Cognitive Dissonance as a Means of Reducing Hypothetical Bias

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Abstract

Hypothetical bias is a persistent problem in stated preference studies. We propose and test a method for reducing hypothetical bias based on the cognitive dissonance literature in social psychology. A central element of this literature is that people prefer not to take inconsistent stands and will change their attitudes and behavior to make them consistent. We find that participants in a stated preference willingness-to-pay study, when told that a nonhypothetical study of similar goods would follow, state significantly lower willingness to pay than participants not so informed. In other words, participants adjust their stated willingness to pay to avoid cognitive dissonance from taking inconsistent stands on their willingness to pay for the good being offered.

Keywords: apples; cognitive consistency; hypothetical bias; instrument calibration; willingness to pay.

JEL classification: C91; Q13; Q51

1. Introduction

The theory of cognitive dissonance proposed by Leon Festinger is one of the most influential and extensively studied theories in social psychology (Aronson et al., 2007, pp. 159–89). In the seminal contribution *A Theory of Cognitive Dissonance* (1957), Festinger defines cognitive dissonance as an uncomfortable feeling caused by simultaneously holding two contradictory cognitions. These cognitions may be attitudes and beliefs or awareness of one's own behavior. Festinger argues that people have a motivational drive to reduce dissonance by changing their attitudes, beliefs, or behaviors.[1] Subsequent research has shown that the dissonance is strongest when people behave in ways that threaten their self-image (Aronson, 1969). Building on this literature, Johansson-Stenman and Svedsäter (2008) argue that people who want to see themselves as rational and thoughtful, as well as honest and trustworthy, have a motivational drive to try to give consistent responses to a series of questions. In this paper, we propose utilizing this motivational drive to reduce the well-known hypothetical bias in valuation studies.

Hypothetical bias is a persistent problem in stated preference studies. It arises when respondents are more willing to spend their money when asked nonconsequential survey questions than when they respond to consequential questions about valuation or willingness to pay (WTP), i.e., questions resulting in the payment of real money. This means that survey-based valuation studies usually yield higher WTP estimates than valuation studies using nonhypothetical methods with real economic consequences. For meta studies on hypothetical bias, see List and Gallet (2001) and Murphy et al. (2005).

[1] Cognitive dissonance is often compared with hunger and thirst: discomfort motivates us to eat or drink (see, for example, Aronson et al., 2007, p. 160).

Several instrumental calibration methods have been suggested to correct for hypothetical bias. Of these, *cheap talk* and *certainty scale calibration* have received the most attention (Harrison, 2006). Cheap talk attempts to eliminate hypothetical bias through explicit discussion of the problem. A script in which the hypothetical bias problem is described is presented by the interviewer or attached to the valuation survey, and the respondents are asked to bear this in mind and answer as if they were in a real situation. Cummings and Taylor (1999), who first presented cheap talk as a method to alleviate hypothetical bias, concluded that the cheap talk design was successful in eliciting responses to hypothetical valuation questions that were indistinguishable from responses to valuation questions involving actual payment. However, later research shows mixed results for the ability of cheap talk to alleviate hypothetical bias (List, 2001; Murphy et al., 2005; Blumenschein et al., 2008).

Certainty scale calibration is an ex post technique used to correct for hypothetical bias in dichotomous choice questions (Blumenschein et al., 2008). In certainty scale calibration, the WTP question is followed by a question that asks how sure respondents are about their response to the valuation question. The follow up certainty questions typically take one of two forms: definitely sure/probably sure (Blumenschein et al., 2008) or a 10-point scale with 10 as very certain (Champ et al., 1997). In the estimation of WTP, only 'Yes' responses followed by definitely sure or at least 8 on a 10-point certainty scale (Champ et al., 1997) are included as valid yes answers. However, as noted by Murphy and Stevens (2004), any method that reduces the number of valid yes responses will reduce WTP in dichotomous choice studies. Further, the empirical results for what should count as a valid yes differ from study to study, and finally, yet importantly, there is no theoretical underpinning for the method. It should also be noted that the

certainty scale calibration is not easily used with valuation formats other than dichotomous choice questions.

In this paper, we propose and test a new type of instrument calibration method based on cognitive dissonance theory. We refer to our method as *real talk.* Real talk is to inform participants in a hypothetical valuation study that a nonhypothetical valuation study with similar but not necessarily identical goods will be conducted after the hypothetical study. In contrast to the certainty scale calibration, the real talk method is not limited by the question format, as most valuation question formats can be made nonhypothetical. Vickrey (1961) auctions and the Becker, DeGrooth and Marschack (1964) mechanism are examples of open-ended nonhypothetical valuation questions, while Lusk and Schroeder (2004) and Alfnes et al. (2006) are examples of nonhypothetical choice experiments. The real talk method, on the other hand, is limited to goods that the participants believe can be included in a nonhypothetical study. These include, for example, private goods, like the apples in this analysis, or public good donations, such as those described by Veisten and Navrud (2006) and Johansson-Stenman and Svedsäter (2008).

So how does the real talk method work? Let us assume that an open-ended hypothetical valuation question is followed by an incentive-compatible nonhypothetical valuation question, and that the participants are informed about this at the beginning of the study, namely, that the real talk method is to be applied. If the participants wish to answer the hypothetical and nonhypothetical valuation questions consistently, they have three possible strategies. They could answer the hypothetical valuation question as if it were a nonhypothetical valuation question; they could answer the nonhypothetical valuation question as if it were a hypothetical valuation question; or they could choose something in between. The latter two strategies come at the cost

of potentially having to buy the product in the nonhypothetical valuation question at a price that is higher than the value they attach to the product. Therefore, if the participants wish to avoid cognitive dissonance from inconsistent answers, the weakly dominant strategy is to answer the hypothetical valuation question as if it were nonhypothetical.

