Cheap Talk Reconsidered: New Evidence From CVM

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Abstract. Two recent studies have shown that "cheap talk" is an effective means of eliminating positive hypothetical bias in experimental and field-auction settings. We further investigate the ability of cheap talk to mitigate positive hypothetical bias in a CVM phone survey administered to over 4,000 households. Positive hypothetical bias is detected in our data by contrasting revealed and stated preference information. However, a short, neutral cheap-talk script appears to exacerbate rather than mitigate the bias. Based on this and mixed evidence from earlier studies, we suggest caution in using cheap talk as an *ex ante* control for hypothetical bias.

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1. Introduction

The contingent valuation method (CVM) is a widely used approach for estimating the value of goods and services when market information on equilibrium prices and quantities is either unavailable or unreliable. CVM has been employed by courts and government agencies such as the U.S. Environmental Protection Agency, the National Oceanographic and Atmospheric Administration, and the U.S. Fish and Wildlife Service to assess the benefits of policies impacting the environment and damages from environmental disasters. Researchers often estimate these values through surveys that ask individuals to place a monetary value on the hypothetical provision of a good or service. Since provision of the good and the associated payment are purely hypothetical, the reliability and validity of information obtained from CVM has been the subject of lively debate (Diamond and Hausman, 1994; Hanneman, 1994). Proponents of CVM have attempted to develop new methodologies that either (1) mitigate *ex ante* any hypothetical bias (i.e., bias associated with the respondent misstating her willingness to pay (WTP) due to the hypothetical nature of the good and payment method), or (2) calibrate the welfare estimates *ex* post (List and Shogren, 1998).

Recently, Cummings and Taylor (1999) and List (2001) have advocated the use of "cheap talk" to mitigate the effects of hypothetical bias in CVM. In the context of CVM, cheap talk refers to explicit warnings about the problem of hypothetical bias provided prior to respondents' valuation of the good. Cummings and Taylor (CT hereafter), in a series of lab experiments with students, find that cheap talk successfully eliminates hypothetical bias in valuation responses for a variety of public goods. List (2001) tests a similar script for private goods using sportscard auctions and finds that cheap talk is effective in eliminating hypothetical bias for non-dealers, but not for dealers. The cheap-talk scripts used in both of these studies are almost identical in

length and content. They each provide lengthy descriptions of *positive* hypothetical bias (i.e., upward bias in the valuation responses of those facing hypothetical rather than actual payment).

In order for cheap talk (such as that applied by CT and List) to be a useful design element in CVM surveys, the script needs to be general so that, unlike *ex post* calibration, it can be easily applied across a wide array of non-market goods without requiring *ex ante* information on the degree of hypothetical bias. Unfortunately, the scripts used in CT and List (2001) are not easily generalized.[1] Both scripts refer to a baseline degree of hypothetical bias by comparing the outcomes of preliminary experiments with hypothetical and real payment mechanisms for the goods in question. In CVM research, such prior information regarding the degree of hypothetical bias is typically unavailable or too expensive to produce in the field.

Our research addresses this concern by testing a version of cheap talk that can easily be generalized to other goods or services. We administered a telephone survey to over 4,000 households regarding their WTP for a curbside recycling service. Unlike the pure public goods used in the CT experiments and the private good in List's field auctions, curbside recycling can be considered an impure public good because it provides both potential private benefits (in the forms of added convenience relative to dropoff recycling, reduced garbage fees, and a "warm glow" from helping the environment), and public benefits by reducing the stream of waste going to landfills (Andreoni, 1990). As part of our survey design, we randomly assign our sample into three groups – the first receiving no cheap talk, the second receiving a short-script version of cheap talk, and the third receiving cheap talk with a reminder of budget constraints and substitutes. Aside from using a shorter script (to be compatible with phone surveys), the primary

[1]In addition to their main script, CT report similar results using a modified script that replaces the specific percentages of people in previous studies who voted "yes" for hypothetical and real referenda, with a statement indicating that "on average" more people voted "yes" for hypothetical referenda (see CT, pages 659-660). Although the modified script is more general in the sense of not reporting the magnitude of hypothetical bias, it still informs the subjects that the bias is positive.

difference between our cheap-talk scripts and those used by CT and List is the manner in which hypothetical bias is described to the survey respondents. While CT and List state that hypothetical bias leads people to *overstate* their true WTP, the cheap-talk scripts in our survey are purposefully neutral. Instead, they inform respondents that hypothetical bias leads people to *misstate* their true WTP.

