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Hypothetical Bias in Choice Experiments: Is Cheap Talk Effective at Eliminating Bias on the Intensive and Extensive Margins of Choice?

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Hypothetical Bias in Choice Experiments: Is Cheap Talk Effective at Eliminating Bias on the Intensive and Extensive Margins of Choice?

Abstract: We use an experimental approach to evaluate the effectiveness of the most commonly employed bias-mitigation tool in nonmarket valuation surveys: the cheap talk script. Our experimental design allows us to estimate treatment effects on two margins of choice separately: the decision to enter the market at all (the extensive margin) and the choices among alternatives offered (the intensive margin). The key result of this study is to show that a cheap talk script appears to affect both margins in ways distinctly different than when choices involve actual payments. Specifically, participants in hypothetical choice experiments including cheap talk are more inclined to enter the market but are also more price-sensitive as compared to when payments are real. Interestingly, the average influence of cheap talk on market participation and price-sensitiveness could result in total willingness to pay (WTP) estimates that are similar to real payment treatments since the two effects identified act in opposite directions when computing WTP. However, they may do so by inducing behavior that is distinctly different than those of consumers facing real choices. As such, future reliance on cheap talk as a bias mitigation tool requires extensive testing for empirical regularities to gain any confidence that the tool can be effective, and under what circumstances.

JEL Classifications: Q50, Q51

Keywords: Cheap Talk, Choice Experiment, Hypothetical Bias

I. Introduction

A central focus of the nonmarket valuation literature continues to be the design of stated preference surveys capable of eliciting unbiased statements of household willingness to pay (WTP) for nonmarketed goods. Beginning with seminal work by Bohm (1972), a large literature has developed that draws upon experimental economics methods to identify the degree of hypothetical bias in stated preference surveys by comparing responses to hypothetical WTP questions to actions taken when the WTP questions have actual monetary consequences. Reviews of this literature indicate that hypothetical valuation statements generally exceed actual payments, and sometimes substantially so (see List and Gallet, 2001; Harrison and Rutstrom, 2005; and Murphy et al, 2005).

A natural extension to the literature employing experimental methods to examine hypothetical bias is to extend the methods to "test-bed" alternative survey designs and determine their efficacy in eliminating or reducing hypothetical bias (for early examples, Bjornstad, et al., 1997, and Cummings and Taylor, 1998). Perhaps one of the most tested mechanisms thus far is the "cheap talk" script developed by Cummings and Taylor (1999) in which survey participants are led through an extensive discussion of the meaning and underpinnings of hypothetical bias in stated preference surveys (see Jacquemet et al., 2011 and Horowitz et al., forthcoming, for a discussion). The robustness of the cheap talk script as written by Cummings and Taylor has met with mixed results in eliminating hypothetical bias.[2] While some studies have suggested that cheap talk scripts are effective at reducing or eliminating bias (e.g., Carlsson et al. 2005; Landry and List 2007; Mozumder and Berrens 2007), others have found that the effectiveness of cheap

[2] Note, we focus our discussion on studies that implement the "full version" of the cheap talk script as written by Cummings and Taylor. Many stated preference surveys include a short reminder or discussion of hypothetical bias or budget constraints. There is no consistency in these reminders across studies, nor has there been a systematic exploration of the efficacy of any one of these reminders on eliminating hypothetical bias.

talk scripts may be limited if the respondents have substantial experience purchasing the good(s) being valued (List, 2001), if the script is shortened and made more neutral (Aadland and Caplan, 2006), or if the monetary consequences of the decisions are small (Brown et al. 2003; Murphy et al. 2005).[3]

Common among the previous literature testing the efficacy of the cheap talk design is the use of a single-choice, contingent valuation elicitation format such as a voting referendum or a dichotomous choice WTP question. However, the nonmarket valuation literature has moved away from contingent valuation formats in recent years towards attribute-based methods, and in particular to choice experiments. In a choice experiment, a respondent is asked to choose among a set of alternatives that vary by the characteristics of the good offered and (usually) its price. Choices are usually over three or more alternatives, and each respondent typically repeats the choice task over a series of questions (for a review, see Louviere, et al., 2000; Bateman et al., 2002; Carson and Louviere, 2011).

The key feature of choice experiments is their ability to offer several choice options with varying characteristics to respondents. This design implies an important distinction between the decision to purchase (i.e., "enter the market") and the choice among the alternatives offered (List et al., 2006). By extension, the influence of cheap talk, or any bias-mitigation tool, may act on these two choice margins differentially: participants may react to the cheap talk script (or real treatment) by changing their decision to 'purchase' or 'not purchase' the offered good; by

[3] Researchers have also compared cheap talk scripts to other techniques for mitigating hypothetical bias such as certainty scales, consequentialism, training subjects in valuation tasks, and Bayesian truth serum (BTS), again with mixed results. For example, Barrage and Lee (2010) find cheap talk and BTS eliminate hypothetical bias only inconsistently but that consequentialism is effective. Blumenschein et al. (2008) find certainty scales are effective but cheap talk is not. See also Morrison and Brown (2009) who find cheap talk less effective than certainty scales or dissonance-minimization, while Jacquemet et al. 2011 find cheap talk more effective than training subjects.

changing their sensitivity to the attributes of the alternatives (especially the price of the good in question); or both.

Recently, List et al. (2006) explore the efficacy of cheap talk scripts within a choice experiment context.[4] They find cheap talk to be effective at generating credible estimates of purchase decisions. However, in these experiments price is not varied. Understanding the impact of bias mitigation tools on price responsiveness is key since this parameter is the basis for computing both marginal and total willingness to pay (WTP) for attributes of a nonmarketed good.

In this research, we report results from choice experiments that are designed to separately estimate the effects of cheap talk on both the decision to enter the market (the extensive margin of choice) and the decision among which alternative to purchase (the intensive margin of choice). We conduct three treatments: a purely hypothetical treatment, a hypothetical-with-cheap-talk treatment, and a real payment treatment.[5] Our experimental design allows us to test for differences in the overall propensity to purchase a tree across treatments separately from differences in the price sensitivity of subjects across treatments. This feature of our analysis is a key contribution of this study. Furthermore, we explore heterogeneity in the treatment effects across subjects based on observable characteristics.

