Willingness to Pay for a Green Energy Program: A Comparison of Ex-ante and Ex-post Hypothetical Bias Mitigation Approaches[1]

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Abstract

The most persistently troubling empirical result in the contingent valuation method literature is the tendency for hypothetical willingness to pay to overestimate real willingness to pay. Two approaches, ex-ante and ex-post, have been developed to mitigate or eliminate the overstatement of hypothetical willingness to pay. The ex-ante approach addresses hypothetical bias in the survey design stage while the ex-post approach addresses hypothetical bias with follow-up questions to the hypothetical willingness to pay question. We find that willingness to pay estimates are similar when either the ex-ante or ex-post approach are employed. Our results suggest that the approaches should be considered as complements and not substitutes. Employing both approaches to mitigate hypothetical bias we estimate that the annual benefits of the regional amenities associated with a green energy program in North Carolina are $186 million.

Key Words: Hypothetical Bias, Willingness to Pay, Green Energy

JEL Code: Q51

Introduction

The Kyoto Protocol, which went into effect on February 16, 2005 with the participation of 141 parties, is an effort to mitigate global warming by reducing emissions of carbon dioxide and other greenhouse gasses. The United States withdrew from the Protocol in 2002, citing high costs, uncertain benefits, and the Bush Administration's preference for voluntary programs aimed at greenhouse gas emission reductions. Since then, many corporations, states and municipalities have pursued policies to address greenhouse gas emissions. One approach is the implementation of green energy programs that allow consumers to voluntarily purchase energy from non-fossil fuel sources such as wind and solar power (e.g., the North Carolina GreenPower Program[3]). By shifting energy production away from sources emitting greenhouse gasses, participation in green energy programs may contribute to reductions in greenhouse gas emissions and improvements in regional and local public goods such as air quality that leads to health, recreation and passive use benefits. Little is known about the economic benefits of these voluntary programs.

The benefits of policies addressing greenhouse gas emissions can be estimated with revealed and stated preference methods. The contingent valuation method (CVM) is a stated preference approach for measuring the use and passive use benefits of government policy. Use values associated with reduced greenhouse gas emissions might be experienced through improved recreation or improvements in one's own health. Much research has compared benefit estimates from the CVM and revealed preference methods. Carson et al. (1996) conduct a meta-analysis of over one hundred studies that compare CVM and revealed preference method

[3] See http://www.epa.gov/greenpower/locator/.

estimates and find a positive correlation suggesting the similarity of value estimates across valuation methodology. They also find that CVM estimates are about 30% lower, on average, than those estimated from revealed preference methods.

One of the advantages of the CVM is its ability to elicit economic values from people who do not directly experience the changes resulting from policy (i.e., passive use values). Preferences for ecological integrity, altruism for others and bequests to future generations contribute to passive use values. For some policies, passive use values may exist but their contribution to total value is not substantial. In these cases revealed preference methods are most appropriate. For some policies, however, ignoring the measurement of passive use values would lead to significant errors in policy analysis. For example, the benefits of policies that address greenhouse gas emissions may be largely determined by passive use values arising from bequests to future generations.

The most persistently troubling empirical result in the CVM literature is hypothetical bias, the tendency for hypothetical willingness to pay to overestimate real willingness to pay (Cummings, Harrison, and Rutström, 1995; Cummings et al., 1997, Blumenschein et al., 1997). Hypothetical bias occurs when CVM respondents state that they will pay for a good when in fact they will not, or they will actually pay less, when placed in a similar purchase decision. Hypothetical bias is usually attributed to the presence of passive use values and lack of familiarity of paying for policies that provide passive use value. However, hypothetical bias has been found in a variety of applications including private goods for which no passive use values should exist (List and Gallet, 2001). Surprisingly, hypothetical bias is downplayed in much of the CVM literature (Harrison, forthcoming).

Hypothetical bias arises because answers to CVM willingness to pay questions have no real consequences other than a weak connection to the influence of government policy. Respondents who state that they would pay for the policy change are not required to actually pay. Some respondents may state that they would pay for the policy when, in fact, they would not if placed in the real situation. Two reasons for this behavior are an attempt to influence policy by signaling their support (e.g., strategic bias, warm glow) and to please the interviewer (e.g., yea saying). Hypothetical bias leads to upwardly biased willingness to pay estimates. Willingness to pay estimates from the CVM must be considered upper bounds of benefits in the context of benefit-cost analysis unless steps are taken to mitigate hypothetical bias.