In the real talk design, we attempt to turn the arguably major weaknesses of within-subject design, that the results of the various parts of the study may be confounded, into something positive. For example, this weakness is the basis of Harrison's (2006) critique of Carlsson and Martinsson's (2001) test of hypothetical bias in choice experiments.

In a test of hypothetical bias in choice experiments, Johansson-Stenman and Svedsäter (2008) conducted both within-subject and between-subject tests, with the subjects split into two groups. In the first group, respondents first took part in a stated choice experiment, and subsequently in a nonhypothetical choice experiment with real economic consequences. However, contrary to real talk design, the subjects were not told that they were to face real money choices immediately after completing the hypothetical choices. In the second group, the participants only took part in the nonhypothetical choice experiment. Comparing the bids given by the two groups, they found the largest WTP estimates were in the hypothetical choice experiment, which is consistent with the literature on hypothetical bias. Second largest were the WTP estimates from the nonhypothetical choice experiment conducted after the hypothetical experiment; smallest were the WTP estimates from the group that took part only in the nonhypothetical choice experiment. Johansson-Stenman and Svedsäter (2008, p. 6) argue that the difference in WTP estimates between the real after-hypothetical treatment and the real-direct treatment is "expected due to the importance of maintaining cognitive consistency." This shows

that respondents are willing to change their bids and, in this case, even risk a potential economic loss to avoid cognitive dissonance.

The design used by Veisten and Navrud (2006) in a contingent valuation study for the World Wildlife Fund (WWF) Forest Fund is one that comes closest to what we propose. Veisten and Navrud found a significant reduction in the level of yea-saying in a dichotomous choice question when the respondents received an invoice for actual payment together with the contingent valuation survey. This is a promising result with respect to the effect of telling the respondents that a nonhypothetical question will follow the hypothetical valuation question. The rest of this paper proceeds as follows. Section 2 describes the market experiment and the econometric models used in the analysis. Section 3 presents and discusses the results. Section 4 concludes the paper.

2. Market Experiment

The purpose of the market experiment was twofold. First, we wished to investigate the impact of cosmetic product damage on consumer WTP for organic and conventional apples. The results of this investigation are presented in Yue et al. (in press). Second, we wanted to investigate the effect of the real talk method. The results of the latter investigation are presented in the current paper.

The experiment was conducted on the campus of a large U.S. Midwestern university in February 2005. Upon arrival, participants were given a folder with U.S.$20, a consent document, and a questionnaire. They were told that the money was now theirs, and were asked to complete the consent document and the questionnaire. We subsequently conducted an open-ended WTP

study phrased as a hypothetical auction in which the alternatives were presented with pictures, followed by a nonhypothetical auction with real products.

2.1 Sample

Participants were recruited by e-mail and advertisements in newsletters at the university campus. The e-mail recruitment of participants went to faculty and staff through college-level and university unit solicitations in order to assure nonstudent participation. See Table 1 for descriptive statistics of the sample. We used Fisher's exact test to see if there was a relationship between the treatments and the socioeconomic characteristics of the subjects. The results suggest that there was no statistically significant relationship between treatment and gender ($p = 0.97$), treatment and income ($p = 0.88$), treatment and education ($p = 0.46$), or treatment and association with the university ($p = 0.47$), while the relationship between treatment and age ($p = 0.06$) was marginally insignificant.

2.2 Products

The products used were 3-pound bags of golden delicious apples obtained from commercial sources and university farm orchards. Prior to the experiment, the apples were sorted according to their production method (conventional and organic) and appearance (level of surface blotches caused by sooty blotch fungi). The four appearance categories included SpotA, SpotB, SpotC, and SpotD with surface blotches, or spots, ranging from SpotA as apples with no spots to SpotD as apples about one-tenth covered with spots. The sorted apples were packed into clear bags for easy inspection. In the hypothetical auctions, the apples were replaced with pictures of apples.

2.3 *Elicitation of value*

We used the full-bidding approach (Lusk and Shogren, 2007) with a uniform-price sealed-bid auction. The participants simultaneously bid on 12 alternatives. The winners are the *n* highest bidders and the price equals the (*n* + 1) highest bid. Vickrey (1961) showed that in uniform-price sealed-bid auctions with single unit buyers and the price determined by the highest rejected price, it is a weakly dominant strategy for people to bid their WTP for products with independent private values. Subjects have an incentive to reveal their private preferences truthfully because the auction separates what they say from what they pay. Underbidding participants risk forgoing profitable purchases, whereas overbidding participants risk unprofitable purchases.

The pictures of apples and the apples were presented on a large table and the participants walked around the table and placed their bids as they considered each alternative. After the auction, each participant randomly drew his or her exclusive binding alternative. The drawing was done without replacement, so each participant could draw a unique alternative as his or her binding alternative. For this to be possible, the number of alternatives had to be greater than or equal to the number of participants in each session. The price of an alternative was equal to the fourth-highest bid for that alternative. If the participants had bid higher than the price for their binding alternative, they were asked to buy the alternative. The apples they evaluated were the same apples they would purchase. For a thorough introduction to the use of experimental auctions to value products and product attributes, see the book by Lusk and Shogren (2007).

2.4 Treatments

The eight sessions were divided into four treatments: No Talk (NT), Cheap Talk (CT), Real Talk (RT), and Real Auction (RA). The four treatments included 18, 18, 20, and 22 participants,

respectively. In three of the four treatments (NT, CT, and RT), we first conducted a hypothetical auction in which the apples were represented by pictures. A hypothetical auction is in fact an ordinary open-ended contingent valuation study phrased as an auction. We specified to participants that no one would buy any apples because of the auction's hypothetical nature. After the hypothetical auctions in the NT, CT, and RT treatments, we replaced the pictures with apples and ran a second auction with a nonhypothetical fourth-price auction.