Given that the goal of a cheap talk script is to induce respondents to reveal the choices they would have made had the choice required a real payment, we test the hypotheses that 1) choices made in the cheap talk and real treatments are different from choices made in the

[4] Carlsson et al. (2005) conduct a choice experiment and include a cheap-talk treatment, finding that estimated willingness-to-pay (WTP) for golden rice is lower when a cheap talk script is employed. However, their study does not include a real payment treatment for comparison.

[5] Taylor et al. (2010) and Carlsson and Martinson (2001) explore hypothetical bias within choice experiments by comparing responses elicited in hypothetical and real-payment choice experiments, but do not test bias-mitigating tools.

hypothetical treatment and 2) choices made in the cheap talk and real treatments are statistically indistinguishable from each other. Results indicate that there are important differences in the behavioral responses to cheap talk and real-payment treatments that may not be apparent in WTP calculations if each margin of choice is not separately analyzed. Specifically, our results indicate that the cheap talk script appears to affect both participant price sensitivity and participants' decisions to purchase in ways that are different than the real treatment. We show that participants in the cheap talk treatment appear to be 1) more price sensitive when choosing among the offered trees than in the real treatment, but 2) more inclined to purchase a tree overall (i.e., to "enter the market") in the cheap talk treatment than in the real treatment.

Our results raise important concerns about the use of cheap talk as a bias mitigation tool, especially when applied in nonmarket valuation tasks where no real-payment benchmark is available to the researcher for comparison (as is typically the case in policy-relevant applications). When computing willingness to pay for an attribute, the increase in price sensitivity in cheap talk treatments would tend to decrease WTP estimates, while the increased propensity to enter the market would have the opposite effect. Because these two factors influence implied WTP in opposite directions, it is possible that calculated WTP in cheap talk treatments might appear to be statistically indistinguishable from WTP in real payment treatments. However, the calculus masks the fact that the underlying behavioral processes leading to these outcomes are statistically different. This is an important finding for the conduct of stated preference surveys. To the degree our results are generalizable, the argument for using cheap talk as a bias mitigation tool choice experiments must rely on empirical regularities, not consistency in the cognitive/behavioral underpinnings of the approach.

The remainder of this paper is as follows. The next section provides a brief overview of the experimental design[6], and Section III provides an overview of the empirical models employed, hypotheses tested, and summary statistics for the data collected. Section IV reports the estimation results and Section V provides concluding comments.

II. Experimental Design

A total of 735 subjects participated in three treatments. All subjects were recruited through principles of economics classes at Georgia State University and surveys were conducted during regularly scheduled class-meetings. After completing a consent form, subjects read written instructions for the survey (referred to as a "decision-making experiment") while the experimenter read them aloud.

The instructions for all three treatments followed the same basic format. First, the introduction informed subjects that this was a decision-making experiment and that they would be asked if they wished to purchase a good. In both the baseline hypothetical and the cheap talk treatment, subjects were told that they were not actually being given the opportunity to purchase the good, but were asked that they answer all questions as if the survey were for real. For the real payment treatments, subjects were told that they were under no obligation to purchase the good unless they wished to do so.

Following the introduction to the survey, the public good was described and a sample choice question was presented. The public good used in our experiments is the planting of trees in public spaces of downtown Atlanta, Georgia through a local non-profit organization called *Trees Atlanta*. Subjects are informed that downtown Atlanta is ranked as having the least amount of downtown tree cover among cities in the U.S., and that the lack of trees, along with

[6] All experimental scripts are available upon request from the authors.

increased asphalt and reflective buildings, has contributed to an 8-10 degree increase in downtown summer temperatures since 1970. *Trees Atlanta* seeks to alleviate this "urban heat-island" effect through the planting and maintaining of trees in the downtown area. The organization has a "gift trees" program in which it will plant a tree in the metro-area for a contribution. Each donation leads directly to the planting of a tree during the current planting cycle (from October through March each year). Thus, the good we offer is to have *Trees Atlanta* plant a tree through the gift trees program.

We chose this good for several reasons. First, "the good" (planting of a tree) is deliverable. If subjects choose to purchase the good, it is credible that it will be provided in return for their payment. Second, the good is divisible in provision. Subjects are able to connect their specific payment with a specific amount of the public good provided. Third, we are able to alter the attributes of the public good and deliver any combination of the attributes we offer to subjects.

There are three possible types of trees that subjects could choose to have planted: Oak, Dogwood, or Crepe Myrtle. As part of the experiment instructions, subjects were shown color photographs of each tree at maturity and presented with a table that summarized a few of the important characteristics of each tree. The focus of the survey was on the shade potential of the trees at maturity, and the Oak tree has the largest shade potential (up to 60 feet across) while the Crepe Myrtle and Dogwood have 20 and 15 feet of shade potential upon maturity, respectively. The trees could be planted in one of two sizes: a small tree (5-feet) or a medium tree (8-feet). The cost to the subject for having a tree planted varied between $8 and $26.

After describing the good and its attributes, subjects were presented with a sample choice question offering two variants of the good (i.e., two trees that vary by type of tree, size of tree,

and cost of the tree) and an option to choose neither tree. Subjects were told they would answer eight questions just like the sample question, but where the attributes of the trees and their costs would be varied. Subjects in the real payment treatment are told in advance that one of the eight questions will be randomly chosen at the end of the experiment and whatever choice they made in that question is the one they will have to abide by. If they choose a good in the binding question, they will purchase it right then and there in the real payment treatment. An important aspect of our design is that payment to *Trees Atlanta* could be made by cash, check or credit card. Having the option to pay by check or credit card helps to relax a potential constraint that subjects would not have anticipated the need for cash-on-hand during class.

In the hypothetical treatments, the scripts followed the real-payment treatment as closely as possible, but were modified using subjunctive language such as "you would pay" and "we would provide you with a receipt." In addition, subjects are reminded at the end of the instructions that the experiment is hypothetical and that no payments will be made as a result of their decisions. In the cheap talk treatment, the cheap talk script as developed by Cummings and Taylor (1999) was inserted at the very end of the instructions. The section was titled "Final Discussion" and followed the script of Cummings and Taylor (1999) as closely as possible, only modifying it to fit the nature of the current choice question experimental designs. For instance, Cummings and Taylor used a simple majority-rule voting mechanism in their experiments, while ours is a choice experiment. As such, when discussing with subjects the differences in behavior observed between hypothetical and real-payment treatments, our cheap talk script refers to the difference in the proportion of subjects who purchase a tree – not the difference in the proportion that vote yes on a proposition to fund a project as in Cummings and Taylor. Full scripts for each treatment are available in an appendix.

