ON THE USE OF CHEAP TALK IN HYPOTHETICAL PRODUCT VALUATION: A FIELD EXPERIMENT

A Thesis

by

ANDRES SILVA MONTES

Submitted to the Office of Graduate Studies of
Texas A&M University
in partial fulfillment of the requirements for the degree of

MASTER OF SCIENCE

December 2007

Major Subject: Agricultural Economics

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Approved by:

Chair of Committee, Rodolfo M. Nayga, Jr.
Committee Members, Brit Grosskopf
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ABSTRACT

On the Use of Cheap Talk in Hypothetical
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Andres Silva Montes, B.S., Universidad Catolica de Chile
Chair of Advisory Committee: Dr. Rodolfo M. Nayga, Jr.

Experimental willingness to pay (WTP) studies can be classified as hypothetical or non-hypothetical. In a hypothetical study, such as conjoint analysis, a subject does not need to make a real economic commitment. In contrast, in a non-hypothetical task such as in experimental auctions, a subject may need to actually buy the product. Subjects in hypothetical studies tend to overstate their true WTP. Consequently, researchers need to correct hypothetical values to obtain reliable WTP estimates. Recently, incentive-aligned and cheap talk approaches have been proposed as ways to correct for hypothetical bias. In a hypothetical task, a cheap talk script explicitly reminds the subject about the hypothetical nature of the task and its expected consequences. In an incentive-aligned task (non-hypothetical), subjects are randomly selected to physically buy the product. The objective of our study is to assess and compare the reduction of hypothetical bias in consumers' willingness to pay for novel products by applying a generic, short, and neutral cheap talk script in a retail setting. To accomplish this objective, we employ non-hypothetical, hypothetical, and hypothetical with cheap talk treatments in our experimental design.

We conducted our experimental retail study using conjoint analysis and open-ended elicitation mechanisms, utilizing Becker DeGroot Marshak (BDM) mechanism for the incentive-aligned treatments. Consistently in both elicitation mechanisms, using seemingly unrelated and random-effect Tobit techniques, we find that our cheap talk script is effective in eliminating the hypothetical bias. As expected, the hypothetical WTP values are significantly higher than the non-hypothetical values but the hypothetical values with cheap talk are not significantly different from incentive-aligned or non-hypothetical estimates. In addition, we find that open-ended estimates are significantly higher than conjoint analysis estimates and that emotions and familiarity can have significant impacts on WTP estimates.

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It is not possible to fit on one page all the ways that Texas A&M University, and in particular, the Department of Agricultural Economics has changed my life. For me, it is not possible to measure it now.

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TABLE OF CONTENTS

LIST OF TABLES

CHAPTER I
INTRODUCTION

Over the last decade, experimental studies have become a popular mechanism to test economic theory (e.g. Coursey, Hovis, and Schulze 2003) or to measure willingness to pay (WTP) (e.g. Lusk and Hudson 2004b). An advantage of experimental studies are that subjects can be studied 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. Study of WTP methods is warranted when considering the number of new products being constantly introduced into the marketplace. This is true wherever these new products are completely novel product or an addition/subtraction of attributes from an existing product.

Following Maynard et al. (2004), WTP experimental studies can be classified as hypothetical and non-hypothetical. Hypothetical studies do not require 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. An example of a hypothetical approach is the traditional conjoint analysis (CA), which has been widely used in marketing research and business applications. In traditional CA, subjects need to rate or rank product profiles (unique combination of attributes). After conducting the survey, the WTP for each attribute is estimated and respondents are clustered into various groups (market segments) that share similar characteristics. In non-hypothetical studies, there is the possibility of a transaction occurring between the researcher and the subject, meaning the subject may have to give up something, such as money, in exchange for the actual product. An example of the non-hypothetical approach is an experimental auction (EA).

This thesis follows the style of the *American Journal of Agricultural Economics.*

EA are more commonly conducted in a lab setting rather than in a field setting. In general, subjects are endowed with a generic product and receive a cash payment to express their WTP to exchange it for a premium good. Later on, we discuss some study specifications and their expected effect on the WTP estimations. There are several types of auctions where the researcher is interested in incentive-compatible approaches. An incentive-compatible auction is designed such that the subject's best strategy is to truly reveal their WTP for the elicited product. The most appropriate type of auction for a particular study is highly dependent on the research objectives and the experimental conditions. Recently, Ding, Grewal, and Liechty (2005) introduced incentive-aligned CA, which takes advantage of the attribute tradeoffs of CA and the incentive compatibility property of EA. In the literature review section, some advantages and disadvantages of a few of the most common types of EA and elicitation mechanisms are reviewed.

In addition to the type of elicitation mechanism, the researcher needs to decide on the experimental conditions. In general, we look for experimental conditions that facilitate subject valuation and minimize biases. In the end, our objective is to generate accurate and reliable estimations of product valuation. Recent literature has extensively examined several experimental situations, including: type of setting (Lusk and Fox 2003) and valuation procedures (Coursey, Hovis, and Schulze 2003).

Problem Statement

It has been suggested that subjects tend to behave differently when they face a hypothetical task compared to a real one (Blumenschein et al. 1997; Neill et al. 1994; List and Gallet 2001). More specifically, there is strong evidence that subjects overstate their true WTP in hypothetical situations. There is a lack of agreement about why respondents do this and also how to calibrate their overstated responses (Murphy et al. 2005). Consequently, there needs to be a method to identify and control the hypothetical bias.

Objectives

With this in mind, we conducted a set of retail experiments to further evaluate the use of hypothetical cheap talk (cheap talk hereafter) to reduce the hypothetical bias using different types of elicitation mechanisms, such as open-ended elicitation (OE) and CA. Using treatments involving hypothetical, cheap talk, and non-hypothetical tasks, our objective is to examine cheap talk's effectiveness in reducing hypothetical bias in product valuation. To our knowledge, there is no prior work that has evaluated the effect of cheap talk using different elicitation mechanisms in a retail setting.

Data and Methods

We conducted our experiments of February 2007 in selected grocery stores in the Bryan-College Station area in Texas. The study was approved by the Office of Research Compliance at Texas A&M University (Protocol Number 2006-0703). The dataset involved four value-added Texas Rio Star grapefruit products. The fresh fruit was provided by Texas Citrus Mutual and processed on-campus by specialized personnel. The grapefruit were cut in segments or cubes and included preservatives in some cases. So, we elicited WTP values for four value-added grapefruit products: cubes without preservatives, cubes with preservatives, segments without preservatives and segments with preservatives.

The respondent instructions were scripted in order to keep the study as similar as possible from one subject to the next. We randomly conducted the experiments at different times of the day, different days of the week, and at selected supermarkets. We obtained demographic, consumption, and attitudinal information from the subjects who expressed their WTP for each of the four products.

In the literature review section, we discuss some of the most popular elicitation mechanisms. After that, in the methodology section, we explain and justify the study specifications for OE and CA. Basically, in the OE approach, subjects directly state their WTP. However, in the CA treatments, subjects rate a set of product profiles. For each type of elicitation mechanism we have four treatments. Consequently, in total, we have two types of elicitation mechanisms and eight treatments. The product profiles were displayed as pictures and labeled with the type of product on the top of each picture to ensure that the subjects completely understood which product was under study. Pictures were used to

limit any variation among product samples and were also used to vary the attribute levels being tested. However, subjects were made aware that despite the use of pictures, bids were binding and the participant might have to exchange money for a randomly selected good, depending on the elicitation mechanism. For CA, the actual number of product profiles had an orthogonal design, which assures that we had enough profiles to compute individual attribute value estimates. Finally, since the WTP data are censored to zero and we asked for more than one price per subject, we analyzed the data using a system of equations as seemingly unrelated regression (SUR) and random-effect Tobit model. Using both hypothetical and non-hypothetical designs of OE and CA elicitation mechanisms, we were able to evaluate how cheap talk can effectively reduce hypothetical bias in hypothetical product valuation.

Expected Results

In the literature, there is evidence, mostly in lab settings, that the provision of cheap talk in hypothetical valuation and the use of incentive-aligned non-hypothetical mechanisms are effective in eliminating hypothetical bias. Consequently, we expect that both mechanisms would generate significantly more realistic WTP estimates than the hypothetical treatments, without a significant difference between them.

Our findings can have managerial implications. First, decision makers will have evidence to validate the use of cheap talk to reduce hypothetical bias for environmental and hypothetical new product valuation applications. Second, we will be able to determine whether the incentive-aligned CA and OE approaches offer a significant improvement with respect to the cheap talk approach in terms of reducing the hypothetical bias in product valuation.

Organization of Remaining Chapters

This thesis is organized as follows. First, we identify the hypothetical bias problem in experimental economics. Second, we describe some of the aspects that need to be considered when designing an experimental study, including the type of subject, nature of the task, and type of setting. In the discussion regarding the elicitation mechanism, we highlight the relevant research within the areas of EA and CA, giving emphasis to areas of

ongoing research regarding measurement of WTP. In particular, we revisit the literature on cheap talk and the incentive-aligned approach by Ding, Grewal, and Liechty (2005). Third, in the theoretical framework section, we discuss the theory behind EA and CA. Fourth, in the empirical model section, we justify the use of SUR and the random-effect Tobit model used to analyze the dataset. In addition, we present the experimental specifications. Fifth, we present and explain our results. Sixth, we discuss our findings, their economic and managerial implications, with recommendations for future research. Finally, the appendix section includes the original questionnaires (Appendix A), cheap talk script (Appendix B), and instruction per treatment (Appendix C).

CHAPTER II
REVIEW OF LITERATURE

Carpenter, Harrison and List (2005) indicate that some of the most important factors that can be used to determine the field context of an experiment are subject background (previous experiences and information), type of commodity, nature of the pool of subjects, study environment, and tasks. The following is a review of some of the most relevant work that has been done within experimental economics focusing on the last three points, which are intrinsic to our study.

Nature of Pool of Subjects

Students are widely used in experimental economics since they are easily prepared and convenient to recruit. In the academic literature, Harrison and List (2004) identify the main issues of working with students. They note that students are non-representative sample (due to selection bias) and that findings based on such as sample are difficult to extrapolate to the rest of the population. The result is a lack of external validity (Carpenter, Harrison and List 2005). According to Harrison and List (2004), most of the selection bias can be corrected by including explanatory variables (e.g. demographics) in the statistical model. However, it is still difficult to predict the behavior of a population based on a model that was developed from a small segment or portion of the population. In other words, interpolation generates a more accurate prediction than extrapolation (Harrison and List 2004). In our study, we use retail walk-in subjects selected in grocery stores. Supermarket shoppers can be more difficult to recruit than students; however, this allows us to expand the range of demographic characteristics.

Nature of the Environment

The setting in which an experiment is conducted has also been shown to influence WTP values. A more realistic setting tends to help participants behave as if they were making a real buying decision. Every market setting has unique characteristics that provide advantages and disadvantages. For instance, a retail setting allows recruitment of a larger sample size, a wider range of demographic characteristics, minimization of the recruiter

payment, and also allows the study to be conducted in a place where decisions are actually being made (i.e., a grocery store). However, it is more difficult to ensure the subject actually reads all of the information that is important to the study, which makes it harder to employ more demanding protocols. With regards to lab settings, more sophisticated experiments can be conducted since there are usually fewer distractions and fewer time constraints. However, lab settings usually provide a hypothetical buying situation and participants may not want the product being offered. In setting up an experiment, the advantages and disadvantages of each setting must be weighed in order to pick which setting is most appropriate for the problem at hand.

In the literature, Shogren et al. (1999) compared mail, lab, and retail study formats. They estimated the WTP for an irradiated chicken product using four sets of prices. The authors found that respondents within the mail group expressed a highest WTP and acceptance rate for irradiated chicken. Furthermore, the main difference between the lab and the retail setting was that in the retail setting, most of the people did not read the available information about irradiated food. This finding is especially relevant since information helps to change attitudes about irradiated food and a positive attitude may lead to increased purchase behavior.

Comparing in-lab and retail settings, Lusk and Fox (2003) did not find a significant difference in bids after controlling for demographic differences. However, when comparing non-zero bids, retail valuations were significantly higher than the lab valuations. The authors explain that WTP should increase when consumers are certain about a good's value. Another possibility that is not presented in the paper is that more people bid zero in a retail setting and a higher bid values could be a product of a biased selection sample.

In addition, the higher WTP in lab valuation exercises compared to actual retail exercises could be associated with the novelty of the product more than inexperience with the elicitation procedure (Shogren, List and Hayes 2000). Consequently, we would expect that subject bids should be stable for familiar goods and decline for unfamiliar goods. This fact was tested by Shogren, List and Hayes (2000) using candy bars, mango, and irradiated pork meat, expected to generate different levels of familiarity by the subjects. They found that the novelty of a product or preference for the learning process had a significant effect

on the results. In other words, subjects bid higher for a new good in order to see how the good fit into their preference set.

Some hypothetical tasks have been criticized based on the lack of realism. The argument here is that subjects behave differently when they feel that they are not in a real purchase situation. Incentive-aligned approaches have been proposed to overcome this hypothetical bias. Incentive-aligned techniques involve a hypothetical situation where and some subjects are randomly selected to buy the elicited good. Some examples in the literature can be found in the work of Voelckner (2006), who randomly selected ten percent of the subjects to actually buy their prior choice, which significantly reduced the bias of the hypothetical setting. Lusk and Schroeder (2004), comparing hypothetical and non-hypothetical choice based responses, found that subjects overestimated the probability of purchasing an elicited good and their WTP in a hypothetical setting, compared to an incentive-aligned one. However, the WTP for a marginal change of product attributes converged in both mechanisms.

Alfnes et al. (2006) modified the Lusk and Schroeder (2004) protocol in order to measure the WTP for artificial color in salmon, using different levels of information and 22 choice scenarios with different combinations of colors and prices. At the end of the experiment, each subject randomly determined their binding scenario. Finally, Ding, Grewal, and Liechty (2005) found that when they compared incentive-aligned, hypothetical CA to contingent valuation (CV), the incentive-aligned mechanisms generated significantly lower WTP estimates than the hypothetical approaches. In summary, incentive-aligned procedures take advantage of an incentive-compatible elicitation protocol that may involve the availability of substitutes in CA; consequently, subjects can make decisions in a more realistic setting. In the remaining part of this section, we review cheap talk as a technique that can be used to reduce or eliminate hypothetical bias. Later, we apply this technique in a retail environment.

Nature of the Task

There are numerous means by which to measure WTP. Three of the most common are CV, EA, and CA. With respect to CV, there is evidence that this approach overestimates WTP values. Brown, Ajzen and Hrubes (2003) argued that overestimation occurs due to hypothetical bias. Blumenschein et al. (1997), compared real and hypothetical second-price EA, and provided empirical evidence that EA produce significant overestimation of results. In some cases, although working in a hypothetical setting, hypothetical answers can be informative when the bias is systematic and predictable (Blackburn, Harrison and Rutstrom 1994). List and Gallet (2001) found that hypothetical WTP values need to be calibrated by a factor between 1.26 and 1.30 to get an estimate of the actual value. Type of good (public/private), elicitation mechanism and type of study (willingness to pay/accept) were taken into account to estimate the appropriate calibration factor. Thus, identifying the bias pattern based on demographic characteristics can allow us to correctly analyze other sample sets. Blackburn, Harrison and Rutstrom (1994) used dichotomic questions for three goods and tried to predict bias patterns in hypothetical and non-hypothetical contexts. Their results indicate that it was possible to predict responses with some accuracy; however, more experiments are needed to have more robust conclusions. In the following paragraphs, we describe EA and CA as methodologies used to elicit WTP values.