In the NT treatment, we did not include any instrument calibration. The monitor simply directed the participants to "*please, try to behave in the hypothetical auctions as if it were a real auction*." In the CT treatment, the monitor used a 493-word-long cheap talk script adopted from List (2001). At this point, participants in the NT and CT treatment were not aware that we had apples in the room next door. In the RT treatment, the monitor said, "*Before you start, I want to inform you that right after the hypothetical auctions we will conduct a similar series of real auctions with real apples. We have apples similar to what you will see in the pictures and we will repeat the whole procedure with real apples. At the end of the real auction, we will sell apples to the participants that have bid higher than the price in their binding real auction. Try to behave in the hypothetical auctions as if you were a part of the real auctions.*" In the fourth treatment, the RA treatment, we did not run a hypothetical auction. Instead, we ran two trials with a real auction. To avoid income and substitution effects in the RA treatment, we randomly drew which of the two auction trials were binding before drawing the individual binding products. Please see Table 2 for details of the four treatments.

2.5 Statistical models

Some of the apple bags deviated slightly from 3 pounds (1,361 grams). To correct for the effect of small variations in weight on participants' bids, we estimated a nonlinear regression model that relates the individual bids to the weight and quality of the apples:

$$Bid_{ni} = (1+\theta * weightdeviation_i)(\beta_1 * SpotA_i + \beta_2 * SpotB_i + \beta_3 * SpotC_i + \beta_4 * SpotD_i + \beta_5 * Org_i) + \varepsilon_{ni} \quad (1)$$

where Bid_{ni} is individual *n's* bid on alternative *i*, $weightdeviation_i$ is the weight of alternative *i* in grams expressed as a ratio of the 3 pound (1,361 gram) weight, and θ is the marginal valuation of apples relative to the valuation of the first 3 pounds. If θ = 0, then the participant places no value on the weight deviation, i.e., they bid as if all bags are exactly 3 pounds. If θ = 1, then the consumer places the same value on the marginal apple as the average apple, i.e., they bid 10 percent more for a bag of apples that weighs 1.1 × 1,361 = 1,497 grams than a bag of apples of similar quality that weighs 1,361 grams. $SpotA_i$, $SpotB_i$, $SpotC_i$, $SpotD_i$, and Org_i are dummies that respectively equal one if alternative *i* is SpotA, SpotB, SpotC, SpotD, and organic, and zero otherwise; and β_1 to β_5 are the WTP values associated with 3 pounds of apples with the respective qualities.

Taking the relative valuation of the marginal apples into account, we calculate participants' WTP for 3 pounds of apples as

$$\widehat{WTP}_{ni} = Bid_{ni} / \left(1+\hat{\theta} * weightdeviation_i\right) \quad (2)$$

where $\hat{\theta}$ is the marginal valuation estimated in equation (1).

We estimate a nonlinear calibration function that relates the difference in WTP between the hypothetical and nonhypothetical auction rounds to the treatment effects, an organic dummy, and the socio-demographic effects. The treatment effects are expressed as a second-degree polynomial of the WTP in the nonhypothetical auction. The socio-demographic variables provide statistical controls for the differences in the sample distribution, although these differences were not found to be significant (as discussed earlier). As the function is estimated with a relatively small sample and includes socio-demographic variables, we do not include any fixed or random effects:

$$WTP1_{ni} - WTP2_{ni} = \left(1 + \alpha_2 * CT_n + \alpha_3 * RT_n\right) * \left(\beta_0 + \beta_1 * WTP2_{ni} + \beta_2 * WTP2_{ni}^2 + \beta_3 * Org_i + \beta_4 * Age0_n + \beta_5 * Gen0_n + \beta_6 * Inc0_n + \beta_7 * Edu0_n\right) + \varepsilon_{ni} \quad (3)$$

where $WTP1_{ni}$ is the WTP of individual *n* for alternative *i* in the first round, $WTP2_{ni}$ is his or her WTP in the second round; CT_n and RT_n are dummies that are equal to one if the respondent was included in the CT and RT treatment, respectively, and zero otherwise; α_2 and α_3 are treatment-specific parameters; β_0, β_1 and β_2 are parameters in the second-degree polynomial; Org_i is a dummy variable equal to one if alternative *i* is organic, and zero otherwise; β_3 is a measure of whether there is a difference between conventional and organic apples; $Age0_n$, $Gen0_n$, $Inc0_n$, and $Edu0_n$ are normalized socio-demographic variables for age, gender, income, and education with a zero mean and a standard deviation of one; and β_4 through β_7 are the corresponding parameters. If there were no treatment effects, the hypothetical bias would be the same for all

three treatments, and so α_2 and α_3 would equal zero. If the hypothetical bias is the same for the organic and conventional apples, β_3 equals zero. If there is no hypothetical bias, then β_0 through β_3 equals zero.

3. Results and Discussion

Table 3 presents the results of the nonlinear regression in Equation (1) on how any weight deviations affect the bids.[2] The bids do not fully account for the variation in weight, and the weight deviation was only 46 percent of the average value of the apples in the first 3 pounds. We use Equation (2) to estimate the WTP for 3 pounds of apples, based on the bids and the marginal valuation of the apples. Table 4 presents the weight-corrected WTP for 3 pounds of conventional and organic apples with various degrees of cosmetic damage.