Two approaches have been developed to mitigate the overstatement of hypothetical willingness to pay. The ex-ante approach addresses hypothetical bias in the survey design stage. Respondents are variously (a) told that there are substitutes for the policy available, (b) reminded that they are income constrained, (c) asked to answer as if they were placed in an actual payment situation and (d) told that hypothetical bias is a significant problem and asked not to succumb to this type of respondent error. The ex-post approach addresses hypothetical bias with follow-up questions to the hypothetical willingness to pay question. Respondents who indicate that they are willing to pay for the policy are asked to rate the certainty they have in their willingness to pay. Respondent certainty is measured on a qualitative or quantitative scale where the low and high ends of the scale allow respondents to express their degree of certainty about their payment. Hypothetical willingness to pay responses are then recoded based on the certainty of the respondent.

Both ex-ante and ex-post approaches have been shown to successfully mitigate and even

eliminate hypothetical bias. An unaddressed question is the comparative performance of the approaches. In this paper we compare ex-ante and ex-post hypothetical bias approaches. Most studies that address hypothetical bias compare hypothetical and real willingness to pay. Instead, due to research budget limits, we conduct a typical hypothetical CVM survey with a scenario that includes a realism and budget reminder in a split-sample treatment. Both samples are asked about their payment certainty with a quantitative follow-up certainty question. We compare the impact of both approaches separately and together.

Related Literature

The ex-ante approach to hypothetical bias mitigation has evolved from simple reminders about economic constraints to elaborate lessons on how to avoid overstating one's willingness to pay. Loomis, Gonzalez-Caban, and Gregory (1994) reminded respondents about substitutes and income constraints and find that these reminders do not affect willingness to pay. In Loomis et al. (1996) respondents are reminded about income constraints and asked to answer as if they would actually pay. They find that the additional survey information moves hypothetical willingness to pay towards real willingness to pay.

Cummings and Taylor (1999), in what is called a "cheap talk" script, define hypothetical bias for respondents, explain why it may occur, and ask respondents to behave as if they are in a real payment situation. They find that the divergence between hypothetical and real willingness to pay is eliminated by the cheap talk script. List (2001) finds that the cheap talk script eliminates hypothetical bias for utility maximizers (card show consumers) but not for profit maximizers (card show dealers) who may have more familiarity with the value of the product. Brown, Ajzen and Hrubes (2003) find that a cheap talk script is able to mitigate hypothetical bias at high bid

levels but not at low bid levels. Aadland and Caplan (2003) find that a "short-scripted" cheap talk design for phone surveys is able to mitigate hypothetical bias for respondents who have strong environmental preferences. Lusk (2003) finds that the cheap talk script eliminates hypothetical bias for respondents with less knowledge about the goods.

Studies that have employed the ex-post correction approach have used qualitative and quantitative certainty scales. Johannesson, Liljas, and Johansson (1998) use two certainty categories and consider only those respondents who indicate they are "absolutely sure" about payment (the other category is "fairly sure"). They find that hypothetical willingness to pay understates real willingness to pay when only those who are "absolutely sure" are considered. Blumenschein et al. (1998), Blumenschein et al. (2001), and Blumenschien et al. (2004) consider only those respondents who indicate they are "definitely sure" about payment (the other category is "probably sure"). They find that hypothetical willingness to pay is no different than actual willingness to pay when adjusted by respondent certainty.

A number of studies have used quantitative certainty scales. Champ et al. (1997) considers only those respondents who indicate that they are very certain at the highest point of a 10 point quantitative scale (i.e., 10 is very certain). The percentage of respondents who are very certain about paying and respondents who would actually pay are no different. Champ and Bishop (2001) find that those respondents who are certain of their willingness to pay at the 8 or higher level on a 10 point scale have similar hypothetical willingness to pay compared to a real willingness to pay sample. Poe et al. (2002) and Vossler et al. (2003) find that those respondents who are certain of their willingness to pay at the 7 or higher level on a 10 point scale have similar probabilities of payment as a real willingness to pay sample.

