# WILLINGNESS TO PAY FOR AGRICULTURAL LAND PRESERVATION AND POLICY PROCESS ATTRIBUTES: DOES THE METHOD MATTER?

ROBERT J. JOHNSTON AND JOSHUA M. DUKE

This article examines relationships between willingness to pay for land preservation and policy process attributes. The approach departs from traditional welfare assessments in that it does not constrain attributes of the policy process to be utility-neutral. Results indicate policy process attributes may influence utility in some circumstances, even after controlling for the influence of land use outcomes often correlated with specific policy techniques. Results further imply that in some cases, even comprehensive specification of land use outcomes by stated preference instruments may be insufficient to prevent systematic shifts in willingness to pay related to unspecified, yet assumed, policy process attributes.

*Key words*: choice experiment, choice modeling, farmland preservation, policy implementation, valuation.

Farmland preservation may be accomplished using a variety of policy techniques and implemented by a range of public and private agents.[1] The stated preference (SP) literature is clear with regard to potential biases associated with SP surveys that provide "few if any details about the goods *and how they would be provided*" (Carson, Groves, and Machina 2000, p. 4, emphasis added). Such guidance notwithstanding, economists frequently assess willingness to pay (WTP) for land preservation outcomes independent of detailed information regarding the policy process or with vague reference to policy implementation techniques.

Stated preference analyses of land preservation, in particular, often suppress information regarding policy implementation or assesses WTP contingent upon a single, often sparsely described policy (e.g., Halstead 1984; Beasley, Workman, and Williams 1986; Ready, Berger, and Blomquist 1997; Bowker and Didychuk 1994; Duke and Ilvento 2004). A recent review of the farmland valuation literature identifies only one SP analysis—an unpublished article by Swallow (2002)[2]—that quantifies the influence of any policy process attribute on WTP (Bergstrom and Ready 2005). In addition, while the land valuation literature often discusses implications of policy process attributes for outcomes of preservation (e.g., public access, as discussed by McGonagle and Swallow 2005), it generally overlooks the possibility that policy process attributes themselves may influence WTP, apart from influences on measurable preservation outcomes.

Such practices reflect a common presumption that policy implementation details do not represent a major consideration in estimating WTP, and follow the standard neoclassical purchase model in which utility and welfare are assumed to be determined solely by policy outcomes, independent of the policy process leading to those outcomes (Bulte

---

Robert J. Johnston is associate professor, Department of Agricultural and Resource Economics and associate director, Connecticut Sea Grant College Program, both at the University of Connecticut. Joshua M. Duke is associate professor, Departments of Food and Resource Economics and Economics, and the Legal Studies Program, University of Delaware.

This research was funded by the National Research Initiative of the Cooperative State Research, Education and Extension Service, USDA, Grant 2003-35400-13875. The experimental design was completed by Lídia Rejtõ and Diccon Bancroft at the University of Delaware STATLAB. Opinions belong solely to the authors and do not imply endorsement by the funding agencies. Any remaining errors are those of the authors.

[1] The land preservation policy process includes both preservation *technique* and implementing *agent*. Duke and Lynch (2006) identify 28 techniques used to preserve farm and forest land. The survey described here focuses on three of the most common and familiar of these: conservation easements (described to respondents as "preservation contracts"); fee simple purchase ("outright purchase"); and enhanced zoning protections ("conservation zoning"). The survey also distinguishes two types of implementing agents: state and local governments and nongovernmental land trusts.

[2] This article addresses the impact of public vs. private land ownership on WTP for land preservation.

*Amer. J. Agr. Econ.* 89(4) (November 2007): 1098–1115

DOI: 10.1111/j.1467-8276.2007.01029.x

et al. 2005). Notwithstanding this common presumption, recent evidence suggests that the means of obtaining environmental outcomes may influence WTP. For example, Bosworth, Cameron, and DeShazo (2006) show that WTP for mortality reductions varies according to whether reductions are achieved using prevention or treatment mechanisms. Similarly, Bulte et al. (2005) show that WTP to reduce wildlife losses depends on whether losses result from man-made or natural events. Anecdotal, qualitative, and indirect evidence suggests that similar patterns may apply to land preservation (Inman and McLeod 2002; McLeod et al. 1998; McLeod, Woirhaye, and Menkhaus 1999; Johnston et al. 2003; Rosenberger et al. 1996). For example, focus groups of McLeod et al. (1998) 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, the published literature provides no systematic, quantitative information indicating whether and how WTP for land preservation is influenced by the attributes of the policy process.

This article examines relationships between WTP for land preservation outcomes and attributes of the policy process, considering stated preferences for farm and forest preservation in two Northeastern states. The approach departs from traditional welfare assessments in that it does not constrain the attributes of the policy process to be utility-neutral. The model is based on 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 and WTP, providing a more complete perspective on relationships between farmland preservation policies and individual welfare.

## 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. For example, in the absence of information regarding public access, respondents might assume that conservation easements are less likely to provide access than fee simple purchase (McGonagle and Swallow 2005). Individuals might also associate particular policy processes with increased probability of preservation success. Still others might associate specific techniques with an increased realization of personal benefits (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 outcomes. Accordingly, they are most appropriately addressed though more complete specification of the vector of relevant land use outcomes, based on evidence from appropriate survey design methods (Kaplowitz, Lupi, and Hoehn 2004; Johnston et al. 1995).

A second possibility, however, is that respondents might maintain systematic preferences for policy tools that are unrelated to measurable land use outcomes. For example, respondents might maintain a systematic preference for government involvement in land preservation, or believe that certain policy actions represent an inappropriate use of public authority (cf., Inman and McLeod 2002; McLeod, Woirhaye, and Menkhaus 1999). Such preferences might manifest as a change in utility associated with government-implemented policies, apart from any measurable outcome of those policies. Individuals might also maintain altruistic preferences for consumption bundles realized by others (McConnell 1997) or for the distribution of program costs across different groups (Mansfield and Smith 2002), leading to varying support for land use policies anticipated to generate particular distributions of costs and benefits.[3] To the extent that such preferences are of the type that should legitimately be incorporated in benefit cost analysis (Freeman 2003, p. 150), associated WTP measures represent a valid component of welfare analysis not associated with traditionally measured land use outcomes.

In such cases, WTP estimates associated with land use outcomes alone may provide misleading or partial welfare guidance.

[3] We distinguish *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 issue, 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 *land use* outcomes typically represented in SP research. Moreover, if certain benefit distributions are unique features of specific policy techniques, it may be difficult to distinguish preferences for the policy from preferences for the distribution. In either case, such preferences are not appropriately captured by SP instruments that omit policy process details.

