Obtaining Unbiased Contingent Values:
Further Tests of Entreaties to Avoid Hypothetical Bias

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Abstract

Over-estimation of willingness to pay in contingent markets has been attributed largely to hypothetical bias. One promising approach for avoiding hypothetical bias is to tell respondents enough about such bias that they self-correct for it. A script designed for this purpose by Cummings and Taylor (1999) was used in hypothetical referenda that differed in payment amount and in secrecy of the vote. In comparisons with behavior observed in otherwise identical real payment referenda, the script worked well whether the vote was open or secret at higher payment levels, but inadequately corrected the bias at a lower payment level. Generally, both hypothetical bias and the effectiveness of the script in correcting for that bias increased as did referendum payment amount.

Introduction

Given the goal of efficient management of environmental resources but the difficulty of estimating the benefits of environmental programs using revealed preference methods, hopes have been high that properly designed contingent markets would emulate actual markets. However, evidence has mounted that willingness to pay measured in contingent markets over-estimates payment observed in otherwise identical real cash markets. Differences between real and hypothetical willingness to pay, which we will call hypothetical bias, have been found for private goods such as a chocolate bar (Kealy, Dovidio, & Rockel, 1988), a houseplant (Boyce, Brown, McClelland, Peterson, & Schulze, 1989), an original landscape painting (Neill, Cummings, Ganderton, Harrison, & McGuckin, 1994), a juicer (Cummings, Harrison, & Rutstrom, 1995), an art print (Loomis, Brown, Lucero, & Peterson, 1996), and a sports card (List, 2001). Significant over-

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estimations have also been found for funding public goods such as removing roads to enhance wilderness character along the North Rim of the Grand Canyon (Brown, Champ, Bishop, & McCollum, 1996), distributing a citizens' guide to households affected by groundwater contamination in Albuquerque (Cummings, Elliott, Harrison, & Murphy, 1997), and helping the Nature Conservancy protect rain forest in Costa Rica (Cummings & Taylor, 1999). These studies used a mixture of bidding mechanisms, suggesting that the over-estimation cannot be ignored.[2]

Approaches for accurately estimating willingness to pay in contingent markets include (1) using real payment bids for other goods as a way to calibrate the hypothetical bids (Blackburn, Harrison, & Rutstrom, 1994; Fox, Shogren, Hayes, & Kliebenstein, 1998), (2) asking respondents how certain they are that they really would be willing to pay as much as they said they would and using the certainty response to adjust the bids (Champ, Bishop, Brown, & McCollum, 1997; Li & Mattsson, 1995), and (3) asking respondents before they give their hypothetical bid to consider their budget constraints (Loomis, Gonzalez-Caban, & Gregory, 1994; Neill et al., 1994). Although sometimes successful, each of these approaches has problems as well. Calibration is quite commodity-specific (Fox et al., 1998); the certainty scale does not provide an obvious cutoff point, reducing its accuracy; and the budgetary reminder has been largely ineffective.

This paper tests an approach similar to the budgetary reminder, in that it attempts to get respondents to avoid over-estimations, but it employs a much more direct attack on hypothetical bias. It instructs people, when answering a contingent willingness to pay question, to respond as if they were *really* spending their money. At least three studies, described next, have used this approach. The latter two show that the approach can achieve considerable success.

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[2] Not all comparisons have found an over-estimation. For example, in an open-ended mail survey requesting donations to a land preservation fund, MacMillan et al. (1999) found no over-estimation when they adopted a campaign-style format, similar to typical solicitations for donations, for the contingent and real cash instruments. See also Dickie, Fisher, and Gerking (1987)

Past Attempts

Studies of attempts to talk people out of their hypothetical bias have compared hypothetical and real payment treatments with a hypothetical treatment employing a script instructing respondents to answer as if they were really spending their money. In the first study, Loomis et al. (1996) obtained sealed bids for an art print. In the "hypothetical with reminder" treatment, participants were encouraged to think about what they would "honestly" be prepared to pay if the auction were for real. In the two-paragraph entreaty, they were reminded of their budget constraint but also told, among other things, "Although the question is hypothetical, we want you to answer as if it were real—as if you were participating in a real sealed-bid auction and would really have to pay your dollar amount if you were the highest bidder" (p. 453). Mean bids from separate samples were $42 in the hypothetical treatment, $26 in the hypothetical-with-reminder treatment, and $14 in the actual payment treatment. The reminder helped reduce hypothetical bias, but was insufficient to remove it.[3]

