WILLINGNESS TO PAY FOR CURBSIDE RECYCLING WITH DETECTION AND MITIGATION OF HYPOTHETICAL BIAS

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In this paper, we estimate willingness to pay for curbside recycling. Using a unique data set, we also test for and detect significant hypothetical bias using stated and revealed preference data. A short-scripted "cheap talk" statement is used to mitigate the bias and provide more efficient estimates of the welfare impacts of curbside recycling programs.

Key words: cheap talk, contingent valuation, curbside recycling, hypothetical bias

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Recycling in the United States has increased dramatically in the past decade. Nationwide, municipalities recycle approximately 32% of their solid waste, up from 8% in 1990. Likewise, the number of community curbside recycling programs (CRPs) has increased eight fold, to approximately 9,250 programs (Goldstein and Madtes). Despite these national trends in recycling, many CRPs in the western United States are losing money or are not retaining earnings adequate for future growth. For example, in Utah two of the state's nine CRPs are not covering their costs, and none of the other seven programs are retaining adequate earnings.[1] While there are well-known cost-side explanations for this lack of earnings–e.g. high collection, processing, and shipping costs relative to materials prices, and lower landfill tip fees (Glenn)–the benefits associated with curbside recycling are not as well understood.

In order for local policymakers to make informed decisions about whether to initiate or maintain a CRP, they need reliable estimates of households' maximum willingness to pay (WTP) for the service, as well as the household- and community-specific characteristics that influence WTP. For voluntary CRPs in particular (where households pay for the service only if they have signed up for it), information regarding its value to households will enable community planners to estimate the relationship between changes in fees and participation rates—a crucial determinant of sustained profitability. A primary aim of this paper is to accurately estimate this relationship using contingent valuation methods (CVM).

There is an active debate regarding the reliability of CVM, and more specifically the issue of hypothetical bias from stated-preference methods (e.g. Arrow et al.; Hanemann; and Diamond and Hausman). The potential for hypothetical bias arises whenever people are asked to state or select a maximum amount they are willing to pay for a good or service, even though they will not have to actually pay for it.[2] To detect and mitigate the effects of this bias, researchers have recently used two methods. The first combines results of actual market outcomes (i.e., revealed preference (RP) data) with stated preference (SP) data (henceforth, the RP-SP method). Examples of this method include Whitehead, Haab, and Huang; Nestor; Adamowicz, Louviere, and Williams; and Cameron (1992). The second involves innovations in survey design.

Our sample consists of over 1,000 households from the state of Utah who were asked to value either their actual CRP, or a hypothetical program if their community does not currently provide one. The sample includes both RP and SP information from households that have chosen whether to participate in an actual CRP, as well as SP information from non-participating households that either do not have a CRP available in their community or are unaware of its existence. This unique nature of our data set enables us to test for hypothetical bias using two distinct approaches to the RP-SP method.

In the first approach, we use a sub-sample of data from two groups: (1) households that have already made a decision about whether to participate in an actual voluntary CRP and (2) households that are making a decision about whether to participate in a hypothetical CRP. Because the CRPs facing each group are similar in their attributes, and because we control for differences across these two groups, any between-group difference in the likelihood of CRP participation is suggestive of hypothetical bias. This is a *direct* RP-SP comparison because we have information on actual household participation decisions rather than indirect information on preferences, such as that provided by travel costs.

The second RP-SP approach is a comparison of households' actual participation decisions in voluntary CRP communities with *their own* stated WTP responses. This approach is similar in spirit to Whitehead, Haab, and Huang; and Huang, Haab, and Whitehead, who detect and control for hypothetical bias by using information from the same individuals concerning actual number of recreation trips at current quality (RP data) and expected number of trips at current and improved qualities (SP data). The advantage of this approach is that it avoids (1) the need to control for differences between two groups of households, and (2) potential sample-selection problems.

The other method for detecting and mitigating hypothetical bias involves innovations in survey design. Recent innovations include: (1) reminder statements of substitutes, budget constraints, or the hypothetical nature of the good in question (e.g., Loomis et al.; Cummings and Taylor; and List), (2) referendum formats (e.g., Loomis; Alberini (1995b); Boyle et al.; and Welsh and Poe), and (3) follow-up questioning (e.g., Li and Mattsson; Champ et al.; Blumenschein et al.; and Berrens et al.).

