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Revisiting Cheap Talk with New Evidence from a Field Experiment

Andres Silva, Rodolfo M. Nayga, Jr., Benjamin L. Campbell, and John L. Park

We assess the reduction of hypothetical bias in consumers' willingness to pay (WTP) for products by applying a generic, short, and neutral cheap talk script in a retail setting. Using an open-ended elicitation mechanism with non-hypothetical, hypothetical, and hypothetical with cheap talk treatments, our results indicate that the hypothetical WTP values are higher than the non-hypothetical values, but the hypothetical with cheap talk values are not significantly different from non-hypothetical estimates.

Key words: BDM, cheap talk, field experiment, hypothetical bias, willingness-to-pay

Introduction

Over the last two decades, experimental studies have become a popular mechanism for testing economic theory or measuring willingness to pay (WTP) for goods. An advantage of experimental studies is that subjects can be put in an environment that closely resembles a real situation, while allowing the researcher to control the conditions the subjects are facing. Recently, there has been an increasing interest in measuring WTP using experimental approaches. For instance, Lusk and Shogren (2007) identified 113 academic publications that used experimental auctions. Of these studies, 73 were completed in 2000 or later. Following Maynard et al. (2004), experimental WTP studies can be classified as hypothetical and non-hypothetical. Hypothetical studies do not require the presentation of an actual product since a transaction does not take place. Therefore, these studies are especially attractive when the actual product is not available or prototypes are too costly to produce. In contrast, in non-hypothetical studies, there is the possibility of a transaction occurring, meaning the subject may have to give up something, such as money, in exchange for the actual product.

Subjects tend to behave differently when they face a hypothetical task compared to a real one (Neill et al., 1994; Blumenschein et al., 1997; List and Gallet, 2001). More specifically, there is strong evidence that subjects overstate their true WTP in hypothetical situations.[1] However, there is a lack of agreement about why respondents do this and also how to calibrate their overstated responses (Murphy et al., 2005). Possible reasons could be due to strategic manipulation (Carson and Groves, 2007), uncertainty (Johannesson et al., 1999), or social desirability (Lusk and Norwood, 2009) on the part of the respondents. Some studies have investigated means of calibrating hypothetical

Andres Silva is a Ph.D. student at the University of Kent, Rodolfo Nayga is a Professor and the Tyson Chair in Food Policy Economics at the University of Arkansas, Benjamin Campbell is a Research Scientist at Vineland Research and Innovation Centre, and John Park is the Roy B. Davis Professor of Agricultural Cooperation and Extension Economist at Texas A&M University. The authors thank Gary Brester and anonymous journal reviewers for helpful comments and suggestions. Support from AgriLife Extension Agricultural Economics Department at Texas A&M University and Texas Citrus Mutual are gratefully acknowledged.

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[1] There is also the issue of deception in the case of hypothetical product auctions. The problems of deception are two-fold. First, there is an ethical problem. Second, if respondents discover later that they were deceived, this can "spoil the well" for other researchers. One way to avoid the latter is to debrief respondents and explain the deception and the reasons for it, but this might not be completely effective in limiting the damage that could be caused by the deception.

studies to non-hypothetical results obtained in experimental settings (Blackburn, Harrison, and Rutström, 1994; List and Shogren, 1998). Murphy et al. (2005) conducted a meta-analysis of 28 WTP studies and found that the median ratio of hypothetical to actual values was 1.35, and that calibration effectively reduced hypothetical bias. Results from previous research, however, imply that calibration factors vary on a case-by-case basis and a specific calibration factor must therefore be determined for each study (List and Shogren, 1998; List and Gallet, 2001).