The omission of utility-relevant policy process attributes, for example, may generate WTP biases related to the methodological misspecification of valuation contexts (i.e., respondents' unobserved yet potentially systematic assumptions regarding applied policy process attributes). In addition, if welfare estimates are contingent upon a single policy technique, their valid application to benefit-cost analysis or benefit transfer would be limited to instances in which similar policy techniques were applied. Both possibilities represent cases in which the welfare associated with farmland preservation policies could depart significantly from that predicted by standard methods. In summary, inappropriate assumptions regarding the utility-neutrality of policy process attributes could have significant implications both for the validity of welfare estimates and for the use of these estimates to guide policy. Nonetheless, standard practice in the land valuation literature is to suppress such possibilities, effectively presuming that details of the policy process are irrelevant for welfare estimation.

## *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 preferences for policy process attributes alone, an important element of the model is the experimental control of utility-relevant land use attributes sometimes omitted from SP analyses, yet potentially associated with particular policy techniques. Without this control, that which appears to be a systematic preference for process attributes might instead be a preference for omitted land use outcomes.[4] 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),

$$(1)\qquad U_{ij}(\cdot) = U_{ij}(\mathbf{X_{ij}}, \mathbf{W_{ij}}, Y_i - F_{ij}) = v(\mathbf{X_{ij}}, \mathbf{W_{ij}}, Y_i - F_{ij}) + \varepsilon_{ij}$$

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;
$\varepsilon_{ij}$ = the unobservable 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 preserved.[5] 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 (McGonagle and Swallow 2005). 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, $j = \text{N}$). 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

[4] A reviewer notes that it may be difficult to control for all potential correlations between outcomes and policy mechanisms that might be assumed by respondents. While it is of course impossible for any survey to control for every possible correlation that might be assumed by each and every respondent, the primary concern is that researchers control for more significant correlations that might be assumed by a non-trivial number of respondents. The importance of identifying shared perceptions and assumptions in SP survey design is discussed by Johnston et al. (1995); this guidance was used in survey design to identify perceptions of the land preservation policy process shared by respondents.

[5] Although one could easily think of counter-examples in which the number of acres that could be preserved would depend on the techniques used for preservation.

the greatest utility. Given (1), individual $i$ will choose plan A if

$$(2) \quad U_{iA}(\mathbf{X_{iA}}, \mathbf{W_{iA}}, Y_i - F_{iA}) \geq U_{ik}(\mathbf{X_{ik}}, \mathbf{W_{ik}}, Y_i - F_{ik}) \quad \text{for } k = B, N,$$

such that

$$(3) \quad v_{iA}(\mathbf{X_{iA}}, \mathbf{W_{iA}}, Y_i - F_{iA}) + \varepsilon_{iA} \geq v_{ik}(\mathbf{X_{ik}}, \mathbf{W_{ik}}, Y_i - F_{ik}) + \varepsilon_{ik}.$$

If the $\varepsilon_{ij}$ 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 $\partial v(\cdot)/\partial \mathbf{W_{ij}} \neq 0$ and respondents make systematic assumptions concerning elements of $\mathbf{W_{ij}}$ based on the correlated elements of $\mathbf{X_{ij2}}$ present in the scenario, the result will be biased, inconsistent estimates for *all model parameters*, including those associated with $\mathbf{X_{ij1}}$ (Greene 2003).[6] 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. This presumption aside, the 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 scenario and associated statistical model. The result is a combination of methodological misspecification (the behavioral implication) and bias in parameter and welfare estimates (the statistical implication).

The implication of this model is that appropriate WTP estimation—including marginal WTP for land use attributes that may not be correlated with policy process attributes ($\mathbf{X_{ij1}}$)—depends on an appropriate specification of utility-relevant policy process attributes, $\mathbf{W_{ij}}$, in SP scenarios. Omission of these attributes will bias estimated 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. To test this hypothesis, a random utility model is estimated for land preservation preferences using choice experiment data. Hypothesis tests (based on both parametric $t$-tests and differences in empirical distributions) test for statistically significant effects of policy process attributes on marginal utility and welfare (WTP), holding associated land use outcomes constant.

## The Data

The data are drawn from the *Mansfield* and *Preston Land Preservation Surveys* in Connecticut and the *Georgetown*, and *Smyrna Land Preservation Surveys* in Delaware. Surveyed communities were selected based on a number of factors, including the presence of similar development pressures, the lack of a major urban center in close proximity, and the existence of substantial areas of undeveloped land.

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 (Kaplowitz, Lupi, and Hoehn 2004). One of the initial purposes of focus groups was to identify respondents' perceptions of the land preservation policy process and the extent to which specific policy implementation techniques were assumed to be correlated with measurable policy outcomes. Extensive pretests were conducted in focus groups and interviews to ensure that the survey language and format could be easily understood by respondents, that respondents shared interpretations of survey terminology and scenarios, that the survey scenarios captured land use and policy attributes viewed as relevant and realistic by respondents, and that respondents were not treating the incorporated policy process attributes as proxies for potentially omitted land use outcome attributes. Focus groups led to a self-administered mail survey, following a choice experiment framework (Adamowicz et al. 1998).

Prior to administration of choice experiment questions, the survey provided information on land use and change in respondents' local areas, tradeoffs implicit in land conservation and reminders of the budget constraint, and techniques used to preserve farm and forest land. The survey also provided instructions

[6] The result is omitted variables bias in a discrete choice model, in which observed choices depend on an assumed set of values for omitted policy process variables. 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.

and information for choice experiment questions, including attribute details. 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 located in their community. Respondents were provided with two preservation options that would each preserve a single parcel with 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, "I would not vote for either program," following standard language in SP surveys. The second option stated, "I support these programs in general, but my household would/could not pay for either Option A or B." This option was included based on focus group results and prior research (Loomis, Traynor and Brown 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 quandary facing "individuals who would not pay the bid amount, but nevertheless want to register (symbolic) support for provision of the public good" (Loomis, Traynor, and Brown 1999). For purposes of estimation the two status quo options—both indicating a choice of no preservation—were combined into a single category.[7]

Each respondent was provided with three choice experiment questions and was instructed to consider each as an independent, nonadditive 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.

[7] This treatment of responses, while simplifying the data, has no substantive impact on model results.

Choice questions also specified elements of $\mathbf{W_{ij}}$, including the specific *technique* 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 easement and fee simple techniques included state government or local land trusts.[8] Combining these possibilities, presented policy alternatives included conservation easements implemented by land trusts using government block grants (*tr_cont*), 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] The survey provided detailed information on each of these policy attributes prior to administration of choice questions (see Appendix). Table 1 describes the attributes that distinguished hypothetical preservation options.

The fractionated experimental design was constructed by the University of Delaware STATLAB based on a D-optimality criterion (Kuhfeld and Tobias 2005). Table 2 shows attributes, levels, and other details of the design. 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 2,763 deliverable surveys, 1,136 were returned, for an average response rate of 41.1%. Returned surveys provide 3,309 complete choice responses.