Along these lines, we employ a "cheap talk" reminder statement similar in spirit to the ones used by Cummings and Taylor and List, but closer in length to that used by Loomis, Gonzalez-Caban, and Gregory. Cheap talk is information provided prior to the

WTP questions reminding respondents that they are valuing a hypothetical program, and as a result, they may inadvertently misstate their true WTP.

Previous research on cheap talk is divided over whether it matters. For example, Cummings and Taylor find that households receiving a long-script version of cheap talk will, on average, report lower WTP values than those that do not. They mention two yet unpublished studies that find no significant effect of shortened cheap-talk scripts, but nonetheless recommend reducing the script length to be more compatible with telephone applications of CVM. Loomis, Gonzalez-Caban, and Gregory reduce the discrepancy between hypothetical and actual WTP in laboratory experiments by issuing a short-script reminder to their subjects that ". . . although the question is hypothetical, we want you to answer as if it were real…" Finally, List finds that the effectiveness of long-scripted cheap talk depends upon respondent experience with the good being valued. Similar to List, we find that cheap talk is more effective for certain types of individuals, but unlike List and Cummings and Taylor, we use a *short-scripted* version of cheap talk. To the best of our knowledge, this is the first evidence that short reminder statements can reduce hypothetical bias and improve the efficiency of the corresponding welfare estimates in contingent valuation surveys.

The next section presents the econometric model used to estimate our welfare measures, as well as the specific methods used to detect and mitigate potential hypothetical bias in our data. We then discuss our survey instrument and the data, followed by our empirical results. The final section summarizes our findings and offers policy recommendations.

Methodology and Econometric Model

This section is divided into four subsections. The first subsection discusses the double-bounded dichotomous-choice (DBDC) model used to obtain our welfare estimates. The second and third introduce our explicit hypothesis for the existence of hypothetical bias in the SP data. The fourth subsection discusses the cheap-talk hypothesis.

Econometric Model

Our econometric approach follows Cameron and James. WTP questions are set in the DBDC format to elicit a household's WTP through a sequence of dichotomous-choice (i.e., yes or no) valuation questions. The first question is: "Would you be willing to pay \$$\tau$ for the service?" The opening bid τ is chosen randomly from a set of pre-determined values.[3] By randomizing the opening bid, the possible effects of "starting-point bias" are reduced (Cameron 1988 and Alberini 1995a and b). Based on her response to the opening bid, the respondent is then asked a similar follow-up question, but with a larger bid, $\tau_H = 2\tau$, if she answered "yes" (i.e., willing to pay at least τ for the service) or a smaller bid $\tau_L = 0.5\tau$ if she answered "no" (i.e., unwilling to pay τ for the service).

Based on the responses to the opening bid and follow-up questions, the respondent's latent WTP may be placed in one of four regions: $(-\infty,\tau_L)$, (τ_L,τ), (τ,τ_H) or (τ_H, ∞). Unlike other CVM studies, we follow up with a third valuation question for those who respond "no" to the first two valuation questions, that is, "Would you be willing to use the service if it were free of charge?" Previous experience with household recycling surveys suggests that some households apparently need to be paid (i.e., have negative WTP values) to participate (Haab and McConnell; and Aadland and Caplan). As a result, our survey generates five rather than four valuation regions with $(-\infty, \tau_L)$ being replaced by $(-\infty, 0)$ and $(0, \tau_L)$.

We posit that the household's true WTP (WTP*) is represented by the equation

(1) $$WTP_i^* = X_i\beta + \varepsilon_i,$$

where X_i is a row vector of household- and community-specific control variables, $\exists$ is a corresponding column vector of coefficients, and ε_i is a normally distributed error term for households $i = 1,\ldots,n$. We allow for possible heteroscedasticity by modeling the variance of the WTP error term as

(2) $$\sigma_i^2(Z_i\gamma) = \exp(Z_i\gamma),$$

where Z_i is a row vector of variables related to the disturbance variances and γ is a column vector of parameters.