In this article, we study the effect of cheap talk, a non-binding communication of actions given to subjects before a hypothetical commitment that potentially minimizes bias introduced by hypothetical methodologies (Cummings and Taylor, 1999; Lusk, 2003; Aadland and Caplan, 2006). Cheap talk can be thought of as a specific type of informational effect. Cheap talk was first described in the game theory literature as a costless communication between subjects that can be effective in experimental coordination games (Santos, 2000; Charness and Grosskopf, 2004). While there have been a few studies that evaluated cheap talk, the scant literature discussed in the next section has produced mixed results as to its effectiveness in reducing hypothetical bias. Our aim in this study is to test a cheap talk script that we designed for a field experiment using an open-ended elicitation mechanism. Even sophisticated experiments can provide misleading results if participants do not clearly understand how to respond to valuation questions (Lusk and Hudson, 2004a). With this in mind and in contrast to other cheap talk studies, we use a generic, short, and neutral cheap talk script suited for studies eliciting WTP values from consumers in retail stores. Shorter scripts are also necessary in other types of studies, such as phone surveys.

Literature Review

The effect of cheap talk was studied by Lusk and Hudson (2004a) in ultimatum games. They found that compared to a non-informed group, the more-informed group behaved significantly differently, providing answers closer to the expected Nash equilibrium. Hence, they concluded that cheap talk could be used to "homogenize" the rationality and beliefs of participants, needed in obtaining Nash equilibrium. The implication is that a sophisticated experiment can easily provide biased results if the researcher is not able to explain the directions of the experiment in a short time.

Cummings and Taylor (1999) introduced cheap talk as a non-binding communication of actions before a hypothetical commitment. This communication specifically included a discussion about the hypothetical bias problem. They tested two alternative versions. In the first version, they included a discussion of the numerical results of a similar hypothetical task. In the second version, the same results were discussed without reference to numerical statistics. The cheap talk scripts were successful in reducing hypothetical bias. Both scripts made explicit references to the expected direction of the bias. Their cheap talk script has been referenced by a number of studies including those by List (2001); Brown, Ajzen, and Hrubes (2003); Murphy et al. (2005) and Landry and List (2007). Table 1 summarizes some of the major contributions to the cheap talk literature.

Empirical evidence can be contradictory in a number of cases. After reviewing the published work in this area, we synthesize the research on cheap talk into four categories: (1) length and content, (2) payment level, (3) participant's background, and (4) survey and experimental setting.

Length and Content

Results from the extant literature lack consistency regarding the effect of cheap talk, especially for short scripts. Loomis et al. (1996) tested the effectiveness of reminding subjects about being honest in their evaluation using three treatments: real, cheap talk, and hypothetical treatments. However, they did not explicitly mention or discuss hypothetical bias and its effect, which is one of the critical points that Cummings and Taylor (1999) included in their original cheap talk script. Loomis et al. (1996) found that a cheap talk script was not able to remove hypothetical bias since their cheap talk estimates and the hypothetical estimates were not significantly different. Brummett, Nayga, and

Table 1. Selected Cheap Talk Papers

Authors	Year	Task	Content	Words	Setting	Public Good
Loomis et al.	1996	CV Hypothetical vs. Non-Hypothetical	Neutral	181	Lab	No
Cummings and Taylor	1999	CV Hypothetical vs. Non-Hypothetical	No neutral	920 and 941	Lab	Yes
List	2001	Experimental Auction	No neutral	941[a]	Field	No
Aadland and Caplan	2003	CV Hypothetical	No neutral	49	Phone	Yes
Brown, Ajzen and Hrubes	2003	CV Hypothetical	No neutral	941[a]	Lab	Yes
Lusk	2003	CV Hypothetical	Neutral	522	Mail	Yes
Carlsson, Frykblom and Lagerkvist	2005	Hypothetical Choice Experiment	No neutral	113	Mail	No
Murphy, Stevens and Weatherhead	2005	CV Hypothetical vs. Non-Hypothetical	No neutral	941[a]	Lab	Yes
Aadland and Caplan	2006	CV Hypothetical	Neutral	75 and 125	Phone	Yes
Brummett, Nayga and Wu	2007	CV Hypothetical	No neutral	128	Field	No
Landry and List	2007	CV Hypothetical vs. Non-Hypothetical	No neutral	941[a]	Field	Yes
This study[b]	2011	OE Hypothetical vs. Non-Hypothetical	Neutral	211	Field	No

Notes: OE means open-ended and CV means contingent valuation [a] This script was adapted from Cummings and Taylor (1999).
[b] This study, included for purposes of comparison.