Johannesson et al. (1999) used the respondent's self assessed quantitative certainty ranking to estimate a statistical bias function to calibrate the hypothetical yes responses. Calibration of the hypothetical yes responses caused no statistical difference between hypothetical and actual yes respondents. Other calibration approaches (e.g., CVM-X) have also incorporated socioeconomic and laboratory data to calibrate hypothetical responses with actual responses (Blackburn et al., 1994, Fox et al., 1998). Studies indicate the usefulness of such calibration techniques is reduced because the resulting functions appear to be commodity-, context-, and even individual-specific (Mansfield, 1988; Fox et al., 1998; List and Shogren, 2002).

A few studies have considered both ex-ante and ex-post approaches. Poe et al. (2002) include a short cheap talk script and find that it has no effect on willingness to pay. In addition to the short cheap talk script Aadland and Caplan (2003) include a three level qualitative certainty rating question but do not recode yes responses. Blumenschien et al. (2004) finds that cheap talk does not mitigate hypothetical bias but that certainty ratings eliminates the bias.

Most recently, Champ, Bishop and Moore (2005) find that cheap talk does not fully mitigate hypothetical bias while certainty rating recoding does. Actual donation and cheap talk treatment hypothetical donation empirical models are statistically different. Actual donation and certainty rating recoded hypothetical donation empirical models are not statistically different. On the other hand, willingness to donate estimates are not statistically different between the actual donations ($24), cheap talk treatment hypothetical donations ($36) and certainty rating recoded hypothetical donations ($30).

Survey Design

Our application is to a green energy program in North Carolina. Previous CVM studies have focused on green energy programs in Wisconsin and New York. Both of these studies compare hypothetical and real willingness to pay. Champ and Bishop (2001) estimate the benefits of a voluntary wind energy program from the Madison Gas and Electric Company. The average annual hypothetical willingness to pay for wind power is $101 and actual willingness to pay is $59 (pre-2001 dollars). As described above, the hypothetical willingness to pay is equal to the real willingness to pay with an ex-post correction. Poe et al. (2002) and Vossler et al. (2003) estimate the voluntary willingness to pay for a renewable energy facility operated by Niagara Mohawk Power Corporation in New York. Almost 31 percent are willing to pay a hypothetical $6 monthly surcharge for the program. After an ex-post correction, 21 percent are willing to pay, which better corresponds to the 20 percent that actually make a donation.

In order to develop a realistic willingness to pay scenario we present a hypothetical market in which survey respondents are first described the good to be valued. The good generated by the green energy program is improved air quality in the western North Carolina mountains. Next, the change in air quality is described. After the payment mechanism and policy implementation rules are described the willingness to pay question is presented. We based our survey design on the prior studies that examined willingness to pay for green power (Champ and Bishop, 2001, Poe et al., 2002).

The first few questions in the survey elicit knowledge, importance and concern about air quality. These questions are primarily used to define air quality and describe threats to air

quality.[4] The hypothetical portion of the survey begins with a question introducing a "Green Energy" program (Appendix). Respondents are told that the hypothetical program is similar to a real program called the North Carolina GreenPower program. Only a few respondents had already heard about the GreenPower program. The next question states that the hypothetical green energy program would offer all North Carolina utility customers power generated from renewable energy sources such as wind and solar. Over eighty percent of the respondents are very interested or somewhat interested in this program. Respondents are told that the goal of the program is to get 10 percent of all North Carolina utility customers to sign up. Almost three quarters of the sample think that it is very likely or somewhat likely that 10 percent would sign up. This result indicates that most respondents find this component of the scenario credible.

The next question defines the scope of the program based on the dimensions of air quality. Respondents are told that if 10 percent of all North Carolina utility customers sign up, air quality in the western North Carolina mountains would improve. Visibility would increase by about Δq miles, the number of streams and acres of forests impacted by acid rain would decrease by about Δq percent, and the number of people who get sick because of breathing problems would decrease by about Δq percent. There are three scope versions that are randomly assigned to respondents: $\Delta q = 2$, 10 and 20. Respondents are asked for their opinion about the likelihood of achieving the air pollution goal in order to determine if they find the scenario credible. Over three quarters think that it is very likely or somewhat likely that the goal will be reached. These frequencies do not vary significantly by the scope of the program.