By assuming independence across error terms, we then form the likelihood function conditional on (1), (2), and the observed data. Letting Φ indicate the standard normal cumulative density function, the probability that household *i*'s true WTP falls in each of the five intervals is:

(3) $$P_{1,i} = \text{Prob}(-\infty < WTP_i^* < 0) = \Phi(-X_i\beta/\sigma_i(Z_i\gamma));$$

$$P_{2,i} = \text{Prob}(0 \le WTP_i^* < 0.5\tau_i) = \Phi((0.5\tau_i - X_i\beta)/\sigma_i(Z_i\gamma)) - \Phi(-X_i\beta/\sigma_i(Z_i\gamma));$$

$$P_{3,i} = \text{Prob}(0.5\tau_i \le WTP_i^* < \tau_i) = \Phi((\tau_i - X_i\beta)/\sigma_i(Z_i\gamma)) - \Phi((0.5\tau_i - X_i\beta)/\sigma_i(Z_i\gamma));$$

$$P_{4,i} = \text{Prob}(\tau_i \le WTP_i^* < 2\tau_i) = \Phi((2\tau_i - X_i\beta)/\sigma_i(Z_i\gamma)) - \Phi((\tau_i - X_i\beta)/\sigma_i(Z_i\gamma));$$

$$P_{5,i} = \text{Prob}(2\tau_i \le WTP_i^* < \infty) = 1 - \Phi((2\tau_i - X_i\beta)/\sigma_i(Z_i\gamma)),$$

where τ_i represents household *i*'s opening bid. Using (1) through (3), the (log) likelihood function for all households in the sample is

(4)
$$\ln(L) = \sum_{i=1}^{n}\sum_{j=1}^{5}\omega_{j,i}\ln(P_{j,i}),$$

where $T_{j,i}$ = 1 if the stated WTP value falls in the j^{th} region and 0 otherwise. Maximizing the (log) likelihood function (4) results in an estimation problem requiring nonlinear optimization techniques to generate estimates of the ∃ parameters (see Greene).[4]

Detecting Hypothetical Bias—Inter-Household Comparison

Typically, CVM researchers are unable to quantify potential hypothetical bias because they do not observe individuals revealing their preferences through actual market decisions. We, however, have observations both from individuals who have made a voluntary choice of whether to participate in an actual CRP and those who were asked to hypothetically value a similar CRP. The hypothetical program was described as follows:

> . . . please imagine that you could have a service that regularly collects paper, plastic, glass, aluminum cans, tin cans, and cardboard. Your household would need to sort your recyclables (into groups of similar materials) and pay a fee for the recycling service, in addition to your current garbage collection fee.

To ensure that similar CRPs are valued in this inter-household comparison, we isolated participating and non-participating Salt Lake City (SLC) respondents who know that a CRP exists in their community. There is no marginal fee for the SLC program. In order to sign up for the service, a resident phones to initiate the service and pays a one-time $6 deposit for the recycling container. As with our hypothetical CRP, the SLC program involves some sorting of recyclable materials and picks up materials two to four times a month.

We then grouped these SLC respondents with the subset of respondents across the 25 communities in our sample that answered the question of whether they would participate in the hypothetical CRP if it were free of charge.[5] With this sub-sample, we estimate a joint RP-SP probit model (controlling for household- and community-specific effects) for the binary choice of whether or not to participate in the CRP if the service were offered free of charge.[6] Our general hypothesis for the detection of hypothetical bias is:

> **H1**: Respondents tend to overstate their maximum WTP for (and willingness to participate in) a hypothetical CRP relative to an actual program.

In other words, we expect respondents who are valuing the hypothetical program to be observed as more likely to participate (and therefore have higher implied latent WTP values) than those making the same decision about an actual CRP with similar attributes, all else equal. This hypothesis is tested directly by examining whether the coefficient on an SP binary variable is statistically greater than zero in an RP-SP probit model for whether households participate in curbside recycling or not. If it is greater than zero, we fail to reject **H1** using this approach.

Detecting Hypothetical Bias—Intra-Household Comparison

A second method for testing **H1** is to compare RP and SP information from the same households. This type of intra-household comparison is possible because our survey asked households residing in communities with a CRP whether they participate in the program (RP data), as well as their maximum WTP for the program (SP data). We have usable data of this type from two communities with voluntary CRPs—SLC and another with a $6 monthly fee. A simple test of **H1** is to examine both the frequency and magnitude of cases where households stated a WTP greater than the monthly fee but do not participate in the program. Evidence of this type of response behavior from non-participants would suggest that positive hypothetical bias exists in the SP data (i.e., failure to reject **H1**). Also, note that it is possible to find cases where the respondent's stated WTP is less than the monthly fee but the household is participating in the program. This type of behavior would suggest *negative* hypothetical bias among participants along the lines of Carson et al. (1996).