Experimental Auctions

Since non-hypothetical CV studies can be difficult to conduct, EA have been presented as a means to estimate a more realistic WTP for novel products. Lusk (2003b) reviewed some advantages and disadvantages of EA, CV, and CA approaches. The author identified several advantages of EA, such as the use of an active market environment where feedback can be attained. The active market allows subjects to face real decisions due to the use of money and goods. However, a couple of disadvantages to EA exist. Notably, many actual products may be needed, depending on the auction type used and the difficulty of creating substitutes in a realistic setting. EA procedures have especially become a popular method for eliciting WTP values for new product attributes, and for examining several aspects of economic theory (e.g. Chow and Sarin 2001). However, most

applications have taken place in lab settings rather than in retail settings (see Wertenbroch and Skiera 2002). Lusk, Feldkamp and Schroeder (2004) conducted a study to compare the performance of four of the most common experimental mechanisms: English, Becker DeGroot Marshak mechanism (BDM hereafter), second-price (also known as Vickrey), and n^{th} price auctions.

In a standard EA procedure, subjects are randomly assigned to either a particular group (or treatment) or a control group. Each group is exposed to a different level of information (e.g. Fox, Hayes and Shogren 2002), endowment (e.g. Loureiro, Umberger and Hine 2003), setting or task (e.g. Voelckner 2006). Subjects express their WTP by submitting a bid for the elicited product in each of the rounds, and at the end, one of the rounds is randomly chosen as the binding round. For the selected round, the auction winners have to buy the product and pay the market price. In the English auction, the subject with the highest bid gets the product and pays his stated price. In the second-price auction, the subject with the highest bid gets the product and pays the second highest price. In the n^{th} price auction, the n-1 subject with the highest bid gets the product and pays the market price that is determined randomly (n^{th} price auction). In the BDM mechanism, the market price is randomly drawn and the subject is able to purchase the good only if their bid is larger than or equal to the market price. Second-price auction, n^{th} price auction, and the BDM mechanism are incentive-compatible mechanisms. In other words, the subject's best strategy is to reveal their true WTP. For example, if a bidder bids an amount that is lower than their true WTP, the chance of being able to purchase the good is lowered, while bidding a higher amount increases the chance that the bidder will have to purchase the good at a price that is higher than what they truly want to spend. However, each elicitation mechanism has some limitations. For instance, the BDM mechanism has no market feedback. In repeated trials, a market-like learning experience helps subjects to show their true preferences and reveal their demand for the good (Coursey, Hovis, and Schulze 2003). However, the BDM mechanism allows us to elicit WTP for each subject while maintaining its incentive compatibility.

Lusk, Feldkamp and Schroeder (2004) carried out a multiple good valuation using four types of auctions: English, BDM, second-price, and n^{th} price. In multiple good valuations, the authors identify a disadvantage in the form of a possible demand reduction

or wealth effect. In other words, once some subjects get an auctioned good, they will reduce their demand in the next round. A way to avoid this problem is to randomly choose the binding round, as proposed in the n^{th} price auction.

In general, an endowment can be used in two ways in EA. First, subjects may receive an endowed good (typically a basic pre-existing substitute) and then are asked to bid to exchange their endowed good for the good of interest (premium product). This also called the endow-and-upgrade method by Corrigan and Rousu (2006a). Second, subjects can bid directly on several competing goods and a random drawing can be used to determine which good is binding (must be purchased), so that demand for a single unit can be elicited.

In the literature, endowment effects have been described as the difference between bids by those endowed with a good and those not given an endowment before bidding. Rutstrom (1998) identified two effects associated with participation fees in an experimental study. First, the income effect, which is based on the subject's increased income, is expected to be low considering the allocation of goods. Second the selection effect considers that subjects need different levels of monetary compensation for different goods. So, the higher participation fee the more likely we will attract a subject with a higher opportunity cost. Regarding the selection effect, authors found that the effect on the residual variance is not constant over elicitation mechanisms. Particularly, for the income effect, the study identified both an expected and unexpected income. The expected income was the known participation fee, and later in the study, the unexpected income was the random income assignment. With respect to income effect, they found that an unexpected endowment was translated into a larger residual variance of the bid, and the expected endowment had no significant effect on the average bid or its residual variances.

Wertenbroch and Skiera (2002) suggested not giving any compensation to participants as a way to avoid distortions that can lead to overbidding. They conducted a point-of-purchase experiment in which their recruited participants were not significantly compensated. However, when subjects are not endowed with money, it can be a liquidity constraint is stronger, and specially, to buy big items. In other words, subjects do not have enough cash to buy the elicited good even when they want to purchase the good. The authors advised that this issue can be avoided by allowing subjects to pay with a check,

credit card, or by taking a loan from the researchers. Alternatively, incentive participation that does not affect the budget constraint can be implemented by compensating subjects via a donation to a third party, such as a church or another similar institution (Cummings, Harrison, and Rutstrom 1995).

Loureiro, Umberger and Hine (2003) designed an experiment to test whether three levels of monetary endowments would cause significantly different bids. Participants were divided into groups and compensated with two, four, or six dollars. Results indicated that the participants of groups endowed with four and six dollars bid significantly higher than subjects receiving two dollars. Thus, the endowment level had a significantly positive influence on the bid level. Finally, they concluded that endowments close to the value of the auctioned good should not have a significant impact on the experimental design. They suggested that participants should bid and then receive final payment at the end of the experiment. In addition, in order to minimize the overbidding behavior, the authors recommended that participants should receive an initial monetary compensation to use in the study task and then a participation fee at the end of the session.

Lusk, Feldkamp and Schroeder (2004) measured the endowment effect using beef steaks as a product. Their experiment involved using two sets of treatments. First, respondents were either endowed with or without a steak. The second treatment was an assignment to an elicitation mechanism: English, BDM, second-price, and n^{th} price auction. Results indicated that the endowment effect was not consistent across elicitation mechanisms. However, for the n^{th} price auction, WTP was significantly lower in the endowment treatment, consistent with loss aversion theory. The subjects valued the endowed good more and bid less in order to get a new one. The opposite was found for the second-price auction, which is consistent with previous research.

Corrigan and Rousu (2006a) also examined endowment effects by utilizing a similar design to that used by Lusk, Feldkamp and Schroeder (2004). Their protocol differed in that subjects were endowed with a bottle of salsa and subjects bid to obtain a second bottle of the same good. By doing this, the endowment effect was isolated, and loss aversion behavior was avoided. Their results indicated that WTP for a second unit of the good was 75% higher when subjects were initially endowed. The highest differences in WTP occurred between the n^{th} price and second-price auctions. The authors explained that

the higher valuation is a reciprocal obligation effect. In other words, subjects felt committed to the researcher who just endowed them with a good. This effect will have the opposite effect rather than the described loss aversion behavior. Consequently, the final effect is ambiguous and will depend mainly on the auction design.

In the substitution area, Marette et al. (2006) developed an experiment to estimate the substitution between two imperfect substitutes and computed the WTP with different levels of health information in France. The experiment involved cans of tuna and sardines. Tuna is known as having higher levels of mercury and lower levels of beneficial omega-3 fatty acids; however, it is consumed more frequently than sardines. Some subjects were endowed with six cans of tuna and others with six cans of sardines. Subjects were then asked to exchange their endowed good for the good they were not endowed with in order to find the point of indifference. Using the point of indifference, they computed WTP estimates. The substitution rate was affected by the type of information presented and its order of presentation. Therefore, the authors recommended presenting the benefits before the risk information to assure that the benefit information is absorbed, since the risk information, in any order, would still be considered.

Lusk, Feldkamp and Schroeder (2004) found that the WTP for a particular steak was not significantly altered by the presence of other types of steaks. Corrigan and Rousu (2006a) explained that bidding for two goods separately can lead to avoidance of the endowment effect. The authors found that the WTP estimations change significantly when the subjects are endowed with a good. So, the endow-and-upgrade method can be the cause of misleading estimates of a good's premium.

Lusk and Hudson (2004b) compared methods to compute WTP and indicated how some of the work in experimental and environmental economics can be applied to an agribusiness context. They presented the significance of cross-price effects in measuring WTP. They found that the market price information about substitutes significantly affects the WTP for a novel product. The cross-price effect was computed in two ways. First, subjects bid with and without the presence of a substitute/complementary good. Second, the novel good was auctioned and the substitute good was also available at a posted selling price. The selling price of the substitute good was varied to analyze the impact on changing bids. However, it is possible to have some incentive compatibility issues. In the authors'

opinion, if the cross-price effect is not considered, there will be a decrease of the decision power of the findings by the business. In summary, with regards to the substitution effect, there is experimental evidence that the presence of substitutes affects WTP estimations. In the discussion section, we use this evidence to explain some of our experimental findings.

Conjoint Analysis

The concept of CA relies completely on the fact that demand for a product is based on the set of attributes that make up the product. According to Lancaster (1966), consumers maximize utility subject to a budget constraint and a technology level. A product is composed of a bundle of attributes that can exist in more than one product. An attribute, not a good, provides utility to the consumer. Therefore, the marginal utility of a good can be expressed as the weighted marginal utility of the attributes.

CA requires participants to evaluate several product profiles, with each profile made up of a unique level combination of the same attributes that mimic a real buying decision. For example, color is an attribute, while blue, red, and green are levels. The main difference between CA and other WTP elicitation mechanisms like open CV and EA, is that price values are provided. In a standard CA, the most important attributes and their levels should be included within the profiles to be evaluated. Some examples of CA studies include: valuations of value-added citrus (Campbell et al. 2006), external citrus appearance (Campbell et al. 2004), genetically modified organisms (Baker and Burnham 2001), edible flowers (Kelley et al. 2002), and food quality labels (Fotopoulos and Krystallis 2001). By evaluating several products with various attribute combinations, it is possible to estimate the WTP for each attribute and its levels.

According to Green, Kreiger, and Wind (2001), CA is “by far, the most used marketing research tool for analyzing consumer tradeoffs.” The main reason for CA popularity is its ease of collecting WTP estimates and obtaining market segmentation results. Ease of application mainly results from the ability to conduct studies in a hypothetical setting. However, the hypothetical nature of the task (i.e., no transaction takes place) can affect the reliability of the estimations. As recent research has shown, hypothetical CA may result in overestimation of WTP estimates (Ding, Grewal, and

Liechty 2005). Since subjects facing a hypothetical buying decision tend to behave differently than subjects in a real buying situation, it can lead to biased WTP estimates.

Informational Effect and Cheap Talk Approach

In the literature, it is common to see experiments where subjects are exposed to different informational treatments. For example, Paradiso and Trisorio (2001) found that letting the subject inspect the product in question reduced the ratio between hypothetical and real WTP from 3.5 to 2.7. Margolis and Shogren (2004), via an experimental protocol and the bootstrapping approach, estimated bid functions of the players, which helped to determine the auction winner, price, and payoff. They concluded that it is possible to get reliable results from inexperienced subjects when there are several repetitions. However, the subjects tended to avoid extreme values, which were interpreted as a lack of the bidder's confidence in their understanding of the auction.

Huffman et al. (2004) took a different approach with regards to information by examining its significance. In their study, after an EA in a lab, subjects chose the most reliable source for information on genetically modified organisms (GMOs). Using a multinomial model and the source of information as the dependent variable, results indicated that personal (education, age, and prior knowledge) and social (religious affiliation) characteristics would highly explain the most trusted source of information. Some specific findings were that income did not affect the trust placed in a specific source of information. Further, more educated people were found to rely more on third party information, while less educated persons relied more likely on governmental sources.

Fox, Hayes and Shogren (2002) compared the effect of favorable and unfavorable information on the WTP for a pork sandwich irradiated to control *Trichinella*. They found that subjects were more influenced by a negative description, even when the information is provided from a questionable source. This fact could be explained by the consumers' risk aversion or by the ambiguous interpretation of contradictory information.

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 (Charness and Grosskopf

2004). The effect of cheap talk in the ultimatum game[1] was studied by Lusk and Hudson (2004a). They found that the more informed group behaved significantly different than a non-informed group by providing answers closer to the expected Nash equilibrium. In this sense, cheap talk could be used to "homogenize" the rationality and belief of participants, which is an assumption for obtaining a Nash equilibrium. This result is critical considering that a sophisticated experiment can easily fail if the researcher is not able to explain the directions of the experiment in a short time. Considering most subjects have no economic background, the reliability of complicated experiments may be in question. This leads to another issue: how do you keep a participant's attention if they know the incentive receive is independent of their effort?

In the applied economics literature, Cummings and Taylor (1999) introduced cheap talk as the non-binding communication of actions before a hypothetical commitment. This communication specifically includes a discussion about the hypothetical bias problem. Their work is heavily referenced in field research. For instance, List (2001), Brown, Ajzen and Hrubes (2003), Murphy, Stevens, and Weatherhead (2005), and Landry and List (2007) have all adapted that transcript to apply to different research conditions. Table 1 summarizes the cheap talk literature. We did not include Lusk and Hudson's (2004a) paper since it is game theory-related or Murphy et al. (2005) since it is a review of other authors' work.

[1] The ultimatum game features two parties that interact anonymously and only once. The first party makes a proposal on how to divide a good and the second party could reject or accept this division.

Table 1. Selected Cheap Talk Papers

			Script			
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 et al.	2003	CV Hypothetical	No neutral	941[a]	Lab	Yes
Lusk	2003	CV Hypothetical	Neutral	522	Mail	Yes
Carlsson et al.	2005	Hypothetical Choice Experiment	No neutral	113	Mail	No
Murphy et al.	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 et al.	2007	CV Hypothetical	No neutral	128	Field	No
Landry and List	2007	CV Hypothetical vs. Non-Hypothetical	No neutral	941[a]	Field	Yes
Silva et al.[b]	2007	OE and CA Hypothetical vs. Non-Hypothetical	Neutral	211	Field	No

[a] The script was adapted from Cummings and Taylor (1999)

[b] This study, included for purposes of comparison.

Overall, experimental studies show that cheap talk script is effective at reducing, or in some cases removing, hypothetical bias. To point, Murphy et al. (2005) conducted a meta-study with data from 28 WTP studies. The authors found that the median ratio of hypothetical to actual value was 1.35 and that cheap talk effectively reduced the hypothetical bias. However, empirical evidence can be contradictory in some cases. After reviewing published work in this area, we synthesize the research on cheap talk into five basic findings that shape our research: First, the impact of cheap talk is affected by the length and the content of the script. Second, the payment level influences the cheap talk effect. Third, the subjects' background can influence the cheap talk effect. Fourth, cheap talk studies have been conducted mainly in lab settings. Fifth, cheap talk studies have mainly utilized in CV methodologies.