3.1 Differences among the auctions

We conduct six nonparametric tests of differences between the auctions. In all six tests, we include the six alternatives included in all auctions (organic SpotA, conventional SpotA, Organic SpotB, conventional SpotB, conventional SpotC, and conventional SpotD). In the first three tests we test for differences in WTP, and in the last three tests we test for differences in marginal WTP. The marginal WTP is the difference in WTP between two alternatives, calculated so that on average the marginal WTP is positive. Because we include six alternatives, there are 15

[2] A participant with only half of the first round pairwise rankings in the expected order and bids on the organic SpotC and SpotD more than ten times higher than the average bid was excluded from the remaining analysis.

marginal WTP estimates ($C_6^2 = 6*5/2 = 15$) for each participant calculated from each of the two auction rounds. Since rational consumers would bid in an auction so that they get at least the same surplus as if they bought a product in the same product category in the market, the WTP in experimental auctions are limited by the prices in the outside market (Alfnes, in press). However, close substitutes are affected the same way, and bid differences and marginal WTP are therefore the preferred WTP measure in most studies. In what follows, we conduct tests on both bids and bid differences. All tests are conducted using SAS.

First, we use a Wilcoxon signed-rank test to see if the WTP is the same in the first and second rounds of the auctions. The average WTP for the six alternatives in rounds 1 and 2 were 1.94 and 1.16 in the NT treatment, 2.67 and 1.60 in the CT treatment, 1.95 and 1.33 in the RT treatment, and 1.45 and 1.39 in the RA treatment. For all of the hypothetical treatments, we reject equal WTP in the first and second round of the auction with *p*-value = 0.00 for each of the treatments. For the RA treatment, we do not reject equal WTP in the first and second rounds of the auctions, as *p*-value = 0.86. It is therefore likely that the change in WTP from the first to the second rounds of the auctions in the NT, CT, and RT treatments are because the first round is hypothetical while the second round is not. It is worth noting that the first rounds of the auctions in the three hypothetical treatments are hypothetical in two senses: first, no real economic incentive, and second, no real products (i.e., product pictures only).

Second, we use a Mann–Whitney test to see if the first round WTP is the same for all four treatments. We reject the null hypotheses of equal WTP in the first round for NT and CT (*p*-value = 0.00), NT and RA (*p*-value = 0.01), CT and RT (*p*-value = 0.00), RT and RA (*p*-value = 0.01), and CT and RA (*p*-value = 0.00), but not for NT and RT (*p*-value = 0.64). The CT

treatment reports a significantly higher WTP than the other two hypothetical treatments, while the RA treatment reports a significantly lower WTP than any of the hypothetical treatments.

Third, we use a Mann–Whitney test to find if the change in WTP between the first and second rounds of the auctions is the same for all four treatments. The average changes in WTP between the first and second rounds of auctions were 0.78, 1.07, 0.62, and 0.07 for the NT, CT, RT, and RA treatments, respectively. We reject the hypothesis that the change in WTP between the first and second auction rounds is independent of treatment. The difference between the two rounds of auctions is significantly lower in the RA treatment than in the NT (p-value = 0.00), CT (p-value = 0.00), and RT (p-value = 0.00). Furthermore, the difference between the two auction rounds is significantly lower in the RT treatment than in the CT treatment (p-value = 0.00), and lower (but not significantly) in the RT treatment compared with the NT treatment (p-value = 0.26). However, there is no evidence of a cheap talk effect, and in fact the difference in valuation between the first and second rounds of auctions is smaller, but not statistically significant, in the NT treatment than in the CT treatment (p-value = 0.16).

Fourth, we use a Wilcoxon signed-rank test to see if the marginal WTP is the same in the first and second rounds of the auctions. The marginal WTP is the difference in WTP between two alternatives, calculated so that on average the marginal WTP is positive. Because we include six alternatives, there are 15 marginal WTP estimates for each participant calculated from each of the two rounds. The average marginal WTP in the first and second rounds was 1.30 and 0.89, 1.32 and 0.78, 0.79 and 0.59, and 0.79 and 0.61 in the NT, CT, RT, and RA treatments, respectively. We reject equal marginal WTP in the first and second rounds of the auctions for all four treatments, with p-values of 0.00, 0.00, 0.03, and 0.00, respectively. The marginal WTP decreases from the first to the second round for all four treatments. From Table 4, we can see that

the spread in valuation from the first to the second rounds of the auctions falls for all of the alternatives in the RA treatment. Furthermore, the difference between the highest and lowest valued alternatives in the RA treatment falls from US$1.74 to US$1.42. In light of this, the rejection of equal marginal WTP for the first and second rounds of the auctions in the RA treatment comes as no surprise.

Fifth, we use a Mann–Whitney test to evaluate whether the first-round marginal WTP is the same for all four treatments. We reject equal marginal WTP for NT and RT (p-value = 0.00), NT and RA (p-value = 0.00), CT and RT (p-value = 0.00), and CT and RA (p-value = 0.00), but not NT and CT (p-value = 0.88), and RT and RA (p-value = 0.83). The levels of the WTP differ across the four treatments, and the marginal WTP is higher in the NT and CT treatments than in the RA treatment. However, the marginal WTP in the RT treatment is not significantly different from the marginal WTP in the RA treatment.

Sixth, we use a Mann–Whitney test to see if the change in marginal WTP between the first and second rounds of the auctions is the same for the four treatments. We reject equal change in marginal WTP for NT and RT (p-value = 0.00), NT and RA (p-value = 0.00), RT and CT (p-value = 0.00), and CT and RA (p-value = 0.00), but not NT and CT (p-value = 0.37) and RT and RA (p-value = 0.59). Once again, we find that the RT treatment is the method that yields results that are closest to the RA treatment.