Mitigating Hypothetical Bias

As mentioned in the Introduction, we test whether short-script cheap talk is effective in mitigating hypothetical bias. Our cheap talk statement reads

> . . . studies have shown that many people say they are willing to pay more for curbside recycling than they actually will pay when (it/curbside recycling) becomes available in their community. For this reason, as I

> read the next two curbside recycling fees, please imagine your household *actually* paying them.

This statement is noticeably shorter than the scripts used by Cummings and Taylor and List. By including a binary explanatory variable in (1) for those that randomly received cheap talk (CHEAP TALK) and interacting it with several household characteristics, we test whether our shortened cheap-talk script is effective in reducing potential response bias among households that were asked to value the hypothetical CRP. Our hypothesis is:

> **H2**: CHEAP TALK will have a significant response effect, resulting in a downward effect on estimated WTP.

Recycling Surveys and Data

We administered two surveys for this study.[7] The first—a household survey—was administered over the phone by the Oregon Survey Research Laboratory (OSRL) to approximately 1,000 households located in 35 different communities throughout Utah with population sizes greater than 1,000 residents.[8] The communities were selected through proportionate random sampling in order to reflect a rough one-to-three split in the population between those communities with and without curbside recycling, respectively. Households were then randomly sampled within each community to ensure a roughly equal number of households with and without curbside recycling. OSRL reports that the average survey took approximately seven minutes to conduct, and response rates were approximately 75%. The second survey—a recycling coordinators survey—was also administered over the phone to each of the recycling coordinators in the 35 communities. This survey provided background information on each of the communities, as well as verification of the households' responses.

We find it convenient to partition the household survey questions into four categories. The first category includes questions regarding a household's motivation for recycling. The variables are (1) "whether you feel an ethical duty to recycle to help the environment" (ETHICS), (2) "whether you believe recycling saves you money by either directly turning in recyclables or by using a smaller garbage container" (MONETARY), and (3) "whether an ethical duty to help the environment is the primary reason why you recycle" (PRIMARILY ETHICS). Including these variables enables us to test whether different motivations for recycling produce systematically higher WTP values, all else equal.

The second category includes a series of questions related to the status and household usage of dropoff recycling in the community. Since dropoff is a substitute for curbside recycling, we test whether certain characteristics of the dropoff-recycling program, such as presence and usage of dropoff facilities, influence a household's WTP for curbside service. Variables in this category include (1) "whether or not there are dropoff recycling facilities in the community" (DROPOFF), and (2) "whether the household is a frequent user of the dropoff facilities" (DROPOFF USER).

Demographic variables such as age, gender, household size, membership in environmental organizations, household income level, and education level comprise the third category of variables. Age, income, and education levels are broken down into binary variables as follows: $18 \leq \text{Age} \leq 35$ (YOUNG); $\text{Age} \geq 65$ (OLD); $\$25{,}000 \leq \text{Income} \leq \$50{,}000$ (MEDIUM INCOME); $\text{Income} \geq \$50{,}000$ (HIGH INCOME); and whether the highest degree earned is either a high school diploma or GED (HIGH SCHOOL), associate's degree (ASSOCIATE), bachelor's degree (BACHELORS), or master's/doctoral/professional degree (GRADUATE).

The fourth category includes variables that elicit household valuation (or refine the manner in which the values are elicited). As mentioned previously, with the DBDC format households valuing the hypothetical CRP are offered a randomly chosen opening bid value (BID) followed by additional bids scaled to the opening amount. Households with an available CRP (whether or not they actually use it) have their opening bids replaced by their perceived monthly fee for curbside service.[9] Additional bids are then scaled to this opening amount. As discussed above, a random sample of households valuing the hypothetical CRP received CHEAP TALK. A subset of households to which the hypothetical program was described, but who did not receive cheap talk, were given the following certainty question immediately after their valuation questions: "How sure are you of the answer you just gave to the previous question?" The options were very sure (VERY SURE), somewhat sure (SOMEWHAT SURE), or not sure (NOT SURE). Finally, we also create a binary variable for whether households claimed to know the CRP fee (FEE KNOWN), as well as differences between the actual and perceived fee (FEE UNDERSTATED and FEE OVERSTATED).

