How Can Behavioral Economics Inform Nonmarket Valuation? An Example from the Preference Reversal Literature

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ABSTRACT. *Psychological insights have made inroads within most areas of study in economics. One area where less advance has occurred is environmental and resource economics. In this study, we examine preference reversals over evaluation modes, in which economic values critically depend on whether a good is valued jointly with others, or in isolation. The question arises because two methods for eliciting stated preferences differ in that one presents objects together and another presents them in isolation. Our empirical evidence demonstrates the import of behavioral economics and sheds new light on the possible insensitivity of valuations to the scope of the good.* (JEL Q51)

I. INTRODUCTION

Over the past few decades economists have increasingly turned to psychologically based explanations of individual behavior. Whether used to explain involuntary unemployment, excessive risk taking, gender differences, firm entry and exit patterns, discrimination patterns, or labor supply, behavioral economists have lent important insights to the underlying data patterns observed. Interestingly, even though environmental economists provided early ammunition to the pioneers of behavioral economics, we have been slower to adopt psychologically based explanations throughout our field.[1] To provide a step in that direction, this study explores how the preference reversal literature can be used by environmental and resource economists.

Beginning with the work of Slovic and Lichtenstein (1968), evidence has accumulated that theoretically equivalent measures of preference elicitation can lead to systematically different preference orderings. The early results from psychology laboratories received added attention from economists after Grether and Plott (1979) found that these inconsistencies were robust to salient incentives and other controls consistent with best practices in experimental economics. Their study confirmed that preference orderings over lotteries could be sensitive to the elicitation's *response mode*, either choices or prices.

More recently, another type of choice anomaly has been discovered: preference reversals that occur over joint and isolated *evaluation modes*. In the isolated evaluation mode, a single good is valued on its own. In the joint evaluation mode, two or more goods are compared. Reversals of preference across evaluation modes have been observed both for hypothetical choices between private goods and in market environments (Bazerman et al. 1999; Hsee, Loewenstein, and Bazerman 1999; List 2002).

The apparent robustness of evaluation mode reversals raises an important practical question for environmental valuation. This is because two prominent methods for eliciting stated preferences differ precisely in whether goods are evaluated jointly or in isolation. Contingent valuation exercises typically ask for the valuation of an isolated program or

[1] A puzzle arose nearly four decades ago when researchers discovered that the willingness-to-pay measure of value differed starkly from the willingness-to-accept measure (see, e.g., Hammack and Brown 1974).

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good.[2] Alternatively, choice-based methods ask consumers to value a *pair* or *set* of programs that are presented simultaneously when constructing marginal values of the characteristics (Adamowicz 2000; Adamowicz et al. 1998; Louviere, Hensher, and Swait 2000).

Although both methodologies are widely used, the small lot of studies that contain estimates using both approaches suggest that valuations can differ substantially across evaluation modes (Boxall et al. 1996; Frederick and Fischoff 1998; Irwin et al. 1993; Magat, Viscusi, and Huber 1988; Takatsuka, Kahn, and Stewart 2002). Further, the direction of the difference is not uniform across studies. For example, Irwin et al. (1993) find that public goods are more likely to be chosen when they are directly compared with a private good that is presented at the same time, while Boxall et al. (1996) report contingent valuation estimates that greatly exceed those from their choice experiments, a result they attribute to subjects ignoring the value of substitute goods in the isolated evaluation mode.

Our experimental treatments test for evaluation mode effects using both public and private goods. The private good treatment is an extension of List's (2002) sportscard market study. To provide a test of joint and isolated valuations over public goods, we examine willingness to pay for farmland preservation and water quality improvements, with the questions structured to parallel the market treatments. The public goods treatments are designed to identify the impact of evaluation mode on the ability of respondents to interpret information, shedding light on one source of scope insensitivity that should be considered in the design of stated preference surveys.[3] Thus, in contrast to earlier studies that argue that respondents are unable to formulate economic values for the goods or are expressing general attitudes (see, e.g., Kahneman and Knetsch 1992; Desvouges et al. 1993), we focus on identifying the cognitive underpinnings of scope effects, using both market and nonmarket valuations for robustness. In this way, our work complements that of Heberlein et al. (2005), who argue that "we need to better understand the *conditions* that produce scope failure."

II. BACKGROUND AND RELATED LITERATURE

Bazerman, Loewenstein, and White (1992) were the first to demonstrate the existence of preference reversals over joint and isolated evaluation modes. They presented subjects with a hypothetical dispute between neighbors and asked them to evaluate alternative resolutions. In the isolated evaluation mode, equitable settlements, in terms of monetary payoffs, were preferred. In the joint evaluation mode, the preference for equity was overturned in favor of settlements that maximized social welfare. Additional studies have elicited the preference reversal across examples that include hiring practices, and the provision of public versus private goods (see, e.g., Hsee 1996, 1998). We present an example from this literature in some detail to make clear the character of the reversal.

Hsee (1998) examined the valuation of goods where one of the goods, good L (where L represents "Less"), is a proper subset of the other, good M ("More"). Hsee compared choices over two sets of dinnerware with the following characteristics: Set M: 40 pieces, 31 in good condition, 9 broken; Set L: 24 pieces, all in good condition. The two sets shared the same 24 pieces (8 dinner plates, 8 salad plates, 8 dessert plates) in good condition. In addition, set M contained cups and saucers of which 7 were intact and 9 were broken. Set L was priced higher in the isolated evaluation mode, between subjects, while set M was preferred when the two were evaluated jointly, within subjects. The reversal has been characterized as a "more is less" preference reversal by Hsee (1998).

[2] Carson, Flores, and Mitchell (1999) emphasize the isolated evaluation aspect of contingent valuation, noting that "contingent valuation surveys are centered around the choice between having a good and not having it. This format serves to focus the respondent's attention on the distinguishing characteristics."

[3] Our interest in evaluation mode effects is not meant to imply that insensitivity to scope is inevitable in contingent valuation studies. Carson, Flores, and Meade (2001) review the evidence and find that the majority of published studies pass scope tests. Our aim is to shed light on the mechanism underlying scope failure when it does occur.