The impact of cheap talk is affected by the length and the content of the script: There is no 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. In the previous literature, sometimes it has been classified as a cheap talk experiment. The authors compared three CV formats, real, cheap talk, and hypothetical treatments. However, they did not explicitly discuss hypothetical bias and its effect, which is one of the critical portions that Cummings and Taylor (1999) included in their original cheap talk script. Loomis et al. (1996) found that the cheap talk script was not able to remove the hypothetical bias. The cheap talk estimates were lower than hypothetical

estimates but were not significantly different. Brummett, Nayga and Wu (2007) did not find statistical significance using a short cheap talk paragraph, while Aadland and Caplan (2003) found that cheap talk can reduce the hypothetical bias, especially in some subjects' profiles. In a different study, Aadland and Caplan (2006) argued that a neutral cheap talk can increase the hypothetical bias. Since their survey asked about a public program, the authors explained that subjects can be worried about the image of losing it, or alternatively, subjects can enhance the hypothetical nature of the task. In the end, they recommended using special caution with the use of words that can have different cognitive effects on the subjects. Using a long cheap talk script, Cummings and Taylor discussed the expected hypothetical bias and tested two alternative versions. In the first version, the authors 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.

The payment level influences the cheap talk effect: Brown, Ajzen and Hrubes (2003), using Cummings and Taylor's script, found that a long cheap talk is successful just in a higher payment context. The authors found that cheap talk reduced the hypothetical bias associated with payments of $5 and $8, but it was not significant at $3 and $1. Consistent with these findings, Murphy, Stevens, and Weatherhead (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. According to them, cheap talk did not conduct to a significant bias reduction when the subject was requested to contribute $4 and $6. However, the reduction was significantly different from zero in higher payment levels.

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

Cheap talk studies have been conducted mainly in lab settings: Of the three studies we found that tested cheap talk in a field setting, two have been done recently. Landry and List (2007), adapting a Cummings and Taylor's script, found that cheap talk was effective in eliminating hypothetical bias in CV and was not significantly different than the estimates in consequential tasks.[2] In addition, Brummett, Nayga and Wu (2007), using irradiated mangos in Texas, did not find significant differences in the WTP estimates between groups, suggesting the absence of cheap talk effects. However, it was not possible to measure the potential hypothetical bias since they did not have an actual product to compare.

Cheap talk studies have mainly utilized in CV methodologies: According to Carlsson, Frykblom, and Lagerkvist (2005), there is very limited experience on the use of cheap talk to measure hypothetical bias in choice experiments. In their analysis, they found seven out of ten WTP attributes were significantly lower for hypothetical choice tasks compared to cheap talk choice tasks. Murphy et al. (2005) found that choice experiments can significantly reduce the hypothetical bias since the choice format makes explicit the substitute effect and forces the subject to express their trade-off. At the same time, the choice format allows room to express uncertainty, which needs to be converted later into a monetary value. In other words, the cut-off value to transform preferences into monetary values will be relevant.

In summary, CA and EA are two highly used experimental approaches to compute WTP for novel products. In the application of these techniques, the study environment, nature of the pool of subjects and tasks play a key role in generating reliable results. The experimental design can involve a hypothetical or non-hypothetical task. In particular, the literature describes that hypothetical studies can be affected by hypothetical bias, which overestimates the true WTP. We discussed some of the experimental work using cheap talk as an approach to reduce or eliminate the hypothetical bias. For the first time, a cheap talk study will allow us to compare its hypothetical bias reduction to incentive-aligned techniques for OE and CA formats in a retail setting.

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

CHAPTER III
METHODOLOGY

In this chapter, we discuss the experimental specifications of the study with respect to the topics that we highlighted before in the literature review section. In addition, we explain the dataset structure and we propose a SUR approach and a random-effect Tobit model as complementary techniques to explain the data variability. In the next chapter, we explain the procedure that we followed with the data, and finally, the results are discussed and presented with some managerial implications. The nomenclature with respect to treatments and their respective tasks that are presented in this chapter are used frequently until the end of the document.

Data

We conducted the survey to each person that we could in the grocery store, so we explicitly did not want to preselect people because of any reason. However, at the time of analyzing the data, we did not include them since they were not sensitive to the task information and we did not want to underestimate the true WTP values based on people that are not in the market. We have a total of 555 completed surveys in our study. We removed 53 subjects from the data who rejected the product (32 subjects in CA and 21 subjects in OE tasks) and 3 subjects who provided of inconsistent answers. In our dataset, we decided to not include people who initially rejected the product (fresh-cut grapefruit). Specifically, some subjects rejected the product because of some external reasons, such as medical reasons. In the end, we analyzed 499 subjects distributed in eight treatments: (1) non-hypothetical OE elicitation, (2) non-hypothetical BDM OE elicitation, (3) hypothetical OE elicitation, (4) cheap talk OE elicitation, (5) non-hypothetical CA, (6) non-hypothetical BDM CA, (7) hypothetical CA, and (8) cheap talk CA (see Table 2). In the rest of the document, each group is called a treatment and is identified with its respective number from 1 to 8.

Table 2. Experimental Treatments

Treatment	Name	Task	Elicited Product	n
Open-Ended Elicitation				
1	Non-Hypothetical	Subject writes down his WTP for each of the four products, a product is randomly chosen until he gets one and pays his state price.	100%	71
2	Non-Hypothetical BDM	Subject writes down his WTP for each product, a product randomly chosen and if he win he will pay his state price if it is higher than the market price.	25%	41
3	Hypothetical	Subject writes down his WTP for each product, he does not get any product nor does he pays anything.	0%	59
4	Cheap Talk	After reading a paragraph, subject writes down his WTP for each product, he does not get any product nor does he pays anything.	0%	73
Conjoint Analysis				
5	Non-Hypothetical	Subject rates twelve pictures, a product is randomly chosen until he gets one and pays the price of the picture for that particular product.	100%	73
6	Non-Hypothetical BDM	Subject rates twelve pictures, a product randomly chosen and if he win he will pay the price of the picture for that particular product if it is higher than the market price.	25%	50
7	Hypothetical	Subject rates twelve pictures, he does not get any product nor does he pays anything. This is known as the traditional conjoint analysis.	0%	73
8	Cheap Talk	After reading a paragraph, subject rates twelve pictures. He does not get any product nor does he pays anything.	0%	59

Study Specifications

In February 2007, we conducted a field experiment utilizing both CA and OE elicitation mechanisms, at selected grocery stores in Texas. We decided to conduct a field experiment after considering the advantages identified by Lusk and Hudson (2004b). For example, subjects are in a more familiar environment, lower compensatory fees are necessary, there is a natural availability of complementary and supplementary goods, and we can better target the population of interest. Particularly, in our case, a field experiment allowed us to get a wide range of demographic characteristics, minimize participation fees, and test cheap talk scripts in a setting close to a real purchase situation. In this sense, the recruited subjects were adult shoppers (at least 18 years old), who were intercepted while exiting the store and asked to participate in the study. None could participate more than once in the protocol. The study was designed to last no longer than ten minutes to reduce respondent fatigue.

Experiment instructions were tested in a focus group using 15 graduate students in the Department of Agricultural Economics at Texas A&M University. Using the focus group information, a pretest in a grocery store was conducted. The pretest and focus group were useful to test the wording and length of the instructions, as a practice for the recruiters, and to get a range of values to be used in the CA treatments; this is similar to the procedure followed by Campbell et al. (2006). Following Ortmann's (2005) recommendations, we developed comprehensive scripts to avoid irregularities between treatments for our experimental study, which can be reviewed in the Appendices in addition to the demographic and consumption questionnaires. The scripts indentified the optimal strategy: to bid your true WTP. The eight treatments utilized similar questionnaires. The only difference was that the cheap talk treatments (treatments four and eight) included two additional questions asking specifically about the effect of the cheap talk paragraph.

With regard to endowments, each subject received four dollars as a monetary payment at the end of the study (in both the hypothetical and non-hypothetical treatments). We do not expect a significant endowment effect in the WTP estimates, since this amount is equivalent to the highest markup price (i.e., highest CA price level) for their participation. This value was chosen to minimize the endowment effect, which is more significant when higher payments are used. Further, it allowed us to engage and commit subjects to the study. For those respondents that purchased a product, the purchase price was deducted from the initial four dollars. If a subject wanted to spend more than the participation fee, they would need to pay the difference by cash or check to the researcher.

We decided to use a single round elicitation mechanism. Corrigan and Rousu (2006b) showed experimental evidence that, in repeated trials, posted prices have a significant effect on the bids of the following rounds. In other words, a subject's bid would be affected by the posted price, which is not a desirable influence on our study. We are not looking for converging values; rather, we want to identify the characteristics of respondents with different WTP.

For our study, we used a generic, short, and neutral script. Our script was inspired by the one utilized by Cumming and Taylor (1999); however, we decided to modify it for our purposes. First, we made it generic and did not make any reference to the elicited

product. Second, we made it shorter in order to be 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 situation." In addition, we did not use words that could create strong visual reference in the subject. For instance, words that recreate images of natural disasters or diseases may generate an overreaction in the subjects. In summary, our cheap talk script is generic with respect to the elicited good, short enough to be applied in a retail or phone study, and neutral with respect to the direction of the hypothetical bias.

We used value-added grapefruit products with attributes consisting of type of cut (segmented or cubed) and preservatives (with or without) in our WTP experiments. The attributes of interest (i.e., segment/cube and with/without preservatives) were identified during our pre-tests as the most important attributes that consumers consider in purchasing value-added grapefruit products. According to Hair (2006), attributes that are most important in a consumer's buying decision should be used in the product profiles. However, attributes should be chosen to minimize collinearity. This method of choosing a markup for a yet-to-be-marketed citrus product was utilized by Campbell et al. (2006). In addition to these attributes, the CA experiments also involved a price attribute ($0.50, $2.50, or $4.00 per half-pound). Our OE elicitation mechanism did not specify price since respondents were required to give their own WTP.

Each product was presented in an 8" by 10" picture, which were all taken in the same light conditions. OE elicitation and CA treatments involved four and twelve pictures, respectively, to consider all the possible combinations. CA required a larger number of pictures since it included three levels of prices, as opposed to the OE elicitation treatments. The picture order, for OE elicitation and CA, were randomized over the study to minimize any order effect. Trying to minimize undesirable patterns, we conducted the survey in three grocery stores, combining time of the day, days of the week, and treatments.

Experimental Design

Type of Subject:	Retail walk-in people over 18 years old in select grocery store in Texas
Elicited Product:	Half-pound of value-added grapefruit product.
Type of Products:	Cubes with preservatives
	Cubes without preservatives
	Segments with preservatives
	Segments without preservatives
Participation Fee:	$4 in cash at the end

Steps per Treatment

Open-Ended Elicitation Mechanism (treatment 1 to treatment 4)

Pictures Sets:	Four pictures for OE treatments
Task:	Write down WTP for each picture for OE treatments

Treatment 1: Non-hypothetical

Step 1: Subject goes over the welcome questionnaire (demographic), which can be found in the Appendix 1, for all treatments.

Step 2: Subject checks the four pictures and writes down his WTP for each one knowing that he has to purchase one of the products. Subject is informed that a binding product will be randomly picked.

Step 3: Randomly, the interviewer selects a binding product.

Step 4: Subject gets the binding product, pays the bid price, and gets the remaining cash up to $4. Consequently, each subject takes a product home and pays his initially stated WTP.

Step 5: Subject goes over the exit questionnaire (consumption), which can be found in the Appendix. The same exit questionnaire is used in all treatments.

Treatment 2: Non-Hypothetical BDM

Step 1: Subject goes over the welcome questionnaire (demographic).

Step 2: Subject checks the four pictures and writes down his WTP for each one. Subject is informed that a binding product will be picked and he will need to buy it. We randomly selected one product from four possible products, which gives them a chance of 25% to take a product home if the stated price was equal or higher than the market price.

Step 3: Randomly, the interviewer selects a binding product and shows the market price, which was previews randomly predetermined. However, the subject did not know the market price until he expressed his WTP for all products.

Step 4: The subject has to purchase the randomly selected product if their stated price was equal to or higher than the market price. He will have to pay the market price and will get the remaining cash up to $4. If he does not get the product, he will get the full $4.

Step 5: Subject goes over the exit questionnaire (consumption).

Treatment 3: Hypothetical Open-Ended

Step 1: Subject goes over the welcome questionnaire (demographic).

Step 2: Subject checks the four pictures and writes down his WTP for each one.

Step 3: There is no binding product; each subject gets the full $4 and no product.

Step 4: Subject goes over the exit questionnaire (consumption).

Treatment 4: Cheap talk Open-Ended

Step 1: Subject goes over the welcome questionnaire (demographic).

Step 2: Subject reads the cheap talk paragraph, which can be found in the Appendix.

Step 3: Subject checks the four pictures and writes down his WTP for each one.

Step 4: There is no binding product; each subject gets the full $4 and no product.

Step 5: Subject goes over the exit questionnaire (consumption).

Conjoint Analysis Treatments (treatment 5 to treatment 8)

Picture Sets: Twelve pictures for CA treatments, each picture had a unique combination of attributes and a pre-determined price. The set of twelve pictures include all the possible combinations of attributes that make an orthogonal design. In the empirical model section, we describe in detail the CA task and the transformation from rating values to monetary WTP estimates, which was used by Voelckner (2006).

Task: Rate each picture for CA treatments. A rating of one means that the subject will definitely not purchase the product, a rating of four that they may or may not and a rating of seven that they would definitely purchase the product.

Treatment 5: Non-hypothetical

Step 1: Subject goes over the welcome questionnaire (demographic).

Step 2: Subject checks the twelve pictures and rates them from one to seven.

Step 3: Randomly, the interviewer selects three binding products.

Step 4: If one of the binding products were rated four or higher, the subject will get a binding product, pays the price in the picture, and gets the remaining cash up to $4. If the subject has more than one binding product rated four or higher, we randomly pick one.

Step 5: Subject goes over the exit questionnaire (consumption).

Treatment 6: Non-Hypothetical BDM

Step 1: Subject goes over the welcome questionnaire (demographic).

Step 2: Subject checks the twelve pictures and rates them from one to seven.

Step 3: Randomly, the interviewer selects a binding product and a market price. If his rating is less than four, then another product was randomly drawn. This procedure continued until a transaction occurred or three products had been drawn randomly. Up to three products were chosen to maintain the same chance of "winning" as in treatment 2 (non-hypothetical BDM OE

elicitation), given that the subject had a favorable rating for the product. In other words, in both non-hypothetical BDM treatments, a subject has a 25% chance to take the product home if he has a real interest in the product.

Step 4: If one of the binding products were rated four or higher and the picture price is equal or higher than the market price, the subject will get a binding product, pays the market price, and gets the remaining cash up to $4.

Step 5: Subject goes over the exit questionnaire (consumption).