We crafted neutral cheap-talk statements for two reasons. First, we wished to avoid criticisms that we are simply layering on another type of bias, one that is independent of the observed hypothetical bias itself.[2] Second, researchers typically do not know *ex ante* whether hypothetical bias will exist or if it will be positive or negative (Dickie et al., 1987; Adamowicz, et al., 1994; Carson et al., 1996; Nestor, 1998; Haab et al., 1999). Unlike CT and List, we had no prior evidence regarding the direction and magnitude of hypothetical bias for our population. Therefore, we decided to err on the side of caution so as not to induce any additional bias. Much to our surprise, this seemingly innocuous change in verbiage from a directed to a neutral script caused respondents receiving cheap talk to state *higher* WTP than those not receiving cheap talk. This counterintuitive result is robust across models, types of households, and types of recycling programs. As a result, we are led to question the efficacy of cheap-talk statements in mitigating hypothetical bias in CVM surveys.

The next section provides examples of the cheap-talk designs used in the previous studies. In Section 3, we briefly describe the survey and cheap-talk scripts used in our study. Section 4 reports our empirical evidence on hypothetical bias and cheap talk. Section 5 summarizes our overall findings.

[2] This hypothesis of a "layering effect" is refuted by CT for one of their sub-groups. However, we feel that it is still an open question, particularly for CVM surveys.

2. Previous Use of Cheap Talk

CT are the first to use the game-theoretic terminology "cheap talk" in the context of CVM. Cheap talk differs from standard reminder statements about substitutes and budget constraints in that the script explicitly warns respondents about the potential problem of hypothetical bias. Loomis et al. (1994) and Neil (1995) find that short-scripted reminder statements (without cheap talk) are ineffective in altering respondents' stated WTP for a public good in a hypothetical setting. However, CT find that a cheap-talk script openly discussing the existence of positive hypothetical bias prior to voting on public good referenda eliminates the bias in an experimental setting, in the sense that the results from the cheap-talk and actual referenda are statistically indistinguishable.[3] An excerpt from CT's cheap-talk script is given below

> *... in a recent study, several different groups of people voted on a referendum just like the one you are about to vote on. Payment was hypothetical for these groups, as it will be for you. No one had to pay money if the referendum passed. The results of these studies were that on average 38 percent of them voted "yes". With another set of groups with similar people voting on the same referendum as you will vote on here, but where payment was real and people really did have to pay money if the referendum passed, the results on average across the groups were that 25 percent voted yes. That's quite a difference, isn't it?*
>
> *We call this a "hypothetical bias." Hypothetical bias is the difference that we continually see in the way people respond to hypothetical referenda as compared to real referenda…*

List reads a nearly identical cheap-talk script to market participants in a field auction for sportscards. Similar to CT, he finds that cheap-talk statements are effective in eliminating hypothetical bias, but only for ordinary consumers (non-dealers). Dealers, who presumably have more market experience in valuing sportscards, are not influenced by cheap talk.

[3] Two other studies – Loomis et al. (1996) using a short script somewhere between a reminder statement and cheap talk and Murphy et al. (2003) using CT's cheap talk script – find that while these statements do not eliminate hypothetical bias, they do reduce it.