List's (2002) study, which is extended here, incorporates a problem similar in character to Hsee's dinnerware example, but in a naturally occurring marketplace for sports memorabilia. Importantly, subjects endogenously select into the market (and select their roles in the market) and, rather than giving hypothetical responses, voluntarily used their own funds to bid in an incentive-compatible auction. List finds that the "more is less" reversal is alive and well in the market setting, although attenuated among a group of super-experienced subjects (sportscard market dealers).

The underlying causes of the evaluation mode reversals have been discussed primarily by psychologists. Bazerman et al. (1999) argue that differences in valuation across modes arise because information about the attributes of a good has different salience—or evaluability—in the different modes. In the "chipped plate" example, discussed above, the *evaluability hypothesis* suggests that the number of plates in the package received increased salience in the joint evaluation mode because quantities are easily compared in that setting. In the isolated evaluation mode, quantity has reduced salience, and the chips in the plates take on importance as valuation cues. Hsee, Loewenstein, and Bazerman (1999) also note that the value function for attributes with little "evaluability information" is relatively unresponsive to changes in the attribute level, consistent with findings regarding insensitivity to scope. The combination of changes in salient cues and scope insensitivity can, in extreme cases, lead to the observed evaluation mode preference reversals.

In his Nobel Prize lecture, Daniel Kahneman discusses evaluation mode reversals in the context of a research program investigating the "architecture of cognition" (Kahneman 2003). The cognitive model posits two systems: System 1 is quick, associative, and intuitive, while System 2 is slower and based on rules and reasoning (Kahneman and Frederick 2002; Chaikin and Trope 1999). The use of automated choice heuristics by System 1, such as representativeness or affect, has been shown to produce biases that may be mitigated by System 2's supervision of intuitive judgments.[4]

The ability of System 2 to carry out its role may be compromised in isolated evaluation, however, since relevant cues, such as the quantity changes in Hsee's (1998) example, are absent. Kahneman (2003) argues that these changes are generally more accessible than absolute values, and their absence makes the use of an automated heuristic more likely. In the case of the cracked dinnerware it seems plausible that in the isolated evaluation mode a System 1 response to the poor-quality items affected the willingness to pay for the bundle. In joint evaluation, the information was available to promote System 2 responses. Differences in behavior across experience levels, reported by List (2002), can also be understood in the context of dual-process theory, which suggests that complex cognitive operations can migrate from System 2 to System 1 with repeated exposure to similar tasks (Kahneman and Frederick 2002).

While the literature has focused on psychological explanations, it is worthwhile to consider whether an economic model of quality signaling can rationalize the preference reversal results as well as, or along with, the cognitive theories. List's (2002) study provides some evidence on this issue since the goods were explicitly graded for quality by an independent third party with an established reputation in the marketplace. If quality signaling was sufficient to explain the pattern of results, we would expect the bundle of goods, in this case 13 sportscards, to be priced similarly across evaluation modes, since identical information on quality is available in both settings. The hypothesis of equal values, however, is rejected in favor of the one-sided alternative that the prices are greater in the

[4] Previous research has distinguished automated from deliberate choice heuristics. The automated heuristic with regard to judgments is operative "when the individual assesses a specified *target attribute* of a judgment by substituting another property of that object—the *heuristic attribute*—which comes more readily to mind" (Kahneman and Frederick 2002; see also Frederick 2002). Kahneman and Frederick (2002) review the representativeness heuristic, and Slovic et al. (2007) the affect heuristic.

joint evaluation mode.[5] This finding suggests that the cues regarding the low-quality cards receive additional emphasis under isolated evaluation. This represents the starting point for our private good market treatments.

III. EXPERIMENTAL TREATMENTS AND HYPOTHESES

The experiments we conduct include both an extension of List's (2002) study of private goods in the sportscard market and contingent valuation treatments over public goods. The sportscard market experiment alters List's original study by changing the information that the participants receive about the cards in a simple way—removing the cards from their sealed, graded cardholders, effectively removing the signal of their quality. We hypothesize that the removal of the grading information will accentuate valuation differences across the valuation modes, although the dual process theories suggest that sportscard dealers may be less affected by the informational change than nondealers. The use of ungraded sportscards in the new experiments also provides a conceptual bridge to the public goods treatments, since the quality of the public goods we present is more difficult to gauge than that of the graded sportscards.

Market Treatment

The market study was conducted on the floor of a sportscard show in Orlando, Florida, and closely followed List (2002). Each participant's experience typically followed three steps: (1) inspection of the goods, (2) learning the auction rules and placing a bid, and (3) conclusion of the transaction. Subjects approached the table voluntarily and, if they agreed to participate, were randomly allocated into one of the four treatments.

In treatment LI (LI denotes "Less, Isolated"), ten 1982 *Topps* baseball cards were auctioned off. The 10-card bundle had a book value of approximately $15. These cards were the same ones that List (2002) used, except in this case the grade of the cards was removed so the professional grader's opinion was not observable to the participants.[6] In treatment MI ("More, Isolated"), a bundle of 13 cards was auctioned: the *identical* 10 *Topps* baseball cards *and* an additional 3 different 1982 baseball cards that were previously professionally graded as in "poor" condition—the worst grade possible. While the three additional cards are of much lower quality than the original 10 cards, they do have economic value: in aggregate, the 13-card bundle has a book value of approximately $18. As in the LI treatment, the grades were not observable to the market participants.