After completing the instructions, subjects then answered eight choice questions. Each choice question presented subjects with three alternatives: two variants of the good (i.e., a tree-type, tree-size, and price combination) and an alternative to purchase neither variant of the good. An orthogonal, fractional-factorial design was selected (Sloane, 2004) and 32 choice questions or choice sets were developed. The design was modified so that medium-sized trees cost more than small trees, but orthogonality was maintained in all other dimensions. The 32 combinations were split into four different questionnaires, each containing eight questions. Each subject is presented with only one of the four questionnaires and thus answered eight choice questions.

Finally, in the real payment treatment, the experiment concluded by rolling an eight-sided die. The number rolled then determined which question was binding. If a subject chose a tree in the binding question, payment was collected and subjects were offered the opportunity to receive direct confirmation of the planting of their tree by *Trees Atlanta* by providing their mailing address.

III. Empirical Model and Data

As described above, each choice set that a subject faces consists of three alternatives: two tree type/size/price combinations and a status quo (no tree) alternative. Individual *i* chooses alternative *j* if the utility associated with alternative *j*, U_{ij}, in a given choice set is greater than the utility associated with the other two alternatives. In the simplest empirical model, individual *i*'s indirect utility from alternative *j* can be represented as:

$$U_{ij} = \alpha ASC_j + \beta p_j + \sum_{n=1}^{5} \gamma_n T_{nj} + \varepsilon_{ij}, \qquad (1)$$

where p_j is the price of tree j and its coefficient β measures price sensitivity (or the negative of the marginal utility of income). ASC_j is an alternative specific constant equal to one if alternative j is either of the tree options and zero for the opt-out or "neither tree/status quo" option. The coefficient θ represents the utility increment from purchasing a tree, irrespective of the attributes of the tree, and can be interpreted as the utility associated with market participation. The dummy variables, T_{nj}, indicate tree type (medium Oak, small Oak, medium Dogwood, small Dogwood, or medium Crepe Myrtle), noting that the omitted category is small Crepe Myrtle. The coefficients on these variables, γ_n, capture the utility increment unique to a particular tree type. Finally, ε_{ij} is the unobserved random component of utility.

Equation (1) represents an average or homogeneous preferences model—the parameters of the model do not vary across treatments, individuals, or tree types. We can easily generalize this model by allowing β and θ to vary systematically with treatment or with individual- or tree-specific attributes, as follows:

$$U_{ij} = \theta_i ASC_j + \beta_i p_j + \sum_{n=1}^{5} \gamma_n T_{nj} + \varepsilon_{ij} \qquad (2)$$

where θ_i is a systematically varying parameter that varies with $\mathbf{X}_i$, a vector of variables describing respondent i: $\theta_i = \theta_0 + \boldsymbol{\theta}'\mathbf{X}_i$. Similarly, β_i can be written $\beta_i = \beta_0 + \boldsymbol{\beta}'\mathbf{X}_i$, where, as before, $\mathbf{X}_i$ is a vector of variables describing individual i. It is key to note that the willingness to pay measures that are of interest for nonmarket valuation purposes depend critically on both the propensity for a subject to enter the market, given by θ_i, and the sensitivity of a subject to prices, given by β_i. Specifically, with a model of utility as given in (2), total willingness to pay (TWTP) for tree type T_n is given by solving for the tree price (p_j^*) that would make an individual with

the given utility function indifferent between the tree at price p_j^* and no tree, or $p_j^* = \left(\theta_i ASC_j + \gamma_n T_{nj}\right)/\left(-\beta_i\right)$. Noting that ASC_j and T_{nj} are dummy variables equal to one when tree type T_n is chosen, we can write TWTP as $p_j^* = \left(\theta_i + \gamma_n\right)/\left(-\beta_i\right)$. Marginal willingness to pay (MWTP) for tree-type T_n in relation to a baseline tree (say, tree-type T_1) is given by finding the derivative of total WTP with respect to T_n, or $\frac{\partial p_j^*}{\partial T_{nj}} = \frac{\gamma_n}{-\beta_i}$.

In this application, we wish to test hypotheses regarding the equivalency of the parameters of the model in equation (2) across treatments. With this goal in mind, we further allow the parameters of the model to vary with treatment. By pooling data from two of the treatments (e.g., hypothetical and cheap talk), we can test the equivalency of parameters across treatments using interaction terms. Because treatment group is an individual-level attribute rather than a choice-specific attribute, this can be accomplished by simply including a treatment indicator variable in the vector **X**. As an illustrative example, suppose an individual is described only by the treatment group they belong to and their gender, and we wish test for preference differences between the hypothetical and cheap talk treatments as related to subjects' price responsiveness. In this case, the systematically varying parameter β_i can be written $\beta_i = \beta_0 + \beta_1 1(CT_i) + \beta_2 1(Male_i)$, where $1(CT_i)$ is equal to one if individual *i* is assigned to the cheap talk treatment and zero otherwise. The variable $1(Male_i)$ is defined analogously. We would then test whether or not the coefficient β_1 is different from zero. In an empirical model, this would be the coefficient on the interaction term between p_j and $1(CT_i)$. The parameter θ_i can be similarly generalized.

Finally, we can generalize the model to an even greater degree by allowing treatment effects to vary with the socio-economic status of the individual. This generalization is motivated by the observation that different types of participants may respond differently to different treatments. We allow for this possibility by allowing interaction effects between treatment and socioeconomic status. To illustrate, consider the example in the preceding paragraph that allows the price coefficient to vary with treatment and with gender, but constrains the treatment effect to be the same for both genders. This constraint can be relaxed by including an interaction term as follows: $\beta_i = \beta_0 + \beta_1 1(CT_i) + \beta_2 1(Male_i) + \beta_3 1(CT_i) * 1(Male_i)$. Now, a test of whether or not the coefficient β_3 is equal to zero would test whether or not the effect of treatment on price sensitivity is different for males and females.