[4] We did not mention global warming due to the political controversy surrounding the issue in 2002.

We adopt the same payment vehicle used by Champ and Bishop (2001) and Poe et al. (2002) -- a voluntary surcharge to the monthly utility bill. The voluntary contribution is a lower bound on maximum willingness to pay (Champ et al., 1997). The magnitude and rationale for the additional monthly fee, is described: "In a voluntary Green Energy program, households that choose to participate would pay an extra *A* dollar fee each month with their power bills. This fee would be fixed and not tax-deductible. The fee would cover the higher production costs of green energy." The fee was randomly assigned to respondents and took on one of four values: $A = 5$, 15, 30, and 50. Respondents were then asked for their average monthly power bill in order to get them to assess the impact the monthly fee would have. The average monthly power bill is over $100.

Respondents were then described the policy implementation rule that includes the provision point design to minimize free riding (Poe et al. 2002; Rose et al. 2002): "If you signed up for the green energy program and were not satisfied you could cancel the program at any time. But if less than 10% signed up, the green energy program would not have enough customers to make it cost effective. The program would stop and you would owe no money." A split-sample survey design is used in which one-third of all respondents were reminded about their budget constraint and substitutes and asked to think of the hypothetical decision as if it were a real decision: "Now please think about the next question just like it was a real decision. If you signed up for the program you would have *A* dollars less each month to spend on other things." This script is similar in length to the so-called "short scripted" cheap talk design of Aadland and Caplan (2003). We refer to this as the *REMINDER* version since it does not incorporate most of the cheap talk characteristics found in Cummings and Taylor (1999).

The willingness to pay question is then presented: "Suppose you were given the opportunity to participate in the green energy program for an extra fee of *A* dollars each month. Would you sign up for the green energy program?"[5] In contrast to previous hypothetical bias research we allow respondents to give yes, no and don't know responses. Respondents are then asked the follow-up questions about how certain they are about their willingness to pay response: "We would like to know how sure you are that you would sign up. On a scale of 1 to 10 where 1 is very uncertain and 10 is very certain, how certain are you that you would sign up?"

Respondents who would (would not) sign up at the monthly fee are asked for the most important reason why. The most popular reasons for signing up are for a better environment, better human health, for future generations, and because "it is the right thing to do." The most popular reasons for not signing up are the cost is too high, not enough income, and "I don't trust the power companies."

Data

During November 2002 we conducted a telephone survey of North Carolina residents. The North Carolina GreenPower program is available statewide, so the sample was randomly taken from all 100 North Carolina counties. The survey treatments were randomly distributed across respondents. We completed interviews with 431 respondents and achieved a response rate of 61%. Due to item nonresponse, data from 353 respondents are included in the empirical

[5] We note that it was not possible to observe actual participation by respondents in this case. The lack of an internal real willingness to pay benchmark is unfortunate, but our relative findings remain instructive when taken in conjunction with previous work on this issue.

analysis.

In the sample without the *REMINDER* the percentage of raw yes responses falls from 61 to 40 percent as the monthly fee rises from $5 to $50 (Table 1). With the *REMINDER* the percentage of yes responses falls from 55 to 24 percent as the fee rises from $5 to $50. At the two higher fees, the *REMINDER* leads to yes percentages that are substantially lower compared to the lower fees. This result is consistent with Brown, Ajzen, and Hrubes (2003) who find that cheap talk is most effective at high bids.

Twenty four percent of all yes responses are certain of their response at the highest level (10). Nineteen percent, 31, and 5 percent are certain at the 7, 8, and 9 levels. As in Poe et al. (2002), we define those who give a 7 or higher as those who are sure about their willingness to pay.[6] Since we include a don't know response option, we recode uncertain yes responses to don't know responses instead of no responses. When the uncertain yes responses are recoded to don't know responses, the percentage of yes responses falls from 52 to 36 percent without the *REMINDER* treatment. With the *REMINDER* treatment the percentage of certain yes responses falls from 52 to 16 percent as the monthly fee rises from $5 to $50.

Empirical Model

Willingness to pay, *WTP*, is the difference in expenditure functions

[6] Recoding all respondents who are certain at the level 8 and above would result in twenty-nine fewer yes responses. Median willingness to pay estimates at this recoding level are not statistically different from zero as in Loomis and Ekstrand (1998).