In the second study, Cummings and Taylor (1999) used a much more thorough script, which they called "cheap talk," in their effort to remove bias from a hypothetical referendum on contributing $10 to fund a public good. In the eight-paragraph script, the authors not only encouraged participants to act as if the referendum were for real, but also explicitly addressed the problem of hypothetical bias—of how people in hypothetical bidding situations tend to over-estimate their willingness to pay—and discussed possible reasons for the phenomenon. Their approach met with notable success. For two of their three goods—the donation to the Nature Conservancy for Costa Rican rain forest protection mentioned above and a separate Nature Conservancy donation to protect land in the state of Georgia—the percentages of participants voting to donate $10 were nearly identical in the real payment and hypothetical-with-cheap-talk treatments, and these percentages were significantly below those of the standard hypothetical treatment. And for the other good—the Albuquerque citizens' guide—the percent voting yes when the cheap talk script was used was within 5 percentage points of its real referendum counterpart. This study is important not only for its demonstration of a promising approach for

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[3] Cummings and Taylor (1999) cite two additional working papers that unsuccessfully used similarly short scripts to remove hypothetical bias.

adjusting for hypothetical bias, but also because it used a referendum format, which is arguably the most appropriate contingent valuation format for valuing public goods (Hoehn & Randall, 1987).

Finally, List (2001) used the Cummings and Taylor cheap talk script in a second-price auction to purchase a baseball card. Respondents from two groups of card collectors (professional card dealers and nondealers) were interviewed individually at a sports card show and given an opportunity to bid (either hypothetically or for real) on a card with a book value of about $200. For nondealers, the real payment and hypothetical-with-cheap-talk treatments each produced a mean bid of $26, compared with a mean bid of $49 in the hypothetical treatment. However, the professional dealers were largely unaffected by the cheap talk script, bidding a mean of $116 in the hypothetical treatment and $108 in the cheap talk treatment, compared with a mean of $60 in the actual auction. As in the Cummings and Taylor study, the cheap talk script worked remarkably well when bidders were unfamiliar with bidding for the good at issue. The lack of success with professional dealers raises the question: Are there other circumstances in which the script does not correct effectively for hypothetical bias?

The study reported in this discussion paper further tested the Cummings and Taylor script. Our bidding format, subject pool, recruitment enticement, and good were similar to theirs, in that students were paid $10 and participated in actual or hypothetical referenda on donating to a public good. We extended their work in two ways. First, whereas they used only one donation amount, $10, we varied the amount across referenda to investigate the effect of the amount on the success of the cheap talk script. The practical reason for such a test is that, if the script is to work in dichotomous choice contingent valuation, it must work at a range of bid levels. The more fundamental objective was to shed some light on when and how the script corrects for hypothetical bias. A side benefit of this study is that we tested for hypothetical bias in referenda at several payment amounts, the first such test of which we are aware. Second, in a further attempt to understand the workings of the script, we tested the effect of anticipating that the vote will be made public on hypothetical bias and on the success of the script.

We accept as given that when placed in a real payment situation, each participant will forms an estimate of willingness to pay (or casts a vote reflecting that willingness to

pay). The question is, in a hypothetical situation, will the cheap talk script lead the participant to the response that would be forthcoming in a real payment situation, or will it create a separate response that only equals the real payment response under certain circumstances? We suspected that the latter possibility was more likely, and specifically that the script—which as seen below is a very direct and lengthy attack on hypothetical bias—would perform best when hypothetical bias was greatest. Our hunch was that a script that worked well when the bias was substantial would be ineffective or over-correct when the bias was smaller.

Methods

In groups, students at the University of Massachusetts participated in a session of three or more stages, for which they were each paid $10 in cash at the outset. The referendum stage(s) of the sessions were preceded by an unrelated task and followed by a related but quite different task, a survey intended to measure tendency to respond in a socially desirable manner.