## The Empirical Model

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 independence from irrelevant alternatives (IIA) property. For these and other reasons, researchers are increasingly considering mixed logit (ML) models for SP applications (McFadden and Train 2000; Hensher and Greene 2003). ML models allow

[8] Given that zoning in Connecticut and Delaware is implemented at the local level, this method was always associated with local government.

[9] Land trust activities were described as involving government block grants to motivate the payment vehicle (taxes) in choice questions.

**Table 1. Variables and Descriptive Statistics**

| Variable | Description | Mean (Standard Deviation)[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 (dummy) 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_cont* | 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. Attributes and Levels for Choice Experiment Design**

| Attribute | Levels |
|---|---|
| Acres (4 levels) | 1. 20<br>2. 60<br>3. 100<br>4. 200 |
| Land type (5 levels) | 1. Active farmland<br>a. Nursery<br>b. Food crop<br>c. Dairy or livestock<br>2. Farmland (currently idle)<br>3. Forest |
| Policy technique and implementing agency (5 levels) | 1. Preservation contracts<br>a. By state<br>b. By land trusts using block grants<br>2. Outright purchase<br>a. By state<br>b. By land trusts using block grants<br>3. Conservation zoning |
| Public access (3 levels) | 1. No access allowed<br>2. Access for walking and biking<br>3. Access for hunting |
| Development risk (3 levels) | 1. Development likely in less than 10 years if not preserved<br>2. Development likely in 10–30 years if not preserved<br>3. Development NOT likely within the next 30 years |
| Cost (6 levels) | 1. $5<br>2. $15<br>3. $30<br>4. $50<br>5. $100<br>6. $200 |
| Design D-efficiency | 50.20[a] |

[a]The D-efficiency score is provided based on the request of a reviewer, as calculated using SAS PROC OPTEX. The calculation and interpretation of D-efficiency are detailed by Lusk and Norwood (2005). While the D-efficiency ordering of designs is invariant to a nonsingular recoding of parameters, the D-efficiency score is not (i.e., it changes with model parameterization). Hence, the D-efficiency score is most useful for comparing designs within a given model and provides less useful information outside of this context. The illustrated D-efficiency is for a model parameterized with all main effects and two-way interactions and accounts for constraints imposed to ensure logical choice sets as well as pair blocking. Including all attributes and levels, the full factorial design for a single policy option (e.g., Option A) would require 5,400 unique profiles. A full factorial design incorporating all attribute levels for both choice options (e.g., Option A and Option B) would require 29.1 million profiles. The size of this full factorial is diminished, however, by infeasible attribute combinations. The final design in the present case is characterized by 1,500 unique question pairs, divided over 500 survey booklets.

for coefficients on attributes to be distributed across sampled individuals according to a set of estimated parameters and researcher-imposed restrictions (Hu, Veeman, and Adamowicz 2005). While such models require a greater number of choices regarding model specification (e.g., the selection of fixed versus random parameters, the distribution and correlation of random parameters), they have greater flexibility and can approximate any random utility model (Hensher and Greene 2003). For comparison, both CL and ML results are applied to the present survey data.

*Specification of the Mixed Logit Model*

The estimation of ML models requires that researchers determine which coefficients in the utility functions should be randomized and the distributions that characterize random components (Hu, Adamowicz and Veeman 2006). Although some ML specifications allow for randomization of the entire coefficient vector, in practice researchers often randomize only a subset of coefficients (e.g., Hensher and Greene 2003; Hu, Adamowicz, and Veeman 2006; Layton 2000). This is at least in part due to potential convergence difficulties that may be associated with large numbers of random coefficients (e.g., Layton 2000) and the tendency of researchers to randomize only those coefficients for which the modeling of heterogeneity is of particular relevance (e.g., Hensher and Greene 2003; Hu, Adamowicz, and Veeman 2006; Johnston et al. 2005). While statistical tests can in some cases help researchers determine initial candidate sets of random parameters (e.g., McFadden and Train 2000), these tests are of low power with difficult-to-retrieve critical values (Hu, Adamowicz, and Veeman 2006). In addition, models incorporating a random parameter on program cost (typically distributed lognormal) often encounter challenges related to the long right-hand tail of the distribution and associated implications for mean WTP (Hensher and Greene 2003). As a result, the most appropriate specification of ML models is often unclear, and involves both quantitative assessments and researcher judgment (Hu, Adamowicz, and Veeman 2006).

Here, following the guidance of Hensher and Greene (2003) and others, the final ML model is based on a combination of researcher expectations based on theory and prior empirical evidence, and consideration of preliminary models incorporating different sets and distributions of random coefficients. When specifying the ML model, we first emphasize the estimation of distributions for the coefficients of main emphasis. Given a primary research focus on the policy process attributes *st_purch*, *tr_purch*, *tr_cont,* and *zoning* (table 1) and the superior statistical fit of models in which the associated coefficients are specified as random (measured using traditional likelihood ratio tests), all are randomized in the ML model. Following common practice for coefficients with no a priori sign or distributional expectations, a normal distribution is assumed. The estimated standard deviation on the program cost coefficient (*cost*) is also significant in all model specifications in which the coefficient is randomized. Accordingly, we also randomize this coefficient. Following common practice, the coefficient is estimated with a lognormal distribution and sign-reversal on *cost* (Hensher and Greene 2003). As we experience repeated convergence difficulties with models incorporating large numbers of correlated random coefficients, we specify independent distributions.

Additional specification choices involve the treatment of coefficients on alternative specific constants (ASCs)—particularly that associated with the choice of "neither plan" (*neither*). Based on evidence from focus groups and prior research (Johnston et al. 2002, 2005), we expect that observable respondent attributes such as age, education, and membership in environmental organizations may be correlated with the likelihood of rejecting all plans. Accordingly, the ASC for neither plan is interacted with a set of these covariates in both the CL and ML model specifications (table 3). ML models in which the coefficient on *neither* is also randomized failed to converge; hence this coefficient is specified as fixed in the final model.

Trials with preliminary models in which the entire set of non-ASC model coefficients is randomized (with independent distributions) provide no evidence of statistical improvement over the simpler model in which random coefficients are restricted to the five primary coefficients of interest (i.e., coefficients on *st_purch*, *tr_purch*, *tr_cont*, *zoning,* and *cost*). Moreover, results for these alternative specifications reveal a relatively high degree of robustness in the primary findings of interest (e.g., statistical significance of policy process attributes). As a result, all less focal model coefficients are specified as fixed. Such results mirror findings elsewhere (e.g., Hu, Adamowicz, and Veeman 2006) that—despite the many choices facing ML practitioners—primary ML model results can be robust to such specification choices. The ML model is estimated using maximum likelihood with Halton draws applied in the likelihood simulation.