(1) $$WTP = e(q,u) - e(q + \Delta q, u)$$

where Δq is the increase in air quality. The yes responses to the willingness to pay question depend on whether willingness to pay is greater than the monthly fee (i.e., yes if $WTP \geq A$). The probability of a yes response is estimated with the multinomial logit model

(2) $$\prod_j(yes) = \frac{\exp(\alpha_j \log A + \beta_j' X_i)}{\sum_{k=1}^{3} \exp(\alpha_k \log A + \beta_k' X_i)}$$

where $\log A$ is the natural log of the fee amount, β is a vector of coefficients, X_i is a vector of independent variables (including a constant), $i = 1, \ldots, n$ respondents, and $j \in k$. The $k = 3$ choices are: k is equal to 1 if yes, 2 if no, and 3 if don't know. The coefficients for the base case (β_1), are normalized to zero. The multinomial logit model produces separate coefficient vectors for the no (β_2) and don't know (β_3) responses.

Willingness to pay is estimated from the censored logit coefficients. The coefficient on the fee amount, α, is equal to Cameron's $1/\kappa$ (Cameron, 1988). Since the dollar amount is varied across respondents, $1/\kappa$ can be identified and willingness to pay can be recovered from the estimated coefficients

(3) $$WTP_2 = \exp(\kappa_2 \beta_2' \overline{X}) = \exp\left(\frac{\beta_2' \overline{X}}{\alpha_2}\right)$$

when don't know responses are discarded by the researcher and

(4) $$WTP_{2=3} = \exp\left(\kappa_{2=3}\beta'_{2=3}\overline{X}\right) = \exp\left(\frac{\beta'_{2=3}\overline{X}}{\alpha_{2=3}}\right)$$

when the don't know response coefficient vector is constrained to be equal to the no response coefficient vector (Carson et al., 1998; Groothuis and Whitehead, 2002). Standard errors for the willingness to pay estimates are constructed using the delta method (Cameron, 1991; Greene, 1997). When the natural log of the fee amount is used in the regression WTP is the median of the willingness to pay distribution. Median willingness to pay is the dollar amount that 50% of respondents would agree to pay. Median willingness to pay is calculated from the estimates of the regression coefficients at the mean of the independent variables, $\overline{X}$.

Empirical Results

The multinomial logit models are presented in Table 2. The independent variables include the monthly fee, scope, a dummy variable for the ex-ante *REMINDER* version (*REMINDER* = 1, 0 otherwise) and household income (in thousands). The average household income, *INCOME*, is \$53 thousand. The natural log of the monthly fee (log *A*) is used because it performed significantly better according to likelihood ratio tests.[7] We present four models. With the raw yes responses, the unconstrained multinomial model estimates separate coefficient vectors for the no and don't know responses. The constrained model is estimated with the no and don't know response coefficient vectors constrained equal. Unconstrained and constrained models are estimated when the uncertain yes responses are recoded as don't know responses.

[7] Multinomial logit results with a linear fee amount are qualitatively similar. These are available upon request.

The probability that the respondent is not willing to pay the monthly fee increases with the magnitude of the fee. The probability that the respondent does not know if they are willing to pay the monthly fee also increases with the magnitude of the fee. The coefficient on *Log A* is positive and statistically significant in each model. This result indicates that respondents behave rationally with respect to cost and allows the estimation of the monthly willingness to pay for the green energy program. The effect of income on the probability of no and don't know responses is negative except in the unconstrained yes response coefficient vector. This indicates that the air quality improvements resulting from the green energy program are normal goods.

The scope test is conducted by including two dummy variables for the $\Delta q = 10$ and $\Delta q = 20$ versions. *SCOPE=10* is equal to 1 if the respondent received the $\Delta q = 10$ version and zero otherwise. *SCOPE=20* is equal to 1 if the respondent received the $\Delta q = 20$ version and zero otherwise. In general willingness to pay is sensitive to the scope of the policy. In the unconstrained yes response model the probability of a no response falls relative to the low scope version when $\Delta q = 20$. There is no difference in the probability of a no response when comparing $\Delta q = 2$ to $\Delta q = 10$. The probability of a don't know response is lower when scope increases. In the constrained model, the probability of no and don't know responses falls when scope is equal to 10 and 20 but the coefficients between the two high scope versions are not statistically different. Results are similar when the uncertain yes responses are recoded to don't know responses.