Each of 491 subjects participated in one of 16 different experimental versions. Each version included one or two referenda. The initial 12 versions included one secret ballot referendum; these 12 different referenda were created by crossing three levels of "reality" of payment (hypothetical, hypothetical with cheap talk script, and real payment) with four payment levels ($1, $3, $5, and $8). The next three versions were identical to the initial three $8 payment referenda except that participants expected that they would have to discuss their votes with the group. A final version included two secret ballot referenda at the $8 payment level, a hypothetical referendum followed by a real payment referendum.

The hypothetical with cheap talk (hereafter just cheap talk) versions of the referenda differed from the comparable hypothetical versions only in that the former included an extensive entreaty to respond as if the vote were for real. Sample sizes for the 12 initial versions are listed in Table 1, and those of the open referenda are listed in Table 5. There were 34 participants in the dual referenda version, who are included among the 52 participants listed in Table 1 for the $8 hypothetical referendum.

Table 1. Sample size of initial 12 referenda			
Payment amount			
$1	$3	$5	$8
Hypothetical	31	37	33	52
Cheap talk	33	43	31	52
Real	28	37	30	17
Total	92	117	94	121

The referendum stage(s) of a session began with a description of and justification for the proposition to be voted on. It was described as an effort to make additional scholarship funds available to deserving students wishing to attend the university (Figure A1, Appendix A). The proposition ($8 version) was then stated as follows:

> "Everyone here in this room will contribute $8.00 to the *University of Massachusetts – Amherst Scholarship Fund*. The contribution is to be used to provide scholarship aid to needy and deserving students at the University of Massachusetts – Amherst."

The potential outcomes of the voting procedure were then explained, to the effect that if more than 50% of the participants in the room voted yes, all participants would donate the stated amount, and otherwise no one would pay. Figure A2 (Appendix A) shows the wording of the explanation in the hypothetical versions. The real and hypothetical versions differed only in the tense used to describe the outcomes. For example, the first sentence of the real version read "If more than 50% of you vote YES on this proposition, all of you pay $8.00 – I will collect $8.00 from each of you…"; whereas the first sentence of the hypothetical version read "If more than 50% of you were to vote YES on this proposition, all of you would pay $8.00 – I would collect $8.00 from each of you…".

At this point, in the cheap talk versions a lengthy discussion of hypothetical bias was presented (Figure A3, Appendix A). The script we used is identical to that used by Cummings and Taylor except for differences related to changes in the good. In this script, participants are told of the problem of hypothetical referenda—that when money payments are at issue people tend to vote differently in a hypothetical referendum than they do in a real referendum. Results of past studies are presented showing a higher percentage voting yes in a hypothetical referendum than in a real referendum. The reasons for this difference are then discussed, including the notion that in a referendum to do something good for

others, people tend to focus on doing good and tend to ignore the cost to them when it is hypothetical, but they temper that inclination with considerations of other options for their money when the referendum is for real. The cheap talk script ended with the following request: "I ask you to vote just exactly as you would vote if you were really going to face the consequences of your vote: which is to pay money if the proposition passes."

The vote was recorded on individual printed ballots that were placed in a ballot box. Before the vote, participants were reminded that their $10 participation fee was their money, which they earned by spending their time helping a research project (the cheap talk version of this reminder is included near the end of Figure A3). In addition, respondents in the open versions were told:

> "Most referenda are conducted as part of general elections where people tend to discuss their votes with friends, coworkers, and acquaintances. Therefore, to make our referendum comparable to other referenda, you will discuss your vote with the other people in this room. After you have cast your votes and handed in your ballots, we will ask each of you to state how you voted and explain *why* you voted the way you did."

Participants in the secret versions received no such instruction.

Results

As will be explained in more detail below, percentages voting yes varied widely among the referenda, from a low of 21 percent to a high of 82 percent.