Based on the incentive-aligned literature, this can be thought of as a modified version of the work done by Ding, Grewal, and Liechty (2005) and Voelckner (2006). We implement a double random procedure; since we randomly selected a product and the market price was determined randomly (non-hypothetical BDM elicitation). Using this mechanism, we minimize the product requirement, while keeping the incentive compatibility properties of the elicitation mechanism intact.

Treatment 7: Hypothetical

Step 1: Subject goes over the welcome questionnaire (demographic).

Step 2: Subject checks the twelve pictures and rates them from one to seven.

Step 3: There is not binding product, each subject gets the full $4 and no product.

Step 4: Subject goes over the exit questionnaire (consumption).

Treatment 8: Cheap talk

Step 1: Subject goes over the welcome questionnaire (demographic).

Step 2: Subject reads the cheap talk paragraph, which can be found in the Appendix.

Step 3: Subject checks the twelve pictures and rates them from one to seven.

Step 4: There is not a binding product, so each subject gets the full $4 and no product.

Step 5: Subject goes over the exit questionnaire (consumption).

Empirical Model

After conducting the study either through the OE elicitation or CA format, the next critical step is the calculation and comparison of WTP estimates. Calculation of the WTP of a product, for both the hypothetical and non-hypothetical auction experiments, involved running a regression to test the significance of both treatment effects and other explanatory variables, such as demographic characteristics. With regards to hypothetical and non-hypothetical CA, when a rating scale was used, the ratings were regressed on the product profiles to obtain part-worth estimates. Next, a transformation is needed to move from utility space to a monetary price space. Voelckner's (2006) transformation utilizes a limit-card to make the conversion from utility to price, and is briefly described below. First, part-worths are estimated for each attribute level and for each respondent using ordinary least squares. Second, the utility is calculated for each product using all part-worths except those associated with price. Third, the utility for those products the respondent is willing to buy and the minimum or limit utility are calculated. Fourth, the price that equates the limit utility with the utility value in step two is then the monetary WTP.

After obtaining all the WTP for every treatment, we propose two models to analyze the data: a SUR system and a random-effect Tobit model. In the remaining part of this section, we justify the use of these models based on the data structure.

Seemingly Unrelated Regression Model

It is more flexible to assume that the variables that explain the bid level can differ by product; in addition, we expected to find correlation between the error term of each equation. In this sense, the SUR approach allows for different coefficient vectors and captures efficiency from the correlation between error terms. The model can be represented by

$$WTP_i = X_i \beta_i + \varepsilon_i ,$$

where the subscript "*i*" identifies the product (1 to 4). Greene (2003) explains that generalized least squares estimation for the SUR model allows a greater gain of efficiency when the equations are related, have a different set of explanatory variables, and when the

dataset is different for each equation. In our case, we run a simple linear regression per product to test for heteroskedasticity and run a Ramsey test[3] to avoid omitted variables. After testing each equation, we generate a system of four equations (one per product) to be estimated using a SUR approach and compute the correlation matrix of the error terms.

Random-Effect Tobit Model

As discussed by Greene (2003), there are several ways to analyze panel data, namely pooled data, fixed-effect, and random-effect models. For a detailed discussion of each approach, we recommend chapter 13, p. 283 in Greene (2003). In summary, the key distinction between approaches depends on the assumption of variability between subjects. Pooled regression assumes no variability across subjects, a fixed-effect model assumes a constant variability, and a random-effect model considers that the subject's variability changes over time (or over product in our case). Considering our dataset structure, pooled regression is too restrictive since we have a wide range of demographic characteristics that would make it difficult to assume a homogeneous group. Initially, a fixed-effect model seems appealing, however, it too is data-demanding since it requires a dummy variable per subject and we have four observations per subject. In general, fixed-effect models are recommended when there are few subjects and many observations per subject. Finally, with random-effect models, the individual differences are treated as random and assume no correlation between explanatory variables and the disturbance term.

We pooled the cross-sectional data by type of product. The panel structure allows capturing product-series and cross-sectional structure of the data. A random-effect single limit (censored at 0) Tobit model was used to assess both attribute and treatment effects on WTP. The model has a panel-like structure of our data when all treatments were merged together (sequence of responses). The dependent variable was the WTP level and the explanatory variables were the demographic and attitudinal characteristics. We include a set of dummy variables to take into account the differences per treatment (a subset per elicitation mechanism, with the first treatment as the base) and per product (1 to 4, product 1 the base). We consider a censored model, since participants were explicitly told that they

[3] The Ramsey test is used to check if a non-linear combination of the current explanatory variables has any explanatory power. If that is the case, it is taken as evidence of a misspecified model.

could assess a WTP of zero dollars if they did not want the product. At the same time, this technique is able to measure the effect of observable and non-observable influences. The last dummy variable explains the cross-sectional heterogeneity that can be expected since we are working with walk-in people. The composite error is assumed to be normally distributed. Consequently, the proposed model considers cross-sectional heterogeneity and censored data and can be described as:

$$WTP_{it} = \alpha' x_{it} + v_{it} + u_i ,$$

where subscript "*i*" identifies the subject and "t" the product (1 to 4), WTP can take zero or positive values, X_{it} is a vector of independent variables, "*u*" is the disturbance term per subject or cross section, and *v* is the overall disturbance term. Taking advantage of the panel structure, we are able to gain degrees of freedom and compare the treatments at the same time independently of the underlying demographic differences of the samples.

With respect to past research, Carlsson, Frykblom, and Lagerkvist (2005) used a random-effect logit model to measure the difference between treatments with and without cheap talk. Lusk and Fox (2003) used a random-effect Tobit model to compare non-hypothetical and hypothetical treatments in lab and field settings. Carlsson and Martinsson (2007), using a CV survey dataset and a random-effect Tobit model, estimated WTP to avoid power outages in Switzerland.

In the linear case, Hausman's test can be used to justify the decision between a fixed and a random-effect model. For the Tobit model, we found no robust evidence of the fixed-effect models. Some relevant work in this area has been done by Honore (1992).

We estimate rho (ρ), the panel unit variance (σ_v^2), and overall disturbance variance (σ_e^2),

$$\rho_i = \frac{\sigma_v^2}{\sigma_e^2 + \sigma_v^2},$$

rho is interpreted as percent contribution of the total variance of the panel level. So, a zero rho means that the panel and pooled estimator are the same.

In summary, we collected a dataset composed of 499 subjects distributed in eight treatments. Each subject expressed their WTP for four products. Considering the data structure, we proposed two complementary techniques to study the data: The SUR approach and random-effect Tobit model. In the next chapter, we formally run diagnostic tests to check the models and explain the results. Finally, in the last chapter, we discuss the managerial implications of the findings.

CHAPTER IV
RESULTS

As presented in Chapter III, the dataset is composed of 499 subjects and organized into eight treatments. In the OE treatments, each subject expressed his WTP for each of four product profiles. In the CA treatments, each subject rated twelve product profiles that mixed both attribute and prices, which were subsequently to estimate the WTP. In this chapter, we start by comparing unconditional means and median per treatment. Then, we analyze the data using a SUR approach and a random-effect Tobit model. Finally, we focus on the cheap talk treatments and check the hypothetical bias reduction.

Initially, we cleaned the data for missing observations or other errors, and then we created a set of indicator (dummy) variables for the categorical answers. The descriptive statistics for continuous and categorical variables in the models are presented in Table 3. The original questionnaires are located in Appendix A. The independent variables can be classified as demographic or consumption variables, and are either categorical or continuous in nature. Only treatments 4 and 8 (cheap talk treatments) include two follow-up questions that are directly related to the cheap talk script. The dependent variables are continuous and correspond to the WTP per product. These values are distributed from zero to six dollars and censored at zero. Comparing the unconditional mean per product, we find that subjects are willing to pay $1.47 per half pound for cubes without preservatives and $1.06 for the same product form with preservatives. In addition, consumers are willing to pay $1.61 for segmented fruit without preservatives and $1.15 for segments with preservatives. Consequently, the non preservative attribute was valued around $0.43, while the segments were valued around $0.12 more than cubes. However, we need a conditional analysis that takes into account the demographic and consumption factors and treatment information. With respect to the econometric software package, we mainly used Stata 9.2 to clean and analyze the dataset. SAS was used for the calculation of the CA part-worth estimation.

Table 3. Descriptive Statistics of the Variables

Independent Variables							
Demographics							
	Type of Variable	Mean[1]	SD	Median	Min	Max	n
Gender							
Male	Indicator	0.4	0.5	0	0	1	499
Female	Indicator	0.6	0.5	1	0	1	499
Marital Status							
Single	Indicator	0.5	0.5	1	0	1	499
Married	Indicator	0.4	0.5	0	0	1	499
Other	Indicator	0.1	0.2	0	0	1	499
Income							
Less than $19,999	Indicator	0.5	0.5	0	0	1	495
$20,000 - $39,999	Indicator	0.2	0.4	0	0	1	495
$40,000 - $59,999	Indicator	0.1	0.3	0	0	1	495
$60,000 - $79,999	Indicator	0.1	0.3	0	0	1	495
$80,000 - $99,999	Indicator	0.1	0.2	0	0	1	495
$100,000 and more	Indicator	0.1	0.2	0	0	1	495
Race or ethnic group							
White	Indicator	0.6	0.5	1	0	1	498
Hispanic	Indicator	0.2	0.4	0	0	1	498
African-American	Indicator	0.1	0.3	0	0	1	498
Asian	Indicator	0.1	0.2	0	0	1	498
Other	Indicator	0.03	0.2	0	0	1	498
Age (years)	Continuous	34.2	15.3	27	18	92	498
People at home under 14 years (number)	Continuous	0.5	0.9	0	0	6	499
People at home between 15-25 years (number)	Continuous	1.3	1.4	1	0	6	499
People at home between 26-50 years (number)	Continuous	0.7	0.9	0	0	3	499
People at home older than 50 years (number)	Continuous	0.4	0.7	0	0	4	499
Education (years)	Continuous	14.5	2.6	14	4	25	499
Education less than 12 years	Indicator	0.04	0.2	0	0	1	499
Education 12 years (graduated from high school)	Indicator	0.3	0.4	0	0	1	499
Education between 12 and 16 years	Indicator	0.3	0.5	0	0	1	499
Education 16 years (graduated from college)	Indicator	0.2	0.4	0	0	1	499
Education more than 16 years	Indicator	0.1	0.3	0	0	1	499
Consumption							
Primary household shopper							
Yes	Indicator	0.8	0.4	1	0	1	493
No	Indicator	0.2	0.4	0	0	1	493
State/Emotion							
Hungry	Indicator	0.1	0.3	0	0	1	498
Thirsty	Indicator	0.3	0.5	0	0	1	498
Both	Indicator	0.2	0.4	0	0	1	498
Neither	Indicator	0.3	0.5	0	0	1	498

Table 3. Continued

		Mean	SD	Median	Min	Max	n
Consume at least once a month							
Yes	Indicator	0.5	0.5	1	0	1	499
No, because of taste	Indicator	0.1	0.3	0	0	1	499
No, because of not sweet enough	Indicator	0.1	0.3	0	0	1	499
No, because of medication	Indicator	0.03	0.2	0	0	1	499
No, because of expensiveness	Indicator	0.02	0.1	0	0	1	499
No, because of difficultness to eat	Indicator	0.02	0.1	0	0	1	499
No, because of allergies	Indicator	0.01	0.1	0	0	1	499
No, because of a combination of above	Indicator	0.1	0.3	0	0	1	499
No, because of other reasons	Indicator	0.1	0.3	0	0	1	499
Purchase location							
Grocery store	Indicator	0.8	0.4	1	0	1	493
Farmers market	Indicator	0.1	0.3	0	0	1	493
Roadside stand	Indicator	0.002	0.0	0	0	1	493
A combination of above	Indicator	0.04	0.2	0	0	1	493
Other	Indicator	0.1	0.3	0	0	1	493
Purchase for special occasion							
Yes	Indicator	0.2	0.4	0	0	1	497
No	Indicator	0.8	0.4	1	0	1	497
Origin preferences							
Texas	Indicator	0.5	0.5	0	0	1	497
Does not matter	Indicator	0.4	0.5	0	0	1	497
Other US state	Indicator	0.04	0.2	0	0	1	497
Other	Indicator	0.1	0.2	0	0	1	497
Exposure to the raw product							
Yes	Indicator	0.7	0.4	1	0	1	498
No	Indicator	0.1	0.3	0	0	1	498
Do not know	Indicator	0.2	0.4	0	0	1	498
Valley (South Texas) origin							
Yes	Indicator	0.1	0.3	0	0	1	499
No	Indicator	0.9	0.3	1	0	1	499
Small town origin							
Yes	Indicator	0.1	0.3	0	0	1	499
No	Indicator	0.9	0.3	1	0	1	499
		1.0					
Monthly fresh product purchase (times)	Continuous	1.4	1.7	1	0	14	482
Purchased quantity (pound)	Continuous	2.1	2.9	1	0	25	493
Rating of product interest (units)	Continuous	5.6	2.6	6	1	10	322
Rating of task complexity (units)	Continuous	2.7	2.4	1	1	10	478
Cheap Talk Questions (just for groups four and eight)							
Rating of cheap talk impact on WTP (units)	Continuous	4.1	2.8	4	1	10	129
Rating of cheap talk paragraph complexity (units)	Continuous	2.1	1.9	1	1	10	131
Dependent Variables							
WTP Product 1 ($)	Continuous	1.5	1.1	1.3	0	5.3	499
WTP Product 2 ($)	Continuous	1.1	1.0	0.9	0	6.0	499
WTP Product 3 ($)	Continuous	1.6	1.2	1.5	0	5.2	499
WTP Product 4 ($)	Continuous	1.2	1.0	1.0	0	5.7	499

[1] Most of the variables are indicators. For instance, in the case of gender, the variable "male" has a value of one if the subject is a male and "zero" if otherwise. In the dataset, 40% of the subjects are men and 60% are women. When the variable is continuous, the units are indicated between parentheses. For further references, the original questionnaires can be found in Appendix A.

Carlsson and Martinsson (2001) checked the internal consistency of a choice experimental, and later on, the external consistency to test for the hypothetical bias. Following the same idea, we check the internal consistency, comparing distributions using a non-parametric approach, and checking the degree of difficulty of the task. After this, we will check the external consistency using the SUR approach and random-effect Tobit models to test for the hypothetical bias.

Internal Consistency

Initially, we checked the internal consistency testing for the differences in the WTP distribution per treatment. Table 4 presents the means per treatment and per product, their respective standard deviations, and proportion of zero values. Overall, we found that 16% of the WTP values were zero. Comparing the treatments, consistent with our expectations, the hypothetical treatments had the lowest percent of zeros compared with the OE and CA treatments with 4% and 19%, respectively.

Comparing the ratio of hypothetical/non-hypothetical means, Murphy et al. (2005) had a ratio of 1.35 in a meta-analysis and Lusk and Schroeder (2004), using a choice experiment found a mean ratio of 1.2. In our study, the hypothetical/non-hypothetical mean ratio is 1.26 for OE and 1.18 for CA treatments. Compared with the non-hypothetical BDM, the mean ratio goes to 1.20 for OE and 1.22 for CA. Finally, when we use cheap talk instead of non-hypothetical, the mean ratio is 1.27 for OE and 1.35 for CA. Now, comparing elicitation mechanisms, the mean ratio of hypothetical OE/hypothetical CA is 1.26. The mean ratio between non-hypothetical treatments is 1.18, the mean ratio between non-hypothetical BDM treatments is 1.28, and the mean ratio between cheap talk treatments is 1.35.