The coefficient on the *REMINDER* dummy variable is not statistically significant in the unconstrained yes response model. When the no and don't know response coefficient vectors are constrained to be equal the *REMINDER* coefficient is statistically significant at the $p = .10$ level,

increasing the probability of a no or don't know response. When the uncertain yes responses are recoded to don't know responses the coefficient on the *REMINDER* dummy variable is statistically significant in the unconstrained model at the p = .05 and p = .10 levels for the no and don't know responses. When the no and don't know response coefficient vectors are constrained equal the *REMINDER* coefficient is statistically significant at the p = .05 level, increasing the probability of a no or don't know response.

Willingness to Pay

Willingness to pay estimates from the four models are presented in Table 3. Six different scenario estimates are presented for each model: willingness to pay at each of the three scope levels with and without the ex-ante *REMINDER* treatment. Scenarios 1, 2 and 3 are without the *REMINDER* treatment and *SCOPE* = 2, 10 and 20. Scenarios 4, 5 and 6 are with the *REMINDER* treatment and *SCOPE* = 2, 10 and 20. For brevity we focus our discussion on the willingness to pay estimates from the constrained models 2 and 4.

The willingness to pay estimates from the yes vs. no and don't know response model 2 without the *REMINDER* are likely prone to hypothetical bias. In Scenarios 1-3 of Model 2 when hypothetical bias is ignored, willingness to pay is $22 when *SCOPE* = 2 and greater than the highest bid amount when *SCOPE* = 10 and *SCOPE* = 20. When the *REMINDER* is used in Model 2 scenarios 4-6 the willingness to pay estimates fall by more than 50 percent at each scope level relative to the willingness to pay estimates in Model 2 scenarios 1-3.

When the uncertain yes responses are recoded to don't know responses, the willingness to pay estimates from Model 4 scenarios 1-6 also fall by more than 50 percent relative to the

willingness to pay estimates in the corresponding Model 2 scenarios. For example in scenario 1, willingness to pay is $12 when the uncertain yes responses are recoded and $22 when they are not. When both approaches are used in Model 4, Scenarios 4-6, willingness to pay estimates are about one-third of the willingness to pay estimates when hypothetical bias is ignored in Model 2, Scenarios 1-3.

We test for differences in willingness to pay estimates in the ex-ante and ex-post correction methods by constructing t-tests with the standard errors estimated from the delta method for the following three willingness to pay comparisons: (1) Model 2, Scenario 4 vs. Model 4, Scenario 1, (2) Model 2, Scenario 5 vs. Model 4, Scenario 2 and (3) Model 2, Scenario 6 vs. Model 4, Scenario 3. The three differences in willingness to pay are not statistically different from zero. The differences are -$1.30 (t = 0.73), -$0.57 (t = 0.04) and $0.36 (t = 0.02)

Conclusions

We find that willingness to pay estimates are similar when either the ex-ante or ex-post hypothetical bias mitigation approaches are used. However, it seems reasonable to remind respondents about their budget constraint and to answer the willingness to pay question as if it was real. In fact, in this study the reminder is more effective when uncertain yes respondents are recoded to don't know responses. This suggests that ex-ante and ex-post approaches to mitigating hypothetical bias are complements instead of substitutes. In other words, it may be that only the relatively certain respondents are those who heed the call to tell the truth. This interpretation may help explain why the Loomis, Gonzalez-Caban, and Gregory (1994) ex-ante reminder did not affect willingness to pay and why the Loomis et al. (1996) ex-ante reminder did not cause hypothetical willingness to pay to equate to real willingness to pay. If ex-ante and ex-

post approaches are complements, studies that employ only one of the approaches in an attempt to mitigate hypothetical bias may still overstate willingness to pay.