Table 2. Percent voting yes in initial 12 referenda				
	Payment amount			
	$1	$3	$5	$8
Hypothetical	71	73	70	69
Cheap talk	64	67	39	25
Real	82	43	33	24

Effect of payment amount. Results of voting in the initial 12 referenda are presented in Figure 1. Voting in the four hypothetical referendum was insensitive to payment amount, with 69% to 73% of participants voting yes (Table 2).[4] At the 95%

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[4] To test whether a yes vote of about 70% was an invariable result for public goods with our subject population, we also ran a hypothetical referendum (with 19 subjects) on donating $8 to a different scholarship fund, one usable at universities throughout the U.S., and observed a 47% yes vote. This extra

confidence level we cannot reject the hypothesis of no difference in voting among the four
referenda (Table 3).[5] However, in the real referenda, percent voting yes decreased
monotonically as payment amount increased, from 82% at the $1 level to 24% at the $8

level (percent voting yes is
significantly greater at the $1
payment level than at the
other three payment levels,
Table 3). Clearly participants
were sensitive to cost when
spending real money. In the
cheap talk referenda, percent
voting yes decreased with
payment amount across the
$3, $5, and $8 amounts (with

Figure 1. Referenda on Contribution to Scholarship
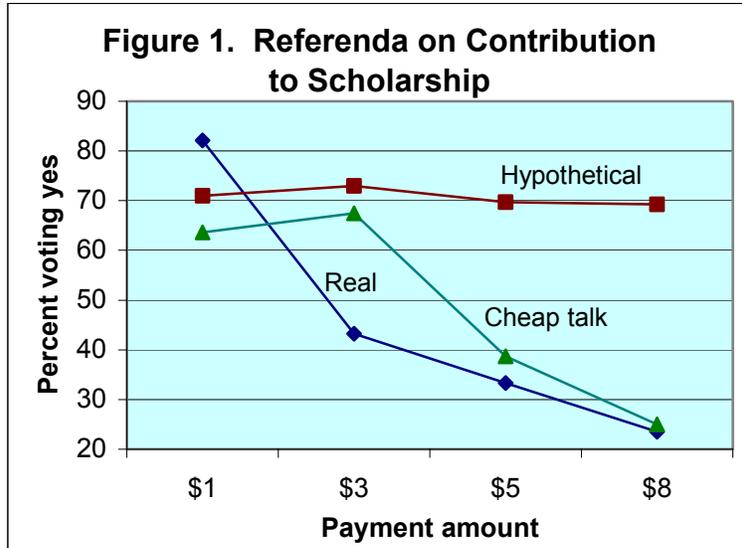
the $3 percentage yes being significantly greater than the $5 and $8 percentages yes, Table
3), but the $1 payment percentage yes was slightly below the $3 payment percentage yes.

Examining the
tendency toward
hypothetical bias,
percentages voting yes in
the hypothetical referenda
were significantly greater
than in the real referenda
at the $3, $5, and $8

Table 3. Tests of equal proportions voting yes by payment amount (Fisher's exact p-value, two-sided test)

Null hypothesis	Hypothetical	Cheap talk	Real
$1 = $3	1.000	0.809	0.002
$1 = $5	1.000	0.079	0.000
$1 = $8	1.000	0.001	0.000
$3 = $5	0.796	0.019	0.458
$3 = $8	0.814	0.000	0.229
$5 = $8	1.000	0.221	0.529

levels (Table 4). At the $1 level, the percentage voting yes was actually greater—though
not significantly so—in the real referendum than in the hypothetical referendum, a result
that may reflect the lack of salience of a $1 payment.

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referendum suggests sensitivity to the nature of the good in the hypothetical condition.
[5] Two-sided tests of hypotheses are reported herein using Fisher's exact test. One-sided tests, of course,
yield lower probability levels, but reach the same conclusions for our data at the 0.05 significance level,
except for the comparison of $1 and $5 in the cheap talk treatment, for which the probability level drops from
0.079 with a two-sided test to 0.040 with a one-sided test.

Table 4. Tests of equal proportions voting yes by treatment (Fisher's exact p-value, two-sided test)

Null hypothesis	Payment amount			
	$1	$3	$5	$8
Real = hypothetical	0.370	0.018	0.006	0.001
Real = cheap talk	0.154	0.042	0.791	1.000
Hypothetical = cheap talk	0.601	0.632	0.023	0.000

The cheap talk script had the desired effect of eliminating the hypothetical bias at the higher payment levels, dropping percent voting yes to within one percentage point of what it was in the real referendum at the $8 level, and to within 6 percentage points at the $5 level. At both of these payment amounts, the hypothetical percentages yes are significantly greater than the corresponding cheap talk percentages (Table 4). But at the $3 payment level the cheap talk script had a very modest effect, producing a percentage yes only 6 points below the hypothetical percentage yes, and significantly greater than the real percentage yes (Table 4). And at the $1 level, no correction was needed but a correction nevertheless occurred, with the cheap talk percentage yes being 18 percentage points below that of the real referendum, though the difference is not statistically significant.