3.2 The effect of the hypothetical auctions on subsequent real auctions

We conduct two nonparametric tests of the effect of the hypothetical auctions on the following real auctions. First, we use a Mann–Whitney test to see if the WTP in the second round of the auctions is equal for all four treatments. The average WTP levels for the six alternatives in the

second round were 1.16, 1.60, 1.33, and 1.39 for the NT, CT, RT, and RA treatments, respectively. We reject the null hypotheses of equal WTP in the second round for NT and CT (*p*-value = 0.00), NT and RA (*p*-value = 0.02), CT and RT (*p*-value = 0.03), and NT and RT (*p*-value = 0.05) (at $\alpha = .10$), but not RT and RA (*p*-value = 0.53) and CT and RA (*p*-value = 0.13). Second, we use a Mann–Whitney test to check for differences in marginal WTP between the second rounds of auctions for the four treatments. We reject equal marginal WTP for NT and RT (*p*-value = 0.00), NT and RA (*p*-value = 0.00), CT and RT (*p*-value = 0.04), and CT and RA (*p*-value = 0.04), but not for NT and CT (*p*-value = 0.14), and RT and RA (*p*-value = 0.90). Once again, we can see that the RT treatment is the hypothetical treatment that has most in common with the RA treatment.

3.3 Calibration function

As second approach to measuring the effect of the RT design, we use a nonlinear least square model to estimate a calibration function described in Equation (3), including the treatment dummies, a second-degree polynomial of the real WTP, an organic specific dummy, and four socio-demographic variables. We reject no hypothetical bias $\left(\alpha_t \beta_i = 0 \ \forall \ \ i = 0,...,7\right)$ for all three treatments (Wald statistic = 100.48, *p*-value = 0.00; Wald statistic = 47.29, d.f = 8, *p*-value = 0.00; Wald statistic = 144.67, d.f = 8, *p*-value = 0.00; respectively). We can see from the treatment effects shown in Table 5 that there is no significant difference between the NT and CT treatments. However, the hypothetical bias is significantly lower in the RT treatment than the other two hypothetical treatments (41 and 43 percent, respectively).

Furthermore, we can see that hypothetical bias is significantly higher for organic apples than for conventional apples. This is consistent with earlier studies finding that calibration

factors are product specific (List and Shogren, 1998). It is also consistent with the literature on social desirability bias: in our context, respondents are trying to project themselves as more concerned about the environment than they really are. [3]

4. Conclusion

In this paper, we propose and test an instrumental calibration method that utilizes the premise that people like to be consistent in their answers and do not like to pay more than what they think a good is worth. We find that the use of real talk, which means informing participants in a stated preference study that a nonhypothetical study of similar goods will follow, significantly reduces the hypothetical bias in the stated preference study. We find no similar effect for cheap talk. We therefore conclude that real talk is a valuable addition to the growing toolbox used to handle the hypothetical bias persistently found in stated preference studies.

Real talk can be used in any valuation study in which respondents can be made to believe that a similar product, not necessarily identical, will be offered in a nonhypothetical setting. For example, real talk can be used when researchers are evaluating consumer preferences toward consumer goods still on the drawing board. The products of interest are first presented in a stated preference study using manipulated pictures. Thereafter, similar products from the same product category are offered in a non-hypothetical setting. In this case, the consumers do not know

[3] Social desirability bias has been found to occur in nearly all types of self-reporting measures and across nearly all of the social science literature (Fisher, 1993; King and Bruner, 2000; Leggett et al., 2003; Nederhof, 1985). As the socially acceptable response comes at no cost in the hypothetical auction, the hypothetical auction is more prone to social desirability bias than the nonhypothetical auction.

exactly what will be offered in the second part and must behave in the first part of the study as if real products are being offered.

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Table 1. Descriptive Statistics for the Sample (Mean and Standard deviation)

Variable	Statistics	NT	CT	RT	RA	Total
Age[a]	Mean	40.88	40.26	32.65	41.67	39.05
	St.dev.	(10.00)	(14.29)	(11.47)	(14.25)	(13.02)
Gender[b]	Mean	0.65	0.68	0.65	0.71	0.68
	St.dev.	(0.49)	(0.48)	(0.49)	(0.46)	(0.47)
Education[c]	Mean	4.82	4.63	5.06	5.10	4.91
	St.dev.	(1.29)	(1.50)	(1.25)	(1.18)	(1.29)
Income[d]	Mean	46.76	52.11	43.24	39.64	45.30
	St.dev.	(35.16)	(34.86)	(33.84)	(20.65)	(31.00)
Undergraduate student[e]	Mean	0.06	0.16	0.06	0.00	0.07
	St.dev.	(0.24)	(0.37)	(0.24)	(0.00)	(0.25)
Graduate student[e]	Mean	0.24	0.21	0.53	0.43	0.35
	St.dev.	(0.44)	(0.42)	(0.51)	(0.51)	(0.48)
Faculty[e]	Mean	0.06	0.05	0.06	0.10	0.08
	St.dev.	(0.24)	(0.23)	(0.24)	(0.30)	(0.50)
Staff[e]	Mean	0.53	0.53	0.24	0.43	0.43
	St.dev.	(0.51)	(0.51)	(0.44)	(0.51)	(0.50)

[a] The age question had seven age classes. The midpoints of the age classes are used in the estimation. [b]Male = 0, Female = 1. [c]Less than high school=0; Some high school or less = 1, High school diploma = 2, Some college = 3, College Diploma = 4, Some graduate school = 5, Graduate degree = 6. [d] In USD 1000. The income question had eight income classes. The midpoints of the income classes are used in the estimation. [e]No = 0,Yes = 1.

Table 2. Overview of the Four Treatments

	No Talk (NT)	**Cheap Talk (CT)**	**Real Talk (RT)**	**Real Auction (RA)**
First auction	Hypothetical auction with NT	Hypothetical auction with CT	Hypothetical auction with RT	Real auction
Second auction	Real auction	Real auction	Real auction	Real auction

Table 3. Results of the Weight Correction Model

Variable	Parameter	Std Error	p-value
Weight deviation	0.47	0.28	0.09
SpotA	1.88	0.09	0.00
SpotB	1.31	0.11	0.00
SpotC	0.99	0.12	0.00
SpotD	0.57	0.10	0.00
Organic	0.25	0.07	0.00
Number of observations	1188		
R-squared	0.18		
Adj. R-squared	0.19		

Note: Estimated with Limdep 8.0.