It is important to note that the empirical models described above allow for variation in the parameters β_i and θ_i with respect to treatment, but not in the γ_n parameters. This is because we expect, *a priori*, that treatment (real, hypothetical or cheap talk) will affect the behavioral response to the market in which subjects are placed, and thus influence the key economic parameters in the utility model (β_i and θ_i). We do not necessarily expect treatment to influence preferences over tree types, captured by γ_n. This is because the cheap talk script is designed to induce respondents to more carefully consider their budget constraint and market participation decision, but not designed to influence their relative preferences over trees (Cummings and Taylor, 1999). In the main results, we therefore present models that allow for heterogeneity in the tree preferences parameters, γ_n, by demographic characteristics, but restrict this variation to be the same across treatments in the pooled models. More general models that relax this assumption are reported in the Appendix and indicate that this restriction does not influence the conclusions drawn from the main results reported here.

To summarize, the coefficient β captures price sensitivity, or the intensive margin of choice, and θ captures utility from market participation or the extensive margin of choice. Tests for behavioral differences across treatments are given by tests of differences in these key parameters across treatments. All the models in this paper are estimated via alternative specific multinomial probits that allow for an unstructured correlation pattern (as well as heteroskedasticity) in the latent variable errors.[7]

Data and Summary Statistics

A total of 745 subjects were approximately evenly divided among the three treatments. The baseline hypothetical treatment involved 263 subjects, and the hypothetical-with-cheap-talk treatment involved 220 subjects. The real-payment treatment was conducted with 252 subjects. Table 1 reports variable definitions and summary statistics for a few key demographic characteristics by treatment. As indicated in Table 1, demographic characteristics of each of the sub-samples are generally similar. The mean age of subjects is approximately 21 years old, and the mean household income is approximately $56,000. The mean amount of income the student personally earns ("own-income") is generally less than $15,000. Subjects in the cheap talk treatment reported own-income that is statistically different than the hypothetical treatment at the 5% level, but not statistically different than the real-payment treatment. Approximately half the subjects are male and between 30 and 40 percent of the subjects in each treatment are African American, reflecting the general undergraduate population of Georgia State University. Lastly, fewer than half of the subjects indicated that they are primarily in control of budget decisions in their household.

[7] Specifically, we use the asmprobit routine in Stata 11.

Table 2 reports market participation by treatment, and the differences in the propensity to choose a tree across treatments is apparent. In the hypothetical treatment, 82.6 percent of all choices were to purchase one of the two trees—this percentage falls to 42.5 percent in the cheap talk treatment and to 10.3 percent in the real treatment. To examine choices by subject, we define a subject as being "in the market" if they chose to purchase a tree in at least one of the eight choice questions. As indicated in Table 2, 97.7 percent of subjects in the baseline hypothetical treatment were in the market. Participation in the market drops off to 74.8 percent in the cheap talk treatment and is dramatically lower in the real payment treatment (21.8 percent). The last three rows of Table 2 further illustrate differences in subject behavior across treatments. Of the subjects that stay in the market and purchase a tree at least once, the majority purchase a tree always in the hypothetical treatment. However, in the cheap talk treatment, the majority of subjects in market are more selective and purchase a tree sometimes, but not always. The same is true in the real payment treatment. Clearly there are substantial differences in choice behavior across treatments—the propensity to choose trees is diminished in when the cheap talk script is read and is dramatically lower when the treatment involves real payments.

Before turning to our empirical models, we explore whether or not there are observable differences across subjects that are market participants versus those who are not. Table 3 provides summary statistics for market participants separately from non-participants for each treatment. Summary statistics highlighted in bold indicate statistically significant differences among the two groups at the 5 percent level. Not surprisingly, we find no statistically significant difference in the mean values of these observable characteristics in the hypothetical treatment given that there are only six individuals who did not participate in the market in the hypothetical treatment. We do find, however, that the average market participant in the cheap talk treatment is

more likely to be female. In the real treatment, market participants are older, have higher "own" income and are more likely to self-budget. These statistics suggest heterogeneity in the effects of both the cheap talk script and real treatment. As such, we now turn to our empirical models that allow us to explore heterogeneity in both the decision to participate in the market and the responsiveness to price.

IV. Preference Models

Table 4 reports the results of pairwise comparison models that pool data from two of the three treatments and are estimated via alternative-specific multinomial probit models that allow for unstructured latent-error correlation patterns (as well as heteroskedasticity). In each model we allow both the price coefficient and alternative-specific constant to vary with treatment. Recognizing that the scale of utility is not estimated separately from the error variance in random utility models, we allow the error variance to vary across treatments and, for each pairwise comparison, estimate a scale parameter via the grid search method described in Swait and Louviere (1993). Under the assumption that cheap talk is truly effective at eliminating hypothetical bias, our null hypotheses are that responses in the cheap talk and real treatments will differ *in the same manner* from the pattern exhibited by those in the hypothetical treatment. In other words, we expect preferences for the cheap talk and real treatments to be 1) statistically indistinguishable from each other and 2) different from the hypothetical treatment in the same way.

The first column of Table 4 reports the results of models that pool the data from the hypothetical and cheap talk treatments. The second column uses pooled data from the hypothetical and real treatments and the third uses pooled data from the cheap talk and real

treatments. In the first two columns the hypothetical treatment is the base category while in the third column, the cheap talk treatment is the base category. In all models presented, a small crepe myrtle tree is the omitted category and as such, each tree-type coefficient represents the change in utility associated with consumption of the tree in question as compared to the small crepe myrtle. As indicated in Table 4, the average preferences for trees correspond to intuition and basic economic theory: we see that larger trees and trees that provide more shade generally have larger associated coefficients, indicating that respondents are willing to pay more for these trees. Furthermore, as expected, the price coefficient is negative (not interacted), indicating positive marginal utility of income.

Our key coefficients of interest are those on the interaction terms between the treatment indicator variables and price or the ASC, highlighted in bold type in Table 4. If preferences are the same across treatments, then we would expect that after controlling for observable sample differences, the interaction terms between the treatment indicators and the price and ASC variables would be statistically insignificant. We find, however, substantial evidence of differences in preferences across treatments. We find that, compared to the hypothetical treatment, participants in the cheap talk treatment are somewhat less likely to enter the market as measured by the coefficient on ASC*1(Cheap Talk), which is significant at the 10% level. However, participants in the cheap talk treatment are significantly more price sensitive as compared to subjects in the hypothetical treatment as indicated by the large (in absolute value) and highly significant coefficient for Price*1(Cheap Talk).