Poe et al. (2002) find that the following short script:

> *I have one caution though. For programs like this it's often the case that more people say they would sign up than actually do sign-up. Utilities in other parts of the country have found that eight times as many people say yes to similar programs as actually take part in them. With this in mind…*

does not have a statistically significant effect on the participation decisions of individuals valuing green power and tree planting in New York via a provision-point mechanism CVM survey. Aadland and Caplan (2003) also employ a shorter version of cheap talk than CT and List. Similar to List, they find that the effectiveness of cheap talk varies by type of household. In particular, those households who might be expected to suffer the most from positive hypothetical bias (e.g. those motivated to recycle by an ethical duty or who are members of an environmental organization) also tend to lower their stated WTP the most in response to cheap talk. Their cheap-talk script reads

> *… studies have shown that many people say they are willing to pay more for curbside recycling than they actually will pay when it becomes available in their community. For this reason, as I read the next two curbside recycling fees, please imagine your household actually paying them.*

To our knowledge, this is the first evidence that short-script cheap-talk statements can be effective in mitigating positive hypothetical bias in CVM surveys.[4]

[4] In a related working paper, Cummings et al. (1995) find that while "heavy" cheap talk tends to offset positive hypothetical bias, "light" cheap talk actually tends to increase the upward bias in a public good valuation experiment. We note, however, that their light version is still a much longer script than those used by Poe et al. (2002) and Aadland and Caplan (2003).

3. Cheap-Talk and Survey Design

During the winter of 2002, we conducted a telephone survey about recycling behavior to over 4,000 households in 40 western U.S. cities with populations over 50,000.[5] Our primary goal was to estimate household WTP for curbside recycling, controlling for a host of household-, program-, and community-specific characteristics. We described the following hypothetical curbside recycling program (CRP) to households who either did not have a CRP in their community or were unaware of its existence:

> *At the beginning of the survey, you said that your community does not currently have a curbside recycling service. For the next few questions, please imagine that you COULD have a service that regularly collects aluminum cans, cardboard, glass, paper, plastic and tin cans. Your household (would/would not) need to sort your recyclables into separate bins and pay a fee for the recycling service, in addition to your current monthly garbage collection fee. Now we are going to ask you some questions about your household's willingness to pay for this type of curbside recycling service.*

After describing the CRP, we then randomly assigned respondents with equal probability to one of three groups. The first group received no cheap-talk statement and proceeded directly to the valuation questions. The second group received the following short-scripted cheap talk statement, which was read prior to the ensuing double-bounded dichotomous-choice (DBDC) WTP questions:

> *As you prepare to answer the next few questions, please keep in mind that in previous surveys we have found that the amounts that people SAY they are willing to pay for curbside recycling are sometimes different from the amounts that they would ACTUALLY be willing to pay when curbside recycling became available in their community. For this reason, as I read the following curbside recycling fees, please imagine your household is ACTUALLY paying them.*

[5] The survey was administered by the survey research laboratory at Washington State University. A list of the 40 cities in our sample is available from the authors by request.

This cheap-talk script is substantially shorter than that used by CT and List (2001) – so as to be compatible with a phone survey – and intentionally does not take a stand regarding the direction of hypothetical bias. The third group of households received the following "long" cheap-talk script, which added explicit reminders to the household about budget constraints and alternatives to recycling:

> *As you prepare to answer the next few questions, please keep in mind the following three things. First, keep in mind your household budget. In a typical month, at what price would your household be able to afford curbside recycling? Second, keep in mind that there are alternatives to curbside recycling such as recycling drop-off centers and landfills. And third, keep in mind that in previous surveys we have found that the amounts that people SAY they are willing to pay for curbside recycling are sometimes different from the amounts that they would ACTUALLY be willing to pay when curbside recycling became available in their community. For this reason, as I read the following curbside recycling fees, please imagine your household is ACTUALLY paying them.*

Our decision to test variations in script length and reminders about budget constraints and substitutes is motivated by mixed results in the literature. As mentioned above, CT find that long-scripted cheap talk is effective in an experimental setting. List (2001) finds that similar long-scripted cheap talk is effective in field auctions for dealers but ineffective for non-dealers. Similar to List, Aadland and Caplan (2003) find a short-scripted version of cheap talk is effective only for certain types of households. Poe et al. (2002) report that short-scripted cheap talk is ineffective in eliminating hypothetical bias. Loomis et al. (1994) and Neil (1995) find that reminders about budget constraints and substitutes are also ineffective. In sum, the evidence regarding the effectiveness of various cheap-talk and reminder statements is anything but clear. Our survey was designed to help sort out the patterns between cheap talk, reminder statements, and hypothetical bias.