Table 4. WTP for 3 Pounds of Apples[a]

	Conventional apples				Organic apples			
	Mean	**StD**	**Median**	**N**	**Mean**	**StD**	**Median**	**N**
NoTalk: Picture								
SpotA	2.41	1.44	2.00	25	3.41	1.59	3.00	26
SpotB	2.03	2.22	1.82	25	2.37	1.12	2.00	26
SpotC	1.16	0.92	1.00	25	1.52	0.91	1.30	26
SpotD	0.31	0.38	0.00	25	0.89	0.97	0.50	26
NoTalk: Auction								
SpotA	1.72	1.07	1.68	51	2.14	0.95	1.96	51
SpotB	0.76	0.57	0.82	17	1.43	0.87	1.45	51
SpotC	0.66	0.59	0.78	17				
SpotD	0.25	0.39	0.00	17				
CheapTalk: Picture								
SpotA	3.49	2.14	2.60	30	4.03	2.01	3.69	30
SpotB	2.65	1.49	2.45	30	3.05	1.87	2.28	30
SpotC	1.92	1.21	1.75	30	2.23	1.46	1.90	30
SpotD	0.83	0.82	0.91	30	1.30	1.54	0.95	30
CheapTalk: Auction								
SpotA	2.30	1.13	2.01	60	2.20	1.01	2.07	60
SpotB	1.71	0.59	1.59	20	1.73	1.00	1.71	60
SpotC	1.03	0.66	1.11	20				
SpotD	0.65	0.54	0.73	20				

RealTalk: Picture								
SpotA	2.32	1.00	2.10	28	2.92	1.39	2.50	26
SpotB	2.13	1.03	1.90	26	2.06	1.04	1.85	28
SpotC	1.56	0.89	1.20	26	1.52	0.74	1.50	28
SpotD	0.87	0.68	0.95	28	0.97	0.80	3.00	26
RealTalk: Auction								
SpotA	1.67	0.61	1.52	54	1.90	0.66	1.86	54
SpotB	1.08	0.56	1.19	18	1.62	0.75	1.48	54
SpotC	1.12	0.83	0.96	18				
SpotD	0.59	0.45	0.76	18				
Real Auction: Auction1								
SpotA	2.02	1.29	1.87	66	2.23	1.37	1.90	66
SpotB	1.22	0.96	1.13	22	1.65	1.16	1.47	66
SpotC	1.05	1.00	0.76	22				
SpotD	0.59	0.68	0.37	22				
Real Auction: Auction2								
SpotA	1.75	0.76	1.88	66	2.00	0.98	1.91	66
SpotB	1.33	0.87	1.23	22	1.54	0.89	1.47	66
SpotC	1.05	0.83	0.87	22				
SpotD	0.68	0.67	0.49	22				

[a]3 pounds = 1361 grams.

Table 5. Estimated Parameters for the Calibration Function

Variable	Parameter	Std Error	*p*-value
Treatment effects			
CT[a]	0.23	0.20	0.25
RT[b]	-0.33	0.15	0.03
General effects			
Constant	0.87	0.15	0.00
WTP2 [c]	-0.32	0.16	0.05
WTP2Sq [d]	0.09	0.04	0.01
Org[e]	0.52	0.15	0.00
Age0 [f]	-0.40	0.11	0.00
Gen0 [f]	-0.01	0.07	0.87
Inc0 [f]	0.36	0.11	0.00
Edu0 [f]	-0.05	0.07	0.47
# Obs	330		
R-squared	0.47		
Adj. R-squared	0.48		

Note: Estimated with Limdep 8.0.

[a]*CT* is the *cheap talk* treatment. [b]*RT* is the *real talk* treatment. [c]*WTP2* is the willingness to pay for 3 pounds of similar apples in the real auction. [d]*WTP2* squared. [e]*Org* is a dummy that is one for organic apples, zero otherwise. [f]Normalized variables: *Age0*=(*Age*-mean(*Age*))/standard deviation(*Age*). Similarly for *Gen0*, *Inc0* and *Edu0*.

Appendix

Instructions for the Experiment

There were four experimental treatments in this experiment. The effect of these treatments is investigated in another paper. In this paper the effect of the treatments is captured by the random element in the picture parameter.

Treatment 1	**Treatment 2**	**Treatment 3**	**Treatment 4**
Introduction	Introduction	Introduction	Introduction
Instruction A	Instruction A	Instruction A	Instruction A
Instruction B1	Instruction B2	Instruction B3	Instruction B4
Instruction C1	Instruction C1	Instruction C1	Instruction C4
Instruction D1	Instruction D1	Instruction D1	Instruction D4

Treatment 1,2,3,4: Introduction (Presented by the monitor)

Hi, my name is ……. and I am going to run this experiment. I have a script that I will follow closely and some parts of the instructions I will read directly from the script.

First, I would like to thank you all for agreeing to participate in today's experiment on market decision making. The purpose of the experiment is to study consumers' willingness to pay for various types of apples and consumers' decision making in experimental auction markets. The whole session will last approximately one hour.

In front of you there is a folder with papers and an envelope. Inside the envelope, you will find 20 dollars as thanks for participating and an ID number. You will use this ID number to identify yourself during this research session. Please make sure that all the papers you hand in have your ID number on them.

Before we begin, I want to emphasize that your participation in this session is completely voluntary. If you **do not** wish to participate in the experiment, please say so at any time. Non-participants will not be penalized in any way. I want to assure you that the information you provide will be kept strictly confidential and used only for the purposes of this research. If you consent to participate in the experiment, please sign the consent form and hand it in to one of us.