Wu (2007) also did not find statistical significance using a short cheap talk paragraph. In contrast, Aadland and Caplan (2003) and Carlsson, Frykblom, and Lagerkvist (2005) found that cheap talk can reduce hypothetical bias. In a different study, however, Aadland and Caplan (2006) argued that neutral cheap talk can increase hypothetical bias. In the end, the authors suggested that researchers use cheap talk with caution, since words can have different cognitive effects on subjects.

Payment Level

Brown, Ajzen, and Hrubes (2003), using Cummings and Taylor's script, found that a long cheap talk script is successful in a higher payment context. The authors found that cheap talk reduced the hypothetical bias associated with payments of $5 and $8 but not with payments of $3 and $1. Consistent with these findings, Murphy et al. (2005), using Cummings and Taylor's script and a payment fee of $10, tested different contribution levels for a public good using real, cheap talk, and hypothetical treatments. Their cheap talk did not result in a significant bias reduction when the subject was requested to contribute $4 and $6. Nevertheless, the reduction was significantly different from zero in higher payment levels.

Participant Background

List (2001), using a long cheap talk script, found that experienced card dealers did not change their WTP based on a cheap talk script. However, inexperienced card dealers were affected by the script. Consistent with these findings, Lusk (2003), using a mailed survey about golden rice, argued that cheap talk is effective in reducing the WTP for unknowledgeable consumers. Along the same line, Aadland and Caplan (2003) found that a short script can reduce the hypothetical bias, which would depend on the type of subject under study.

Survey and Experiment Setting

Only three known studies have tested cheap talk in a field setting. In table 1, we see that most of the studies have been conducted by mail, by phone, and in controlled lab settings. Of the three studies we found that tested cheap talk in a field setting, two are recent. Landry and List (2007), adapting a Cummings and Taylor's script, found that cheap talk was effective in eliminating hypothetical bias in CV but was not significantly different than the estimates in consequential tasks.[2] In addition, Brummett, Nayga, and Wu (2007), studying irradiated mangos in Texas, did not find significant differences in the WTP estimates between treatment groups, suggesting an absence of cheap talk effects. However, it was not possible to measure the potential hypothetical bias, since they did not have an actual product that can be used in a real treatment.

Based on the review of cheap talk studies, we conclude that there is a need to revisit the cheap talk issue using a field experiment. We conducted a set of retail WTP experiments to re-evaluate the use of cheap talk in reducing hypothetical bias using an open-ended elicitation mechanism. The open-ended mechanism is simple and has been used numerous times in marketing and economic studies (Balistreri et al., 2001; Goldar and Misra, 2001; Lusk and Hudson, 2004a). There is a consensus that incentive-aligned open-ended elicitations that use the Becker-DeGroot-Marschak (BDM) mechanism produce more accurate WTP valuations than hypothetical studies. Miller et al. (2011) also recently found that hypothetical open-ended approach can accurately forecast demand curves.

We employed three treatments: hypothetical, hypothetical with cheap talk, and non-hypothetical tasks. For the non-hypothetical treatment, we utilized the BDM mechanism in the conduct of the actual transactions to ensure that it is in the participant's best interest to state his or her true WTP. In the BDM mechanism, a subject only gets the product if his or her bid is greater than a randomly picked market price. Hence, a bidder who submits a bid greater than the market price receives the good and pays an amount equal to the market price (Noussair, Robin, and Ruffieux, 2004). If the subject's WTP is lower than the randomly picked price, no transaction takes place. Bidders have an incentive to bid truthfully because the number of winners is unknown before the bidding and does not have to be limited to one or few bidders. Since subjects have incentive to reveal their true WTP, the BDM mechanism is considered incentive compatible (Lusk, Feldkamp, and Schroeder, 2004). Precedence for this can be seen in Irwin et al. (1998), Wertenbroch and Skiera (2002), and Ding (2007), who all utilized elicitation procedures that involved the BDM mechanism.