4. Empirical Results

In this section, we report three sets of empirical results. The first set documents the existence of positive hypothetical bias in our data. That is, we find that households who are making a hypothetical decision to participate in a CRP are, all else equal, more likely to participate than households who have made an actual decision of whether to participate in a real CRP. Next, we document the unconditional and conditional effects of cheap-talk scripts on households' responses. For our unconditional evidence, we contrast stated participation rates for the CRP referendum with and without cheap talk. The advantage of examining the unconditional participation rates is that they are transparent and are not dependent on any particular econometric model. For our conditional evidence, we report the coefficient estimates associated with cheap-talk dummy variables from an econometric model where we control for a host of potential differences across treatments and groups.

4.1 Estimates of Hypothetical Bias

We begin by documenting the existence of positive hypothetical bias in our CVM responses. Toward this end, we compare the participation decisions of households who are voting on a hypothetical CRP referendum with the actual participation decisions of households in communities with existing voluntary CRPs.[6] Our survey was designed to facilitate such a comparison by choosing appropriate bid values and describing hypothetical CRPs that are similar to existing voluntary programs in our sample.[7]

Our discussion of the econometric model used to document the existence of positive hypothetical bias is purposefully brief, as it closely follows the procedure outlined in Cameron

[6] By "voluntary" we mean that the household pays for the program only if it has signed up for it.
[7] Since households in our sample are spread across 40 western U.S. cities, we adjust CRP fees and income levels for differences in costs of living using the city comparison calculator at http://list.realestate.yahoo.com/re/neighbor/.

(1988). WTP questions are set in the DBDC format to elicit a household's WTP through a sequence of 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 the integers \$2 through \$10 in order to mitigate the effects of starting point bias. 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). Those who answer "no" to the first two questions are then asked whether they would participate in a program that is free of charge. Based on the responses to the opening bid and follow-up questions, the respondent's latent WTP may be placed in one of five regions: $(-\infty,0)$, $(0,\tau_L)$, (τ_L,τ), (τ,τ_H) or (τ_H, ∞).

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

$$WTP_i^* = X_i\beta + \varepsilon_i , \tag{1}$$

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

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

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

To detect hypothetical bias we pool respondents who have made actual participation decisions in communities with voluntary CRPs with respondents who have valued similar

hypothetical CRPs at similar bid values.[8] We then estimate a simple (random-threshold) probit model for the decision of whether to participate in a CRP, while controlling for a host of demographic, program, and community attributes such as age, education, income, employment status, motivation for recycling, degree of sorting required for CRP, availability and use of substitutes to recycling, etc.[9] Our null hypothesis of no hypothetical bias is tested by observing the statistical significance of the coefficient on the dummy variable for whether the participation decision is hypothetical (i.e., based on the household's valuation of the hypothetical program) or real (i.e., based on the household's actual decision of whether to participate in its community's existing CRP). If we find that there is no statistical difference between the participation rates of households in communities with voluntary CRPs and those hypothetically valuing a CRP then, all else equal, we conclude that hypothetical bias is unlikely to be a problem in our population. If instead, the coefficient on the hypothetical dummy variable is positive and statistically significant, we conclude that positive hypothetical bias is likely to exist, and its average level is measured by the value of the coefficient. The estimation results are shown in Table 1.

[INSERT TABLE 1 HERE]

The signs and significance levels of the demographic, program- and community-specific variables are similar to the results in Aadland and Caplan (2003). We therefore focus on the result germane to this section – the sign and magnitude of the estimated coefficient on the dummy variable for whether the participation decision was hypothetical (HYPOTHETICAL).