When we compare the real treatment to the hypothetical treatment (see column 2, Table 4), we similarly find that participants are less likely to enter the market in the real treatment as compared to the cheap talk treatment, but now the effect is quite large and highly significant. In

stark contrast to the hypothetical/cheap talk comparison, we find no statistically significant differences in price sensitivity across subjects in the hypothetical and real treatments. The evidence here suggests that hypothetical bias exists in the form of a higher propensity to purchase a tree when the situation is hypothetical, but the revealed price sensitivity at the intensive margin of choice is not significantly different across hypothetical and real scenarios.[8]

Turning now to the comparison between cheap talk and real treatments, we find that subjects in the cheap talk treatment are more likely to be in the market and less price sensitive than subjects in the real treatment (see coefficients in bold, column 3, Table 4). Ideally, if cheap talk were influencing behavior in a manner identical to being placed in a real-payment situation, the coefficient estimates would indicate similar propensities to be in the market in the cheap talk and real treatments as well as similar price sensitivity. However, the comparisons of behavior in the cheap talk treatment to real and hypothetical treatments suggest that cheap talk scripts may actually induce participants to "overreact" in term of price sensitivity but "underreact" in terms of the purchase decision. It is important to note that when calculating willingness to pay for benefits estimation, the increased proclivity to choose a tree the cheap talk treatment would tend to offset the increased price sensitivity in this treatment.[9] Thus, a researcher could conclude that a cheap talk script "works" because it results in WTP estimates that are indistinguishable from real payment treatments, but fail to recognize that it does so by inducing behavior that is distinctly different from what would occur if the situation were in fact real.

[8] Lusk and Schroeder (2004) and Taylor et al. (2010) also find loosely suggestive evidence that price responsiveness may not be different across hypothetical and real choice experiments, while market participation is dramatically different. Carlsson and Martinsoon (2001) also find little evidence that price responsiveness is different in hypothetical and real-payment choice experiments, however their experimental design involves a forced-choice so that market participation is not a choice margin.

[9] See Haab and McConnell (2002) for an extensive discussion of WTP for discrete choice models. Although we focus on differences in utility parameters for this research, we note that the two effects described in the text (lower price sensitivity and lower likelihood of purchasing a tree) would work in opposite directions with respect to WTP. Lower price sensitivity results in higher estimated WTP due to the lower implied marginal utility of income in the denominator of the calculation. A lower estimate of the coefficient on the ASC implies lower utility from planting a tree and would result in a smaller numerator and lower WTP.

Heterogeneity in Treatment Effects

Having established the presence of differences in preferences across treatments (while controlling for variation in socio-economic status), a natural extension of our model is to allow for heterogeneity in treatment response across individuals. This extension, in principle, can allow us to answer the question of whether different types of participants react differently to the hypothetical, cheap talk, and real treatments. Table 5 reports the key results of models that allow three-way interactions between price (or ASC), participant characteristics, and treatment. Full results are available in the appendix Table A1. As with previously reported pairwise comparison models, we use the grid search method to identify the appropriate scale parameter across different treatments.

The first panel in Table 5 indicates that the effect of treatment on the market participation choice is relatively homogeneous across participants. The exception is that African American participants appear to be more likely to enter the market and choose a tree in the real treatment as compared to either the cheap talk or hypothetical treatment. Note that once this heterogeneity is directly modeled, we see that the average effect of the cheap talk treatment on participants' decisions to enter the market is not significantly different than the real payment treatment for the other respondents (see Table 5, column 3, coefficient estimate for ASC*1(Real)).

The primary heterogeneity in treatment effect appears to be related to the price sensitivity of participants. As indicated in the second panel of Table 5, relative to the hypothetical treatment, the cheap talk treatment induces a higher level of price sensitivity overall (relative to hypothetical), but this effect is relatively smaller for older respondents and for males and relatively higher for higher-income participants. Comparing the hypothetical treatment to the real

treatment, we observe that the estimated change in price sensitivity is greater for African American participants, but less for older participants and for self-budgeters. The same is true in the comparison of the cheap talk to real treatment, except that age loses statistical significance. Thus, despite the relatively homogeneous samples and large parameter space, these models indicate some evidence of statistically significant heterogeneity in treatment response by socio-economic status.

V. Conclusion

Improving the ability of stated preference methods to accurately estimate economic values is important because revealed preference techniques are not always viable for valuing some environmental goods. Cheap talk scripts have demonstrated potential for improving stated preference estimates in some contexts, although the efficacy of the script as written by Cummings and Taylor (1999) remains ambiguous. In this study, we explore the influence of the cheap talk script within a choice experiment context. As noted by List et al. (2006), choice experiments provide an opportunity to explore the influence of treatment on the decision to purchase (the extensive margin) separately from the choice among the alternatives offered (the intensive margin). This is an important distinction and different from earlier studies, our experimental design and econometric modeling explicitly allow us to separately estimate treatment effects on both these margins of choice.

The key result of this study is to show that even though cheap talk scripts can provide willingness to pay estimates that are similar to real treatments, they may do so by inducing behavior that is distinctly different than consumers facing real choices. Specifically, the cheap talk script appears to affect both participants' price sensitivity and decisions to purchase in ways that are different than the real treatment. We show that on average, participants in the cheap talk treatment appear to be *i*) more price sensitive when choosing among the offered trees than in the real treatment, and *ii*) more inclined to enter the market and purchase a tree relative to the real treatment. In other words, the cheap talk treatment appears to induce respondents to "overreact" to prices as compared to the real-payment treatment, but "underreact" in terms of their proclivity to be in the market and purchase one of the two trees offered in a given choice set. These two distinct effects of the cheap talk script have important implications for estimated WTP for

environmental goods. As discussed in Section III, the formula to calculate total WTP for a specific tree involves the both the coefficients capturing price sensitivity and proclivity to the be in the market. However, the formula for marginal WTP does not include the coefficient for the variable describing proclivity to be in the market. If our results are robust to further tests, they imply that calculated marginal WTP in cheap talk treatments will be unambiguously lower than those elicited in real-payment treatments, even though total WTP maybe identical. Depending on the statistic of interest in the nonmarket valuation exercise, a research could infer deeply discounted WTP estimates for environmental goods when elicited with hypothetical choice experiments employing the cheap talk script (as originally written by Cummings and Taylor).