## Results

Results for three models are illustrated in tables 3 and 4, including two CL specifications and one ML specification. table 3 illustrates parameter estimates; table 4 shows summary statistics and hypothesis test results. Model one is an unrestricted CL model, with dummy variables allowing for systematic variation in estimated parameters across the CT and DE samples.[10] 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 across the CT

[10] Slopes are permitted to vary systematically for all linear (non-interaction) variables.

**Table 3. Conditional and Mixed Logit Parameter Estimates**

| 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) |
|---|---|---|---|
| Parameters fixed in both mixed and conditional logit | | | |
| *neither* | −0.5969 (0.2553) | 0.1012 (0.2193) | −1.3629 (0.3667)*** |
| *env_neither* | −0.5770 (0.1018)*** | −0.5876 (0.1009)*** | −0.6668 (0.2353)*** |
| *age_neither* | 0.0130 (0.0034)* | 0.0130 (0.0033)* | 0.0089 (0.0061) |
| *prot_neither* | 2.9498 (1.0480)*** | 2.9359 (1.0480)*** | 4.1276 (0.9856)*** |
| *coll_neither* | −0.6731 (0.0791)*** | −0.6815 (0.0779)*** | −1.0863 (0.1780)*** |
| *option_A* | 0.1761 (0.0764)** | 0.1054 (0.0486)** | 0.1096 (0.0432)** |
| *acres* | 0.0014 (0.0006)** | 0.0019 (0.0004)*** | 0.0024 (0.0005)*** |
| *nursery* | −0.0905 (0.1192) | −0.1172 (0.0766) | −0.0763 (0.0951) |
| *forest* | −0.1594 (0.1215) | −0.0559 (0.0776) | −0.0024 (0.0956) |
| *idle* | −0.0024 (0.1170) | −0.0130 (0.0760) | 0.1144 (0.0961) |
| *walking* | 0.4876 (0.1402)*** | 0.5666 (0.0922)*** | 0.9443 (0.1425)*** |
| *hunting* | 0.2052 (0.1444) | 0.2530 (0.0944)*** | 0.3591 (0.1379)*** |
| *dev_not_30* | −0.2700 (0.2443) | −0.3921 (0.2339)* | −0.6184 (0.2581)** |
| *dev_10_30* | −0.2087 (0.2502) | −0.2405 (0.2413) | −0.6072 (0.3132)* |
| *age_not 30* | 0.0066 (0.0042) | 0.0067 (0.0042) | 0.0109 (0.0046)** |
| *age_dev 10_30* | 0.0045 (0.0043) | 0.0043 (0.0043) | 0.0112 (0.0056)** |
| *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_cont* | 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 (0.0010)** | – | – |
| Parameters random and fixed in mixed logit in conditional logit | | | |
| *st_purch* | −0.2732 (0.1803) | −0.1972 (0.1185)* | −0.2987 (0.1771)* |
| *tr_purch* | −0.3779 (0.1807)** | −0.2009 (0.1187)* | −0.3165 (0.1712)* |
| *tr_cont* | −0.3092 (0.2048) | −0.1628 (0.1315) | −0.4423 (0.2072)*** |
| *zoning* | −0.5843 (0.2028)*** | −0.4099 (0.1314)*** | −0.6271 (0.2224)*** |
| *cost* (sign-reversal applied all models; lognormal in ML) | 0.0051 (0.0008)*** | 0.0038 (0.0005)*** | −4.4574 (0.3516)*** |
| Standard deviations of random parameters (mixed logit only) | | | |
| *sd_st_purch* | – | – | 0.5628 (0.3103)* |
| *sd_tr_purch* | – | – | 0.4527 (0.3508) |
| *sd_tr_cont* | – | – | 1.1883 (0.4207)*** |
| *sd_zoning* | – | – | 1.3275 (0.3358)*** |
| *sd_cost* | – | – | 7.1325 (0.7104)*** |

Note: Single (*), double (**), and triple (***) asterisks denote $p$ values of 0.10, 0.05, and 0.01, respectively.

**Table 4. Model Summary Statistics and Hypothesis Tests**

| | Model One Conditional Logit, Unrestricted | Model Two Conditional Logit, Restricted | Model Three Mixed Logit, Restricted |
|---|---|---|---|
| −2 Log likelihood $\chi^2$ | 552.098*** | 535.829*** | 1,725.191*** |
| Pseudo-$R^2$ | 0.08 | 0.08 | 0.24 |
| Chow test: Equal scale parameter (CT and DE) | $\chi^2 = 0.95, p = 0.33$ | – | – |
| Likelihood ratio test: Restricted vs. unrestricted model | – | $\chi^2 = 16.628, p = 0.36$ | – |
| Likelihood ratio test: Mixed vs. conditional logit model | – | – | $\chi^2 = 1{,}189.362, p < 0.01$ |
| Observations (N) | 3,309 | 3,309 | 3,309 |

Note: Single (*), double (**), and triple (***) asterisks denote $p$ values of 0.10, 0.05, and 0.01, respectively.

and DE data ($\chi^2 = 0.95$, $p = 0.33$), 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 ($\chi^2 = 8.13, p = 0.36$). The combination of these two tests provides no evidence of statistically significant preference heterogeneity or difference in scale between responses from the two states (table 4). Hence, subsequent discussion emphasizes results of the simpler pooled model.

Model three is a ML specification of the pooled model. As noted above, parameters on the four policy process attributes (*st_purch*, *tr_purch*, *tr_cont*, *zoning*) are specified as random and normally distributed, while the parameter on sign-reversed program cost (*cost*) is specified as random with a lognormal distribution. 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 ($\chi^2 = 1{,}189.36$, $p < 0.01$) for these parameters, with the estimated standard deviation on four of the five random parameters significant at $p < 0.10$ (three are significant at $p < 0.01$). Given the superior performance of the ML model relative to the CL model, subsequent discussion emphasizes ML results (model three).

Of 21 estimated parameters in the ML model (not including those characterizing the distribution of random parameters), 17 are statistically significant at $p < 0.10$ or better, with signs of significant parameters conforming to prior expectations (table 3). 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 (*walking*) is preferred to public access for hunting (*hunting*), supporting prior findings that 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 some negative consequences (e.g., McGonagle and Swallow 2005; Johnston et al. 2005).

Parameters on land type attributes, in contrast, are not statistically significant (*nursery*, *forest*, *idle*). This pattern is robust across a wide range of model specifications. These results suggest that while respondents distinguish between some attributes of preservation programs, the type of farm or forest preserved is not a significant determinant of preferences. While these results may be subject to the specific land types considered, they contradict findings of some prior stated preference research demonstrating statistically significant public preferences associated with agricultural productivity and/or type of farmland (e.g., Kline and Wichelns 1996; Ozdemir et al. 2004).