Summary statistics for each of these variables are shown in table 1. Notice that respondents appear to recycle out of both an ethical duty to help the environment (90%)

and to save money (80%), with an ethical duty being the primary motivation (68%). For those respondents given the preference-certainty question, most said they were "very sure" (47%) about their responses to the valuation questions. People residing in communities with CRPs who stated that they knew the curbside recycling fee (45%), mostly tended to overstate the amount by an average of $4.14 per month. Also, a little over one third (38%) of the respondents who live in a community with dropoff facilities consider themselves frequent dropoff users.

[Insert table 1]

In terms of demographics, our sample appears fairly representative of the Utah population, except for an over-sampling of women. Since women tend to be willing to pay more for curbside recycling, all else equal, the WTP of the "typical" household is ultimately evaluated at the population average, which is approximately a 50-50 split between men and women.

Econometric Results

We begin by estimating model (1), which combines all (n = 876) households in our sample—those residing in a community without an actual CRP and those with a CRP.[10] The results are also provided in table 1. First, note that the mean estimated WTP across our entire sample is approximately $7.00 per month. This estimate is considerably higher than those reported in Lake, Bateman, and Parfitt; Tiller, Jakus, and Park; and Aadland and Caplan, and is higher than the current fee of all but one of the communities in our sample whose recycling coordinators have reported inadequate earnings from their CRPs.

Second, we find evidence of heteroscedasticity. The likelihood ratio statistic used to test the null hypothesis that $\gamma = 0$ in (2) is 65.11 with a 5% critical value equal to 11.07. We therefore reject the null in favor heteroscedasticity. The variables included in the Z vector are shown in the lower portion of table 1. The coefficient on CHEAP TALK is negative and significant at the 5% level indicating a reduced error variance for those receiving cheap talk. This result indicates that cheap talk reminder statements, in addition to reducing hypothetical bias for certain types of individuals, may also be effective in reducing the uncertainty associated with stated WTP values. By construction of the bid design, BID is also likely to be systematically related to the variance of the latent WTP errors. Recall that the opening bids are even integers between two and 10, with subsequent bids equal to either half or twice the opening amount. Therefore, the bid design generates larger WTP intervals (and thus more uncertainty regarding the true WTP) for higher opening bids. As expected, the coefficient on BID is positive and statistically significant at the 1% level.[11]

We also include preference-certainty variables in the spirit of Li and Mattsson. However, since our preference-certainty information is discrete rather than continuous, we are precluded from adopting their methodology. In contrast to Li and Mattsson, who impose a constant variance across households, we allow households stating different levels of certainty to have different error distributions. As table 1 shows, however, the coefficients associated with the three preference-certainty categories are not statistically different than zero.

Third, we find several individual- and community-specific characteristics that are significantly related to WTP for curbside recycling. Those willing to pay the most are (1) young; (2) female; (3) highly educated; (4) motivated to recycle because of an ethical duty to help the environment; (5) members of an environmental organization; (6) residing in a large household; (7) not frequently using dropoff-recycling facilities; and (8) overstating the current fee for their curbside service.

Most of these effects are similar to those found in Aadland and Caplan; Tiller, Jakus, and Park; and Lake, Bateman, and Parfitt. Effects (6) and (7), however, deserve further attention. We had no prior expectation on (6) and, therefore, entertain several possible hypotheses regarding its sign. On the one hand, a larger household may experience larger costs associated with organizing the recycling task among its members, and thus is willing to pay less, all else equal. On the other hand, if the household derives passive-use value from recycling, then its payoff will be larger due to its corresponding generation of more recyclable materials. Our results suggest that the latter effect outweighs the former. One explanation for effect (7) is that households presently using dropoff facilities may be revealing a relatively low travel cost associated with dropoff recycling. The marginal value associated with curbside pickup (due to its added convenience) is therefore likely to be lower for these households.

We turn next to the results for our tests of **H1**—i.e., the existence of positive hypothetical bias. Estimation results for the Inter-Household Comparison, where the binary participation variable is the dependent variable, are shown in table 2. The important variable is the dummy variable SP, which equals one for those valuing the hypothetical CRP and zero otherwise. The SP variable is positive and statistically significant, indicating an upward hypothetical bias in the SP data (i.e., we fail to reject **H1**) after controlling for potential differences across households. However, as in standard probit models, where the threshold value is not randomized, the coefficients are only identifiable up to a scale factor involving the standard deviation of the error term. Nonetheless, the marginal effect on the probability of the true WTP being greater than $0.00 is identified. As shown in table 2, this estimate equals 0.17 and is statistically significant at the 5% level. Thus, households making the hypothetical decision of whether to participate in a CRP with no monthly fee are approximately 17% more likely to participate, all else equal, than households making an actual decision to participate in a similar program. We interpret this as evidence of positive hypothetical bias in the SP data.