Finally, the importance of our findings for the cheap talk script also applies to other ex-ante bias mitigation tools such as cognitive dissidence minimization or oath taking (e.g., Morrison and Brown, 2009; Jacquemet, et al., 2009). If the tool in question does not induce the same behavior as the target behavior, then one must rely on extensive testing to discover empirical regularities and gain confidence that the tool will be effective, and under what circumstances. It appears this could be the case for the cheap talk script if our results are replicated in future research.

Table 1: Summary Statistics for Experiment Subjects by Treatment[a]

Variable	Definition	Hypothetical (N=263)	Cheap Talk (N=220)	Real (N=252)
		Mean (standard deviation)		
Age	Age in years	21.53 (4.23)	20.68 (3.09)	22.67 (6.69)
HH Income	Income of household with which respondent shares income, in 1,000s.	56.88 (38.2)	57.98 (41.66)	55.82 (22.67)
Own Income	Income that the respondent personally earns, in 1,000s.	16.75 (19.6)	11.86 (12.7)	14.24 (15.4)
		Percent = 1		
Male	=1 if respondent is male; =0 otherwise	42%	52%	48%
African Am.	=1 if respondent self-identified as African American; =0 otherwise	39%	27%	35%
Self-Budget	=1 if respondent indicated he/she is in charge of their budget and expenses; =0 otherwise=	41%	34%	40%

[a]The number of respondents for which each summary statistic is computed may not equal the total number of subjects in that treatment due to non-responses. Across all treatments, the maximum non-response was to the income question wherein 11, 2, and 15 subjects in the hypothetical, cheap talk, and real-payment treatment, respectively, chose not to answer the question.

Table 2: Market participation.[a]

	Hypothetical	Cheap Talk	Real Payment
Number of respondents	263	220	252
Number of choices	2,104	1,760	2,016
Percentage of **choices** that were:[b]			
to purchase one of the two trees	82.6	42.5	10.3
to not purchase either tree	17.4	57.5	89.7
Percentage of **subjects** who:[b]			
chose a tree in at least one question (i.e., subjects are "in the market")	97.7	74.8	21.8
never chose a tree	2.3	25.2	78.2
Percentage of **subjects** who: [b]			
always chose one of the two trees	55.6	17.7	4.0
chose a tree at least once, but not always	42.1	57.1	17.8
never chose to purchase a tree	2.3	25.2	78.2

[a] Subjects are "in the market" if they chose a good in at least one of the eight choice questions.

[b] Percentages sum to 100.

Table 3: Summary Statistics by Market Participation.[a]

	Hypothetical		Cheap Talk		Real	
	Market Participant (N=237)[b]	Non-Participant (N=5)	Market Participant (N=149)	Non-Participant (N=53)	Market Participant (N=55)	Non-Participant (N=197)
	Mean (standard deviation)					
Age	21.59 (4.38)	22.00 (3.00)	20.95 (3.60)	20.21 (1.76)	**25.32 (8.89)**	**21.75 (5.70)**
HH Income	16.49 (19.08)	34.00 (53.08)	12.90 (14.31)	10.09 (8.69)	**22.25 (21.56)**	**11.62 (12.26)**
Own Income	57.14 (38.39)	52.50 (55.42)	59.62 (41.67)	57.31 (39.96)	56.72 (36.62)	55.67 (36.59)
	100 Percent =1					
Male	0.42	0.80	**0.46**	**0.73**	0.45	0.48
African Am.	0.39	0.60	0.25	0.33	0.34	0.35
Self-Budget	0.43	0.40	0.37	0.25	**0.58**	**0.35**

[a] Entries in bold indicate a statistically significant difference (at the 5% level) in the demographic characteristic between market participants and non-participants for that treatment. We use t-tests for the continuous variables and tests of proportions (Fisher's two-sided exact) for dummy variables.

[b] The number of respondents for which each summary statistic is computed may not equal the total number of subjects in the category due to non-responses for some questions or exclusion restrictions based on prior experience with experiments of this type or expressed confusion about the experiment.

Table 4: Pairwise Comparison Choice Models.[a]

VARIABLES	Hypothetical vs. Cheap Talk	Hypothetical vs. Real	Cheap Talk vs. Real
	Tree Types		
1(Medium Oak)	1.5333***	1.2957***	0.7502***
	(0.149)	(0.142)	(0.136)
1(Small Oak)	1.0976***	0.9080***	0.6535***
	(0.100)	(0.094)	(0.081)
1(Medium Dogwood)	0.9455***	0.7130***	0.4017***
	(0.131)	(0.118)	(0.143)
1(Small Dogwood)	0.6035***	0.4932***	0.3520***
	(0.083)	(0.078)	(0.081)
1(Medium Crepe)	0.4134***	0.3433***	0.2970**
	(0.123)	(0.113)	(0.139)
	Market Participation		
ASC	2.2455***	2.1585***	0.8767***
	(0.503)	(0.316)	(0.327)
…*1(Cheap Talk)	**-0.3972***	**n/a**	**n/a**
	(0.227)		
…*1(Real)	**n/a**	**-3.1847*** **	**-1.7412*** **
		(0.188)	**(0.183)**
…*(Age)	0.0093	0.0015	0.0014
	(0.022)	(0.012)	(0.011)
…*1(Male)	-0.4217**	-0.2252	-0.4505***
	(0.181)	(0.153)	(0.165)
…*1(African Am.)	-0.7475***	-0.3947**	-0.2692
	(0.187)	(0.156)	(0.180)
…*(HH Income)	0.0002	-0.0004	0.0032
	(0.002)	(0.002)	(0.002)
…*1(Self-Budget)	0.3993**	0.5191***	0.3590*
	(0.202)	(0.167)	(0.191)
	Price Responsiveness		
Price	-0.1275***	-0.1515***	-0.1859***
	(0.028)	(0.020)	(0.022)
…*1(Cheap Talk)	**-0.1204*** **	**n/a**	**n/a**
	(0.014)		
…*1(Real)	**n/a**	**-0.0089**	**0.0253** **
		(0.012)	**(0.012)**
…*(Age)	-0.0004	0.0016**	0.0027***
	(0.001)	(0.001)	(0.001)
…*1(Male)	-0.0017	-0.0110	0.0113
	(0.010)	(0.009)	(0.011)

continued, next page

Table 4. Continued.