[8] More precisely, our pooled dataset is created by first including all households that know a voluntary CRP exists in their community (and therefore have revealed their preferences via the decision of whether to participate). These CRPs vary in terms of their monthly cost-of-living-adjusted fees (which are roughly between $1 and $5 per month), and whether they require sorting of the recyclables. Second, we include all households that valued hypothetical sorting and non-sorting CRPs with opening bids that were within the $1 to $5 interval for existing voluntary CRPs. Although our WTP questions are set in the DBDC format, we consider only opening bids so that the design of the hypothetical decision mimics the actual decision as closely as possible. The pooled dataset contains 1,782 households – 994 revealed preference and 788 stated preference.

[9] Detailed definitions of all the variables used in this analysis are included in the Appendix.

The coefficient is statistically significant and indicates that, all else equal, respondents valuing a hypothetical CRP stated a maximum WTP that is, on average, $3.39 more than those valuing an existing voluntary CRP. Stated in terms of likelihood of participation, the respondents for the hypothetical CRP are 17.7 percent more likely to participate than those valuing an existing CRP. This suggests that positive hypothetical bias is present in our data. Additional evidence of positive hypothetical bias can also be inferred from the unconditional participation rates presented in Table 2 of the next section, to which we now turn.

4.2 Unconditional Cheap-Talk Effects

Does cheap talk mitigate the positive hypothetical bias in our data? In Table 2 we report average rates of participation across bid levels, cheap-talk scripts, and types of CRP (actual or hypothetical). It is important to note that unlike the analysis in the previous section, we are not interested in the actual participation decisions of households residing in communities with voluntary CRPs. Instead, we focus on how cheap talk influences the stated maximum WTP of households that are either placing a value on the existing CRP in their community or the hypothetical CRP.

[INSERT TABLE 2 HERE]

Before discussing the effects of cheap talk, notice that, as expected, participation rates generally decline as the bid levels increase. Also, notice that for the cases where no cheap talk is given (i.e., No C-Talk columns), the participation rates are higher (51.0 versus 40.3 percent) when respondents are valuing a hypothetical, as opposed to an actual, CRP. Although this is not a pure test of hypothetical bias as in Section 4.1, it lends further credibility to the existence of positive hypothetical bias in our data.

Based on the unconditional participation rates in Table 2, there is strong evidence that our neutral cheap-talk statements are associated with *higher* rates of participation and WTP for curbside recycling. The positive effect of cheap talk appears to be strongest for households who are valuing an existing CRP in their community (as opposed to the hypothetical CRP described in our survey), and who received the *longer* script with cheap talk and a reminder about substitutes and budget constraints (52.7 versus 40.3 percent). For the case of households valuing the hypothetical CRP, there is no statistical difference between the participation rates of those who received the short cheap-talk script and those who did not receive any cheap talk. Nevertheless, considering the large overall sample size (N = 4268), it is clear that there is absolutely no evidence that cheap talk (either with or without a reminder about substitutes and budget constraints) is effective in mitigating the positive hypothetical bias in our data. To the contrary, cheap talk appears to exacerbate the bias!

We now offer a few potential explanations for this counterintuitive result. To begin, it could be argued that by including the word "landfills" in our long-script version we unwittingly created an environmental "flashpoint", triggering images of overflowing garbage dumps in the minds of respondents who then reacted by inflating their WTP responses. The problem with this hypothesis, however, is that it cannot completely explain our results, as those valuing an actual CRP and receiving short-scripted cheap talk (without the landfill reference) also tend to be more likely to participate (49.5 versus 40.3 percent – see Table 2 – and, as we will see in Section 4.3, to state conditionally higher values than those not receiving any cheap talk.

A second possible explanation, provided by Cummings et al. (1995), is that cheap talk "might assure those doubting the hypothetical nature of the experiment that it is indeed hypothetical" and as a result encourages positive hypothetical bias. The difficulty with this

explanation is in understanding why additional positive bias is elicited from some scripts but not others. A third possibility is that when they hear a neutral cheap-talk script stating only that "people tend to misstate their true WTP," as opposed to the standard script stating that "people tend to overstate their true WTP," survey respondents may be induced to rely more heavily on their own heuristics and inferences in an attempt to guess what type of bias the researchers have in mind. If this hypothesis is true, our results indicate that respondents may be guessing that we would expect them to correct this bias by stating higher WTP values.[10]