The complementarity between ex-ante and ex-post approaches was furthered explored by examining the influence of the reminder text on (1) the 1-10 certainty responses and (2) the likelihood that yes respondents are above or below the level 7 cutoff. In a variety of tests we find no statistically significant evidence that the reminder text affects certainty responses ($p = .10$). However, we note that our sample of yes respondents is relatively small, $n = 151$, and only $n = 31$ of these responded with a certainty level less than 7. Larger samples and more elaborate reminder text might lead to the conclusion that the ex-ante and ex-post approaches are substitutes. The substitutability of the ex-ante and ex-post approaches warrants further research.

Combining both ex-ante and ex-post approaches to mitigating hypothetical bias has considerable promise in addressing the most serious of criticisms of the contingent valuation method. This paper contributes to this line of research by comparing the two approaches to mitigating hypothetical bias. Future studies should examine cheap talk, budget, and other ex-ante reminders in conjunction with certainty rating recodes instead of as a competing approach. A major impediment to the implementation of cheap talk script is its length, especially in the more commonly used mail and telephone surveys. We find that the probability of a yes response falls when a short reminder is included as in Aadland and Caplan (2003). This is in contrast to other studies that examine short cheap talk scripts. This result provides hope that shorter cheap talk scripts could be a practical method for mitigating hypothetical bias.

Our application is to a green energy program in North Carolina. Without a real willingness to pay value with which to compare hypothetical willingness to pay the most

conservative WTP estimate, adjusted with both the ex-ante and ex-post approaches, should be used in benefit-cost analysis. Our most conservative willingness to pay estimate is $4.24/household/month for the least ambitious scope version. The annual willingness to pay is $51 which is very similar to the annual willingness to pay for a wind energy program found by Champ and Bishop (2001). The 2000 U.S. Census estimates that there are about 3.5 million housing units in North Carolina in 2000. Aggregating the annual willingness to pay across the housing units yields an estimate of the annual benefits of the green energy program: $186 million (2002 dollars). According to our results, the benefits of the regional amenities associated with a green energy program in North Carolina are substantial. Future research should estimate the benefits of other voluntary green energy programs and compare the benefits to their costs.

Appendix: Hypothetical Scenario

10. Now consider a hypothetical Green Energy program. The hypothetical program is based on a real program that is being considered right now by power plants that affect air quality in North Carolina. How much have you heard about the real program, called the North Carolina Green Power program? Have you heard a lot, some, a little or nothing?

____ A lot

____ Some

____ A little

____ Nothing

11. With the hypothetical Green Energy Program all utility companies in North Carolina would offer their customers power generated from renewable energy sources such as wind and solar. How interested are you in the Green Energy program? Are you very interested, somewhat interested or not interested?

____ Very interested

____ Somewhat interested

____ Not interested → We would still like your opinions about the following questions.

12. The goal of this program would be to get 10% of all North Carolina utility customers to sign up. In your opinion, how likely do you think it is that 10% of all North Carolina

utility customers would sign up? Do you think it is very likely, somewhat likely, somewhat not likely, or not likely at all?

____ Very likely

____ Somewhat likely

____ Somewhat not likely

____ Not likely at all

13. If 10% of all North Carolina utility customers sign up, air quality in the western North Carolina mountains would improve. Visibility would increase by about Δq[8] miles, the number of streams and acres of forest impacted by acid rain would decrease by about Δq percent, and the number of people who get sick because of breathing problems would decrease by about Δq percent. In your opinion, how likely do you think it is that these goals would be reached? Do you think it is very likely, somewhat likely, somewhat not likely, or not likely at all?

____ Very likely

____ Somewhat likely

____ Somewhat not likely

____ Not likely at all

[8] *Δq = 2, 10 or 20.*

14. In a voluntary Green Energy program households that choose to participate would pay an extra A^{9} dollar fee each month with their power bills. This fee would be fixed and not tax-deductible. The fee would cover the higher production costs of green energy. Not including water, what is your average monthly power bill now?

$ _____

15. If you signed up for the green energy program and were not satisfied you could cancel the program at any time. But if less than 10% signed up, the green energy program would not have enough customers to make it cost effective. The program would stop and you would owe no money. Suppose you were given the opportunity to participate in the green energy program for an extra fee of *A* dollars each month. *Now, please think about this question just like it was a real decision. If you signed up for the program, you would have A dollars less each month to spend on other things.* Would you sign up for the green energy program?

____ Yes

____ No → *Skip 16*

____ Don't know → *Skip 16*

16. We would like to know how sure you are that you would sign up. On a scale of 1 to 10 where 1 is very uncertain and 10 is very certain, how certain are you that you would sign up?