VARIABLES	Hypothetical vs. Cheap Talk	Hypothetical vs. Real	Cheap Talk vs. Real
…*1(African Am.)	0.0208**	0.0029	0.0044
	(0.010)	(0.009)	(0.012)
…*(HH Income)	0.0002	0.0002*	-0.0001
	(0.000)	(0.000)	(0.000)
…*1(Self-Budget)	-0.0084	-0.0027	-0.0014
	(0.011)	(0.009)	(0.012)
n (Alternatives)	10,254	10,962	9,948
Log-likelihood	-2828	-2107	-1788

[a]Scale Parameter, standard deviation, and correlation parameters not shown to save space.

Table 5: Pairwise Comparison Choice Models with Heterogeneity in Treatment Response[a]

VARIABLES	Hypothetical vs. Cheap Talk	Hypothetical vs. Real	Cheap Talk vs. Real
	Market Participation		
ASC	2.5856***	2.6290***	0.0451
	(0.543)	(0.551)	(0.693)
…*1(CT)	-2.1984*	--	--
	(1.315)		
…*1(CT)*Age	0.0858	--	--
	(0.061)		
…*1(CT)*1(Male)	-0.6865	--	--
	(0.453)		
…*1(CT)*1(African Am.)	-0.5459	--	--
	(0.500)		
…*1(CT)*1(HH Income)	0.0079	--	--
	(0.006)		
…*1(CT)*1(Self-Budget)	0.2824	--	--
	(0.548)		
…*1(Real)	--	-3.3917***	-0.5941
		(1.031)	(0.843)
…*1(Real)*Age	--	0.0035	-0.0489
		(0.035)	(0.035)
…*1(Real)*1(Male)	--	-0.0681	0.3290
		(0.536)	(0.357)
…*1(Real)*1(African Am.)	--	1.1029**	0.9470**
		(0.559)	(0.395)
…*1(Real)*1(HH Income)	--	0.0033	-0.0027
		(0.008)	(0.005)
…*1(Real)*1(Self-Budget)	--	-0.6853	-0.5393
		(0.626)	(0.432)
	Price Responsivness		
Price	-0.1177***	-0.1157***	-0.1763***
	(0.030)	(0.030)	(0.037)
…*1(CT)	-0.2139***		
	(0.069)		
…*1(CT)*Age	0.0057*	--	--
	(0.003)		
…*1(CT)*1(Male)	0.0471*	--	--
	(0.027)		
…*1(CT)*1(African Am.)	0.0360	--	--
	(0.029)		
…*1(CT)*1(HH Income)	-0.0007*	--	--
	(0.000)		

continued, next page

Table 5. Continued.

VARIABLES	Hypothetical vs. Cheap Talk	Hypothetical vs. Real	Cheap Talk vs. Real
…*1(CT)*1(Self-Budget)	-0.0474	--	--
	(0.032)		
…*1(Real)	--	-0.2793***	-0.0355
		(0.069)	(0.050)
…*1(Real)*Age	--	0.0060***	0.0002
		(0.002)	(0.002)
…*1(Real)*1(Male)	--	-0.0036	-0.0270
		(0.037)	(0.024)
…*1(Real)*1(African Am.)	--	-0.0906**	-0.0723***
		(0.040)	(0.027)
…*1(Real)*1(HH Income)	--	-0.0001	0.0004
		(0.001)	(0.000)
…*1(Real)*1(Self-Budget)	--	0.1278***	0.0977***
		(0.045)	(0.030)
n (Alternatives)	10,110	10,746	9,732
Log-likelihood	-2773	-2045	-1746

[a] Select coefficients reported. See Appendix Table A.1 for full results.

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APPENDIX

Table A-1: Full Results for Table 5.

VARIABLES	Hypothetical vs. Cheap Talk	Hypothetical vs. Real	Cheap Talk vs. Real
Price	-0.1177***	-0.1157***	-0.1763***
	(0.030)	(0.030)	(0.037)
…*Age	-0.0008	-0.0008	0.0026
	(0.001)	(0.001)	(0.002)
…*1(Male)	-0.0111	-0.0116	0.0197
	(0.011)	(0.011)	(0.014)
…*1(African Am.)	0.0128	0.0126	0.0273*
	(0.011)	(0.011)	(0.016)
…*(HH Income)	0.0003*	0.0003*	-0.0002
	(0.000)	(0.000)	(0.000)
…*1(Self-Budget)	-0.0046	-0.0047	-0.0280*
	(0.011)	(0.012)	(0.017)
…*1(CT)	-0.2139***	--	--
	(0.069)		
…*1(CT)*Age	0.0057*	--	--
	(0.003)		
…*1(CT)*1(Male)	0.0471*	--	--
	(0.027)		
…*1(CT)*1(African Am.)	0.0360	--	--
	(0.029)		
…*1(CT)*1(HH Income)	-0.0007*	--	--
	(0.000)		
…*1(CT)*1(Self-Budget)	-0.0474	--	--
	(0.032)		
…*1(Real)	--	-0.2793***	-0.0355
		(0.069)	(0.050)
…*1(Real)*Age	--	0.0060***	0.0002
		(0.002)	(0.002)
…*1(Real)*1(Male)	--	-0.0036	-0.0270
		(0.037)	(0.024)
…*1(Real)*1(African Am.)	--	-0.0906**	-0.0723***
		(0.040)	(0.027)
…*1(Real)*1(HH Income)	--	-0.0001	0.0004
		(0.001)	(0.000)
…*1(Real)*1(Self-Budget)	--	0.1278***	0.0977***
		(0.045)	(0.030)
ASC	2.5856***	2.6290***	0.0451
	(0.543)	(0.551)	(0.693)
…*Age	-0.0090	-0.0096	0.0467
	(0.024)	(0.024)	(0.032)
…*1(Male)	-0.2700	-0.2749	-0.5350**
	(0.207)	(0.209)	(0.228)
…*1(African Am.)	-0.6491***	-0.6552***	-0.6743***
	(0.205)	(0.208)	(0.261)

