5%)	(-23.1%)	(-27.5%)
$10	1.133	1.454	1.995	1.325
	(-50.0%)	(-46.1%)	(-41.0%)	(-47.5%)
$20	0.563	0.778	1.170	0.693
	(-75.2%)	(-71.2%)	(-65.4%)	(-72.6%)

Note: Percentage Declines in Parentheses.

TABLE 5

Predicted Number of Trips When Flat Fee is imposed for only specific mode

Trips per two-month period by income with a daily flat fee to only offshore boats						
Daily	Low Income		Middle Income		High Income	
Fee	Total	Charter+Private	Total	Charter+Private	Total	Charter+Private
$0	2.267	1.599	2.700	2.000	3.381	2.678
(Base)	(0%)	(0%)	(0%)	(0%)	(0%)	(0%)
$5	2.252	1.412	2.681	1.812	3.356	2.496
	(-0.7%)	(-11.7%)	(-0.7%)	(-9.4%)	(-0.7%)	(-6.8%)
$10	2.241	1.214	2.668	1.609	3.337	2.299
	(-1.1%)	(-24.1%)	(-1.2%)	(-19.6%)	(-1.3%)	(-14.2%)
$20	2.229	0.822	2.650	1.184	3.312	1.864
	(-1.7%)	(-48.6%)	(-1.9%)	(-40.8%)	(-2.1%)	(-30.4%)

Trips per two-month period by income with a daily flat fee to only shore fishing						
Daily	Low Income		Middle Income		High Income	
Fee	Total	Shore	Total	Shore	Total	Shore
$0	2.267	0.667	2.700	0.699	3.381	0.703
(Base)	(0%)	(0%)	(0%)	(0%)	(0%)	(0%)
$5	1.619	0.367	2.003	0.406	2.625	0.438
	(-28.6%)	(-45.0%)	(-25.8%)	(-41.8%)	(-22.4%)	(-37.7%)
$10	1.159	0.198	1.488	0.233	2.040	0.270
	(-48.9%)	(-70.3%)	(-44.9%)	(-66.7%)	(-39.7%)	(-61.6%)
$20	0.602	0.055	0.830	0.075	1.242	0.101
	(-73.4%)	(-91.7%)	(-69.2%)	(-89.3%)	(-63.3%)	(-85.6%)

Note: Percentage Declines in Parentheses.

TABLE 6

Expected Compensating Variation Associated with Policy Scenarios

E(CV) by Income When A $5 Daily Fee is Imposed for Only Offshore Boats		
Income Group	E(CV) per day	E(CV) as % of daily income*
Low	-3.34	-0.88%
Middle	-3.55	-0.45%
High	-3.84	-0.22%

E(CV) by Income When A $5 Daily Fee is Imposed for Only Shore Fishing		
Income Group	E(CV) per day	E(CV) as % of daily income
Low	-1.30	-0.34%
Middle	-1.15	-0.15%
High	-0.93	-0.05%

E(CV) by Income When Catch Rate decreases by 20%		
Income Group	E(CV) per day	E(CV) as % of daily income
Low	-0.60	-0.16%
Middle	-0.69	-0.09%
High	-0.85	-0.05%

*The mean levels of specific 11 income categories with the highest category of $175,000 are used. The mean income levels for each income group are 23,187, $48,372, and $108,198, respectively.

[1] The random utility model's leading competitor these days is probably some sort of count-data model and within that structure, income plays a role that is often difficult to discern, especially in the single-site count data model.

[2] The usual apology to those authors of important papers we missed pertains.

[3] Shonkwiler and Shaw (2003) consider the impact of a $5 increase in the fee at one of the Columbia River main-stem reservoirs within a finite mixture model that allows for income effects, but this is quite different than the usual RUM model. They find that recreational users within one regime lose almost twice the consumer's surplus as those in another income regime.

[4] As will be shown below, our econometric model has a correction for potential intercept bias.

[5] Texas fishing trips were not included in the survey.

[6] Choosing which sites to put in the model was complicated and involved choices of aggregation and selection because of the size of the "Gulf" marine fishery. The focus here is on income effects, and we readily admit there may be interesting extensions involving aggregation schemes and potential bias (see Haener et al. 2004; Parsons and Hauber 1998).

[7] Note that if the popular nested logit specification were used, for example, this would have the advantage of breaking the independence of irrelevant alternatives (IIA) assumption, but for any nesting structure we can think of, this would come at the expense of having to use an average of all prices and catch rates and some aggregate of the unobserved destination sites. As we do not know the other destinations, this assumption seems rather unacceptable. In any case, concerns about aggregation issues (see footnote 6) would quickly be exacerbated, likely overwhelming any gains from nesting.

[8] See the intercept bias correction discussed in MSR (1991), their equation 14, and the relevant text where results are discussed.

[9] Unfortunately, we do not have information to calculate catch rates by species.

[10] In 1997, 50% of the U.S. households had an income less than $40,699, 80% less than $78,638 - See (http://www.census.gov/hhes/income/histinc/ie4.html).

[11] The dummy variable trap is avoided because there no default coefficient.

[12] The authors thank Daniel Hellerstein at ERS/U.S.D.A. for his generous assistance in obtaining that.

[13] Results of the hedonic wage regression models and other details are available on request of the authors, however, because there are 38 specific destinations used to set the seven sites for each angler, summary statistics are not easily presented.

[14] We add that we estimated a simplified version of the model using a random parameters logit (Train 1998), which is in theory, possible to do with our likelihood function and which would also relax the IIA. We specified the model with only one travel cost coefficient, assumed to be normally distributed, and one catch rate coefficient. The model converged after over 6 hours, resulting in a mean travel cost coefficient that is about the same as the one obtained using a similar flat conditional multinomial logit, and a significant standard deviation on this parameter. Were this true in the complex case, it would suggest little difference, on average, in welfare measures, at least if the normal distribution is the best one to use. Attempts to let the travel cost coefficient be log-normally distributed failed to achieve convergence, as is apparently typical (personal communication with Kenneth Train). Estimation of the more complex likelihood function with over 3,000 anglers will be quite a challenge that lays ahead.