You have the general instructions for the market experiment in your folder. Please read the instructions. After you have read the instructions, I will summarize them and give some additional information about today's session.

Importantly, from this point forward, I ask that there be no talking among participants. Failure to comply with these instructions will result in disqualification from the experiment.

Are there any questions before we begin?

Treatment 1,2,3,4: Instructions A (Read by the participants)

In this market experiment, we are going to use **experimental auctions**. We will first go through the general features of the auctions and then give more details about how we are going to implement the auctions in this session.

The auctions we are using are fourth-price auctions. In a fourth-price auction each participant gives one written bid, and the price of the product is determined by the fourth-highest bid.

We will ask you to submit bids on 12 products. After we have collected all the bids, each of you will randomly draw a unique number between 1 and 12. The numbers you draw will determine which one of the 12 product auctions is binding for each of you. Your bids in the other 11 auctions are not binding. It is also possible that you will draw a zero. In that case, none of the 12 auctions will be binding for you. We will sort through all the bids to find the fourth-highest bid in each auction and this will be the price. The price of all 12 products will be posted on the board. If you bid higher than the price in your binding auction you will be required to buy the product with the price paid being the auction price.

The products we are offering in this experimental market are 3-pound bags of Golden Delicious apples. We have sorted the apples based on production method and appearance. The apples in each bag are of the same quality.

Example

Let us first look at an example of how a fourth-price auction works. We assume that participant 1, 2, 3, 4, and 5 have submitted the bids 90, 80, 70, 60, and 50, respectively, for a product. Also, we assume that these five bids are the five highest bids for the product. The fourth-highest bid is 60 and this will be the price. See Figure 1. If participant 1, 2 or 3 were to draw this auction as their binding auction they would be required to buy the product at a price equal to the fourth highest bid (60 in this example). If participant 4 or 5 were to draw this auction as their binding auction they would not be allowed to buy this product, since their bids are equal or lower than the price.

Figure 1

	Participant	**Bid**	
Potential buyers	**1**	90	
	2	80	
	3	70	
	4	**60**	**Price**
	5	50	

What should you do?

In a fourth-price auction, it is in the participant's own interest to bid the highest amount he or she is willing to pay for each of the products. Let's assume that the bids in Figure 1 represent the highest amounts that the participants 1 to 5 are willing to pay for this product.

What happens if participant 1 submits a higher bid? His or her bid is still above 60, and participant 1 is still a potential buyer and the price is still 60. What happens if participant 1 tries to buy the product at a lower price by submitting a lower bid? If his or her bid is above 60, participant 1 is still a potential buyer and the price is still 60. If the bid is below or equal to 60, then participant 1 is no longer a potential buyer and is not allowed to buy the product even though the price is below what he or she would be willing to pay for the product. The same argument applies for participant 2 and 3.

What happens if participant 4 tries to buy the product by submitting a lower bid? His or her bid is still below 60, and participant 4 is still not a potential buyer. What happens if participant 4 tries to buy the product by submitting a higher bid? If the bid is below or equal to 70, participant 4 is still not a potential buyer. If the bid is above 70, participant 4 is a potential buyer. However, the price is now 70 and above what the product is worth to him or her. The same argument applies for participant 5 and all other participants with a valuation below the price.

Hence, none of the participants can do better than bidding the highest amount he or she is willing to pay for the product.

Summary Instructions

1) You are to carefully examine each alternative and write the highest amount that you are willing to pay for each of the 12 alternatives on the bidding sheet.

2) After we collect all bids, each of you will draw a unique number between 1 and 12 from a deck of cards. The number you draw will determine which one of the 12 auctions will be binding for you. Your bids in the other 11 auctions are not binding. It is also possible that you will draw a zero. In that case, none of the 12 auctions will be binding for you.

3) The price in each auction is set equal to the fourth-highest bid in that auction. The prices will be posted on the board after the auction.

4) If you have bid higher than the price in your binding auction you will be required to buy the apples.

Treatment 1: Instructions B1 (Presented by the monitor)

Each session of this experiment will be unique. In this session we are going to use hypothetical auctions. That means that the apples will be represented by pictures and at the end of the auction no one will actually buy any apples. Before we took the pictures we sorted the apples based on production method and appearance. You can assume that all the apples in each picture are of the same quality. You should carefully examine the apples before you make your bids. We will go through the whole auction procedure as described above, but after we have posted the prices no one will actually buy any apples.

Please, try to behave in the hypothetical auctions as if they were real auctions.

Treatment 2: Instructions B2 (Presented by the monitor)

Each session of this experiment will be unique. In this session we are going to use hypothetical auctions. That means that the apples will be represented by pictures and at the end of the auction no one will actually buy any apples. Before we took the pictures we sorted the apples based on production method and appearance. You can assume that all the apples in each picture are of the same quality. You should carefully examine the apples before you make your bids. We will go through the whole auction procedure as described above, but after we have posted the prices no one will actually buy any apples.

Real Talk

Before you start, I want to inform you that right after the hypothetical auctions we will conduct a similar series of real auctions with real apples. We have similar apples as you will see on the pictures and we will repeat the whole procedure with real apples. At the end of the real auction, we will sell apples to the participants that have bid higher than the price in their binding real auction. Try to behave in the hypothetical auctions as if they were a part of the real auctions.

Treatment 3: Instructions B3 (Presented by the monitor)

Each session of this experiment will be unique. In this session we are going to use hypothetical auctions. That means that the apples will be represented by pictures and at the end of the auction no one will actually buy any apples. Before we took the pictures we sorted the apples based on production method and appearance. You can assume that all the apples in each picture are of the same quality. You should carefully examine the apples before you make your bids. We will go through the whole auction procedure as described above, but after we have posted the prices no one will actually buy any apples.