Each of these hypotheses and our empirical results suggest that we simply do not understand how the human cognitive process receives and then reacts to signals such as cheap talk. Several theories of how human cognition reacts to signals are discussed in Fischhoff (2002). As Fischhoff points out, artifacts (such as unexpected responses to cheap talk) could emanate from "the subtle ways that interviewers communicate their expectations." He also notes that "...elicitation is a reactive measurement procedure...The process assumes that people sometimes need help, in order to understand what they believe and want. That help may include presenting a balanced selection of opinions, lest clients miss a critical perspective just because it did not occur to them at the time." It is possible that in erring on the side of conciseness, our short but balanced cheap-talk script provided insufficient detail regarding "selection of opinions," resulting in unpredictable effects on WTP.

As a final note, we acknowledge that our counterintuitive cheap-talk results could simply be due to systematic differences in the treatment and control groups. This seems unlikely, however, as the cheap-talk scripts were assigned randomly across such a large number of households and

[10] Alternatively, respondents may be reacting to what they believe the university-sponsored interviewer wants to hear, thus creating a "social desirability bias" in favor of curbside recycling. We thank John Loomis for this insight.

communities. Nevertheless, for the sake of thoroughness, we now turn to a conditional analysis of cheap talk's effects.

4.3 Conditional Cheap-Talk Effects

Our conditional estimates are based on the DBDC model first introduced by Carson et al. (1986) and applied in Aadland and Caplan (2003). We use maximum likelihood to estimate the full model that incorporates the responses to both the initial and follow-up bids. As in the participation probit model in Section 4.1, we control for a number of demographic and community attributes.[11] Based on our earlier work and List (2001), we also estimate models that allow for differential cheap-talk effects across household characteristics.

The results from this exercise are reported in Table 3. The first row of the Table 3 shows the effect of short and long cheap talk across all households that are valuing either a hypothetical or an actual CRP. The remaining rows report the results of the two cheap-talk scripts interacted with certain demographic groups. Consistent with the unconditional cheap-talk results in the previous section, all of the coefficients are either positive or not statistically different than zero. Overall, survey respondents are more sensitive to cheap talk when they are valuing an actual CRP and when cheap talk is accompanied by a reminder about substitutes and budget constraints.

[INSERT TABLE 3 HERE]

Furthermore, households with at least one member holding a Ph.D. or equivalent professional degree and who received short (long)-scripted cheap talk are, all else equal, willing to pay approximately $1.07 (1.38) more per month for an actual CRP than their counterparts who did

[11] The estimated coefficients on these control variables are similar in sign and significance to the results in Table 1, and therefore are omitted.

not receive short (long)-scripted cheap talk. Similarly, respondents who belong to an environmental organization and who received the short script are willing to pay an additional $1.54 per month, on average, while those who received the long script are only willing to pay an additional $1.45 per month. In sum, our results indicate that cheap-talk statements may be ineffective or even counter-productive in mitigating hypothetical bias, with the degree of the problem varying across types of households.

5. Conclusion

The evidence from our CVM survey draws into question the efficacy of cheap talk as a reliable and robust *ex ante* correction mechanism for positive hypothetical bias. Although initial research has shown that a long-scripted version of cheap talk can be effective in eliminating this bias in lab experiments and field auctions, shorter and more neutrally worded scripts tailored for phone interviews have clearly demonstrated a sensitivity to script length and content. Indeed, we first establish the existence of positive hypothetical bias by contrasting revealed and stated preference information, but then find that our neutral cheap-talk scripts actually exacerbate the problem. Moreover, the degree of exacerbation seems to increase with script length and with respect to household characteristics (e.g., education, environmental advocacy, etc.) that are likely to be systematically related to the degree of observed hypothetical bias.