Experimental Design

We conducted experiments at retail stores at different times of the day and different days of the week. Our sample consists of 173 subjects. We also obtained demographic, consumption, and attitudinal information from the subjects. As presented in tables 2 and 3, we have three treatments in our experimental design:[3] 1) non-hypothetical, 2) hypothetical, and 3) hypothetical with cheap talk. The non-hypothetical treatment utilized a BDM mechanism to conduct the actual transactions.

Our study was conducted in February 2007 at selected grocery stores in the south-central United States. We conducted a field experiment after considering the advantages identified by Lusk and Hudson (2004b), which include the following: subjects are in a more familiar environment, lower compensatory fees are necessary, natural availability of complement and substitute goods, and a greater ability to target the population of interest. In our case, a field experiment also allows us to get a wide range of demographic characteristics, minimize participation fees, and test cheap talk in a setting close to a real purchase situation. The recruited subjects were adult shoppers (at least 18

[2] In a consequential task, subjects have to believe that their responses have a direct impact on a public policy, so it is in their best strategy to truly reveal their preferences.

[3] The study was conducted in retail stores. The sample sizes in the treatments are different since some stores (days/time) were busier than others. However, we did not find any significant store and day of the week effects in the analysis.

Table 2. Descriptive Statistics of the Variables

Variables	Type of Variable	Mean[a]	SD	Min	Max	n
Non-hypothetical treatment	Indicator	0.24	0.43	0	1	173
Hypothetical treatment	Indicator	0.34	0.48	0	1	173
Hypothetical with cheap talk treatment	Indicator	0.42	0.50	0	1	173
Gender (female)	Indicator	0.54	0.50	0	1	173
Income (less than 19,000 dollars)	Indicator	0.47	0.50	0	1	173
Race (black)	Indicator	0.12	0.33	0	1	173
Education (less than 12 years)	Indicator	0.02	0.15	0	1	173
Grapefruit consumption (pounds)	Continuous	0.50	0.50	0	1	173
People at home under 14 years (number)	Continuous	0.43	0.95	0	6	173
Rating of overall study complexity (units)	Continuous	2.41	2.28	1	10	166
Rating of cheap talk paragraph understanding (units)	Continuous	1.89	1.70	1	10	72
Cubes without preservatives ($)	Continuous	1.58	0.93	0	5	173
Cubes with preservatives ($)	Continuous	1.38	0.94	0	5	173
Segments without preservatives ($)	Continuous	1.66	0.93	0	5	173
Segments with preservatives ($)	Continuous	1.41	0.94	0	5	173

Notes: [a] Most of the variables are indicators. For instance, in the case of non-hypothetical open-ended group, the variable has a value of "one" if the subject belongs to treatment 1 and "zero" otherwise. In the dataset, 24% of the subjects were from that group.

Table 3. Mean and Proportion of Zeros per Treatment and Product

Treatment	n		Cubes wo/p	Cubes w/p	Segments wo/p	Segments w/p
Non-Hypothetical	41	Mean ($)	1.5	1.35**	1.59*	1.32**
		SD	0.85	1.12	0.93	0.95
		Proportion of zeros	2%	7%	0%	5%
Hypothetical	59	Mean ($)	1.83	1.56	1.87	1.67
		SD	0.93	0.84	0.94	0.93
		Proportion of zeros	3%	8%	0%	5%
Hypothetical with	73	Mean ($)	1.44**	1.26**	1.52*	1.25**
Cheap Talk		SD	0.93	0.91	0.9	0.91
		Proportion of zeros	3%	10%	1%	10%

Notes: wo/p indicates without preservatives and w/p indicates with preservatives. Single and double asterisks (*) denote statistical significance at the 10% and 5% using the Kolmogorov-Smirnov test to compare hypothetical treatments.

years old) randomly chosen in the retail stores. No subject was allowed to participate in more than one treatment. Each interview or session was designed to last no longer than ten minutes in order to avoid respondent fatigue.