In the third treatment, treatment JP (Joint Price), the same two bundles were auctioned off side-by-side. Accordingly, each subject submits two bids, one for each commodity bundle. To provide comparable budget sets across the three treatments, we informed subjects in treatment JP that if they were winners in both auctions, a random coin toss would determine which auction was binding. Finally, to provide an explicit link to the extant preference reversal literature on evaluation scales, and provide insights into behavior over choices, a fourth treatment, treatment JC (Joint Choice), was conducted in which market participants simply paid $3 and chose their most preferred bundle (rather than bidding in an auction). Table 1 summarizes the 2×3 experimental design. Akin to List (2002), we al-

TABLE 1
Experimental Design: Sportscard Market

	Auction Bids		Choice
Subject	Isolated Evaluation	Joint Evaluation	Joint Evaluation
Nondealers	LI and MI	JP	JC
Dealers	LI and MI	JP	JC

Note: List (2002) conducted a parallel design with the cards graded for quality. LI = Less, Isolated: 10-card bundles valued in isolation; MI = More, Isolated: 13-card bundles valued in isolation; JP = More and Less: 10- and 13-card bundles priced jointly; JC = More and Less: Choice between 10- and 13-card bundles.

[5] A summary of List's (2002) results is in Table 4 of this paper.

[6] To remove the grades we had to crack the cards out of their protective containers. Great care was taken not to damage any of the cards.

low both experts (dealers) and nonexperts to participate in the experiment.

To gather individual values, we use an incentive-compatible mechanism: the random *n*th price auction. As described by Shogren et al. (2001), the random *n*th-price auction can be characterized by four simple steps: (1) each bidder submits a bid; (2) all bids are rank-ordered from lowest to highest; (3) the monitor selects a random number (n) uniformly distributed between 2 and Z, where Z is the number of bidders; and (4) the monitor sells one unit of the good to each of the $(n-1)$ highest bidders at the *n*th price. Akin to Vickrey's (1961) second-price auction, the random *n*th-price auction is theoretically incentive compatible. Determining the number of goods available through the random device is useful to us, since it means that every participant is potentially in the market. Thus bidders have an incentive for truthful revelation even if they believe they are not near the upper tail of the value distribution.[7] Since we are not testing the incentive compatibility of the institution, and want to avoid excess noise, we inform the subjects that it is in their best interest to bid their true value in the auctions. We reinforce this notion via several examples that illustrate the optimal strategy of truth-telling.

After learning the auction rules, the subjects placed their bid(s) to complete Step 2. Finally, in Step 3 the experimenter concluded the experiment by informing the subjects that they should return at 6:00 p.m. on Sunday to find out the results of the auction. Subjects were informed that if they could not return for the specified transaction time, they would be contacted and would receive their cards in the mail (postage paid by the experimenter) within three days of receipt of payment.

Public Goods Treatments

In the public goods treatments, respondents valued either wetlands restoration (W) or farmland preservation (F). The experimental design parallels the market study in that sets of goods are valued either jointly (J) or in isolation (I). Further, the sets of goods can be characterized as L or M with $L \subset M$. As in the sportscard study, the additional goods in the M bundle are of lower quality than those in L. In the farmland preservation treatment the L question asks for a contribution to the permanent preservation of 500 acres at a specified price. The good in the M question includes the 500 acres and an additional *temporary* preservation of 50 acres. The wetlands example similarly augments a full cleanup with a partial cleanup of an additional area of polluted wetlands. As in the sportscard treatment a "more is less" interpretation of the results depends on the fact that the lower-quality goods do in fact have positive economic value.[8]

Treatments were conducted at contribution levels of \$50 and \$100 for the wetlands study and at the \$50 level for the farmland study and are denoted as W50, W100, and F50.[9] In the isolated evaluation mode subjects had the hypothetical choice of contributing the indicated amount or refusing. In the joint evaluation mode subjects could choose to contribute to either the L or the M goods, or to not contribute (N).

Subjects for the W50 and W100 treatments were recruited from visitors to a booth displayed by the Department of Agricultural and Resource Economics at the University of Maryland on Maryland Day in April 2002. Participants were paid \$1 for their responses

[7] Shogren et al. (2001) provide evidence on the effectiveness of the random *n*th price auction. They note also that simply making n large would diminish incentives for truthful revelation for high-value bidders.

[8] If the additional low-quality good was considered a bad, we would expect the L good to be preferred in joint evaluations. This is not the case in either the public or private good treatments. The L good is always presented first in the joint evaluation questions for the public goods. While variation in the order of the stimulus is preferred, any unmeasured order effects in the protocol are likely to work against our observing the $MJ > LJ$ relationship associated with an evaluation mode effect.

[9] An open-ended willingness-to-pay question was also developed for farmland preservation, however, the structure of the question differs from the others in this study and in the literature, and the results are omitted. The questions in the remaining public goods treatments are structured to parallel existing investigations of evaluation mode reversals. We believe that the results of this inquiry suggest the need for additional research with more detailed stated-preference protocols.

TABLE 2
Experimental Design: Public Goods

Good and Price	Isolated Evaluation	Joint Evaluation
Watershed $50 (W50)	LI and MI	L, M, and N
Watershed $100 (W100)	LI and MI	L, M, and N
Farmland preservation $50 (F50)	LI and MI	L, M, and N

Note: Questions are dichotomous (yes/no) and ask about willingness to pay for the public good at the price indicated in Column 1. Separate: LI = Less Separate; MI = More Separate. Joint: L and M bundles in joint evaluation mode with N = "no contribution" option also available.

and completed the protocol in two to three minutes. Sixty-five percent of the Maryland Day subjects were students at the university. In the F50 treatment, subjects were University of Maryland undergraduates who responded to the questionnaire before participating in an unrelated set of experiments. Table 2 summarizes the public goods experimental design.

Hypotheses

We examine the impact of evaluation mode on bids in the market treatments and on contribution rates in the public good treatments. To clarify the hypotheses and results we make use of the following definitions:

Definition 1. A strong evaluation mode effect is observed when, in aggregate, preferences over the bundles are: LI (Less, Isolated) $\succ$ MI (More, Isolated) and MJ (More, Joint) $\succ$ LJ (Less, Joint).

Definition 2. A weak evaluation mode effect is observed when, in aggregate, preferences over the bundles are: LI $\sim$ MI and MJ $\succ$ LJ.