…*(HH Income)	-0.0011	-0.0011	0.0037
	(0.003)	(0.003)	(0.003)
…*1(Self-Budget)	0.4138*	0.4174*	0.3740
	(0.223)	(0.226)	(0.283)
(Continued…)			
…*1(CT)	-2.1984*	--	--
	(1.315)		
…*1(CT)*Age	0.0858	--	--
	(0.061)		
…*1(CT)*1(Male)	-0.6865	--	--
	(0.453)		
…*1(CT)*1(African Am.)	-0.5459	--	--
	(0.500)		
…*1(CT)*1(HH Income)	0.0079	--	--
	(0.006)		
…*1(CT)*1(Self-Budget)	0.2824	--	--
	(0.548)		
…*1(Real)	--	-3.3917***	-0.5941
		(1.031)	(0.843)
…*1(Real)*Age	--	0.0035	-0.0489
		(0.035)	(0.035)
…*1(Real)*1(Male)	--	-0.0681	0.3290
		(0.536)	(0.357)
…*1(Real)*1(African Am.)	--	1.1029**	0.9470**
		(0.559)	(0.395)
…*1(Real)*1(HH Income)	--	0.0033	-0.0027
		(0.008)	(0.005)
…*1(Real)*1(Self-Budget)	--	-0.6853	-0.5393
		(0.626)	(0.432)
…*1(Med Oak)	1.5069***	1.5030***	0.7476***
	(0.148)	(0.167)	(0.142)
…*1(Sm Oak)	1.1014***	1.0982***	0.6833***
	(0.100)	(0.116)	(0.085)
…*1(Med Dogwood)	0.9195***	0.8487***	0.3743**
	(0.130)	(0.138)	(0.150)
…*1(Sm Dogwood)	0.5870***	0.5749***	0.3524***
	(0.083)	(0.090)	(0.085)
…*1(Med Crepe)	0.3951***	0.3754***	0.2554*
	(0.123)	(0.131)	(0.146)
lnsigma3 (St. Dev.)	0.1514**	0.1858**	0.1427
	(0.071)	(0.090)	(0.099)
atanhr3_2 (Corr.)	0.9065***	0.9847***	0.9454***
	(0.097)	(0.120)	(0.125)
scale parameter	1.76	1.44	0.82
n (Alternatives)	10,110	10,746	9,732
Log-likelihood	-2773	-2045	-1746

Table A-2: Pairwise Comparison Choice Models allowing for Treatment Heterogeneity in Tree-type Parameters (models are otherwise comparable to Table 4 in the main text).[a]

	Heterogeneity in Price & ASC		
	Hypothetical vs. Cheap Talk	Hypothetical vs. Real	Cheap Talk vs. Real
VARIABLES			
Price	-0.1267***	-0.1442***	-0.1798***
	(0.028)	(0.016)	(0.021)
…*1(Cheap Talk)	-0.1247***	n/a	n/a
	(0.027)		
…*1(Real)	n/a	0.0932***	0.0579***
		(0.014)	(0.019)
…*(Age)	-0.0004	0.0007***	0.0021***
	(0.001)	(0.000)	(0.001)
…*1(Male)	-0.0020	-0.0061	0.0072
	(0.010)	(0.005)	(0.009)
…*1(African Am.)	0.0206**	-0.0090*	-0.0013
	(0.010)	(0.005)	(0.010)
…*(HH Income)	0.0002	0.0001	-0.0000
	(0.000)	(0.000)	(0.000)
…*1(Self-Budget)	-0.0081	0.0123**	0.0058
	(0.011)	(0.005)	(0.011)
ASC	2.2826***	2.2348***	0.9563***
	(0.504)	(0.189)	(0.297)
…*1(Cheap Talk)	-0.4989	n/a	n/a
	(0.359)		
…*1(Real)	n/a	-2.3822***	-1.5081***
		(0.171)	(0.242)
…*(Age)	0.0086	-0.0016	-0.0015
	(0.022)	(0.004)	(0.009)
…*1(Male)	-0.4215**	-0.0335	-0.3456**
	(0.182)	(0.067)	(0.140)
…*1(African Am.)	-0.7490***	0.0191	-0.1554
	(0.187)	(0.071)	(0.151)
…*(HH Income)	0.0001	-0.0004	0.0022
	(0.002)	(0.001)	(0.002)
…*1(Self-Budget)	0.3998**	0.0378	0.2420
	(0.202)	(0.076)	(0.161)
1(Medium Oak)	1.5321***	1.5586***	0.7850***
	(0.162)	(0.179)	(0.170)
…*1(Cheap Talk)	-0.0600	n/a	n/a
	(0.336)		
…*1(Real)	n/a	-1.3736***	-0.2784
		(0.188)	(0.230)
1(Small Oak)	1.0507***	1.0744***	0.6921***
	(0.109)	(0.122)	(0.106)
…*1(Cheap Talk)	0.2578	n/a	n/a

	(0.213)		
…*1(Real)	n/a	-0.9030***	-0.2554*
		(0.123)	(0.133)
1(Medium Dogwood)	0.9147***	0.9260***	0.6135***
	(0.140)	(0.148)	(0.174)
…*1(Cheap Talk)	0.2210	n/a	n/a
	(0.351)		
…*1(Real)	n/a	-1.0962***	-0.9774***
		(0.197)	(0.347)
1(Small Dogwood)	0.5776***	0.5772***	0.3962***
	(0.091)	(0.095)	(0.104)
…*1(Cheap Talk)	0.1604	n/a	n/a
	(0.216)		
…*1(Real)	n/a	-0.4956***	-0.1868
		(0.101)	(0.139)
1(Medium Crepe)	0.3930***	0.3905***	0.3069*
	(0.133)	(0.137)	(0.171)
…*1(Cheap Talk)	0.1738	n/a	n/a
	(0.349)		
…*1(Real)	n/a	-0.3260**	-0.0945
		(0.153)	(0.242)
Observations	10,254	10,962	9,948
Log-likelihood	-2827	-2082	-1781