[Insert table 2]

Next, we consider the Intra-Household Comparison. Between the two voluntary CRP communities in this sub-sample, there were 190 households that stated they knew a CRP existed in their respective communities. Of these 190 households, 59 (31%) placed themselves in intervals implying a true WTP value that is higher than the fee, but failed to participate in the program. The mean difference between the stated WTP (using the more conservative lower bound of the intervals) and the CRP fee for these non-participants was $6.99 per month. The magnitude of this difference indicates substantial positive hypothetical bias associated with these 59 non-participants. Alternatively, there were only six instances of downward hypothetical bias, whereby a participant stated a

WTP amount less than the fee. In sum, these inter- and intra-household comparisons support the hypothesis that positive hypothetical bias exists in our SP data.

Next, we consider the effect of short-scripted cheap talk in model (1). From table 1, notice that although cheap talk, on average, appears to mitigate hypothetical bias (i.e., the coefficient on CHEAP TALK is negative and equal to -$0.59), it is not statistically different than zero at the 10% level. Therefore, we investigate the possibility (as suggested in List) that cheap talk may be more effective for certain types of individuals. Toward this end, we re-estimate model (1) and interact CHEAP TALK with several individual characteristics. The estimated coefficients on the interaction terms are shown in table 3. Notice that the coefficients on the interactive terms are negative and generally larger in magnitude than the coefficient on the non-interactive term. Moreover, several of the interaction terms are statistically significant. This indicates that certain types of individuals (typically those stating relatively high WTP) are more prone to reduce their stated WTP when receiving cheap talk. To our knowledge, this is the first evidence that short-scripted cheap talk (for phone surveys) may reduce the effect of hypothetical bias.

[Insert table 3]

Conclusions and Policy Implications

Based on a sample of over 1,000 households from Utah, we find that the mean WTP for curbside recycling is approximately $7.00 per month. Young, well-educated women who are members of environmental organizations, who recycle out of an ethical responsibility for the environment, who are not frequent dropoff users, and who reside in large households are willing to pay the most for a curbside service. Further, using combined SP-RP data we find statistically significant positive hypothetical bias in the SP data. We are able to partially mitigate this bias by introducing short-scripted “cheap talk” reminder statements to a subset of households that currently do not participate in a CRP prior to the valuation questions.

The WTP estimates presented above (with or without cheap talk) may represent overly optimistic values for risk-averse policymakers who are concerned about overstating projected revenues. To generate a more conservative WTP estimate using the preference-certainty information, we re-coded the valuation responses in a manner similar to Champ et al., Blumenschein et al., Carson et al. (1998), and Berrens et al. In our re-coding scheme, we assume that WTP* is in the $(0, \tau_L)$ region for all households that stated they were “unsure” of their responses to the valuation questions. Using this approach, the estimated mean WTP in model (1) for the sub-sample who received the preference-certainty questions falls \$0.29, from \$7.00 to \$6.71 per month. In sum, by setting CHEAP TALK equal to one, re-coding the WTP data using the preference-certainty information, and adjusting for an over sampling of females, the predicted WTP for curbside recycling in model (1) falls by \$0.59, \$0.29, and \$0.12, respectively, for an overall reduction of \$1.00 per month.

Local policymakers can use our analysis to estimate a revenue function that relates projected revenues to recycling fees. In figure 1, we present a graph relating projected revenues from a voluntary CRP to a range of fees. Projected revenues are calculated using our estimates from the entire sample under the assumption that households will participate if their predicted WTP is greater than the fee. The product of the fee and the number of participating households then gives total projected revenue.

[Insert figure 1]

Coupled with data on the projected costs of providing a curbside recycling service in their community, policymakers can equate the incremental cost and revenue associated with providing the service to additional individuals (through the relationship between fee and participation changes) to determine the efficient allocation of resources toward curbside recycling.

Future research on these issues should proceed along two lines. First, a broader study of household recycling behavior should be undertaken at a larger regional or national level to test the robustness of the welfare estimates presented in this paper. Second, the effects of cheap talk in mitigating hypothetical bias deserve further testing. Short-scripted cheap talk was found to be effective in mitigating bias for certain types of households in this data set. Future studies might vary the nature and length of the script to examine its marginal effectiveness in mitigating hypothetical bias.