A potential criticism of our cheap-talk design is that we did not *a priori* establish a baseline degree of hypothetical bias in our data. Had we known the extent of positive hypothetical bias in our data beforehand, we could have chosen a script informing respondents of the direction and (possibly the magnitude) of the bias, rather than using a neutral script. CT, List, and Aadland and Caplan (2003) report some success with these types of directed scripts. However, the

primary attraction of cheap talk as an *ex ante* control for hypothetical bias is its apparent generalizability – in the form of a standard script – to CVM studies across a wide array of non-market goods and services. Our findings suggest that standardized cheap-talk scripts can produce undesirable results. As a result, we feel caution is warranted in using cheap talk to correct for hypothetical bias until we better understand how the length and content of cheap talk statements influence the cognitive processes of survey respondents.

Appendix

Explanatory Variables	Description
Ethical Duty	Do you feel an ethical duty to recycle to help the environment? 1 = yes, 0 = no.
Monetary	Are you motivated to recycle in order to save money? For example, are you able to use a smaller garbage container because you recycle or you get money for your aluminum cans? 1 = yes, 0 = no.
Primarily Ethics	Which one would most encourage your household to recycle: an ethical duty to help the environment, or saving money? 1 = ethical duty, 0 = save money.
Dropoff Distance	Distance in miles to the nearest dropoff site.
Dropoff User	In the past 12 months has your household taken any recyclable materials to one of your community's drop-off recycling centers? 1 = yes, 0 = no.
Young	1 if 18<Age<35, 0 otherwise.
Old	1 if 65<Age, 0 otherwise.
Male	1 = male, 0 = female.
High School	What is the highest level of education attained by any member of your household? 1 = high school graduate, 0 = otherwise
Associates	1 = associates degree, 0 = otherwise
Bachelors	1 = bachelors degree, 0 = otherwise
Masters	1 = masters degree, 0 = otherwise
Ph.D.	1 = Ph.D. or equivalent professional degree, 0 = otherwise
Household Size	Number of adults currently living in the household who are older than 18 years of age, other than the respondent.
Environmental Organization	Does anyone in your household belong to an environmental club, group, or organization? 1 = yes, 0 = no.
Med Income	1 if \$35K/yr<Household Income<\$75K/yr, 0 otherwise
High Income	1 if \$75K/yr<Household Income, 0 otherwise
Employed	Is the adult with the highest income currently working for pay, either full time or part time? 1 = yes, 0 = no.
Retired	Is the adult with the highest income currently retired? 1 = yes, 0 = no.
Short Cheap Talk	1 = received short cheap-talk statement, 0 otherwise.
Longer Cheap Talk	1 = received longer cheap-talk statement, 0 otherwise.
Sorting Required	1 = CRP requires some sorting of recyclable materials, 0 otherwise.
Polite	1 if polite refusal for first call attempt, 0 otherwise.
Angry	1 if angry refusal for first call attempt, 0 otherwise.
Certainty	On a scale from 1 to 100, how certain are you to the last WTP question?
Landfill Visit	Has anyone in your household ever visited your community's landfill? 1 = yes, 0 = no.
Landfill Distance	Distance to nearest landfill in miles.
Landfill Distance > 2 mi.	Distance to nearest landfill in miles if greater than 2 miles, 0 otherwise.
Hypothetical	1 = respondent valued a hypothetical CRP, 0 = otherwise.

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Table 1. Random-Threshold Probit Model For CRP Participation (N = 1782)

Explanatory Variables	Estimates	
	Coefficient	P –Value
Ethical Duty	5.380***	0.000
Monetary	-3.051***	0.000
Primarily Ethics	1.848***	0.005
Dropoff Distance	0.106	0.145
Dropoff User	-0.785	0.112
Young	1.136**	0.018
Old	-1.769**	0.029
Male	-0.249	0.281
High School	1.401	0.160
Some College	1.259	0.182
Associates	1.944*	0.096
Bachelors	2.383**	0.047
Masters	2.927**	0.028
Ph.D.	2.824**	0.040
Household Size	0.066	0.387
Environmental Organization	2.125***	0.007
Med Income	-0.077	0.450
High Income	0.226	0.368
Employed	1.147	0.111
Retired	1.872*	0.058
Short Cheap Talk	0.828	0.144
Longer Cheap Talk	2.336***	0.006
Sorting Required	-1.886***	0.003
Polite	-1.661***	0.008
Angry	1.683	0.252
Landfill Visit	0.315	0.265
Landfill Distance	-10.098*	0.084
Landfill Distance > 2 mi.	9.923*	0.087
Certainty	-0.002	0.412
Hypothetical	3.385***	0.000
Heteroscedasticity Variables	Coefficient	P –Value
Constant	3.392***	0.000
Bid	0.101	0.162