_____ (Record response, 1 to 10)

[9] *A* = 5, 15, 30 or 50.

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Table 1. Willingness to Pay Responses

	NO REMINDER				REMINDER[a]			
A = $5	All Yes Responses		Recoded Yes Responses		All Yes Responses		Recoded Yes Responses	
Yes	39	61%	33	52%	17	55%	16	52%
No	13	20%	13	20%	6	19%	6	19%
Don't Know	12	19%	18	28%	8	26%	9	29%
A = $15								
Yes	26	43%	22	36%	14	42%	7	21%
No	16	26%	16	26%	11	33%	11	33%
Don't Know	19	31%	23	38%	8	24%	15	45%
A = $30								
Yes	23	40%	16	28%	5	18%	3	11%
No	24	41%	24	41%	15	54%	15	54%
Don't Know	11	19%	18	31%	8	29%	10	36%
A = $50								
Yes	21	40%	19	36%	6	24%	4	16%
No	19	36%	19	36%	11	44%	11	44%
Don't Know	13	25%	15	28%	8	32%	10	40%

[a]The reminder version includes information about budget constraints and substitutes.

Table 2. Multinomial Logit Models

	All Yes Responses						Recoded Yes Responses					
	Model 1				Model 2		Model 3				Model 4	
	No Response		Don't Know Response		No and Don't Know Response		No Response		Don't Know Response		No and Don't Know Response	
	Coeff.	t-ratio	Coeff.	t-ratio	Coeff.	t-ratio	Coeff.	t-ratio	Coeff.	t-ratio	Coeff.	t-ratio
CONSTANT	-0.962*	-1.76	-1.068*	-1.90	-0.974*	-2.12	-0.792	-1.38	-0.514	-0.96	-0.608	-1.28
LogA	0.674*	4.22	0.378*	2.35	0.530*	3.98	0.744*	4.40	0.410*	2.64	0.555*	4.00
SCOPE=10	-0.433	-1.38	-0.685*	-2.05	-0.552*	-2.03	-0.466	-1.39	-0.562*	-1.74	-0.523*	-1.82
SCOPE=20	-0.741*	-2.22	-0.542	-1.61	-0.646*	-2.27	-0.778*	-2.19	-0.474	-1.43	-0.605*	-2.02
REMINDER[a]	0.435	1.55	0.375	1.28	0.400*	1.64	0.590*	1.94	0.552*	1.90	0.564*	2.15
INCOME	-0.020*	-4.20	-0.005	-0.99	-0.013*	-3.23	-0.022*	-4.46	-0.008*	-1.76	-0.014*	-3.52
Log likelihood function	-355.44				-363.40		-361.98				-368.61	
Restricted log likelihood	-379.05				-379.05		-387.76				-387.76	
Model $\chi 2$	47.23				31.30		51.54				38.29	
Cases	353				353		353				353	

*Significant at $p < .10$.

[a]The reminder version includes information about budget constraints and substitutes.

Table 3. Monthly Median Willingness to Pay Estimates

			All Yes Responses				Recoded Yes Responses			
			Model 1 Yes vs. No Response		Model 2 Yes vs. No and Don't Know Response		Model 3 Yes vs. No Response		Model 4 Yes vs. No and Don't Know Response	
Scenario	SCOPE	REMINDER[a]	WTP	t-ratio[b]	WTP	t-ratio	WTP	t-ratio	WTP	t-ratio
1	2	NO	20.09	2.73	22.18	2.45	14.33	2.82	11.73	2.47
2	10	NO	38.21	2.59	62.88	2.01	26.80	2.90	30.13	2.47
3	20	NO	60.37	2.12	75.03	1.82	40.82	2.42	34.91	2.21
4	2	YES	10.54	2.33	10.43	2.11	6.48	2.18	4.24	1.76
5	10	YES	20.04	2.26	29.56	1.96	12.12	2.27	10.89	1.96
6	20	YES	31.66	2.08	35.27	1.88	18.46	2.20	12.62	1.94

[a]The reminder version includes information about budget constraints and substitutes.

[b]The t-ratio is calculated with the delta method and tests HO: WTP = 0.