Our experimental instructions were pre-tested using focus groups involving college students and again using a small sample of individuals in a grocery store.[4] These pre-tests were useful in evaluating the wording and length of the questionnaire and instructions. They also served as practice for our interviewers. We made sure that the interviewers followed the exact protocol and procedures to avoid interviewer effects. Following Ortmann's 2005 recommendations, we also developed a comprehensive experimental protocol to avoid irregularities between treatments for our experimental study. The only difference was that the hypothetical treatments with the cheap talk script included two additional questions about the cheap talk paragraph that were given to subjects at the end of the experiment after the WTP elicitation.

Each subject received $4 for participating at the end of the survey (in both the hypothetical and non-hypothetical treatments). For those respondents who purchased an elicited product, the purchase price was deducted from the initial four dollar endowment. We used a generic, short, and

[4] The questionnaire and cheap talk script are available from authors upon request.

neutral cheap talk script. Our script was inspired by the one utilized by Cummings and Taylor (1999), which we modified for our purposes. First, we made it generic and did not make any reference to the elicited product. Second, we made it shorter to make it more applicable in a field experiment. Third, our wording took into account the recommendations expressed by Aadland and Caplan (2006). We did not use words such as "overstate" or "higher" to avoid biasing the responses to a certain side. Instead, we phrased statements such as "people tend to act differently when they face a hypothetical decision." In addition, we did not use words that could create a strong visual reference for the subject. For instance, words that bring to mind images of disease may generate an overreaction in the subject. In summary, our cheap talk script is unique in the sense that it is generic with respect to the elicited good, short enough to be applied in a retail/field study, and neutral with respect to the direction of the hypothetical bias.

In our WTP study, we used four value-added grapefruit products that varied in attributes related to product form (segmented or cubed) and presence of preservatives. This project was part of a larger study of value-added grapefruit product marketing. Grapefruit is a familiar product to many people in our study area. We conducted focus groups prior to conducting the WTP study to assess the attributes most important to consumers. In these focus groups, the attributes that were identified as most important when purchasing value-added grapefruit products were segments, cubes, and presence of preservatives. Segments are the naturally-occurring grapefruit sections while cubes refer to chopped grapefruit products.

For all the treatments, respondents were shown the products that were randomly ordered and were then asked to indicate, in an open-ended manner, the amount they would be willing to pay for each product. The only difference between the hypothetical treatments and non-hypothetical treatment is that in the latter, we used the BDM mechanism. Specifically, respondents in the non-hypothetical treatment were told that after they have informed us of their WTP for each of the products, one product will be randomly picked as the binding product. In addition, before the WTP elicitation, we randomly picked a cutoff price for each product for each participant to determine if he or she will get the product and pay the randomly chosen cut-off price. If his or her WTP is lower than the randomly picked price, he or she does not get the product and hence, no transaction takes place. This BDM mechanism was thoroughly explained to all subjects in the non-hypothetical treatment and we informed them that it is in their best interest to state their true WTP. These respondents were also encouraged to ask questions about the BDM mechanism prior to the actual WTP elicitation.