Effect of openness of the vote at the $8 payment level. As seen in Figure 2, knowing that one had to reveal and explain one's vote to others produced a lower percent voting yes in the hypothetical referendum (63%, Table 5) than in the comparable secret ballot referendum (69%, Table 2), and a higher percent yes in the real referendum (43%) than in the comparable secret ballot referendum (24%), narrowing the difference in percent yes between the hypothetical and real payment versions. Neither of these changes in percent voting yes are significant (p = 0.606 for the

Table 5. Effect of anticipating open discussion of one's vote, $8 payment amount

	Sample size	Percent voting yes
Hypothetical	24	63
Cheap talk	22	45
Real	21	43

Figure 2. Referenda on Contribution to Scholarship: **$8 payment**
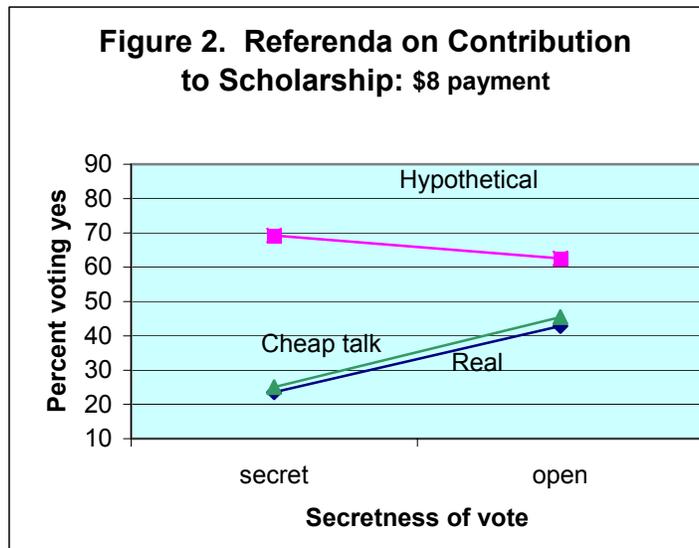
hypothetical referenda and p = 0.307 for the real referenda, Fisher's exact two-sided tests), but the changes do suggest the possibility that in the hypothetical referendum the prospect of explaining one's vote leads some people to more carefully consider their true willingness to pay, and that in the real referendum some people value appearing to others as generous.

More importantly for our interest here, the cheap talk script again worked well, dropping hypothetical percent yes to within two percentage points of what it was in the real referendum. Although neither the real nor the cheap talk percentage yes is significantly lower than the hypothetical percentage (p = 0.238 and p = 0.375, respectively, Fisher's exact two-sided tests), this result for open referenda at least does not detract from the earlier finding of effectiveness of the script at the $8 payment level.

Effect of a prior hypothetical referendum on a real referendum, secret ballot at the $8 payment level. Having established that, at the $8 payment level, the cheap talk script yields voting in a hypothetical referendum that mimics that of a real referendum, we wondered whether having voted yes in an ordinary (without cheap talk) hypothetical referendum would raise percent yes in a subsequent real referendum to what it was in the prior hypothetical referendum. That is, we wondered whether voting in a real referendum was sensitive to prior hypothetical responses. To address this we held a version that included both referenda, allowing a within-subject comparison.

The hypothetical referendum results are very similar to those of the comparable solo hypothetical referendum, with 68% and 72%, respectively, voting yes (the combined percent yes is 69%, Table 2). However, in the real payment referendum that followed the hypothetical referendum 44% voted yes, which is substantially above the 24% voting yes in the solo real payment referendum, though the difference is not significant (p = 0.222, Fisher's exact two-sided test).[6] Neither is the 44% voting yes in the real referendum significantly different from the 68% in the prior hypothetical referendum (p = 0.087, Fisher's exact two-sided test). Thus, while we have a suggestion that a hypothetical referendum can increase the likelihood of a yes vote in a subsequent real referendum, the evidence is weak.