A strong evaluation mode effect requires an inverse relationship between the scope of the good and the valuation in the isolated evaluation mode—the "more is less" reversal. A weak evaluation mode effect requires insensitivity to scope in the isolated evaluation mode. The two definitions are identical with respect to the joint evaluation mode where MJ $\succ$ LJ.

In addition to testing for the existence of evaluation mode effects, we examine subsidiary hypotheses on (1) the impact of grading on the extent of evaluation mode effects in the market treatments and (2) the impact of evaluation mode on aggregate contributions to public goods. The nature of the test for (1) requires examining bids from the joint and isolated modes simultaneously. To control for the two bids per person in the joint mode we estimate an error components model that is described in detail below. Parameter estimates from this model also provide evidence on the tests of the primary hypotheses on the existence of strong and weak evaluation mode effects.

The specific hypotheses are presented in Table 3. The table also presents the linear combinations of coefficients from the regression model that is used to test the hypotheses in the market setting. The model contains indicator variables for good type (*More*), evaluation mode (*Joint*) and their interaction, along with dealer status (*Dealer*) and the information condition (*Grade*), and all interactions. Each variable is coded one when the condition is consistent with the variable name and zero otherwise, and so the baseline (constant only) estimate represents the bids on ungraded cards of nondealers in the Less and Isolated condition, a structure we exploit in constructing the tests of hypotheses outlined in Table 3.[10]

[10] The linear combinations of coefficients for the sports-card market hypothesis tests in Table 3 are derived as follows:

Market evaluation mode effect hypotheses for nondealers:

$MS < LS \rightarrow Constant + More < Constant \rightarrow More < 0.$
$MJ > LJ \rightarrow Constant + More + Joint + More \times Joint > Constant + Joint \rightarrow More + More \times Joint > 0.$

Market evaluation mode effect hypotheses for dealers:

$MS < LS \rightarrow Constant + Dealer + More + Dealer \times More < Constant + Dealer \rightarrow More + Dealer \times More < 0.$
$MJ > LJ \rightarrow Constant + Dealer + More + Joint + Dealer \times More + More \times Joint + Dealer \times Joint + Dealer \times More \times Joint > Constant + Dealer + Joint + Dealer \times Joint \rightarrow More + Dealer \times More + More \times Joint + Dealer \times More \times Joint > 0.$

Market information hypothesis (nondealers):

$(LS - MS)_U > (LS - MS)_G \rightarrow Constant - (Constant + More) > Constant + Grade - (Constant + Grade + More + Grade \times More) \rightarrow Grade \times More > 0.$

TABLE 3
Hypothesis Tests

Hypothesis	Alternative	Coefficients of Interest
Market: evaluation mode	MI $\prec$ LI & MJ $\succ$ LJ	Nondealers: *More* <0 & *More* + *More* $\times$ *Joint* >0 Dealers: *More* + *More* $\times$ *Deal* <0 & *More* + *More* $\times$ *Deal* + *More* $\times$ *Joint* + *More* $\times$ *Deal* $\times$ *Joint* >0
Market: information	$(\text{LI}-\text{MI})_{\text{U}} > (\text{LI}-\text{MI})_{\text{G}}$	Nondealers: *Graded* $\times$ *More* >0
Public: evaluation mode	MS $\prec$ LS & MJ $\succ$ LJ	N/A
Public: contribution	MS + LS $\neq$ MJ + LJ	N/A

Note: The market evaluation mode hypothesis is tested on both the dealer and nondealer subject pools. The market information hypothesis is tested on nondealers across the ungraded (U) and graded (G) sportscards. Public evaluation mode effect and public contribution hypotheses are tested by treatment and pooled over treatments. Mean values of bids are used for the market hypotheses and proportion contributing for the public goods hypotheses. Coefficients of interest are associated with tobit models for the sportcards, which predict bids using indicators and their interactions. N/A, not applicable.

The subsidiary hypothesis (2) for the public good treatments examines whether contribution rates differ between the evaluation modes by comparing the proportion of respondents who offer to contribute in the joint versus isolated modes.

IV. EXPERIMENTAL RESULTS

Market Treatment

Table 4 summarizes the sportscard market data. For each treatment two rows of data are included. The first row contains the results from the new field experiments conducted with the ungraded cards. In the row below, in italics, are the results from List's 2002 study in which the identical cards were graded. We present three results from the sportscard market treatments, the first two from the new ungraded card data, and the third comparing the bids for the graded and ungraded sportscards.

Result 1. Inexperienced agents exhibit a strong evaluation mode effect consistent with a preference reversal. For this group average bids are significantly higher for the 10-card L bundle than for the 13-card M bundle in the isolated evaluation mode. The relationship is reversed under joint evaluation.

Result 2. The experienced agent subject pool is characterized by a weak evaluation mode effect. Bids on average are higher for the 10-card L bundle in the isolated evaluation mode and for the 13-card M bundle under joint evaluation. However, the differences are not significant at conventional levels. Evidence of a weak effect of evaluation mode exists, with M preferred to L in the joint evaluation mode.

Result 3. Among inexperienced agents the strong evaluation mode effect is accentuated when the information on quality is removed.

Statistical inference to support these three results makes use of nonparametric and parametric tests. The parametric tests include *t*-tests for independent and matched samples, and a tobit regression model in which the bid, censored at zero, is regressed on treatment indicator variables and their interactions. The model uses a random effects error structure and is given by $y_{it} = \mathbf{x}_{it}\boldsymbol{\beta} + u_{it} + e_i$ if $y_{it} > 0$ and zero otherwise, with y_{it} representing bidder i's tth bid with $t = 1$ ($t = 1, 2$) in the isolated (joint) evaluation mode, and $\mathbf{x}$ the vector of indicator variables and their interactions. The distribution of the error components is $u_{it}|\, x_{it}, u_{it} \sim N(0,\sigma_u^2)$, $e_i|\, x_{it} \sim N(0,\sigma_e^2)$, and the vector of parameters, $\boldsymbol{\beta}$, is estimated with maximum likelihood techniques (Wooldridge 2002). Results presented in Table 5 include the magnitude and statistical significance of individual coefficients as well as combinations of coefficients relevant for identifying the existence of evaluation mode effects (Results 1 and 2), and the impact of grading on their extent (Result 3).