We pooled our cross-sectional data by type of product, creating a panel-like structure when all treatments were merged. A random-effects tobit model was used to assess treatment effects on WTP. We used a censored model due to the presence of zero WTP values in the data. The participants were explicitly told that they could provide a WTP of zero dollars if they did not want the product. Consequently, the proposed model considers cross-sectional heterogeneity and censored data and can be described as:

$$WTP_{ip} = \boldsymbol{x}'_{ip}\beta + u_i + v_{ip} \,, \tag{1}$$

where subscript i identifies the subject and p the grapefruit product (cubes without preservatives, cubes without preservatives, segments without preservatives and segments with preservatives), WTP can take zero or positive values, $\boldsymbol{x}_{ip}$ is a vector of independent variables (gender, income, education, consumption level, children and complexity of the task), u_i is the disturbance term per subject (cross section), and v_{ip} is the overall disturbance term. Taking advantage of the panel structure, we are able to gain degrees of freedom while comparing the treatments. Some past studies have also used similar models. For example, Lusk and Fox (2003) used a random effects tobit model to compare non-hypothetical and hypothetical treatments in lab and field settings. Haab, Huang, and Whitehead (1999) found that the difference between hypothetical and non-hypothetical WTP estimates can be caused by heteroskedasticity rather than differences in consumer's valuations. The authors proposed a scale factor to provide a common base of comparison. An illustrative example can be found in

the work done by Lusk, Fields, and Prevatt (2008) for an incentive compatible conjoint ranking mechanism. Alternatively, Cummings and Osborne Taylor (1998) used clustering to control for heteroskedasticity patterns in experimental data; this is the procedure that we followed.

Results

Overall, 188 subjects agreed to participate in our survey. Due to missing responses, we ended up using 173 subjects in our analysis. While we did not record the number of consumers who refused to participate in our study, this number was quite small based on field observations. Table 2 presents descriptive statistics. In addition to the treatment variables, other control variables include gender, income, race, education, typical grapefruit consumption (in pounds per month), presence of children, and rating on the overall complexity of the study (ranges from 1 or "very easy" to 10 or "very difficult"). Respondents rated the complexity of the study with an average rating of 2.41, suggesting that the study was perceived to be not very complicated or difficult. Subjects in the hypothetical with cheap talk treatment were also asked an additional question about how difficult was the cheap talk script to understand (CT understanding), ranging from 1 (very easy) to 10 (very hard). Average rating from this question is 1.89, suggesting that respondents in the cheap talk treatment considered the script as easy to understand.

Table 3 presents the means and standard deviation per treatment and per product and the proportion of zero values. Interestingly, in our study, the hypothetical/non-hypothetical WTP mean ratio is about 1.2 and the mean ratio of the hypothetical without cheap talk and hypothetical with cheap talk WTP is 1.27. Murphy et al. (2005) observed a ratio of 1.35 in their meta-analysis and Lusk and Schroeder (2004) also found a mean ratio of 1.2 using a choice experiment. We conducted Kolmogorov-Smirnov (KS) tests to compare the WTP distributions from the treatments. Results show no significant differences between the non-hypothetical and the hypothetical with cheap talk treatments. However, in most of the products, there is a significant difference between the hypothetical (without cheap talk) treatment and the non-hypothetical/hypothetical with cheap talk treatment.

Table 4 shows the random effects tobit model parameter estimates. The results suggest that, as expected, the WTPs in the hypothetical without cheap talk treatment are significantly higher than the WTPs in the non-hypothetical treatment, suggesting the existence of hypothetical bias in the hypothetical without cheap talk treatment. However, the WTPs in the hypothetical with cheap talk treatment are not statistically different from the WTP in the non-hypothetical treatment, suggesting that our cheap talk script was effective in eliminating the hypothetical bias. In addition, we found that product effects are evident. These effects suggest that the two products with preservatives tend to be valued less than cubes without preservatives (base product). Results also show that presence of children, income, race, education, and grapefruit consumption level significantly affect WTP. Notably, less educated respondents (i.e., those with less than 12 years of education) bid more than higher educated respondents, those with children in the household bid more than others, and blacks bid more than non-blacks. Grapefruit consumption level is negatively related to WTP. Also, those who thought our study was complicated tend to bid more than others. However, less educated respondents who also thought that the study was complicated have lower bids than others.