In Table 5, we computed the median, maximum, and minimum value per treatment and per product. The WTP values ranged from $0 to $6. However, more formal statistical methods are needed in order to test for the possible distributional differences.

Table 4. Mean per Treatment and Product

Treatment		Product 1	Product 2	Product 3	Product 4
1	Mean	1.5	1.3	1.5	1.2
	SD	1.2	1.2	1.0	1.0
	Proportion of zeros	4%	17%	6%	21%
2	Mean	1.5	1.3	1.6	1.3
	SD	0.8	1.1	0.9	0.9
	Proportion of zeros	2%	7%	0%	5%
3	Mean	1.8	1.6	1.9	1.7
	SD	0.9	0.8	0.9	0.9
	Proportion of zeros	3%	8%	0%	5%
4	Mean	1.4	1.3	1.5	1.3
	SD	0.9	0.9	0.9	0.9
	Proportion of zeros	3%	10%	1%	10%
5	Mean	1.4	0.8	1.6	1.0
	SD	1.1	0.8	1.2	0.9
	Proportion of zeros	21%	32%	15%	26%
6	Mean	1.4	0.7	1.5	0.8
	SD	1.2	0.9	1.2	1.1
	Proportion of zeros	16%	40%	16%	32%
7	Mean	1.5	0.8	2.0	1.2
	SD	1.2	0.8	1.4	1.0
	Proportion of zeros	12%	32%	11%	22%
8	Mean	1.2	0.8	1.2	0.9
	SD	1.2	0.9	1.3	1.0
	Proportion of zeros	31%	36%	27%	37%

Table 5. Median per Treatment and Product

Treatment		Product 1	Product 2	Product 3	Product 4
1	Median	1.0	1.0	1.1	1.0
	Max	4.5	6.0	4.0	4.0
	Min	0.0	0.0	0.0	0.0
2	Median	1.5	1.0	1.5	1.0
	Max	4.0	5.0	4.0	4.0
	Min	0.0	0.0	0.3	0.0
3	Median	2.0	1.5	1.8	1.5
	Max	4.0	3.5	5.0	5.0
	Min	0.0	0.0	0.3	0.0
4	Median	1.1	1.0	1.5	1.1
	Max	5.0	4.0	5.0	4.0
	Min	0.0	0.0	0.0	0.0
5	Median	1.4	0.5	1.5	0.8
	Max	3.6	3.1	4.3	3.5
	Min	0.0	0.0	0.0	0.0
6	Median	1.2	0.6	1.4	0.5
	Max	4.4	3.0	4.3	5.7
	Min	0.0	0.0	0.0	0.0
7	Median	1.3	0.7	1.9	1.1
	Max	4.3	3.3	4.9	3.9
	Min	0.0	0.0	0.0	0.0
8	Median	1.1	0.5	0.8	0.4
	Max	5.3	3.7	5.2	3.5
	Min	0.0	0.0	0.0	0.0

Considering that 16% of the WTP bids are zero values, and we are aware that WTP has a censored distribution, we did not assume normality and decided to use a non-parametric test. Consequently, we ran a series of two-sample Kolmogorov-Smirnov tests for equality of distribution functions. We tested all of the possible pair combinations for the eight treatments. Table 6 presents all of the pair combinations that are significantly different. It is possible to distinguish three overall trends. First, hypothetical treatments are significantly different from the other treatments. Second, the OE non-hypothetical treatment and cheap talk OE treatment are not significantly different from each other but are significantly different from the CA. Third, the non-hypothetical CA treatment and cheap talk CA treatment are not significantly different from each other but are significantly different from the OE treatments. This analysis checks for the differences between WTP value distributions; however, it is not possible to infer the sign or magnitude from these differences. Later in the chapter, we propose a random-effect Tobit model and SUR approach to identify and determine the magnitude of the differences.

Table 6. Mean Differences between Treatments

Treatment	Level of Significance	Product 1	Product 2	Product 3	Product 4
1	Difference at 5%	3,8	3,5,6,7,8	3,7,8	3,6,8
	Difference at 10%	3,5,8	3,5,6,7,8	3,7,8	3,6,8
	Difference at 20%	3,4,5,8	3,5,6,7,8	3,7,8	3,6,8
2	Difference at 5%	5,8	3,5,6,7,8	7,8	3,5,6,8
	Difference at 10%	5,8	3,5,6,7,8	3,7,8	3,5,6,8
	Difference at 20%	3,5,6,8	3,5,6,7,8	3,5,7,8	3,5,6,8
3	Difference at 5%	1,4,5,6,7,8	1,2,4,5,6,7,8	1,5,6,8	1,2,4,5,6,7,8
	Difference at 10%	1,4,5,6,7,8	1,2,4,5,6,7,8	1,2,4,5,6,7,8	1,2,4,5,6,7,8
	Difference at 20%	1,2,4,5,6,7,8	1,2,4,5,6,7,8	1,2,4,5,6,7,8	1,2,4,5,6,7,8
4	Difference at 5%	3,5,8	3,5,6,7,8	5,7,8	3,5,6,8
	Difference at 10%	3,5,6,8	3,5,6,7,8	3,5,7,8	3,5,6,8
	Difference at 20%	1,3,5,6,8	3,5,6,7,8	3,5,7,8	3,5,6,8
5	Difference at 5%	2,3,4	1,2,3,4	3,4	2,3,4
	Difference at 10%	1,2,3,4	1,2,3,4	3,4	2,3,4
	Difference at 20%	1,2,3,4	1,2,3,4	2,3,4,7,8	2,3,4
6	Difference at 5%	3	1,2,3,4	3	1,2,3,4,7
	Difference at 10%	3,4	1,2,3,4	3,7	1,2,3,4,7
	Difference at 20%	2,3,4	1,2,3,4	3,7	1,2,3,4,7
7	Difference at 5%	3	1,2,3,4	1,2,4,8	3,6,8
	Difference at 10%	3,8	1,2,3,4	1,2,3,4,6,8	3,6,8
	Difference at 20%	3,8	1,2,3,4	1,2,3,4,5,6,8	3,6,8
8	Difference at 5%	1,2,3,4	1,2,3,4	1,2,3,4,7	1,2,3,4,7
	Difference at 10%	1,2,3,4,7	1,2,3,4	1,2,3,4,7	1,2,3,4,7
	Difference at 20%	1,2,3,4,7	1,2,3,4	1,2,3,4,5,7	1,2,3,4,7

As an additional means to check the internal validity, we included a question asking each respondent to rate the degree of complexity of the experiments (Table 7). The idea behind this question was to better understand the complexity of the treatment for each subject and to determine whether the subject actually behaved as expected in a hypothetical task compared to a non-hypothetical task. As anticipated, the hypothetical treatments had a lower complexity index score than the non-hypothetical treatments. The hypothetical treatments required little training to complete their task, while the non-hypothetical treatments required more respondent effort to understand the experiment. In addition, non-hypothetical treatments involved some type of randomization that can be a source of misunderstanding for some people, especially subjects with no training in economics. Further analysis revealed that the hypothetical CA (the most popular approach) had the lowest complexity score, while non-hypothetical CA treatments had the highest score. In addition, the random-effect estimations show that the complexity variable was not significantly different from zero. Considering the level of complexity expressed by the subjects overall is 2.72 out of 10 (considering the eight treatments), and since the estimated coefficient is not statistically significant from zero, we do not believe that the complexity of the treatments played a significant role in their WTP estimates or in explaining the WTP variation. In addition, we were able to verify that subjects rated the complexity task as expected.

Table 7. Level of Complexity of the Task

Treatment	Complexity Level
1	2.88
2	2.92
3	2.64
4	2.15
Average Open Ended	2.60
5	3.13
6	3.07
7	2.26
8	3.26
Average Conjoint	2.88
Total Average	2.72

Note: The subject classifies the task from 1 (very easy) to 10 (very hard)

Consequently, to test internal validity, we compared the WTP unconditional means using a non-parametric approach. As predicted, we found that hypothetical treatments are significantly different and have a higher percent of zero values. The non-hypothetical approaches were not significantly different from the OE and CA groups. Finally, the subjects rated the complexity of the hypothetical tasks lower than non-hypothetical tasks. Based on these arguments, further evaluation of the data is justified.

In the next section, we estimate the sign and magnitude of the differences between treatments. We analyze the conditional behavior of the dataset using the SUR approach and random-effect Tobit model. Considering the findings up until this point, we expect to find significant differences between treatment indicator variables and also between hypothetical and non-hypothetical treatments. However, it remains to be seen whether there will be a significant difference between non-hypothetical and hypothetical WTP estimations.

Seemingly Unrelated Regression

After testing for internal validity, we performed several diagnostic tests. First, we checked the variance inflation factor, condition index, and variance decomposition for collinearity. Second, we ran an ordinary least square (OLS) regression per product in order to recover the residuals. By regressing the residuals against the continuous explanatory variables, we were able to check for heteroskedasticity using an F-test. Results indicate that the F-test was not significantly different from zero, indicating homoskedastic error terms. In addition, we used Ramsey's test to evaluate model specification. Given one equation per product, we have a system of four equations that we solve using the SUR technique, assuming correlated error terms. We expect to find significant correlation in the error terms since each subject needed to express his WTP for four similar products.

In Tables 9 and 10, we present two SUR systems. In Table 9, we present a system for the OE treatments (using treatments 1 to 4). In Table 10, we present a system for the CA treatments (using treatments 5 to 8). In both cases, we used the first treatment (non-hypothetical) as the base to compare with non-hypothetical DBM, hypothetical, and cheap talk treatments. By taking advantage of a systems approach, we are able to determine

whether cheap talk treatments offer a significant hypothetical bias reduction compared to the non-hypothetical treatment of OE and CA WTP estimates.

From the empirical results, we see some common trends associated with the OE and CA systems. First, hypothetical treatments generated significantly higher estimates than non-hypothetical treatments. Second, non-hypothetical treatments are not significantly different from cheap talk treatments. Third, in general there are no clear demographic and consumption patterns than can be used to significantly explain WTP. Even considering that the products are very similar, there is no common vector of significant explanatory variables for the four products. Fourth, there is not a statistical difference in the cheap talk effect across products. In the next section, we discuss these results with the random-effect Tobit to present a scenario complementing both.

Table 8 shows the residual correlation matrix for OE and CA treatments. As expected, the residuals are highly correlated, which is one of the assumptions that we made in using the SUR approach.

Table 8. Residual Correlation Matrix for SUR Model

Open-Ended Treatments (1 to 4)

	Product 1	Product 2	Product 3	Product 4
Product 1	1			
Product 2	0.70	1		
Product 3	0.74	0.54		
Product 4	0.59	0.78	0.63	1

Breusch-Pagan test of independence: Chi-Squared(6) = 647.404, Pr = 0.00

Conjoint Analysis Treatments (5 to 8)

	Product 1	Product 2	Product 3	Product 4
Product 1	1			
Product 2	0.39	1		
Product 3	0.79	0.21	1	
Product 4	0.20	0.65	0.49	1

Breusch-Pagan test of independence: Chi-Squared(6) = 360.285, Pr = 0.00

Table 9. SUR Model for Open-Ended Treatments

	Open-Ended Treatments (1 to 4)			
	Product 1	Product 2	Product 3	Product 4
Subjects	242	242	242	242
R-Squared	0.05	0.08	0.07	0.13
Prob > Chi-Squared	0.01	0.08	0.01	0.00
Treatment Indicators				
Non-Hypothetical	Base	Base	Base	Base
Non-Hypothetical BDM	0.02	0.02	0.07	0.05
	(0.19)	(0.19)	(0.19)	(0.18)
Hypothetical	0.29*	0.18	0.35**	0.39**
	(0.17)	(0.17)	(0.17)	(0.16)
Cheap Talk	-0.07	-0.08	0.05	0.03
	(0.16)	(0.16)	(0.16)	(0.15)
Demographics				
Age	0.004			-0.004
	(0.003)			(0.003)
People at home under 14 years		0.11**	0.17**	0.23**
		(0.05)	(0.05)	(0.05)
People at home between 26-50 years	-0.21**	-0.15*		-0.05
	(0.07)	(0.08)		(0.08)
Education Indicators				
Education less than 12 years		0.43*	-0.22	
		(0.23)	(0.24)	
Education 12 years		0.19	-0.19	
		(0.13)	(0.14)	
Education between 12 and 16 years		0.19	-0.10	
		(0.13)	(0.14)	
Education 16 years		0.19	-0.17	
		(0.13)	(0.14)	
Education more than 16 years		Base	Base	
Gender Indicators				
Female		Base		Base
Male		0.24**		0.26**
		(0.10)		(0.09)
Race Indicators				
White		Base		Base
Hispanic		-0.06		-0.005
		(0.12)		(0.12)
African-American		-0.07		0.04
		(0.16)		(0.15)
Asian		0.03		-0.15
		(0.20)		(0.19)
Other		0.09		0.00
		(0.27)		(0.26)
Primary Household Shopper Indicators				
Yes	0.23**	0.15		
	(0.10)	(0.10)		
No	Base	Base		
Situational Indicators				
Familiar with the Raw Product			-0.02	0.02
			(0.10)	(0.09)
Unfamiliar with the Raw Product			Base	Base
Hungry or Thirsty	-0.04	0.05	-0.02	
	(0.12)	(0.09)	(0.11)	
Not Hungry or Thirsty	Base	Base	Base	
Intercept	1.28**	0.93**	1.59**	1.16**
	(0.19)	(0.20)	(0.18)	(0.16)