Notes. (***), (**), and (*) refer to statistical significance at the 1, 5 and 10 percent levels respectively. The dependent variable is participation in a CRP. The estimates for the constant term, as well as binary variables for "don't know" and missing responses are not shown.

Table 2. Unconditional Participation Rates Across Cheap-Talk Scripts (N = 4268)

		Hypothetical CRP				Actual CRP			
		No C-Talk	Short C-Talk	Long C-Talk	N	No C-Talk	Short C-Talk	Long C-Talk	N
COLA-adjusted Opening Bids ($/month)	≤ 2	58.3	63.0	63.0	109	61.9	75.7**	82.3***	195
	(2, 3]	64.0	64.9	77.5**	243	65.9	71.2	65.8	306
	(3, 4]	49.3	72.6***	77.9***	234	56.9	66.0	70.6**	298
	(4, 5]	61.8	55.7	65.4	233	39.8	59.6***	60.3***	328
	(5, 6]	60.0	52.2	62.4	222	44.9	51.6	55.6*	319
	(6, 7]	54.1	44.9	54.8	192	33.0	39.8	43.4*	272
	(7, 8]	39.1	38.2	36.8	213	24.4	32.4	41.7**	221
	(8, 9]	44.4	47.6	41.7	168	33.3	28.6	35.6	196
	(9, 10]	47.2	31.7	43.1	164	23.3	32.6	41.9**	132
	> 10	30.8	43.2	41.9	132	19.2	37.1*	30.0	91
Totals		51.0	51.4	56.4**	1910	40.3	49.5***	52.7***	2358

Notes: *, ** and *** indicate significantly different than No C-Talk at the 10, 5 and 1 percent significance levels respectively. Estimated variances for the differences in proportions are calculated as described in Freund (1992, p. 414-6). CRP ≡ Curbside Recycling Program. C-Talk ≡ Cheap Talk. COLA ≡ Cost of Living Adjusted.

Table 3. Conditional Estimates of Cheap-Talk Scripts on Maximum WTP (N = 4253)

Interactive Terms	Cheap-Talk Coefficients			
	Hypothetical CRP		Actual CRP	
	Short C-Talk	Long C-Talk	Short C-Talk	Long C-Talk
None	0.188 (0.256)	0.584** (0.259)	0.486** (0.227)	0.794*** (0.223)
Young	0.403 (0.437)	0.647* (0.436)	0.796** (0.433)	1.037*** (0.419)
Female	-0.286 (0.342)	0.272 (0.337)	0.582** (0.290)	0.973*** (0.287)
Env. Org.	0.674 (0.989)	0.789 (1.018)	1.539** (0.754)	1.454** (0.758)
Bachelors	0.048 (0.459)	0.751* (0.482)	1.051* (0.748)	-0.100 (0.748)
Ph.D.	0.485 (0.944)	0.578 (0.952)	1.074* (0.799)	1.383** (0.767)
Sort	0.237 (0.366)	0.720** (0.372)	0.323 (0.363)	0.819** (0.363)
Ethical Duty	0.312 (0.285)	0.579** (0.284)	0.435** (0.238)	0.825*** (0.235)
Certainty ≥ 90	0.307 (0.316)	0.712** (0.322)	0.631** (0.277)	0.935*** (0.270)

Notes: *, ** and *** indicate significantly different than No C-Talk at the 10, 5 and 1 percent significance levels respectively. Standard errors are in parentheses. CRP ≡ Curbside Recycling Program. WTP ≡ Willingness To Pay. C-Talk ≡ Cheap Talk.