As mentioned previously, subjects in the hypothetical with cheap talk treatment were asked to answer an additional question: "How difficult was the cheap talk script to understand: from 1 (very easy) to 10 (very hard) (i.e., CT understanding variable)? We calculated a random effects tobit model using only data from the hypothetical with cheap talk treatment with and without this variable and its interaction effects (see table 5). We included the interaction terms of the "CT understanding" variable and demographic variables in the expanded model to check if the effect of level of difficulty of understanding our cheap talk script varies with demographic groups. Results indicate that rating on how difficult or easy it is to understand the cheap talk script is not statistically significant along with the interaction effects. This result suggests that perceived level of understanding of the cheap

Table 4. Three-Treatment Random-Effect Tobit Model

Product Indicators	**Open-Ended**
Cubes without preservatives	Base
Cubes with preservatives	−0.23**
	(0.06)
Segments without preservatives	0.09
	(0.06)
Segments with preservatives	−0.20**
	(0.06)
Treatment Indicators	
Non-hypothetical	Base
Hypothetical	0.30**
	(0.16)
Hypothetical with cheap talk	−0.06
	(0.15)
Demographics	
People at home under 14 years	0.15**
	(0.06)
Gender (female)	−0.17
	(0.12)
Income (less than 19,000 dollars)	0.23**
	(0.12)
Race (black)	0.37*
	(0.20)
Education (less than 12 years)	0.53**
	(0.19)
Grapefruit consumption (pounds)	−0.04*
	(0.02)
Rating of overall study complexity (units)	0.10**
	(0.04)
Complexity×Education	−0.10*
	(0.05)
Intercept	1.12**
	(0.20)
Sigma_u	0.68
	(0.04)
Sigma_e	0.54
	(0.02)
Rho	0.62
	(0.04)
n (full data set)	660
Subjects	165
Prob $> \chi^2$	0

Notes: Single and double (*) denote statistical significance at the 10% and 5% levels, respectively. Values in parentheses denote standard errors.

talk script did not have a bearing on bids of subjects in this treatment and that this insignificant effect does not vary with demographics.[5] This finding is likely driven by the fact that most of our subjects

[5] We also estimated the model in table 4 with interaction terms between the treatment variables and demographics and did not find these effects statistically significant.

Table 5. Three-Treatment Random-Effect Tobit Model

Product Indicators	Without Interaction Effects	With Interaction Effects
Cubes without preservatives	Base	Base
Cubes with preservatives	−0.23**	−0.23**
	(0.08)	(0.08)
Segments without preservatives	0.08	0.08
	(0.08)	(0.08)
Segments with preservatives	−0.24**	−0.24**
	(0.08)	(0.08)
Demographics		
People at home under 14 years	0.22	0.47*
	(0.17)	(0.27)
Gender (female)	−0.17	−0.50*
	(0.18)	(0.28)
Income (less than 19,000 dollars)	0.34*	0.43
	(0.19)	(0.28)
Race (black)	0.28	0.53
	(0.31)	(0.57)
Education (less than 12 years)	0.55**	0.29
	(0.20)	(0.32)
Grapefruit consumption (pounds)	−0.08	−0.12*
	(0.06)	(0.07)
Rating of overall study complexity (units)	0.03	0.02
	(0.05)	(0.05)
Cheap Talk Variables		
Cheap talk paragraph understanding (units)		−0.003
		(0.11)
CT understanding×Under 14 years		−0.13
		(0.12)
CT understanding×Gender		0.16
		(0.12)
CT understanding×Income		−0.11
		(0.12)
CT understanding×Race		−0.06
		(0.14)
CT understanding×Education		0.12
		(0.13)
Intercept	1.13**	1.30**
	(0.23)	(0.30)
Sigma_u	0.67**	0.64**
	(0.06)	(0.06)
Sigma_e	0.47**	0.47**
	(0.02)	(0.02)
Rho	0.67	0.65
	(0.05)	(0.05)
n (full data set)	280	280
Subjects	70	70
Prob > χ^2	0	0

Notes: Single and double asterisks (*, **) denote statistical significance at the 10% and 5%, respectively. Values in parentheses denote standard errors.

considered our cheap talk script very easy to understand, as evidenced by the very low mean of our "CT understanding" variable.