(*) significance value at 10% level; (**) significance value at 5% level

Table 10. SUR Model for Conjoint Analysis Treatments

	Conjoint Analysis Treatments (5 to 8)			
	Product 1	Product 2	Product 3	Product 4
Subjects	237	237	237	237
R-Squared	0.08	0.12	0.11	0.09
Prob > Chi-Squared	0.00	0.00	0.00	0.00
Treatment Indicators				
Non-Hypothetical	Base	Base	Base	Base
Non-Hypothetical BDM	0.09	0.00	0.01	-0.06
	(0.22)	(0.15)	(0.24)	(0.18)
Hypothetical	0.36*	0.05	0.66**	0.34**
	(0.20)	(0.14)	(0.22)	(0.16)
Cheap Talk	-0.15	0.05	-0.18	0.02
	(0.21)	(0.14)	(0.23)	(0.17)
Demographics				
Age		-0.005	-0.01**	-0.01**
		(0.003)	(0.003)	(0.004)
People at home under 14 years	-0.23**		-0.26**	
	(0.09)		(0.09)	
People at home between 26-50 years	0.26**		0.24**	
	(0.10)		(0.10)	
Education Indicators				
Education less than 12 years	-0.31		-0.68*	-0.30
	(0.36)		(0.42)	(0.24)
Education 12 years	0.22		0.25	-0.10
	(0.24)		(0.27)	(0.16)
Education between 12 and 16 years	0.41*		0.14	-0.26*
	(0.22)		(0.26)	(0.15)
Education 16 years	0.36		0.09	-0.28*
	(0.23)		(0.26)	(0.15)
Education more than 16 years	Base		Base	Base
Gender Indicators				
Female		Base		Base
Male		0.21**		0.25**
		(0.10)		(0.10)
Race Indicators				
White	Base	Base		
Hispanic	0.53**	0.42**		
	(0.12)	(0.10)		
African-American	0.42**	0.38**		
	(0.13)	(0.11)		
Asian	0.06	0.05		
	(0.20)	(0.18)		
Other	0.11	0.06		
	(0.26)	(0.23)		
Primary Household Shopper Indicators				
Yes		0.06		0.14
		(0.12)		(0.13)
No		Base		Base
Situational Indicators				
Familiar with the Raw Product	-0.01	0.002	-0.01	0.01
	(0.01)	(0.01)	(0.01)	(0.01)
Unfamiliar with the Raw Product	Base	Base	Base	Base
Hungry or Thirsty	0.01	0.01	0.01	0.01
	(0.01)	(0.01)	(0.01)	(0.01)
Not Hungry or Thirsty	Base	Base	Base	Base
Rating of Task Complexity		0.04**	0.02	0.072**
		(0.02)	(0.02)	(0.03)
Intercept	0.87**	0.54**	1.57**	1.01**
	(0.24)	(0.19)	(0.30)	(0.27)

(*) significance value at 10% level; (**) significance value at 5% level

Random-Effect Tobit Model

Pooling all the data provides 499 subjects (cross sections) and four WTP estimates per subject. The dependent variable is the subject's bid level, and independent variables include a set of indicator variables for both product and treatment. In this way, we are able to isolate the cheap talk treatments and compare them to the hypothetical and non-hypothetical treatments. As was the case with the SUR approach, we are able to test for significant differences conditional on the demographic and consumption factors. However, a key difference is that we impose a common vector of explanatory variables using the random-effect Tobit model in place of SUR. Considering the data set characteristics, we believe that the SUR approach and the random-effect Tobit can offer complementary approaches to help us fully understand cheap talk effect on the consumer purchase process.

With respect to the random-effect Tobit model, each of the following Tables (Tables 11 and 12) has models using non-hypothetical and non-hypothetical BDM treatments as base. Table 11 shows the random-effect Tobit model for OE treatments using a restricted and full datasets. We use a restricted dataset in order to isolate the cheap talk effect and the full dataset in order to compare our results to the SUR model. At the attribute level, product 2 and product 4 (both with preservatives) have a lower WTP compared to product 1, and with a difference close to -$0.25. This fact is consistent with previous findings that show that non-preservative attributes are more valuable to consumers than the change in type of product form (from cubes to segments). The WTP for the change in product form was not significantly different from zero. At the treatment level, the non-hypothetical and cheap treatments generated statistically lower WTP estimations than the hypothetical treatment. In other words, we tested that cheap talk is effective in eliminating the hypothetical bias in both OE tasks conditional on demographic and consumption factors. Consistent with our findings using the SUR approach, we do not have clear significant trends with respect to the demographic and consumption variables. In this case, most of the WTP variation is captured by the type of product and treatment.

Table 11. Random-Effect Tobit Model for Open-Ended Treatments

	Treatment 1 as Base		*Treatment 2 as Base*	
Data Set Treatments	1,3 and 4	1 to 4	2,3 and 4	1 to 4
n	812	976	692	976
Subjects	203	244	173	244
Prob > Chi-Squared	0	0	0	0
Product Indicators				
Product 1	Base	Base	Base	Base
Product 2	-0.25**	-0.24**	-0.22**	-0.24**
	(0.06)	(0.05)	(0.06)	(0.05)
Product 3	0.03	0.04	0.08	0.04
	(0.06)	(0.05)	(0.06)	(0.05)
Product 4	-0.27**	-0.25**	-0.19**	-0.25**
	(0.06)	(0.05)	(0.06)	(0.05)
Treatment Indicators				
Non-Hypothetical	Base	Base		-0.02
				(0.16)
Non-Hypothetical BDM		0.02	Base	Base
		(0.16)		
Hypothetical	0.28*	0.30**	0.29*	0.29*
	(0.15)	(0.15)	(0.17)	(0.17)
Cheap Talk	0.02	0.00	-0.04	-0.01
	(0.14)	(0.14)	(0.16)	(0.17)
Demographics				
Age	0.004	0.002	-0.001	0.002
	(0.01)	(0.005)	(0.005)	(0.005)
People at home under 14 years	0.28**	0.19**	0.18**	0.19**
	(0.08)	(0.07)	(0.07)	(0.07)
People at home older than 50 years	-0.19*	-0.13	-0.11	-0.13
	(0.11)	(0.10)	(0.11)	(0.10)
Gender Indicators				
Female	Base	Base	Base	Base
Male	0.20	0.20*	0.18	0.20*
	(0.13)	(0.11)	(0.12)	(0.11)
Marital Indicators				
Married or Other	Base	Base	Base	Base
Single	-0.01	0.002	-0.04	0.002
	(0.15)	(0.14)	(0.14)	(0.14)
Situational Indicators				
Familiar with the Raw Product	-0.005	0.05	0.02	0.05
	(0.14)	(0.13)	(0.14)	(0.13)
Unfamiliar with the Raw Product	Base	Base	Base	Base
Hungry or Thirsty	0.13	0.16	0.12	0.16
	(0.13)	(0.12)	(0.14)	(0.12)
Not Hungry or Thirsty	Base	Base	Base	Base
Intercept	1.14**	1.12**	1.33**	1.14**
	(0.27)	(0.27)	(0.30)	(0.29)
Sigma_u	0.79**	0.79**	0.74**	0.79**
	(0.05)	(0.04)	(0.05)	(0.04)
Sigma_e	0.60**	0.59**	0.54**	0.59**
	(0.02)	(0.02)	(0.02)	(0.02)
Rho	0.63	0.64	0.65	0.64
	(0.03)	(0.03)	(0.03)	(0.03)

(*) significance value at 10% level; (**) significance value at 5% level

Table 12 shows the random-effect Tobit model for CA using both the restricted and the full dataset, as we did with Table 11 for OE treatments. At the attribute level, product 2 and product 4 (both with preservatives) have a lower WTP compared to product 1. The WTP for the product with preservatives is around -$0.80 and -$0.50 for the cubed and segmented forms, respectively. Respondents indicated a WTP around $0.30 for the segmented product. Consistent with the OE estimations, the non-preservative attribute is more valuable to the subjects than the change in product form. At the treatment level, the non-hypothetical and cheap talk treatments generated statistically lower WTP estimations than the hypothetical treatment. Also, as found before, we do not find clear demographic or consumption trends. Consequently, consistent in sign with our previous results, we find that hypothetical estimations are statistically different from non-hypothetical treatments. In addition, we see that non-hypothetical BDM treatments generate significantly lower WTP estimations than hypothetical treatments. Finally, the non-preservative attribute is consistently negative and significant. Thus, these results show the robustness of our previous findings.

Table 12. Random-Effect Tobit Model for Conjoint Analysis Treatments

	Treatment 1 as Base		*Treatment 2 as Base*	
Data Set Treatments	5,7 and 8	5 to 8	6,7 and 8	5 to 8
n	816	1016	728	1016
Subjects	204	254	182	254
Prob > Chi-Squared	0	0	0	0
Product Indicators				
Product 1	Base	Base	Base	Base
Product 2	-0.76**	-0.80**	-0.83**	-0.80**
	(0.10)	(0.09)	(0.11)	(0.09)
Product 3	0.31**	0.28**	0.28**	0.28**
	(0.10)	(0.09)	(0.11)	(0.09)
Product 4	-0.46**	-0.52**	-0.53**	-0.52**
	(0.10)	(0.09)	(0.11)	(0.09)
Treatment Indicators				
Non-Hypothetical	Base	Base		0.02
				(0.22)
Non-Hypothetical BDM		-0.02	Base	Base
		(0.22)		
Hypothetical	0.36*	0.34*	0.36*	0.36*
	(0.19)	(0.20)	(0.22)	(0.21)
Cheap Talk	-0.19	-0.20	-0.18	-0.18
	(0.20)	(0.21)	(0.24)	(0.23)
Demographics				
Age	-0.01	-0.02**	-0.02**	-0.02**
	(0.01)	(0.01)	(0.01)	(0.01)
People at home under 14 years	-0.05	0.02	0.05	0.02
	(0.10)	(0.09)	(0.11)	(0.09)
People at home older than 50 years	0.01	0.19	0.13	0.19
	(0.15)	(0.14)	(0.16)	(0.14)
Race Indicators				
White	Base	Base	Base	Base
Hispanic	0.41*	0.34*	0.25	0.34*
	(0.22)	(0.20)	(0.24)	(0.20)
African-American	0.32	0.27	0.19	0.27
	(0.22)	(0.21)	(0.25)	(0.21)
Asian	0.31	0.25	0.54	0.25
	(0.40)	(0.36)	(0.44)	(0.36)
Other	0.58	0.47	0.48	0.47
	(0.52)	(0.46)	(0.62)	(0.46)
Gender Indicators				
Female	Base	Base	Base	Base
Male	0.03	0.13	0.27	0.13
	(0.17)	(0.16)	(0.18)	(0.16)
Marital Indicators				
Married or Other	Base	Base	Base	Base
Single	0.19	0.14	0.01	0.14
	(0.21)	(0.19)	(0.23)	(0.19)
Situational Indicators				
Familiar with the Raw Product	-0.01	-0.01	-0.06	-0.01
	(0.01)	(0.01)	(0.22)	(0.01)
Unfamiliar with the Raw Product	Base	Base	Base	Base
Hungry or Thirsty	0.02	0.02	0.02	0.02
	(0.01)	(0.01)	(0.01)	(0.01)
Not Hungry or Thirsty	Base	Base	Base	Base
Intercept	1.30**	1.43**	1.52**	1.40**
	(0.37)	(0.33)	(0.43)	(0.34)
Sigma_u	1.00**	1.03**	1.04**	1.03**
	(0.07)	(0.06)	(0.08)	(0.06)
Sigma_e	0.95**	0.94**	0.98**	0.94**
	(0.03)	(0.03)	(0.03)	(0.03)
Rho	0.53	0.55	0.53	0.55
	(0.04)	(0.03)	(0.04)	(0.03)

(*) significance value at 10% level; (**) significance value at 5% level

Specifically, we address the cheap talk effect by analyzing subsets of data. With the first sub-dataset, we want to isolate cheap talk treatments and be able to check the significance of two specific variables regarding cheap talk effectiveness. These questions were included only in the cheap talk treatments. So, the first subset of data only utilized the cheap talk treatments (treatments 4 and 8). In Table 13, we find that the cheap talk effects on the WTP estimates highly depend on the elicitation mechanism. Cheap talk CA estimates are significantly lower than OE estimations. From a product point of view, consistent with previous findings, products 2 and 4 (with preservatives) have the lowest WTP estimates, whereas the non-preservatives attribute had a value of $0.30 to $0.32.

Murphy, Stevens, and Weatherhead (2005) found that 56% of people who read cheap talk paragraph expressed that they reduced their WTP after considering the cheap talk script. The rest of the sample explained that they were not influenced by the script. In our case, 8% of the subjects expressed that their evaluation was highly influenced, 31% were somewhat affected, and 61% experienced a low effect from the cheap talk script. On a scale from 1 (not affected at all) to 10 (really affected), we found the cheap talk script had, on average, an influence of 4.23 in the OE task and 4.01 for CA. In order to test these differences, we use a two-sample Kolmogorov-Smirnov test for equality of distribution functions. We do not find a significant difference in the elicitation mechanism in the cheap talk treatment estimations (OE and CA) with respect to the difficulty of the cheap talk task was for the subject and how much influence it is expected to have in the valuation. With respect to treatment, on a scale from 1 (very easy) to 10 (very difficult), subjects gave an average rating for the OE elicitation mechanism of 1.89; while the CA average rating was 2.47.

In the second subsets of data (Table 14), we compared non-hypothetical BDM and cheap talk treatments. So, the sub-dataset includes treatments 2 and 4 for the OE group and treatments 6 and 8 for the CA group. In doing, we are able to isolate non-hypothetical BDM and cheap talk treatments. As we expected, treatment and product indicators are highly significant. Products 2 and 4 have negative coefficients and the CA estimates were more negative than the OE estimates. As shown previously, there is no significant difference between non-hypothetical BDM and cheap talk treatment estimates and no clear demographic or consumption consumer profiles found within the analysis.

Table 13. Comparison between Cheap Talk Treatments

Data Set Treatments	4 and 8 (cheap talk)
n	516
Subjects	129
Prob > chi2	0
Product Indicators	
Product 1	Base
Product 2	-0.32**
	(0.09)
Product 3	0.10
	(0.09)
Product 4	-0.30**
	(0.09)
Treatment Indicators	
Open-Ended Cheap Talk	Base
Conjoint Analysis Cheap Talk	-0.66**
	(0.18)
Demographics	
Age	-0.01
	(0.01)
People at home under 14 years	0.20*
	(0.12)
People at home older than 50 years	-0.10
	(0.16)
Gender Indicator	
Female	Base
Male	0.30*
	(0.17)
Marital Indicators	
Married or Other	Base
Single	-0.20
	(0.20)
Situational Indicators	
Familiar with the Raw Product	-0.09
	(0.19)
Unamiliar with the Raw Product	Base
Hungry or Thirsty	0.27
	(0.18)
Not Hungry or Thirsty	Base
Cheap Talk	
Impact in WTP	0.01
	(0.03)
Understanding Level	0.12**
	(0.04)
Intercept	1.22**
	(0.38)
Sigma_u	0.87**
	(0.07)
Sigma_e	0.72**
	(0.03)
Rho	0.59
	(0.04)

(*) significance value at 10% level; (**) significance value at 5% level

Table 14. Comparison between BDM[1] and Cheap Talk Treatments

Data Set Treatments	2 and 4 (open-ended)	6 to 8 (conjoint analysis)
n	456	436
Subjects	114	109
Prob > chi2	0	0
Product Indicators		
Product 1	Base	Base
Product 2	-0.19**	-0.72**
	(0.07)	(0.14)
Product 3	0.09	0.13
	(0.07)	(0.14)
Product 4	-0.20**	-0.59**
	(0.07)	(0.14)
Treatment Indicators		
Non Hypothetical BDM	Base	Base
Cheap Talk	-0.05	-0.22
	(0.15)	(0.25)
Demographics		
Age	-0.005	-0.01
	(0.01)	(0.01)
People at home under 14 years	0.10	0.25*
	(0.09)	(0.15)
People at home older than 50 years	-0.10	0.40*
	(0.14)	(0.23)
Gender Indicator		
Female	Base	Base
Male	0.26*	0.48**
	(0.15)	(0.25)
Marital Indicator		
Married or Other	Base	Base
Single	-0.31*	0.22
	(0.18)	(0.30)
Situational Indicator		
Familiar with the Raw Product	0.06	0.05
	(0.17)	(0.29)
Unfamiliar with the Raw Product	Base	Base
Hungry or Thirsty	0.19	0.51*
	(0.18)	(0.28)
Not Hungry or Thirsty	Base	Base
Intercept	1.52**	0.67
	(0.37)	(0.61)
Sigma_u	0.76**	1.12**
	(0.05)	(0.10)
Sigma_e	0.51**	0.95**
	(0.02)	(0.04)
Rho	0.69	0.58
	(0.04)	(0.05)

(*) significance value at 10% level; (**) significance value at 5% level

[1] Becker, Degroot, and Marshack mechanism

Table 15 shows the estimated coefficients of the difference between the OE and the respective CA treatment. We found that the non-hypothetical BDM mechanism reduces, and cheap talk increases, the difference in the estimation between elicitation mechanisms.