### *Policy Process Attributes and Preservation Preferences*

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 conservation easements or contracts (tables 1 and 2). Associated parameters capture the marginal influence of these attributes on utility, relative to that which would be realized under the use of state contracts. Model results indicate that attributes of the policy process can have a significant influence on the utility of land preservation, holding constant other attributes such as public access and household cost. The mean of the parameter distribution for *zoning* is statistically significant at $p < 0.01$; *tr_cont* is significant at $p < 0.04$; *tr_purch* and *st_purch* are statistically significant at $p < 0.07$ and $p < 0.10$, respectively. These results indicate that marginal utilities associated with these policy process attributes differ from that associated with state conservation easements (the omitted default), holding all else constant.

The model specification was designed so that the effects of both preservation *techniques* and *agents* could be estimated. For example, conservation easements were described in the survey as generating preservation that was "contractually and permanently" 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_cont*, $p < 0.04$) is lower than that associated with otherwise identical state easements (the omitted default). In contrast, the magnitudes of mean coefficient estimates associated with fee-simple purchase are similar for land trust and state agents (*st_purch*, *tr_purch*). This implies that respondents prefer state agencies to implement conservation easements but do not distinguish (in terms of utility) between fee simple purchase conducted by public and private agents.

Holding program cost and other attributes constant, the least preferred preservation technique is conservation zoning (*zoning*, $p < 0.01$)—a result consistent with focus group findings.[11] Not only did focus group respondents associate zoning with the potential for additional restrictions on land use community-wide, but the survey noted that "while 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 implemented at the local level in both Connecticut and Delaware, this technique was not allowed to vary according to implementing agency.

The statistical significance of the four policy process attributes is a finding which implies the potential risks of suppressing information regarding policy implementation from SP scenarios. Consequences could include omitted variables, bias in estimated parameters, and inappropriate policy guidance. Results also suggest a fair degree of subtlety in respondents' policy preferences—with, for example, preferences for some policy techniques depending on the implementing agency. This is another result not well reflected in the literature, yet of potential policy relevance. However, we also emphasize that relative preferences may remain unaffected by the choice of policy implementation method. For example, model results indicate that utility associated with state purchase of land is statistically equivalent to that associated with land trust purchase. Hence, the relative importance of process attributes may vary across policy contexts.

An additional relevant finding is the statistically significant standard deviations on random parameters associated with policy process attributes, implying significant preference heterogeneity. In all cases, estimated standard deviations indicate that a nontrivial segment of respondents have marginal utilities for policy process attributes that are of opposite sign from the reported mean of the parameter distribution. For example, simulation results based on the estimated mean and standard deviation of the coefficient on *st_purch* (1,000 draws, based on ML coefficients from table 3) indicate that approximately 30% of respondents have a positive marginal utility for this attribute (relative to the omitted default of state contracts), despite a negative and statistically significant mean of the distribution.[12] Similar results hold for *tr_purch* (26% with contrary signs for marginal utility), *tr_cont* (37%), and *zoning* (30%).

### *Willingness to Pay Implications*

We illustrate WTP implications of policy process attributes using the welfare simulation approach of Hu, Veeman, and Adamowicz (2005), following the general framework of Hensher and Greene (2003). The procedure begins with a *parameter simulation* following the parametric bootstrap of Krinsky and

[11] 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.

[12] Similar results may be calculated using large-sample statistical theory, given known properties of the normal distribution of random coefficients.

Robb (1986), with $R = 1{,}000$ draws taken from the mean parameter vector and associated covariance matrix. For each draw, the resulting parameters are used to characterize asymptotically normal empirical densities for fixed and random coefficients. For each of these $R$ draws, a *coefficient simulation* is then conducted for each random coefficient, with $S =$ 1,000 draws taken from simulated empirical densities.[13] Welfare measures are calculated for each draw, resulting in a combined empirical distribution of $R \times S$ observations from which summary statistics and hypothesis tests may be derived (Hu, Veeman, and Adamowicz 2005).

The resulting empirical WTP distributions accommodate both the sampling variance of parameter estimates and the estimated distribution of random parameters (Hu, Veeman, and Adamowicz 2005). Using this procedure, we simulate WTP differences associated with each possible pair of policy process attributes (i.e., the difference in marginal WTP associated with changes in the method of policy implementation, holding all else constant), with differences directly bootstrapped from model results as part of the simulation (cf., Poe, Giraud, and Loomis 2005, p. 356). While the lognormal distribution of the program cost coefficient in this case ensures a negative impact of *cost* on utility, and hence is the most commonly chosen distribution for SP payment vehicle parameters, it also frequently leads to unrealistic mean WTP estimates over simulated distributions (Hensher and Greene 2003). This is particularly true here, given the large estimated standard deviation on the *cost* coefficient. To avoid these problems, we follow Hu, Veeman, and Adamowicz (2005) and present welfare estimates as the mean over the parameter simulation of median WTP calculated over the coefficient simulation.[14]

Results are presented in table 5, along with two-tailed $p$ values for the null hypothesis that the estimated WTP difference is equal to zero. Because marginal welfare measures are calculated as the quotient of normally distributed parameters on policy process attributes and the lognormally distributed parameter on *cost*, the resulting distributions have unknown properties (Hu, Veeman, and Adamowicz 2005). Hence, $p$-values are estimated directly from simulated empirical distributions following the percentile method (Efron and Tibshirani 1993). This approach avoids the potential for misleading statistical inferences related to inappropriate normality assumptions or the use of nonoverlapping confidence intervals (Poe, Giraud, and Loomis 2005).

Point estimates of the WTP difference associated with variations in policy process attributes range from $-\$62.14$ to $\$30.34$. For purposes of discussion, however, we deemphasize these point estimate magnitudes, which vary depending on details of the simulation (e.g., whether one simulates mean or median WTP differences) and whether the parameter on *cost* is specified as fixed or random in the ML model. Instead, we emphasize hypothesis test results for the null hypothesis of zero WTP difference across policy process attributes, which are more robust.

As shown by table 5, four out of ten WTP differences associated with changes in policy process attributes are statistically significant at $p <$ 0.10 or better. Holding all other conservation attributes constant, these represent WTP differences associated with *st_purch* (fee-simple state purchase), *tr_purch* (fee-simple land trust purchase), *tr_cont* (land trust conservation easement), and *zoning* (conservation zoning), compared to the default of state conservation easements. This suggests that one of the more common preservation techniques (conservation easements) is also associated with the highest levels of individual WTP, holding all else constant. Moreover, when costs are considered state conservation easements may further dominate fee simple techniques, because easements are generally less expensive than full purchase. Narrowly missing statistical significance at traditional levels are differences between marginal WTP associated with *zoning* and that associated with *st_purch* ($p =$ 0.12) and *tr_purch* ($p = 0.13$). Other attribute pairs show no evidence of statistically significant marginal WTP differences.