Footnotes

[1]Based on our own survey of a random sample of recycling coordinators throughout 17 western U.S. states.

[2]Our definition of hypothetical bias encompasses any deviation of an individual's stated WTP from their true WTP that is due to the hypothetical nature of the good. With this definition, hypothetical bias could arise from households' incentives to influence policy decisions (frequently referred to as strategic bias). The distinction between hypothetical and strategic bias has been extensively reviewed in the CVM literature. Cummings, Brookshire, and Schulze, and Freeman originally suggested that strategic bias can be reduced by careful design of the survey instrument. Their specific criteria have been repeated by Arrow et al. and Hanemann. Our survey generally meets these criteria. One implication from the recent literature, however, is that strategic and hypothetical biases are in some sense substitutable for one another (Carson, Groves, and Machina). For instance, any attempt to reduce strategic bias by construing the survey as purely hypothetical or "inconsequential" to policy decisions will tend to increase hypothetical bias. Conversely, any attempt to reduce hypothetical bias by tying the survey more closely to policy decisions may contribute to strategic bias. Although our survey instrument states "we are conducting a scientific survey for professors at . . . ," it does not explicitly state that policy decisions will not be influenced by survey responses. Furthermore, we provide plausible prices for curbside recycling, which is a service that households are generally familiar with, either through direct participation in their community's CRP or through participation in a similar weekly curbside garbage pickup.

Therefore, while we acknowledge that our estimates may contain strategic bias, they are unlikely to suffer from the problem sometimes associated with CVM survey designs that make the scenario seem entirely inconsequential to the respondent. We thank an anonymous referee for alerting us to this issue.

[3]In this study, opening bids were randomly chosen integers from \$2 to \$10, reflecting the approximate range of values for actual CRPs in our sample.

[4]Some respondents answered "Don't Know" to one or more of the valuation questions. For these households, their unknown WTP does not fit into one of the five categories, but instead overlaps one or more of the intervals. For example, if a respondent answered "Don't Know" to whether they would be willing to pay \$$\tau$ and "Yes" to whether they would be willing to pay \$$\tau_L$, their unknown WTP falls in the region (τ_L, ∞). The likelihood function is then adjusted accordingly.

[5]Unlike the SLC program, the hypothetical CRP did not include a one-time \$6 fee. There are two reasons why this may not significantly affect our results. First, microeconomic theory tells us that marginal (rather than fixed) fees determine consumers' decisions. Second, a one-time \$6 fee is rather small relative to the income levels of our households, especially when spread over several months of service. Nevertheless, to the extent that the one-time fee matters in households' decisions, it suggests that our estimates of hypothetical bias could be considered upper-bound estimates.

[6]Ideally, we would include RP data from communities other than Salt Lake City. By doing so, we would improve our ability to detect hypothetical bias in two ways. First,

variation in recycling fees across communities would allow us to identify the scale parameter ($1/\sigma$) in our likelihood function and thus directly estimate the *magnitude* of potential hypothetical bias. Second, we would be able to make more precise statements about the extent of potential hypothetical bias at other bid levels. Since we employ SP and RP data exclusively at the $0 level (recall that there is no monthly fee for SLC households), the estimates of hypothetical bias are only suggestive of the degree of bias at other bid levels. Unfortunately, our inter-household comparison precludes us from using RP data from communities other than SLC because our sample contains only three other communities with *voluntary* CRPs. Two of the three are relatively small communities and as a result of proportionate sampling, we have insufficient observations for econometric analysis. In the third community, the voluntary CRP is substantially different from the one described in the hypothetical program and therefore these responses are not directly comparable to the SP responses.

[7]Copies of the survey instruments are available upon request.

[8]OSRL uses random-digit-dialing to select households into the sample, and the computer-aided-telephone-interviewing system (CATI) for interviewing and recording responses (http://darkwing.uoregon.edu/~osrl/).

[9]Ultimately, only households stating values between $2 and $10, which conform to the range of opening bids for the hypothetical CRP, are included in the econometric analysis.

[10]Although approximately 1,000 households were sampled, only 876 provided usable information for the econometric analysis.

[11]We also estimated equation (1) using only the first WTP question to form a single-bounded dichotomous choice model. Our primary econometric results are robust to this change.