Descriptive statistics supporting Result 1 are presented in the upper part of Table 4. In the isolated evaluation mode the average bid

TABLE 4
Experimental Results: Sportscard Market

Treatment	Bundle: 10 Cards	Bundle: 13 Cards
Nondealers	*Auction Bids*	
LI ($n=33$)	\$4.05 (0.45)	—
LI ($n=35$)	*\$4.86* (0.65)	—
MI ($n=30$)	—	\$1.82 (0.26)
MI ($n=37$)	—	*\$3.06* (0.60)
JP ($n=31$)	\$2.89 (0.51)	\$3.32 (0.55)
JP ($n=33$)	*\$3.72* (0.53)	*\$4.52* (0.69)
	Choices	
JC ($n=20$)	1/20 (5%)	19/20 (95%)
JC ($n=25$)	*2/25* (8%)	*23/25* (92%)
Dealers	*Auction Bids*	
LI ($n=30$)	\$3.52 (0.33)	—
LI ($n=35$)	*\$3.20* (0.44)	—
MI ($n=30$)	—	\$3.36 (0.65)
MI ($n=35$)	—	*\$2.70* (0.41)
JP ($n=30$)	\$3.21 (0.53)	\$3.48 (0.53)
JP ($n=28$)	*3.09* (0.47)	*3.45* (0.50)
	Choices	
JC ($n=15$)	0/15 (0%)	15/15 (100%)
JC ($n=13$)	*0/13* (0%)	*13/13* (100%)

Source: Adamowicz, Alevy, and List 2006.
Note: Mean auction bids are reported. Numbers adjacent to bids in parentheses are standard errors (percentages for choice treatments). In the pairs of rows, the first row is for the ungraded sportscard treatments; the second row (italics) is for the comparable results for the graded sportscards, reported originally by List (2002).

for nondealers is \$4.05 for the 10-card bundle (LI) but only \$1.82 for the 13-card bundle (MI), a difference of approximately 121%. Both a large-sample *t*-test and a Mann-Whitney rank-sum test indicate that prices across the evaluation modes differ significantly at a level of $p<0.01$, with the LI bundle strongly preferred to the MI bundle. The tobit estimation provides additional support for Result 1 due to the large negative and statistically significant coefficient on the *More* variable, reported in Column 1 of Table 5.

Treatment JP yields the opposite result, with the M bundle valued more highly. The mean bid for the 10-card LJ bundle is \$2.89, whereas the 13-card MJ bundle's mean bid is \$3.32, a difference of approximately 13%. Using both a matched pairs *t*-test and a Wilcoxon signed-rank test for matched pairs, we find that the null hypothesis of identical values is rejected at the $p<0.01$ level. Further, the sum of *More* and the *More* × *Joint* interaction term from the tobit model, presented as Result 1 in Table 5, is positive (0.548) and significant ($p<0.01$). Synthesizing the results across evaluation modes, we find that preferences do reverse; the L (M) bundle is valued more highly under isolated (joint) evaluation. This is the "more is less" result found in List's initial study and elsewhere in the literature.

Descriptive statistics for the market professionals, which support Result 2, are presented in the lower part of Table 4. While directionally the values are in accord with the data gathered from nondealers, the magnitudes of the differences are smaller. In the isolated evaluation mode, the dealers' average bid is \$3.52 (\$3.36) for the L (M) bundle, and the difference is not significant using any of the statistical methodologies. In the joint evaluation mode, the dealers bid \$3.48 (\$3.21) for the M (L) bundle. The statistical significance of this result differs across methodologies. A matched pairs *t*-test yields a significant difference ($p<0.01$), but the tobit coefficients (Result 2b in Table 5) do not ($p=0.14$). Hence, by the first measure, we do find evidence of a weak evaluation mode effect and conclude that evaluation mode has a minor impact on the dealers' willingness to pay.

Result 3 considers how the information on sportscard quality interacts with evaluation mode by examining willingness-to-pay across the graded and ungraded cards. We limit attention to the nondealer subject pool, where significant evaluation mode effects are observed, and find that the preference reversal in the isolated evaluation mode is accentuated for nondealers when the information on card quality is removed. Comparing the LI and MI treatments in the first panel of Table 4, the \$4.05 and \$1.82 mean values for the ungraded cards represent a 122% difference in pricing. For the graded cards the L good is priced 58% higher than the M good (\$4.85 vs. \$3.06). The coefficient on the interaction term *Graded* × *More* is positive (1.71) and significant ($p=0.027$), indicating that the magnitude of the preference reversal increases when the grading is removed. This finding provides fresh evidence and support for the hypothesis that information across evaluation modes is critical in determining the strength of valuation anomalies. The result further heightens the stakes when con-

TABLE 5
Tobit Estimates of Auction Bids: Pooled Sportscard Market Treatments

Dependent Variable Bid	Parameters	Linear Combinations	
Graded	0.007 (0.539)	Result 1	0.548*** (0.198)
More	–3.246*** (0.598)	Result 2a	0.138 (0.657)
Joint	–1.487*** (0.515)	Result 2b	0.293 (0.199)
Dealer	–0.804 (0.671)		
Graded × *More*	1.707** (0.771)		
Graded × *Joint*	1.500** (0.693)		
Dealer × *More*	3.384*** (0.893)		
Dealer × *Joint*	0.692 (0.797)		
More × *Joint*	3.794*** (0.101)		
Graded × *Dealer*	–0.752 (0.858)		
Dealer × *More* × *Joint*	–3.639*** (0.937)		
Graded × *More* × *Joint*	–1.334 (0.819)		
Graded × *Dealer* × *More*	–2.284** (1.151)		
Graded × *Dealer* × *Joint*	–0.620 (1.090)		
Graded × *Dealer* × *More* × *Joint*	2.089* (1.221)		
Constant	4.337*** (0.395)		
N	509		
Log likelihood	–1069.80		
Chi-squared (15)	122.78		
Prob > chi-square	0.000		

Note: Bids on graded cards are from List's (2002) study. Standard errors are in parentheses. Result 1 tests *More* + *More* × *Joint* > 0. Result 2a tests *More* + *More* × *Deal* < 0. Result 2b tests *More* + *Dealer* × *More* + *More* × *Joint* + *Dealer* × *More* × *Joint* > 0.
* $p < 0.10$; ** $p < 0.05$; *** $p < 0.01$.

sidering applications to public goods, where the quality may not always be as easily observed.