## 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,

[13] Because the coefficient on *cost* is specified with a lognormal distribution, an exponential transformation is required subsequent to the coefficient simulation to obtain a simulated empirical density for the coefficient itself (Hensher and Greene 2003; Hu, Veeman, and Adamowicz 2005).

[14] As noted by Hu, Veeman, and Adamowicz (2005), there is no strong theoretical preference for either mean or median welfare measures. Here, however, unrealistic mean WTP estimates simulated for the ML model suggest that median measures are more policy relevant.

**Table 5. Welfare Differentials Associated with Policy Process Attributes**

| | Mixed Logit Welfare Simulation Results: Difference in Mean of Median Willingness to Pay Pay (WTP for Column Attributes [A] Minus WTP for Row Attributes [B])[a] | | | |
|---|---|---|---|---|
| | [A] | | | |
| Attribute | State Purchase (*st_purch*) | Trust Purchase (*tr_purch*) | Trust Contract (*tr_cont*) | Conservation Zoning (*zoning*) |
| [B] | | | | |
| State purchase (*st_purch*) | 0 | – | – | – |
| Trust purchase (*tr_purch*) | 0.06 ($p = 0.87$) | 0 | – | – |
| Trust contract (*tr_cont*) | 16.50 ($p = 0.52$) | 15.39 ($p = 0.59$) | 0 | – |
| Conservation zoning (*zoning*) | 30.34 ($p = 0.12$) | 28.88 ($p = 0.13$) | 7.82 ($p = 0.41$) | 0 |
| State contract (omitted default) | −30.13 ($p = 0.07$) | −30.84 ($p = 0.05$) | −44.45 ($p = 0.04$) | −62.14 ($p < 0.01$) |

[a] Reported results reflect the mean over the parameter simulation of median WTP difference over the coefficient simulation (cf., Hu, Veeman and Adamowicz 2005). Table entries are calculated as the difference between WTP for the attribute named in the column heading and WTP for the attribute named in the row heading. For example, the WTP for *st_purch* is $30.34 *greater* than the WTP for *zoning*, and $30.13 *smaller* than the WTP for state contracts. Numbers in parentheses are two-tailed *p*-values for the null hypothesis of zero difference in mean of median WTP, calculated from the empirical WTP distribution.

Woirhaye, and Menkhaus 1999; Rosenberger et al. 1996), emphasize the relationships between policy techniques and constituents' support for land preservation. Further supporting the importance of the policy process is evidence from the hedonic literature that property value impacts of open space depend on policy techniques used to prevent development (Irwin 2002; Ready and Abdalla 2005). Such evidence notwithstanding, the dominance of the purchase model has led researchers to suppress details of policy implementation in SP 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. Model results illustrate potential marginal utility and welfare implications associated with attributes of the policy process, after controlling for possibly confounding factors such as public access, cost, land type, and likelihood of preservation permanency.[15] Results indicate that preferences may extend both to the type of preservation techniques applied as well as implementing agents, and that statistically significant welfare differences may be associated with changes in policy implementation.

There are at least three potential explanations for these results, each of which implies some reconsideration of 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 process attributes as proxies for heretofore unsuspected outcome attributes—a variant of methodological misspecification (Mitchell and Carson 1989). If such is the case, despite our extensive and explicit efforts to eliminate such possibilities, it 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 policy outcomes.

Second, it is possible that respondents' choices reflected a genuine individual WTP for policy process attributes, yet one that is not appropriate for inclusion in 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*[16] or related concerns,

[15] The exception, of course, is zoning, which cannot be divorced from the possibility of future change.

[16] 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."

the associated WTP, while measurable, would be irrelevant for welfare analysis (Lazo, McClelland, and Schulze 1997; McConnell 1997). In such cases, despite the welfare irrelevance of WTP measures, it is nonetheless important to account for such factors to avoid statistical biases in discrete choice models. Moreover, given that WTP for policy process attributes would be irrelevant for welfare estimation, it would be critical to ensure that welfare estimates did not incorporate inappropriate WTP associated with assumed policy techniques.

A final possibility is that model results reflect a genuine and welfare-relevant WTP 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 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 land for private purposes. If such preference patterns hold, then process attributes would be welfare-relevant in their own right, and should either be incorporated, or at least controlled for, in applied welfare analysis.

These possibilities make clear that the benefit-cost analysis implications of model findings may depend on the reasons why policy process attributes (appear to) influence marginal utility and welfare. Some of these possibilities are most appropriately addressed by heeding the advice of Carson, Groves and Machina (2000) and others, who recommend clearly specifying the details of the policy process in survey scenarios. Others may involve careful survey design, including qualitative research to more fully explore respondents' potential assumptions regarding the impact of the policy process on expected utility. Still another possibility would follow suggestions of Bulte et al. (2005) that the causes of environmental outcomes may be welfare-relevant and should be incorporated in benefit assessments.

The ultimate welfare implications of preservation techniques will also depend on the entire package of combined attributes that characterize a preservation policy (Johnston et al. 2002b). Although the present experimental design allowed for orthogonality of outcome and process attributes, in actual policy situations preservation instruments are often correlated with policy outcomes (e.g., public access is more likely to be provided by fee simple purchase instruments). The present findings suggest that analysts may wish to consider the possibility that, for valid assessment of welfare implications, relevant attributes of policy packages may include the details of policy implementation. Moreover, correlation between outcomes and policy instruments implies that welfare-relevant tradeoffs may be involved. Here, for example, there is a tradeoff implied between the most highly-favored preservation instrument (state conservation easements, which typically do not provide public access) and the value that respondents place on access for walking, biking, or hunting.

Like many research efforts, the present analysis perhaps raises more questions than it answers. Results of the analysis cannot establish which of the above patterns apply here, nor do results indicate the extent to which similar results hold for other policy contexts. However, results clearly reveal significant preference patterns associated with land preservation policies and agents that implement those policies. 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. Finally, results imply the potential benefit of additional research into the implications of the policy process for welfare estimation. While our 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. At a minimum, these findings suggest that researchers should consider the possibility that policy process attributes may not be utility-neutral in some instances, and recognize the potential consequences of suppressing related preferences in welfare analysis.

*[Received May 2006; accepted January 2007.]*

## References

Adamowicz, W., P. Boxall, M. Williams, and J. Louviere. 1998. "Stated Preference Approaches for Measuring Passive Use Values: Choice Experiments and Contingent Valuation." *American Journal of Agricultural Economics* 80:64–75.