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Table 1. Estimation Results for the DBDC WTP Model (n= 876)

Explanatory Variables	Descriptive Statistics		Estimates	
	Mean	Sample Size	Coefficient	P -Value
Ethical Duty	0.8978	871	3.184***	(0.000)
Monetary	0.7971	818	1.112	(0.137)
Primarily Ethics	0.6844	583	1.235***	(0.004)
Dropoff	0.8151	795	0.574	(0.121)
Dropoff User	0.3787	647	-0.926**	(0.015)
Young	0.3196	876	2.643***	(0.000)
Old	0.1450	876	-1.850***	(0.001)
Male	0.3619	876	-0.900***	(0.008)
High School	0.2592	872	-0.186	(0.404)
Associates	0.3704	872	-0.126	(0.434)
Bachelors	0.2076	872	0.424	(0.304)
Graduate	0.0940	872	1.864**	(0.029)
Household Size	3.2255	869	0.258***	(0.006)
Environ. Org.	0.0708	876	1.291**	(0.039)
Med Income	0.4203	759	0.057	(0.459)
High Income	0.3953	759	0.608	(0.154)
Cheap Talk	0.5000	258	-0.587	(0.159)
Fee Understated	0.5614	22	-1.667*	(0.054)
Fee Overstated	4.1400	84	0.403**	(0.037)
Fee Known	0.4473	237	-1.052	(0.152)
Heteroscedasticity Variables				
Constant	1.0000	876	2.434***	(0.000)
Cheap Talk	0.5000	258	-0.422**	(0.011)
Bid	5.0171	876	0.126***	(0.000)
Very Sure	0.4747	375	0.000	(0.499)
Somewhat Sure	0.3200	375	-0.089	(0.334)
Not Sure	0.0933	375	-0.385	(0.140)
Mean WTP			$7.00 / month	

Notes. (***), (**), and (*) refer to statistical significance at the 1%, 5%, and 10% levels, respectively. The dependent variable is WTP. The results for variables such as the constant, "Don't Know" and community dummies are not shown. The heteroscedasticity and overall likelihood ratio statistics are 65.11 and 201.70, respectively. McFadden's pseudo R^2's is 0.080. The number of correct predictions is 41%. The varying sample sizes for the means reflects the elimination of "Don't Know" responses, refusal to answer, or households that did not receive the survey question.

Table 2. Estimation Results for Inter-Household Detection of Hypothetical Bias

Explanatory Variables	Coefficient	P-Value
Ethical Duty	1.324***	(0.000)
Monetary	0.246	(0.275)
Primarily Ethics	0.009	(0.486)
Dropoff	-0.302	(0.119)
Dropoff User	-0.610***	(0.002)
Young	-0.050	(0.412)
Old	0.027	(0.453)
Male	-0.160	(0.181)
High School	-0.114	(0.366)
Associates	0.138	(0.335)
Bachelors	0.138	(0.347)
Graduate	0.158	(0.338)
Household Size	-0.022	(0.327)
Med Income	0.065	(0.383)
High Income	0.173	(0.233)
Fee Known	-0.097	(0.355)
Fee Overstated	-0.049*	(0.054)
SP	0.495*	(0.062)
	Marginal Effect	P-Value
Prob(WTP* > $0 | SP) – Prob(WTP* > $0 | RP)	0.170**	(0.049)

Notes. (***), (**), and (*) refer to statistical significance at the 1%, 5%, and 10% levels, respectively. The estimates are obtained via probit estimation for the decision whether to participate in a CRP. The estimated coefficients associated with community dummy variables are excluded. The number of RP and SP observations are 165 and 170, respectively. Standard errors for the marginal effect are calculated using the delta method.

Table 3. Interactive Cheap Talk Estimated Coefficients

	Interaction Variables							
	None	Ethical Duty	Primarily Ethics	Young	Female	Graduate	Environ. Org.	Dropoff User
Coefficients	-0.587	-0.832*	-1.274*	-0.606	-0.922	-4.143**	-8.096**	-0.380
P Values	0.159	0.096	0.077	0.273	0.105	0.050	0.012	0.364

Notes. (***), (**), and (*) refer to statistical significance at the 1%, 5%, and 10% levels, respectively. The model is the same as reported in Table 2 except the cheap-talk binary variable is replaced with interactive variables. The respective omitted categories are the variables interacted with those that did not get cheap talk.