Table 15. Random-Effect Tobit Coefficient Comparing Pairs of Treatments

Non-Hypothetical	-0.41**
	(0.19)
Non-Hypothetical BDM	-0.37*
	(0.22)
Hypothetical	-0.44**
	(0.16)
Cheap Talk	-0.66**
	(0.18)

(*) significance value at 10% level; (**) significance value at 5% level
Notes: The complete model includes the same vector of variables. The base is the respective open-ended treatment.

In the next two Tables, we present estimations that are discussed and compared with past research in the discussion section. First, regarding familiarity, Lusk (2003a), using a mailed survey for golden rise, found that subjects who had prior knowledge of the elicited product did not have a significant reduction in WTP because of the cheap talk. In addition, subjects with no previous knowledge had a significant reduction in WTP after the cheap talk script. In addition, List (2001) found that card dealers did not have hypothetical bias and non-card dealers were biased. We used a familiarity index if the subject was from the production area or had previously tried the raw product. Texas grapefruit production is located exclusively in the Rio Grande Valley (South Texas), so we consider it likely that a subject from the production area or who had consumed the product before would have a significantly different prior knowledge or experience than the general population, and thus affect the WTP estimation. Using that definition of a familiar subject, we found that 75% and 77% of the subjects were familiar with the product for the OE and CA treatments, respectively.

Table 16 shows that subjects that are familiar with the product (also called familiar subjects) do not have significant hypothetical bias, since non-hypothetical BDM, hypothetical, and cheap talk treatments are not significantly different between them. At the same time, subjects that are unfamiliar with the product (also called unfamiliar subjects) do have hypothetical bias considering that non-hypothetical BDM and cheap talk treatments

are not significantly different between them, but they are significantly different from the hypothetical treatments. These results are consistent for the OE and CA treatments.

Table 16. Testing for Hypothetical Bias in Familiar Subjects

	Open-Ended		Conjoint Analysis	
	Familiar	Unfamiliar	Familiar	Unfamiliar
Data Set Treatments	2,3 and 4	2,3 and 4	6,7 and 8	6,7 and 8
n	520	172	560	164
Subjects	130	43	140	41
Prob > Chi-Squared	0	0	0	0
Product Indicators				
Product 1	Base	Base	Base	Base
Product 2	-0.21**	-0.27**	-0.84**	-0.79**
	(0.07)	(0.11)	(0.13)	(0.22)
Product 3	0.08	0.08	0.25**	0.33
	(0.07)	(0.10)	(0.12)	(0.22)
Product 4	-0.17**	-0.24**	-0.57**	-0.40*
	(0.07)	(0.11)	(0.13)	(0.22)
Treatment Indicators				
Non-Hypothetical BDM	Base	Base	Base	Base
Hypothetical	0.25	0.55*	0.19	1.17**
	(0.19)	(0.34)	(0.25)	(0.42)
Cheap Talk	-0.11	0.17	-0.39	0.54
	(0.18)	(0.31)	(0.27)	(0.43)
Demographics				
Age	-0.0002	-0.02	-0.02**	0.01
	(0.01)	(0.02)	(0.01)	(0.02)
People at home under 14 years	0.09	0.54**	0.07	0.21
	(0.08)	(0.14)	(0.12)	(0.26)
People at home older than 50 years	-0.17	0.49	0.13	-0.08
	(0.13)	(0.46)	(0.18)	(0.37)
Gender Indicators				
Female	Base	Base	Base	Base
Male	0.11	0.31	0.29	0.47
	(0.15)	(0.24)	(0.21)	(0.34)
Marital Indicators				
Married or Other	Base	Base	Base	Base
Single	-0.02	0.19	0.10	-0.37
	(0.17)	(0.29)	(0.26)	(0.50)
Intercept	1.51**	1.33**	1.73**	0.52
	(0.31)	(0.62)	(0.45)	(0.92)
Sigma_u	0.75**	0.65**	1.07**	0.90**
	(0.05)	(0.08)	(0.09)	(0.14)
Sigma_e	0.56**	0.49**	0.99**	0.95**
	(0.02)	(0.03)	(0.04)	(0.07)
Rho	0.64	0.64	0.54	0.47
	(0.04)	(0.06)	(0.05)	(0.09)

(*) significance value at 10% level; (**) significance value at 5% level

Second, recently, there has been some research related with emotions and their effect on WTP estimations. Capra, Meer, and Lanier (2006) studied the effect of moods on bidding behavior using an n^{th} price auction. The authors found that moods have a significant impact on WTP estimations. Particularly, a subject in a positive mood overbid around 18% higher and a subject in a negative mood did not overbid. The authors explained the overbidding behavior by suggesting that the subject in a positive mood can be less detail-oriented. According to Martin and Clore (2001), people in a negative mood tend to focus more critically on a task than people in a neutral or positive mood. In our case, we expect that being hungry or thirsty can be interpreted as a negative feeling. We found that 72% and 68% of the subjects in the OE and CA task reported they were hungry, thirsty, or both at the time of answering the survey. Table 17 shows that being hungry or thirsty can make a difference estimating the hypothetical bias in an OE mechanism. However, we did not find the same significance in CA estimates.

As a summary of this chapter, complementing the results obtained using both the SUR and random-effect Tobit approaches; we find that there is consistency in the results of both approaches. Overall, the hypothetical treatments are significantly higher than the non-hypothetical and cheap talk treatments. Also, the last two were not significantly different when compared to each other. Specifically regarding cheap talk, treatment effects are not significantly different across products; however, they are significantly different with respect to elicitation mechanisms, and some other factors such as familiarity and being hungry or thirsty. In the next chapter, we discuss these findings and present their managerial implications.

Table 17. Testing for Hypothetical Bias Hungry/Thirsty Subjects

	Open-Ended		Conjoint Analysis	
	Hungry/Thirsty	Not Hungry/Thirsty	Hungry/Thirsty	Not Hungry/Thirsty
Data Set Treatments	2,3 and 4	2,3 and 4	6,7 and 8	6,7 and 8
n	496	196	492	232
Subjects	124	49	123	58
Prob > Chi-Squared	0	0	0	0
Product Indicators				
Product 1	Base	Base	Base	Base
Product 2	-0.19**	-0.29**	-0.78**	-0.95**
	(0.07)	(0.10)	(0.14)	(0.19)
Product 3	0.10	0.03	0.25*	0.31*
	(0.07)	(0.10)	(0.13)	(0.18)
Product 4	-0.14**	-0.32**	-0.49**	-0.63**
	(0.07)	(0.10)	(0.14)	(0.19)
Treatment Indicators				
Non-Hypothetical BDM	Base	Base	Base	Base
Hypothetical	0.25	0.61**	0.37	0.15
	(0.19)	(0.30)	(0.24)	(0.55)
Cheap Talk	-0.003	0.12	-0.17	-0.39
	(0.19)	(0.29)	(0.26)	(0.57)
Demographics				
Age	-0.004	0.01	-0.03**	0.001
	(0.01)	(0.01)	(0.01)	(0.02)
People at home under 14 years	0.19**	0.14	0.11	-0.09
	(0.08)	(0.18)	(0.11)	(0.27)
People at home older than 50 years	-0.07	-0.33**	0.26	-0.22
	(0.16)	(0.16)	(0.18)	(0.34)
Gender Indicators				
Female	Base	Base	Base	Base
Male	0.15	0.25	0.28	0.44
	(0.16)	(0.18)	(0.20)	(0.41)
Marital Indicators				
Married or Other	Base	Base	Base	Base
Single	0.04	-0.08	0.01	-0.26
	(0.18)	(0.23)	(0.24)	(0.50)
Intercept	1.48**	0.77	1.85**	0.98
	(0.32)	(0.53)	(0.45)	(0.94)
Sigma_u	0.78**	0.61**	0.91**	1.27**
	(0.06)	(0.08)	(0.09)	(0.16)
Sigma_e	0.56**	0.49**	1.01**	0.88**
	(0.02)	(0.03)	(0.04)	(0.06)
Rho	0.66	0.61	0.45	0.67
	(0.04)	(0.07)	(0.05)	(0.06)

(*) significance value at 10% level; (**) significance value at 5% level

CHAPTER V
CONCLUSIONS AND IMPLICATIONS

There is an agreement that people in hypothetical situations behave differently compared to those in real situations. Particularly in WTP studies, subjects tend to overstate their actual WTP. Evidence of this hypothetical bias is widespread in CV studies (Cummings, Harrison, and Rutstrom 1995; List and Gallet 2001; Loomis et al. 1997; Neill et al. 1994) and is less frequent in CA (see Ding, Grewal, and Liechty 2005). Over the last decade, experimental economics have contributed to the increased realism of study settings, and therefore to the increased reliability of WTP estimates.

In the literature review section, we reviewed some aspects that need to be taken into account for experimental design and implementation. In addition, we presented EA and CA as alternative ways to measure WTP. We discussed the cheap talk approach and incentive-aligned CA as techniques to reduce hypothetical bias and reviewed results of previous cheap talk studies, which have been conducted mainly in lab settings.

We conducted a study that involved 499 subjects, eight treatments in select grocery stores in Texas. We decided to conduct a retail study since that allowed us to take advantage of some of the benefits of field experiments. Lusk and Hudson (2004b) identified them as a way to easily focus in on a target population, lower experimental costs, reduce bias of high or non-uniform fees, and give a natural buying setting. In addition, CA allows for the creation of an environment that considers substitute goods. With respect to the task, we discussed some of the aspects that need to be covered in a cheap talk paragraph. According to Cummings and Taylor (1999), the length of their scripts makes their application in phone surveys difficult. For our study we were inspired by the Cummings and Taylor scripts, which have been widely used in experimental economics. However, we made it generic, shorter, and with neutral content. We were looking for a generic script that would be easy to apply to other products. Since we conducted our study in a retail setting, we needed to use a short cheap talk script that subjects could easily understand. Finally, we wanted to use neutral content to avoid manipulating the subject's answers to a particular side. In other words, we wanted to make people aware about hypothetical bias, but not explicitly tell them what to do about it. Using the SUR approach

and random-effect Tobit models in the results section, we found that our cheap talk paragraph was effective in removing hypothetical bias in a retail setting. Also, non-hypothetical, non-hypothetical BDM, and cheap talk treatments did not generate significantly different WTP estimations. These findings were consistent for OE and CA estimations. At the same time, we found that cheap talk consistently eliminated the hypothetical bias, but did not have significant difference effect across products. In general, we found that hypothetical treatment and preservative content variables were significantly different from zero. In terms of the other variables, we did not find a significant set of demographic or consumption variables in common.

We identified two factors that can effect WTP estimations: familiarity and hunger/thirst. With respect to familiarity, Lusk (2003a), in a mailed cheap talk study, did not have a non-hypothetical reference point for comparison, so he could test whether hypothetical and cheap talk treatments are significantly different between them, but we do not have a non-hypothetical treatment to state that the hypothetical bias was removed. In contrast, List (2001) was able to test the existence of hypothetical bias since he had non-hypothetical and hypothetical treatments. Using sport cards, List found that card dealers did not have hypothetical bias and non-card dealers were biased. Consequently, the cheap talk effect is less severe or not significant for experienced subjects, since they are closer to the real value of the good. In other words, it does not lower the WTP estimates when there is no hypothetical bias to begin with. Consistent with the previous research, in our study, we find that familiar subjects do not have significant hypothetical bias and unfamiliar subjects do have significant hypothetical bias. Considering a significance level of 5%, these results were robust for OE and CA treatments.

With respect to emotions, we controlled for hunger/thirst. Since we do not have evidence how hunger/thirst affects non-hypothetical estimations, we cannot state that the hypothetical bias was removed. We can limit our analysis to test if there is statistical difference. As we expected, hunger and thirst datasets did not have significant difference with CA and OE treatments. In other words, non-hypothetical BDM, hypothetical, and cheap talk treatments are not significantly different from one another. For the non-hungry/thirsty group, as expected, the hypothetical treatment is significantly different. However, for non-hungry/thirsty subjects, the hypothetical treatment in the CA dataset is

not significantly different, which can be explained in two ways. First, it could be that mood effects are a function of the type of task. Even for the non-hungry/thirsty dataset, since the CA treatments involve a bigger number of tradeoffs, it could be the case that the subject truly reveals his WTP. Second, it is possible that we need to control for the interaction of other emotional states like anger, empathy, or happiness. These variables can be significant by themselves or interact with other demographics or consumption variables. We suggest more research in this area, focusing on how emotions affect hypothetical and non-hypothetical elicitation mechanisms.

When using experiments in a retail setting, our results suggested that indeed, consumers' WTP values are influenced by the type of valuation or elicitation mechanism used, and also by the hypothetical/non-hypothetical nature of the valuation process. Interestingly, our findings suggest that OE WTP values are significantly higher than those obtained from CA. Consistent with Voelckner (2006), we found a significant difference in the values using OE and CA approaches. We offer two alternative explanations. First, as noticed by Champ et al. (1997), leaving room to express uncertainty in an experimental task can reduce the hypothetical bias and possibly some of the difference between elicitation mechanisms too, especially considering that uncertainty is often presented as a source of hypothetical bias. In other words, CA tasks involve purchase intention and not necessarily purchase action. Second, CA tasks involve attribute trade-offs between close substitutes. In the literature, List and Shogren (1998) found that the inclusion of substitutes reduced the WTP in hypothetical and non-hypothetical studies, which is consistent with our findings. Along the same line, Murphy et al. (2005) found that choice studies have less hypothetical bias. The authors suggest that fact since a choice-based experiment involves a trade-off of products, which makes explicit the substitution effect between close products. There is evidence that expressing WTP with substitutes generates lower estimates than without them.

The natural question is to ask which elicitation mechanism generates the most reliable WTP estimation? We have shown evidence that a single subject can have more than a single WTP value for a product based on their emotions or study specifications. Therefore, we believe that it is more fruitful to focus the WTP debate on the characteristics of the study and its subjects rather than try to shoot for the perfect number. For instance, a

reliable elicitation mechanism for market purposes would be incentive-compatible or it would have to have a way to control for hypothetical bias, consider substitution effects, and be elicited in a situation that mimics the point of purchase. We also recommend that market researchers view these elicitation mechanisms as complementary to each other rather than substitutes. For instance, experiences and values from a CA study can be used to better design EA and vice-versa.