Product effects are consistent with the results from table 4 that included subjects from all treatments and are also robust with and without the "CT understanding" variable. Interestingly, in the model without the interaction effects, the less educated respondents tend to bid higher than higher educated respondents, suggesting that education level can have an influence on the effectiveness of cheap talk script in reducing hypothetical bias. Similar education effects are also observed in our overall model in table 4.

Concluding Remarks

There is considerable evidence and agreement that people in hypothetical situations behave differently compared to those in real situations. Particularly in WTP studies, subjects tend to overstate their WTP in hypothetical valuation experiments. Evidence of this hypothetical bias is widespread (Neill et al., 1994; Cummings, Harrison, and Rutström, 1995; Loomis et al., 1997; List and Gallet, 2001) and could be due to strategic manipulation (Carson and Groves, 2007), uncertainty (Johannesson et al., 1999), or social desirability (Carson and Groves, 2007) on the part of the respondents. While the use of cheap talk is not new, our objective in this study is to test a relatively new cheap talk script that has not been examined in the past. Specifically, we used a (1) generic script that did not make any reference to the elicited product; (2) made it shorter to make it more applicable in a field experiment; (3) did not use words such as "overstate" or "higher" to avoid biasing the responses to a certain side; and (4) did not use words that could create a strong visual reference for the subject. In short, our cheap talk script is relatively different from what has been used in previous studies in the sense that it is generic with respect to the elicited good, easy to understand, and short enough to be applied in a retail/field study, and neutral with respect to the direction of the hypothetical bias.

Another reason for revisiting cheap talk is that the literature has provided mixed results in terms of the effectiveness of cheap talk in reducing hypothetical bias. For example, Aadland and Caplan's (2006) results using contingent-valuation method draw into question cheap talk's effectiveness as an ex-ante correction tool for hypothetical bias. Applications of cheap talk in field experiments are also limited. To accomplish our goal, we used a field experiment involving a hypothetical without cheap talk, hypothetical with cheap talk, and non-hypothetical treatments using an open-ended elicitation mechanism.

In contrast to Aadland and Caplan's finding that neutral cheap-talk scripts actually exacerbate the hypothetical bias problem, our results suggest that the WTP values are higher in hypothetical without cheap talk treatment than in non-hypothetical treatment, suggesting the existence of hypothetical bias. However, the WTP values in the hypothetical with cheap talk treatment are not significantly different from the values in the non-hypothetical treatment. This finding suggests that our cheap talk script eliminated the hypothetical bias. As indicated above, unlike other studies on cheap talk (i.e., Cummings and Taylor, 1999; Landry and List, 2007), we used a generic, short, and neutral cheap talk script. Since we conducted our study in a retail setting, we needed to use a short cheap talk script. Finally, we wanted to use neutral content to avoid potentially biasing the subject's responses to a particular side. In other words, we wanted our subjects to be aware of the hypothetical bias issue, but not explicitly tell them what to do about it.

Our study has provided more insights into how cheap talk can be an effective correction tool for hypothetical bias in private good valuation in a field setting using an open-ended elicitation mechanism. A limitation of our study is that we did not test different types of cheap talk scripts. We tested, however, a relatively short and neutral script that can easily be generalized to other private goods. While we tested our script in a field setting, it should also be useful in other settings such as in phone surveys where a short clear script would be necessary. Future studies, however, should test the effectiveness of our cheap talk script in other settings (e.g., lab or other field settings) and using other types of elicitation mechanisms (e.g., dichotomous choice, choice experiments) to test the robustness of our findings.

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