Public Goods Treatments

Hypotheses tests on the existence of evaluation mode effects and on aggregate contributions in the public goods treatments are reported as Results 4 and 5, and are as follows:

Result 4. Contributions to public goods are characterized by a weak evaluation mode effect in which M is preferred to L under joint evaluation and L and M do not differ significantly under isolated evaluation.

Result 5. Contribution rates are higher in the joint evaluation mode for the pooled wetlands treatments and in the aggregate public goods data.

Table 6 summarizes the data for Result 4, which reflects a pattern of results consistent with a modest effect of evaluation mode. Columns LI and MI report results for the isolated evaluation mode. In all treatments we find that the proportion contributing to the L good is greater than that contributing to the M good, although the results are not statistically significant even if responses are pooled across all treatments. Thus we observe that LI ~ MI; contributions are insensitive to the scope of the good in the isolated evaluation mode, a result on contribution rates similar to that observed on the bidding behavior for the professionals in the sportscard market.

The columns labeled Joint Evaluation Mode in Table 6 provide some evidence that M is preferred to L in this setting. Column J reports the proportions choosing the L and M public goods as well as those declining to contribute (N).[11] Column LM reports on tests of proportions between the L and M goods conditional on contribution. For the farmland preservation data presented as treatment F50, there is clear evidence that MJ > LJ. The same

[11] Statistical tests cannot distinguish between the contribution rates to the W50 and W100 treatments. As a result, we also report the results of the pooled watershed treatments as WP.

TABLE 6
Proportion Contributing to Public Good

Treatment	Isolated Evaluation Mode		Joint Evaluation Mode	
	Less (LI)	More (MI)	J (LMN)	LM, χ^2_1
W50	0.67	0.63	L = 0.30, M = 0.50, N = 0.20	$\chi 2 = 1.50$
	$n = 30$	$n = 30$	$n = 29$	$p = 0.22$
W100	0.62	0.61	L = 0.31, M = 0.45, N = 0.24	$\chi 2 = 0.73$
	$n = 29$	$n = 31$	$n = 30$	$p = 0.39$
W pooled	0.64	0.62	L = 0.30, M = 0.48, N = 0.22	$\chi 2 = 2.17$
	$n = 59$	$n = 61$	$n = 59$	$p = 0.14$
F50	0.58	0.51	L = 0.17, M = 0.53, N = 0.30	$\chi 2 = 5.76$
	$n = 36$	$n = 39$	$n = 30$	$p = 0.02$
W and F pooled	0.62	0.58	L = 0.26, M = 0.49, N = 0.25	$\chi 2 = 6.58$
	$n = 95$	$n = 100$	$n = 89$	$p = 0.01$

Source: Adamowicz, Alevy, and List 2006.
Note: Isolated evaluation mode: Less and More columns present the portion contributing to the public goods in the isolated evaluation modes. Although contributions to the inferior good are uniformly higher, these differences are not significant at conventional levels in any treatment. Joint evaluation mode: Column J displays proportions contributing to less (L), more (M) public goods or not contributing (N). Column ML reports on the significance of one-sample tests of proportions for the L and M goods in the joint evaluation mode, conditional on contribution.

TABLE 7
Contribution Rates across Isolated and Joint Evaluation Modes

Treatments	Isolated Evaluation	Joint Evaluation	S vs. J, χ^2
W pooled	0.63	0.79	3.90
	$n = 120$	$n = 59$	$p < 0.05$
F50	0.55	0.70	2.08
	$n = 75$	$n = 30$	$p = 0.15$
W and F pooled	0.60	0.75	6.26
	$n = 195$	$n = 89$	$p < 0.02$

Note: Entries are the proportions contributing to the public good, pooling the L and M goods within each evaluation mode.

is true for the pooled public goods data. Combining these results with those in the isolated evaluation mode, where there is no statistical difference between LI and MI contribution rates, yields results consistent with the definition of a weak evaluation mode effect.

Finally, as noted in Result 5, we observe that contributions are higher in the joint evaluation mode. Table 7 presents a summary of the data that compares contribution rates in the joint and isolated evaluation modes, where a contribution represents a willingness to contribute to either the L or M good. We find that contribution rates are uniformly higher in the joint evaluation mode, ranging between 70% and 80% in contrast to rates between 55% and 63% under isolated evaluation. These differences are statistically significant in the pooled watershed data and in the data pooled over all the public goods treatments (treatment WFP). These results also indicate that the evaluation mode affects the valuation results.

V. DISCUSSION

The experimental sessions yield evidence of evaluation mode effects in the valuation of private goods, and weak evaluation mode effects in the valuation of public goods. The extent of the effects, where we see variability, depends on both experience and the provision of information that help bridge the differences in information evaluability across modes. Dual process theories of cognition can deepen our understanding of these results, and guide stated preference design.

The strong preference reversal observed by the nonprofessionals in the sportscard market provides the most dramatic evidence of the importance of these cognitive processes. The inability to compare goods in the isolated evaluation mode places individuals in a situation of relative uncertainty. As a result, the observable low quality of a subset of the goods has a strong effect on the overall valuation, consistent with the idea that rapid Sys-

tem 1 processes are less likely to be moderated or constrained by System 2 cognition in the isolated evaluation mode. The weak preference reversal exhibited by the market professionals provides additional evidence consistent with dual-process theory, since experience can provide an internal referent of value, mitigating the importance of information evaluability. The informational treatment provides additional evidence consistent with the evaluability hypothesis. The impact of mode on valuation is accentuated when information readily available across modes—the sportscard grades—is removed.