In the extant literature, random-effect models have been used to explain WTP estimations. From a methodology point of view, we suggested that SUR can play an interesting role in experimental economics. The SUR approach allowed us to gain degrees of freedom, to have independent explanatory variable vectors, and to assume correlated error terms. Empirically, we found that the error terms were correlated and the set of variables which were significantly different from zero, were different for each product, even though the products were very similar. In addition, the SUR approach allowed us to join hypothesis and test coefficient values, while imposing some theoretical restrictions. We do not believe that one approach is necessarily the best in all cases, but rather the SUR approach is a possible way to explain data variability. At present, we like the idea of being able to double-check our main findings using both mechanisms.

From a managerial point of view, we identify two contributions. First, managers can use the cheap talk approach to generate more reliable WTP estimations and avoid calibrating hypothetical estimations. As previously stated regarding calibration functions, List and Gallet (2001) recommended consideration of the type of good (public/private), elicitation mechanisms, and the type of study (willingness to pay/accept). In addition, List and Shogren (1998) stated that calibration functions are good and context-specific and presented evidence that elicited goods in the presence of substitutes and experienced subjects reduced the hypothetical bias. In summary, a reliable calibration function needs to be tailored to a particular study. Instead, we offer a generic, short, and neutral script that allows itself to be applied in several formats and is a hypothetical approach that does not require a real product. Consequently, the cheap talk approach is presented as an attractive mechanism to obtain, eliminate, or at least to reduce hypothetical bias without further considering characteristics of the good. We recommend using this cheap talk script in future applications to have more robust findings.

As a second managerial contribution, the numerical estimations showed that subjects are more willing to pay for the non-preservative attribute rather than for additional value added to the product form. This finding has direct managerial implications, since decision makers can focus more to avoid the use of preservatives and use the product form that allows the longest shelf life. At the same time, since the non-preservative coefficients were around 0.25-0.75, we have an estimation of the WTP for that attribute that can be used to evaluate the feasibility of avoiding the use of preservatives.

With regards to future research in the cheap talk area, research can test alternative cheap talk specifications and identify the most sensitive areas of it. Another topic can be to test whether our cheap talk script (short and neutral) generates significantly different estimates than Cummings and Taylor's scripts (long and non-neutral), which is a reference in many cheap talk studies. Finally, we recommend continued work on the effect that emotions can have on WTP estimations and their interactions with other study specifications, such as setting, elicitation mechanism, or type of subjects. In the future, market researchers can use these psychological factors to determine proper marketing practices to commercialize the product in the marketplace.

In summary, previous research shows that a cheap talk script would be effective when the subject is committed to reading it, which is more likely to happen when it is short or there is a high payment level. However, a high payment level can create endowment effects. So, a short cheap talk script can help researchers to commit subjects with a low payment level. In contrast, long paragraphs in mail surveys add to the researcher uncertainty that the subject read or what portion the paragraph. Specifically, the researcher does not know of the paragraph was actually read.

In our study, we have applied a generic, short, and neutral cheap talk script that eliminates the hypothetical bias in a retail setting. Our results showed that the cheap talk treatment and the incentive-aligned estimates were not statistically different from one another. Since it is a short and neutral paragraph, it cannot be argued that the subjects were manipulated to either side, and the script is more compatible with time constraint study formats, as in phone surveys or retail studies. We encourage the continued use of this paragraph in future applications.

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APPENDIX A
QUESTIONNAIRES

Marketing Study

TAMU project…please help us!!

You need to be at least 18 years old

Anonymous

Payment: Up to $4 in cash per person

Marketing study that lasts 5/10 minutes

Number	:
Pic Set	:
Interviewer	:
Time	:
Date	:
TT	:

Welcome Questionnaire

1. What is your gender?

a) Male
b) Female

2. What year were you born?
19____

3. Which best describes your marital status?

a) Single
b) Married (or with a live-in partner)
c) Other_____(specify)

4. What was your household income before taxes in 2005?

a) Less than $19,999
b) $20,000 - $39,999
c) $40,000 - $59,999
d) $60,000 - $79,999
e) $80,000 - $99,999
d) $100,000 and more

5. How many people/roommates live in your household (fill each and include yourself)?
____ under 14 years
____ 15-25 years
____ 26-50 years
____ > 50 years

6. What race/ethnic group best describes you?

a) White
b) Black/African-American
c) Asian or Pacific Islander
d) Native American
e) Hispanic
f) Other_____(specify)

7. What was the last level of education that you completed?
(12 years = graduated from high school
16 years = graduated college)

___ number of years

Product Weight 0.5 pound…please be careful with the order!!

Price ($)

A. Cubes with preservatives _____

Comments……..……

B. Cubes without preservatives _____

Comments……..……

C. Segments with preservatives _____

Comments……..……

D. Segments without preservatives _____

Comments……..……

General Comments……...……..

…….…..…….…...……

…….…..…….…...……

Product Weight 0.5 pound…please be careful with the order!!

Scale: from 1 (the worst) to 10 (the best)

1-10 Scale (No price)

A. Segments with preservatives $2.5 _____

Comments……

B. Segments without preservatives $2.5 _____

Comments……

C. Cubes with preservatives $1.0 _____

Comments……

D. Cubes without preservatives $2.5 _____

Comments……

E. Cubes with preservatives $2.5 _____

Comments……

F. Segments without preservatives $1.0 _____

Comments……

1-10 Scale (No price)

G. Segments with preservatives $3.5 _____

Comments……

H. Cubes with preservatives $3.5 _____

Comments……

I. Segments with preservatives $1.0 _____

Comments……

J. Cubes without preservatives $1.0 _____

Comments……

K. Cubes without preservatives $3.5 _____

Comments……

L. Segments without preservatives $3.5 _____

Comments……

General Comments……

……

Bidding Letter _____

Paid Price : $ _____

Exit Questionnaire

1. Are you the primary household shopper?

a) Yes
b) No

2. With regards to the product you purchased, rate how much interest you had in that product (It could be decimals) from 1 (very disinterested) to 10(very interested) _____

3. Rate how complex the study was for you (you can use decimals) from 1 (very easy) to 10 (very hard) _____

4. At this moment, are you hungry or thirsty?

a) Hungry
b) Thirsty
c) Both
d) Neither

5. Do you eat grapefruit at least once a month from October to March (including juice)?

a) Yes, please go to question 7
b) No

6. Why do you not eat grapefruit regularly (circle as many as apply)?

a) Not sweet enough
b) Taste (other than sweetness)
c) Difficult to eat
d) Allergies
e) Medication
f) Too expensive

g) Other_____(specify)

7. How often do you purchase fresh grapefruit?

____ times per month

8. How much do you purchase when you buy?

____ pounds

9. Where do you generally purchase grapefruit?

a) Grocery store
b) Farmers market
c) Roadside stand
d) Other_____(specify)

10. Do you purchase grapefruit for a special occasion (circle as many as apply)?
a) No
b) Yes, so when?
i1) Christmas
i2) Special family events
i3) Thanksgiving
i4) New Year
i5)Other_____(specify)

11. Where do you prefer your grapefruit to come from?

a) Texas
b) The US, other than Texas
c) Origin does not matter
d) Other_____(specify)

12. Have you ever eaten a Texas grapefruit?

a) Yes
b) No
c) Do not know

13. Are you originally from the Valley (South Texas)?

a) Yes
b) No

14. Are you originally from a town that has less than 1,000 people?

a) Yes
b) No

Just for the Cheap Talk Survey…

15. Rate how much your evaluation was affected by the cheap talk script? (you can use decimals) from 1 (not affected at all) to 10 (really affected) _____

16. How difficult was the cheap talk to understand? (you can use decimals) from 1 (very easy) to 10 (very hard) _____

APPENDIX B
CHEAP TALK SCRIPT

"Studies show that people tend to act differently when they face hypothetical decisions. In other words, they say one thing and do something different. For example, some people state a price they would pay for an item, but they will not pay the price for the item even when they see this product in a grocery store.

There can be several reasons for this different behavior. It might be that it is too difficult to measure the impact of a purchase in the household budget. Another possibility is that it might be difficult to visualize themselves getting the product from a grocery store shelf and paying for it. Do you understand what I am talking about?

We want you to behave in the same way that you would if you really had to pay for the product and take it home. Please take into account how much you really want the product, as opposed to other alternatives of fresh-cut products that you like or any other constraints that might make you change your behavior, such as taste or your grocery budget. Now could you please tell me what price you are willing to pay for each of the following products? Please try to really put yourself in a realistic situation."

APPENDIX C
INSTRUCTIONS PER TREATMENT

General Instructions

I would like to ask your cooperation in a marketing study; you need to be older than eighteen years old. Your participation is completely voluntary and you can leave at any minute. If you stay until the end you will get four dollars or its equivalent in products to thank you for your cooperation. Please start answering a welcome questionnaire and please let me know if you have any questions.

The whole survey will take around ten minutes; this should be the first and only time that you will participate in this study. All the information collected is anonymous and will be used in group comparisons; no individual records will be published. Please do not communicate with anyone while you are answering the questions and let me know of any questions that you have at any time. Please fill out the following questionnaire, and once you finish return it to me and I will explain the next step. *The surveyor checks that the questionnaire is complete and turns the page, which corresponds to the answer sheet for the conjoint analysis or open-ended procedure.*

Open-Ended Study (treatments 1 to 4)

Please check this set of four pictures. Every picture represents a real product and we have a sample of it. These pictures represent real grapefruit products. You cannot buy more than one product. Now, please write down the price that you will be willing to pay for every one of them. Because we are interested in the value that you personally place on the items for sale, it is very important that you not talk to or try to communicate with the other participants.

The four dollars that you received as a participation fee do not relate to your bid, so, you are free to choose to spend any amount in a grapefruit product of the pictures. The numbers that you write are your bids for these products and you would have to pay that price to get the specific product later on. Please feel free to express your valuation of the good. It is okay to write a zero or any larger number.

Before doing the real valuation, I want to ask you some questions to help to understand the directions. How many products will you be able to buy today? What do the pictures represent?

For the Non-Hypothetical Open-Ended Treatment (treatment 1)

After you finish writing down the prices, I will randomly determine the binding product. You will be able to get the product if you just write down any positive number. If not, I will randomly pick another product until you get one. In the case that all your prices are zero, you will be able to go home keeping the full four dollars. Do you have any question? So, at the end of the study, if you write the product four or more, you will go home with a half-pound grapefruit product and the remaining cash up to four dollars.

For the Non-Hypothetical BDM Open-Ended Treatment (treatment 2)

After you finish writing your willingness to pay for each product, I will randomly determine a binding product. You will be able to get a product if you just state that the price for that product is equal or higher than the random price. If not, you will be able to go home keeping the full four dollars. Do you have any questions? So, at the end of the study, if you price the binding product equal or higher than the market price, you will go home with a half-pound of that grapefruit product and the remaining cash up to four dollars.

In this study, it is in your best interest to bid the amount that you are truly willing to pay to buy a grapefruit product. If you bid more than your true willingness-to-pay you increase your chances of purchasing the product but you may have to pay a price that is greater than what you are willing to pay. On the other hand, if you bid less than the amount that you are truly willing to pay, then you may lose the chance to purchase a grapefruit product at a price that you would be willing to pay.

Please remember that we will randomly determine the binding product and the price that you wrote is your bid. Finally, we will draw another random number. If your bid is equal to or higher than the random number, you will have to buy and take home the product and pay the amount that you wrote for it. If your bid is less than the random number, you

will not be able to buy the product. There is no right or wrong answer. Do not hesitate to ask if you have questions.

For the hypothetical open-ended treatment (treatment 3)

Please write out the prices that truly represent your willingness to pay for each product. You will receive a full four dollars as a participation fee. Let me know if you have any questions or something is not completely clear for you.

For the hypothetical open-ended cheap talk treatment (treatment 4)

I would ask you to go over the following paragraph. After, please write out the prices that truly represent your willingness to pay for each product. You will receive the full four dollars as a participation fee. Let me know if you have any question or something is not complete clear for you.

Read aloud the picture label when you show it and let the subject bid for it

Do these bids represent your real willingness to pay for each product? If you want, at this time, you can change your bid.

Let the subject change his bid.

I would like to ask you to fill out the exit questionnaire. Thank you very much for your cooperation. We will be performing this study most of the day; please do not reveal details of the study to anybody.

Money payment minus actual product price for non-hypothetical treatments and $4 for hypothetical treatments.

Conjoint Analysis Study (treatments 5 to 8)

You will see a set of pictures of value-added grapefruit product. Each picture is labeled with its content, which corresponds to a half-pound of the product. I need you to rate them on a scale from one to seven, one being the worst and seven the best. If you don't like any of them you can choose rate one all options. There is no right or wrong answer. Do not hesitate to ask if you have any questions.

For the non-hypothetical conjoint analysis treatment (treatment 5)

After you finish rating, I will randomly determine the binding product. You will be able to get the product if you just rate that the product is equal to or higher than four. If not, I will randomly pick another product until you get one. In the case that all your rates are lower than four, you will be able to go home keeping the full four dollars. Do you have any questions? So, at the end of the study, if you rate a picture four or more, you will go home with a half-pound grapefruit product and the remaining cash up to four dollars.

For the non-hypothetical BDM conjoint analysis treatment (treatment 6)

After you finish rating, I will randomly determine three binding products. You will be able to get one of them if you just rate that the product is equal to or higher than four. If not, you will be able to go home keeping the full four dollars. Do you have any questions? So, at the end of the study, if you rate a binding product a four or a higher score, you will go home with a half-pound of that grapefruit product and the remaining cash up to four dollars.

Please remember that I will randomly determine the binding products, and the price that you list for that product is your bid. We will draw another random number, which will be the market price. If your bid is equal to or higher than the random number, you will have to buy and take home the product and pay the amount that you bid for it. If your bid is less than the random number, you will not be able to buy the product. There is no right or wrong answer. Do not hesitate to ask if you have any questions.

For the hypothetical conjoint analysis treatment (treatment 7)

Please write out the prices that truly represent your rate for each product. You will receive full four dollars as a participation fee. Let me know if you have any questions or if something is not completely clear for you.

For the cheap talk conjoint analysis treatment (treatment 8)

I would ask you to go over the following paragraph. After that, please write out the prices that truly represent your rate for each product. You will receive a full four dollars as a participation fee. Let me know if you have any questions or if something is not completely clear for you.

Read aloud the picture label when you show it and let the subject rate it

Do these rates represent your real willingness to pay for each product? If you want, at this time, you can change your rates.

Let the subject change his rate

I would like to ask you to fill out the exit questionnaire. Thank you very much for your cooperation. We will be performing this study most of the day; please do not reveal details of the study to anybody.

Money payment minus actual product price for non-hypothetical treatments and $4 for hypothetical treatments.

VITA

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Awards:	First Place in the Case Study Competition at the International Food and Agribusiness Management Association Conference. Parma, Italy, June 23-26, 2007 (three-person team).