Cheap Talk

Before you start I want to talk to you about a problem that we have in studies like this one. As I told you a minute ago, this is a hypothetical auction - not a real one. No one will actually pay money at the end. But, I also ask you to bid as though the result would involve a real cash payment. And that's the problem.

In most studies of this kind, folks seem to have a hard time doing this. They act differently in a hypothetical situation, where they don't really have to pay money, than they do in a real situation, where they really have to pay money. For example, in a recent study, several different groups of people bid in an auction. Payment was hypothetical for these groups, as it will be for you. No one had to pay money if he or she won the auction. The results of this study were that on average, across the groups, people overstated their actual willingness-to pay by 150 percent in the hypothetical auction. That's quite a difference, isn't it?

We call this “hypothetical bias.” “Hypothetical bias” is the difference that we continually see in the way people respond to hypothetical situations as compared to real situations—just like the overbidding example presented above.

How can we get people to think about their choices in a hypothetical situation like they think in a real situation, where a person will really have to pay money? How do we get them to think about what it means to really dig into their pocket and pay money, if in fact they really aren’t going to have to do it?

Let me tell you why I think that we continually see this hypothetical bias, why people behave differently in a hypothetical situation than they do when in a real situation. I think that when we behave in a hypothetical situation we place our best guess of what we would really like to do. But, when the choice is real, and we would actually have to spend our own money if we win, we think a different way: if I spend money on this, that’s money I don’t have to spend on other things ... we act in a way that takes into account the limited amount of money we have ... This is just my opinion, of course, but it’s what I think may be going on in hypothetical situations.

So, if I were in your shoes, and I was asked to bid for a series of products, I would think about how I feel about spending my money this way. When I got ready to bid, I would ask myself: if this was a real situation, and I had to pay, am I really willing to pay so much money for this product?

Please keep this in mind when making your bids and try to behave in the hypothetical auctions as if it were real auctions.

Treatment 4: Instruction B4: Real Auction 1 (Presented by monitor)

Each session of this experiment is unique. In this session we will conduct the auctions twice. After we have conducted the second set of auctions, we will randomly draw one of the two trials as the binding auctions trial. We have sorted the apples based on production method and appearance. The apples in each bag are of the same quality. You should carefully examine the apples before you make your bids. But please, don't lift the bags.

Important notes:

1) You have the opportunity to buy one bag of apples here today. After we have collected all bids, each of you will randomly draw a unique number between 1 and 12. The number you draw will determine which one of the 12 auctions that are binding for you. Your bids in the other 11 auctions are not binding. It is also possible that you will draw a zero. In that case, none of the 12 auctions will be binding for you.

   Under no circumstance will anyone buy more than one bag.

2) You must pay for any apple you buy. If your bid in your binding auction is higher than the price in that auction, you are required to buy the bag of apples at a price that is set by the fourth-highest bid for that bag of apples.

Treatment 1, 2, and 3: Instructions C1 Bidding Sheets (Presented by the monitor)

(Show bidding sheet)

There are 12 alternatives on the table in front of you. Each are 3 pounds of apples. In your folder you will find a bidding sheet for the 12 alternatives. You are going to write down your bids for each of the 12 alternatives on the bidding sheet. If you are not interested in an alternative at any price, you can bid zero.

You can start wherever you want, so please spread around the table. Mark on the bidding sheet which alternative you started with and please go clockwise around the table. After you are finished you can hand in your bidding sheet to one of us.

Then we will sort through the bids, find the fourth-highest bid for each alternative, post the prices and each of you will draw a unique number determining your binding auction.

Remember, this is a hypothetical auction and no one will actually buy any apples in this auction.

Are there any questions before we begin the auction?

Treatment 4: Instructions C4 Bidding Sheets (Presented by the monitor)

(Show bidding sheet)

There are 12 alternatives on the table in front of you. Each is 3 pounds of apples. In your folder you will find a bidding sheet for the 12 alternatives. You are going to write down your bids for each of the 12 alternatives on the bidding sheet. If you are not interested in an alternative at any price, you can bid zero.

You can start wherever you want, so please spread around the table. Mark on the bidding sheet which alternative you started with and please go clockwise around the table. After you have finished you can hand in your bidding sheet to one of us.

Are there any questions before we begin the auction?

Treatment 1, 2 and 3: Instructions D1 Real Auction (Presented by the monitor)

We have apples that are similar to those you have seen on the pictures. We will now repeat the whole procedure with real apples. As in the hypothetical auction, the apples differ in production method and appearance. The placement of the various qualities of apples around the table differs from the placement in the hypothetical auction.

You should carefully examine the apples before you make your bids. But please, don't lift the bags. At the end, we will sell apples to the participants that have bid higher than the price in their binding real auction. What you did in the hypothetical auction will have no effect on the outcome of this auction.

Important notes:

1) You have the opportunity to buy one bag of apples here today. After we have collected all bids, each of you will randomly draw a unique number between 1 and 12. The number you draw will determine which one of the 12 auctions that are binding for you. Your bids in the other 11 auctions are not binding. It is also possible that you will draw a zero. In that case, none of the 12 auctions will be binding for you.

   Under no circumstance will anyone buy more than one bag.

2) You must pay for any apple you buy. If your bid in your binding auction is higher than the price in that auction, you are required to buy the bag of apples at a price that is set by the fourth-highest bid for that bag of apples.

Are there any questions before we begin the auction?

You can start wherever you want, so please spread around the table.

Treatment 4: Instructions D4 Second real auction (Presented by the monitor)

We will now conduct the auctions a second time. After you are finished, we will randomly draw one of the two trials as the binding auctions trial.

Are there any questions before we begin the auction?

You can start wherever you want, so please spread around the table.