The similarity of the public good results to those of the market professionals is interesting. The weak evaluation mode effect observed in the public good setting indicates that the expression of preferences is affected by framing, consistent with insensitivity to scope. In analogy with the sportscard dealers, we hypothesize that those valuing the public goods have internal referents that mitigate Type 1 responses, perhaps due to the extensive publicity that these issues have received from governmental and nongovernmental sources in the region, which, in effect, provide respondents with experience in valuing the goods.

The source of the insensitivity to scope observed in this study differs from that most frequently discussed in the literature. Rather than arguing that respondents are expressing general attitudes or are unable to formulate economic values for the goods (see, e.g., Kahneman and Knetsch 1992; Desvouges et al. 1993), we focus on identifying the cognitive underpinnings of scope effects, using both market and nonmarket valuations for robustness. Our emphasis complements recent work by Heberlein et al. (2005), who also argue that "we need to better understand the *conditions* that produce scope failure." Their study examines the impact of affect and cognition on sensitivity to scope, restricting attention to the isolated evaluation mode. They find lack of scope in valuations is widespread, but note that it is consistent with the respondents' cognitive and affective focus on the smaller, usually local, goods. Our study introduces an additional important consideration by demonstrating that the cognitive and affective contributions to valuation can differ significantly across joint and isolated valuation modes.

VI. CONCLUSION

Mainstream economists have begun to more fully recognize that a firm understanding of the psychological processes of individuals is necessary to model human behavior and prescribe efficient public policies. In the environmental arena, where nonmarket valuation is a critical tool, the stakes are quite high, and one can learn a great deal by more fully understanding psychological concepts and how they influence individual choices. Using two related but distinct experimental frameworks to compare value statements across joint and isolated evaluation modes, this study presents results directly at the intersection of the disciplines of environmental economics and psychology.

Our findings confirm and extend previous work on the importance of differences in information evaluability across evaluation modes. Perhaps most provocatively, the results reaffirm the importance of the "more is less" preference reversal phenomenon and extend it to the domain of public goods. Overall, our data suggest that preferences are inconsistently expressed over the joint and isolated evaluation modes for both private and public goods. The effect is accentuated when uncertainty exists about the good's quality, but is reduced with the experience of the respondent.

Needless to say, this research has raised more questions than it has answered. We believe, however, that researchers and policy makers who must decide which valuation technique to use should consider the issues raised by the cognitive processes associated with the joint and isolated evaluation modes. From one perspective, the joint evaluation mode is preferred since the monotonicity of valuations in the joint evaluation mode yields results consistent with the fundamental idea that "more is more." Decisions made in the isolated evaluation mode, however, have a relevance that is also undeniable: once a policy is implemented, after all, its impact is experienced largely in isolation.

More generally we believe that it is important to understand the nuances and biases of our valuation techniques, particularly since the U.S. federal government requires that every economically significant proposal (about 50–100 per year) receive a formal analysis of the benefits and costs. In this sense, our hope is that the pattern of results discovered herein will eventually lead to theories and behavioral generalizations that become part of the analyst's tool box.

APPENDIX A

Detailed Procedures

Welcome to Lister's Auctions. You have the opportunity to bid in an auction for the goods on the table. The number of auction participants, denoted *n* below, will be determined by how many subjects choose to participate in the auction during this sportscard show.

Auction Rules

You are asked to submit one bid in the auction, and there will be a total of *n* bids submitted, where *n* is unknown at this time and depends on how many people agree to participate. The monitor will rank all the bids from highest to lowest and the winning bidder(s) will be determined in a random fashion. Here is how it works: The monitor will put all the bids in a bag and randomly draw out one of them. If the monitor randomly selects the bid ranked #20 (the 20th highest bid), then each of the 19 bidders who bid more than this bid would win in the auction and receive the goods after they sent me the value of the 20th highest bid. There is an equal chance that the selected bid will be the 2nd, 3rd, 4th, 5th, 6th, . . . or *n*th-highest bid. Let's go through an example to be sure you understand the auction rules.

In this example the number of bidders, *n*, is equal to 10. After receiving all the bids, I will rank the bids from highest to lowest as follows:

$C	High bidder
$D	2nd highest bidder
$A	3rd
$B	4th
$F	5th
$G	6th
$L	7th
$K	8th
$V	9th
$Z	Low bidder

I will draw from the bag one of these bids. Assume that the bid drawn randomly is the 8th highest, which corresponds to $K. If this bid is drawn, the top 7 bidders win and pay the value of the 8th highest bid for the cards. In this case all the players would pay $K.

In this type of auction you should bid your true value for the goods (i.e., what they are worth to you). If you bid too much, you increase your chance of winning but face the risk of paying more than the cards are worth to you. If you bid below your true value, then you risk not being among the winners when a bid of your true value might have won and you would have paid less than your true value for the cards. This is true because in this type of auction your bid never affects the price you pay in the auction, just whether you win or lose. In this example,

FIGURE A1
Random *n*th-Price Auction

figure continued on next page

note that since all seven winners pay the 8th highest price, they will all pay less than their true value, when that is their bid.

Here are a couple of examples that demonstrate that it is best for to bid your true value. First consider the case where I bid more than my true value. Suppose I bid $C and turn out to be the highest bidder. In this case I am guaranteed to win in the auction since the winning bid is drawn from those ranked 2nd through 10th. Suppose now that the random draw brings up the 3rd highest bid, $A, which is the price I must pay as an auction winner. If my true value is less than $A, I have won the auction with my bid of $C but actually suffer a loss. My loss is the difference between the price I pay, $A, and my true value for the cards. Similarly consider the case where I bid less than my true value. Suppose that I bid $Z and am the low bidder. Assume this time that the random draw pulls up the 9th highest bid, $V. If my true valuation is greater than $V, I have missed an opportunity to buy at a price less than my true value, by bidding too low.