Bateman, I.J., M. Cole, P. Cooper, S. Georgiou, D. Hadley, and G.L. Poe. 2004. "On Visible Choice Sets and Scope Sensitivity." *Journal*

*of Environmental Economics and Management* 47:71–93.
Beasley, S.D., W.G. Workman, and N.A. Williams. 1986. "Estimating Amenity Values of Urban Fringe Farmland: A Contingent Valuation Approach." *Growth and Change* 17:70–78.
Bergstrom, J.C., and R.C. Ready. 2005. "What Have We Learned from 20 Years of Farmland Amenity Valuation Research?" In S. Schultz, ed. *Western Regional Research Publication* W-1133. *Benefits and Costs of Resource Policies Affecting Public and Private Land*. Eighteenth Interim Report. Fargo, ND: North Dakota State University, pp. 9–360.
Bosworth, R., T.R. Cameron, and J.R. DeShazo. 2006. "Is An Ounce of Prevention Worth a Pound of Cure?" Paper presented at W-1133 Annual Meetings, San Antonio, TX, 23–25 February.
Bowker, J. M., and D. D. Didychuk. 1994. "Estimation of the Nonmarket Benefits of Agricultural Land Retention in Eastern Canada." *Agricultural and Resource Economics Review* 23:218–25.
Brown, T., P. Champ, R. Bishop, and D. McCollum. 1996. "Which Response Format Reveals the Truth about Donations to a Public Good?" *Land Economics* 72:152–66.
Bulte, E., S. Gerking, J.A. List, and A. de Zeeuw. 2005. "The Effect of Varying the Causes of Environmental Problems on Stated WTP Values: Evidence from a Field Study." *Journal of Environmental Economics and Management* 49:330–42.
Carson, R.T., T. Groves, and M.J. Machina. 2000. "Incentive and Informational Properties of Preference Questions." Working Paper, Department of Economics, University of California, San Diego.
Dillman, D.A. 2000. *Mail and Internet Surveys: The Tailored Design Method*. New York: John Wiley & Sons.
Duke, J.M., and T.W. Ilvento. 2004. "A Conjoint Analysis of Public Preferences for Agricultural Land Preservation." *Agricultural and Resource Economics Review* 33:209–19.
Duke, J. M., and L. Lynch. 2006. "Four Classes of Farmland Retention Techniques: Comparative Evaluation and Property Rights Implications." *Land Economics* 82:162–73.
Efron, B., and R. Tibshirani. 1993. *An Introduction to the Bootstrap.* New York: Chapman and Hall.
Freeman, A.M. III. 2003. *The Measurement of Environmental and Resource Values: Theory and Methods*. Washington DC: Resources for the Future.
Greene, W.H. 2003. *Econometric Analysis*, 5th ed. Upper Saddle River, NJ: Prentice-Hall.
Halstead, J. 1984. "Measuring the Non-market Value of Massachusetts Agricultural Land: A Case Study." *Journal of the Northeastern Agricultural Economics Council* 13:12–19.
Hanemann, W.M. 1984. "Welfare Evaluations in Contingent Valuation Experiments with Discrete Responses." *American Journal of Agricultural Economics* 66:332–41.
Hensher, D.A., and W.H. Greene. 2003. "The Mixed Logit Model: The State of Practice." *Transportation* 30:133–76.
Hu, W., M.M. Veeman, and W.L. Adamowicz. 2005. "Labeling Genetically Modified Food: Heterogeneous Consumer Preferences and the Value of Information." *Canadian Journal of Agricultural Economics* 53:83–102.
Hu, W., W.L. Adamowicz, and M.M. Veeman. 2006. "Labeling Context and Reference Point Effects in Models of Food Attribute Demand." *American Journal of Agricultural Economics* 88:1034–49.
Inman, K., and D. McLeod. 2002. "Property Rights and Public Interests: A Wyoming Agricultural Lands Study." *Growth and Change* 33:91–114.
Irwin, E.G. 2002. "The Effects of Open Space on Residential Property Values." *Land Economics* 78:465–81.
Johnston, R.J., G. Magnusson, M. Mazzotta, and J.J. Opaluch. 2002. "Combining Economic and Ecological Indicators to Prioritize Salt Marsh Restoration Actions." *American Journal of Agricultural Economics* 84:1362–70.
Johnston, R.J., J.J. Opaluch, M.J. Mazzotta, and G. Magnusson. 2005. "Who Are Resource Nonusers and What Can They Tell Us about Nonuse Values? Decomposing User and Nonuser Willingness to Pay for Coastal Wetland Restoration." *Water Resources Research* 41:W07017. DOI: 10.1029/2004WR003766.
Johnston, R.J., S.K. Swallow, C.W. Allen, and L.A. Smith. 2002b. "Designing Multidimensional Environmental Programs: Assessing Tradeoffs and Substitution in Watershed Management Plans." *Water Resources Research* 38:IV1–IV13.
Johnston, R.J., S.K. Swallow, D.M. Bauer, and C.M. Anderson. 2003. "Preferences for Residential Development Attributes and Support for the Policy Process: Implications for Management and Conservation of Rural Landscapes." *Agricultural and Resource Economics Review* 32:65–82.
Johnston, R.J., T.F. Weaver, L.A. Smith, and S.K. Swallow. 1995. "Contingent Valuation Focus Groups: Insights from Ethnographic Interview Techniques." *Agricultural and Resource Economics Review* 24:56–69.

Kaplowitz, M.D., F. Lupi, and J.P. Hoehn. 2004. “Multiple Methods for Developing and Evaluating a Stated-Choice Questionnaire to Value Wetlands.” In S. Presser, J.M. Rothget, M.P. Coupter, J.T. Lesser, E. Martin, J. Martin, and E. Singer, eds. *Methods for Testing and Evaluating Survey Questionnaires*. New York: John Wiley & Sons, pp. 503–24.

Kline, J., and D. Wichelns. 1996. “Public Preferences Regarding the Goals of Farmland Preservation Programs.” *Land Economics* 72:538–49.

Krinsky, I., and A.L. Robb. 1986. “On Approximating the Statistical Properties of Elasticities.” *Review of Economics and Statistics* 68:715–19.

Kuhfeld, W.F., and R.D. Tobias. 2005. “Large Factorial Designs for Product Engineering and Marketing Research Applications.” *Technometrics* 47:132–41.

Layton, D.F. 2000. “Random Coefficient Models for Stated Preference Surveys.” *Journal of Environmental Economics and Management* 40:21–36.

Lazo, J.K., G.H. McClelland, and W.D. Schulze. 1997. “Economic Theory and Psychology of Non-Use Values.” *Land Economics* 73:358–71.

Loomis, J., K. Traynor, and T. Brown. 1999. “Trichotomous Choice: A Possible Solution to Dual Response Objectives in Dichotomous Choice Contingent Valuation Questions.” *Journal of Agricultural and Resource Economics* 24:572–83.