Do you have any questions about the bidding process?

Final Transaction

At 6:00 p.m. Sunday night I will determine the winners of the auction. After the winners pay me (cash or check) they will receive the goods. Note that, regardless of the price, the goods will be awarded to the winners. In case you cannot attend the "determination of winners" session at 6:00 p.m., please provide your name, mailing address, and phone number below.

Name_____

Address_____

Phone # _____

If you are unable to attend at 6:00 p.m., I will contact you by phone. Upon receipt of your check or cash, I will send you the goods that you have won. All postage will be paid by Lister's Auctions for goods mailed to winners.

Note that I guarantee to sell the goods to the winners no matter what the final auction price turns out to be. Your bid represents a binding commitment to buy the goods you win at the prices specified by the auction outcomes.

Good luck—please write your bids on the sheets provided.

Thanks for participating.

Confidential Bidding and Survey Sheet

BID: $ _____

Signature: _____

I verify that if I am determined a winner I will be liable for paying the determined amount in exchange for the bundle of cards.

Please complete the information below. THIS INFORMATION WILL BE KEPT STRICTLY CONFIDENTIAL.

1. How long have you been active in the sportscards and memorabilia market? _____ years

2. Are you a sportscard or sports memorabilia professional dealer? _____

3. Gender: _____ Male _____ Female

FIGURE A1
Random *n*th-Price Auction

figure continued on next page

4. Age _____ Date of birth _____

5. What is the highest grade of education that you have completed? (circle one)

   a. Eighth grade b. 2-year college c. 4-year college

   d. High school e. Other post–high school f. Graduate school

6. What is your approximate yearly income from all sources, before taxes?

   a. Less than $10,000 e. $40,000 to $49,999

   b. $10,000 to $19,999 f. $50,000 to $74,999

   c. $20,000 to $29,999 g. $75,000 to $99,999

   d. $30,000 to $39,999 h. $100,000 or over

7. Have you ever seen these goods before this show? _____

FIGURE A1
Random *n*th-Price Auction

APPENDIX B

Question L

Thanks for participating!

A special Maryland Day 2002 environmental conservation program has been discussed at the University of Maryland. This program, if funded, would entirely clean up 500 acres of wetlands in the Chesapeake Bay. The cleanup program would by funded by individual contributions. Each individual who contributes to the cleanup program would receive a certificate indicating that they helped the effort.

I would contribute $X to a program that would result in an entire cleanup of 500 acres of wetlands:

Yes No

Question M

Thanks for participating!

A special Maryland Day 2002 environmental conservation program has been discussed at the University of Maryland. This program, if funded, would entirely clean up 500 acres of wetlands in the Chesapeake Bay and partially clean up 50 acres. The cleanup program would by funded by individual contributions. Each individual who contributes to the cleanup program would receive a certificate indicating that they helped the effort.

I would contribute $X to a program that would result in an entire cleanup of 500 acres of wetlands and a partial cleanup of 50 acres:

Yes No

FIGURE A2
Wetlands Treatments W50 (X Is 50) and W100 (X Is 100)
figure continued on next page

Question J

Thanks for participating!

A special Maryland Day 2002 environmental conservation program has been discussed at the University of Maryland. This program, if funded, would clean up wetlands in the Chesapeake Bay. The cleanup program would by funded by individual contributions. Each individual who contributes $X to the cleanup program would receive a certificate indicating that they helped the effort.

Please choose your most preferred choice from below:

A. For a contribution of $X, an entire cleanup of 500 acres of wetlands would occur.
B. For a contribution of $X, an entire cleanup of 500 acres and a partial cleanup of 50 acres would occur.
C. I would not contribute $X to this program.

FIGURE A2
Wetlands Treatments W50 (X Is 50) and W100 (X Is 100)

Question L

Thanks for participating!

A special Maryland Day 2002 environmental conservation program has been discussed at the University of Maryland. This program would permanently preserve 500 acres of Maryland farmland. The farmland conservation program would be funded by contributions to conserve farmland in parcels 1/10 of an acre in size (1/10 of an acre is about the size of a basketball court). Each individual who purchases 1 unit (basketball court size) of farmland for the conservation program would receive a certificate indicating that they helped the conservation effort.

If a farmland conservation package were offered to you at a price of $50 (to permanently preserve 500 acres), would you purchase one?

Yes No

Question M

Thanks for participating!

A special Maryland Day 2002 environmental conservation program has been discussed at the University of Maryland. This program would permanently preserve 500 acres of Maryland farmland and temporarily (5 years) preserve 50 acres. The farmland conservation program would be funded by contributions to conserve farmland in parcels 1/10 of an acre in size (1/10 of an acre is about the size of a basketball court). Each individual who purchases 1 unit (basketball court size) of farmland for the conservation program would receive a certificate indicating that they helped the conservation effort.

If a farmland conservation package were offered to you at a price of $50 (to permanently preserve 500 acres) and temporarily (5 years) preserve 50 acres, would you purchase one?

Yes No

FIGURE A3
Closed-Ended Farmland Preservation Question, F50

figure continued on next page

Question J

Thanks for participating!

A special Maryland Day 2002 environmental conservation program has been discussed at the University of Maryland. This program would preserve farmland throughout Maryland. The farmland conservation program would be funded by contributions to conserve farmland in parcels 1/10 of an acre in size (1/10 of an acre is about the size of a basketball court). Each individual who purchases 1 unit (basketball court size) of farmland for the conservation program would receive a certificate indicating that they helped the conservation effort.

Please choose your most preferred choice from below:

A. I would purchase a farmland conservation package offered to me at a price of $50 if it would *permanently preserve 500 acres* of Maryland farmland.

B. I would purchase a farmland conservation package offered to me at a price of $50 if it would *permanently preserve 500 acres* and *temporarily (5 years) preserve 50 acres* of Maryland farmland.

C. I would not purchase a farmland conservation package if it were offered to me at a price of $50

FIGURE A3
Closed-Ended Farmland Preservation Question, F50

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