Lusk, J.L., and F.B. Norwood. 2005. “Effect of Experimental Design on Choice-Based Conjoint Valuation Estimates.” *American Journal of Agricultural Economics* 87:771–85.

Maddala, G.S. 1983. *Limited Dependent and Qualitative Variables in Econometrics*. Cambridge, UK: Cambridge University Press.

Mansfield, C.A., and V.K. Smith. 2002. “Tradeoff at the Trough: TMDL’s and the Evolving Status of Water Quality Policy.” In J. List and A. de Zeeuw, eds. *Recent Advances in Environmental Economics*. Cheltenhen, UK: Edward Elgar, pp. 257–85.

McConnell, K.E. 1990. “Models for Referendum Data: The Structure of Discrete Choice Models for Contingent Valuation.” *Journal of Environmental Economics and Management* 19:19–34.

——. 1997. “Does Altruism Undermine Existence Value?” *Journal of Environmental Economics and Management* 32:22–37.

McFadden, D., and K. Train. 2000. “Mixed MNL Models for Discrete Response.” *Journal of Applied Econometrics* 15:447–70.

McGonagle, M.P., and S.K. Swallow. 2005. “Open Space and Public Access: A Contingent Choice Application to Coastal Preservation.” *Land Economics* 81:477–95.

McLeod, D., J. Woirhaye, and D. Menkhaus. 1999. “Factors Influencing Support for Rural Land Use Control: A Case Study.” *Agricultural and Resource Economics Review* 28:44–56.

McLeod, D., J. Woirhaye, C. Kruse, and D. Menkhaus. 1998. “Private Open Space and Public Concerns.” *Review of Agricultural Economics* 20:644–53.

Mitchell, R.C., and R.T. Carson. 1989. *Using Surveys to Value Public Goods: The Contingent Valuation Method*. Washington DC: Resources for the Future.

Opaluch, J.J., S.K. Swallow, T. Weaver, C. Wessells, and D. Wichelns. 1993. “Evaluating Impacts from Noxious Facilities: Including Public Preferences in Current Siting Mechanisms.” *Journal of Environmental Economics and Management* 24:41–59.

Ozdemir, S., K.J. Boyle, M. Ahearn, A. Alberini, J. Bergstrom, L. Libby, and M.P. Welsh. 2004. “Preliminary Report: Farmland Conservation Easement Study for the United States, Georgia, Ohio and Maine Samples.” Working Paper, Department of Resource Economics and Policy, University of Maine, Orono.

Poe, G.L., K.L. Giraud, and J.B. Loomis. 2005. “Computational Methods for Measuring the Difference in Empirical Distributions.” *American Journal of Agricultural Economics* 87:353–65.

Ready, R.C., and C. Abdalla. 2005. “The Amenity and Disamenity Impacts of Agriculture: Estimates from a Hedonic Pricing Model.” *American Journal of Agricultural Economics* 87:314–26.

Ready, R.C., M.C. Berger, and G.C. Blomquist. 1997. “Measuring Amenity Benefits from Farmland: Hedonic Pricing vs. Contingent Valuation.” *Growth and Change* 28:438–58.

Rosenberger, R.S., R.G. Walsh, J.R. McKean, and C.J. Mucklow. 1996. *Benefits of Ranch Open Space to Local Residents. Extension Bulletin XCM-201*. Fort Collins: Cooperative Extension Agricultural Experiment Station, Colorado State University.

Swait, J., and J. Louviere. 1993. “The Role of the Scale Parameter in the Estimation and Comparison of Multinomial Logit Models.” *Journal of Marketing Research* 30:305–14.

Swallow, S.K. 2002. “Critical Lands Conservation with Development: Using Contingent Choice to Establish Impact Fees for Open Space.” Working Paper, Department of Environmental and Natural Resource Economics, University of Rhode Island, Kingston.

Yatchew, A., and Z. Griliches. 1984. “Specification Error in Probit Models.” *Review of Economics and Statistics* 66:134–39.

## Appendix

The following definitions were provided by the survey instrument (Figure A1), to ensure appropriate understanding of policy mechanisms by potential respondents.

To ensure that respondents could consistently match definitions to policy process attributes listed in choice experiment questions, these attributes were presented in capital letters in both the definitions page and in choice questions. Although there was initial concern among researchers that this might draw excessive attention to these attributes, pretests showed no evidence of such patterns. Figure A2 illustrates an example choice experiment question.

***CONSERVATION ZONING***: Zoning with special protections for farm and forest. Parts of the Smyrna area are restricted to only farm, forest, and open space uses. Only in rare cases is development allowed. While CONSERVATION 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.

***OUTRIGHT PURCHASE****:* Purchase and preservation of farm and forest by government or nonprofit groups. Land is purchased in voluntary transactions from interested landowners at a negotiated price, and is preserved as farm or forest. Preservation is guaranteed forever.

***PRESERVATION CONTRACTS***: Interested landowners are paid a fee in return for placing a legal contract on their land that prevents all future development. Ownership and use of the land does not change, but development is contractually and permanently prohibited. Governmental agencies or private nonprofit groups typically administer these programs.

***BLOCK GRANTS:*** State tax dollars are given to nonprofit organizations for the express purpose of preserving farm and forest. Nonprofit organizations, such as land trusts, may use block grants to fund either OUTRIGHT PURCHASE or PRESERVATION CONTRACTS on parcels that they select for preservation.

**Figure A1. Definitions of policy process attributes (from Smyrna, Delaware survey version)**

3. Assume that the following two preservation options were proposed *for the Smyrna area*. Assume that these options are in addition to any existing preservation programs. How would you vote? Choose A, B or neither.

| | **Option A** | **Option B** |
|---|---|---|
| **Type of Land Preserved** | Active Farmland (food crop) | Farmland (idle/not currently used) |
| **Acres Preserved** **(1 acre is the same size as 3/4 of a football field)** | 200 Acres (single lot) | 20 Acres (single lot) |
| **Method Used** | OUTRIGHT PURCHASE by Land Trusts, using BLOCK GRANTS | OUTRIGHT PURCHASE by the State of Delaware |
| **Public Access** | No access allowed | Access for Walking/Biking |
| **What Happens if Not Preserved?** | Development NOT likely within the next 30 years | Development likely in 10-30 years if not preserved |
| **Cost to Your Household** **(for purchase, enforcement & other costs)** | $50 per year (Increased State/Town Taxes & Fees) | $50 per year (Increased State/Town Taxes & Fees) |

**Please check <u>one</u>:**

❑ 1 I would vote for **Option A**, and pay $50 per year.

❑ 2 I would vote for **Option B**, and pay $50 per year.

❑ 3 I support these programs in general, but my household **would/could not pay for either Option A or B**.

❑ 4 **I would not vote for either program.**

**Figure A2. Example choice experiment question (from Smyrna, Delaware survey version)**