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[6] At the individual subject level, 9 of 23 subjects (39%) switched from a yes vote in the hypothetical referendum to a no vote in the subsequent real referendum.

Discussion

This study joins a number of prior studies demonstrating hypothetical bias in elicitations of willingness to pay by comparing responses from hypothetical and real payment conditions. Two of these studies (Cummings et al., 1997; Cummings & Taylor, 1999) used referenda about public goods, but employed only a $10 payment amount. We extend prior work by testing for bias in several secret ballot referenda differing by payment amount. Among the four payment amounts ($1, $3, $5 and $8), the percent voting in favor in the hypothetical referenda remained nearly constant, whereas when the referendum was for real the percent in favor steadily dropped as the amount was raised, causing hypothetical bias to increase with payment amount.

The sensitivity of the vote to payment amount in the real referenda is expected,[7] but the insensitivity to payment amount in the hypothetical referenda is not, as previous studies of hypothetical responses have found that proportion yes drops as bid level increases. Such sensitivity has been found for a private good at bid levels ($1 and $5) similar to those used here (Blumenschein, Johannesson, Blomquist, Liljas, & O'Connor, 1998). And dichotomous choice contingent valuation studies of public goods regularly show a significant negative relation between stated willingness to pay and bid level (for contingent referenda examples, see Carson et al., 1998; Loomis et al., 1994). However, unlike our study, most contingent valuation studies use a wide range of payment amounts. Payment amount in our study was limited by the $10 participation fee. Thus, the lack of sensitivity to payment amount that we found cannot be generalized to higher payment amounts or to bids for private goods.

Why was the hypothetical vote invariant to payment amount, thereby causing the substantial hypothetical bias we observed? Four possibilities come to mind, the first three of which are suggested in the cheap talk script. First, some participants in hypothetical referenda may try to estimate their real willingness to pay but may over-estimate. Second, some may ignore their budget constraint, thereby responding as if they faced none. Third, some may use their vote to indicate whether or not they think the cause is worthy of

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[7] In addition to the obvious effect of rising payment amount on willingness to vote yes—that being the personal opportunity cost of making the payment to fund the scholarship—voters may also be sensitive to payment amount because the do not want to force others to pay. Such other-focused preferences would probably not arise in the hypothetical referenda.

support. Fourth, some may use their vote to indicate whether or not they think such a referendum should be held for real. In essence, participants acting according to the latter three of these possibilities are answering different questions from the one they were asked in the hypothetical referendum.

The Cummings and Taylor cheap talk script worked remarkably well at the two larger payment amounts, lowering the percent voting in favor to within 2 ($8) and 6 ($5) percentage points of what it was in the real referenda. This result is similar to those obtained by Cummings and Taylor in their public good referenda, which used a $10 payment with a similar subject pool. In addition to these secret referenda, we also held referenda at the $8 payment level when participants knew they had to reveal how they had voted (the open condition). Hypothetical bias was lower in this condition than in the $8 secret condition, but still substantial, and the cheap talk script once again lowered the percent voting in favor to within 2 percentage points of what it was in the real referendum, further enhancing confidence in the robustness of the script at the higher payment level.

However, the script significantly under-corrected at the $3 payment level, lowering percentage voting in favor only 6 percentage points below that of the hypothetical condition. One possible explanation for the script's ineffectiveness at the $3 payment level is that some participants may have thought it implausible that they would vote against the referendum at such a low payment. That is, they may not have envisioned significant bias at that level.

Finally, at the $1 level the script caused a 7-point drop in percentage yes, nearly the same correction as occurred at the $3 payment level. However, in this case a correction was not needed, as percentage yes was higher in the real than in the hypothetical referendum.[8] In any case, the percentages yes of the three $1 referenda were not significantly different from each other.

In general, when used with public good referenda, the cheap talk script appears to increase in effectiveness as payment level, and correspondingly hypothetical bias, increase—though the evidence of this is limited to the relatively modest payment levels used here and by Cummings and Taylor. If this trend holds at higher bid levels, it bodes

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[8] Cummings and Taylor (1999) tested for over-correction in a referendum where the percentage voting yes was also slightly but not significantly higher in the real referendum than in the hypothetical referendum, and found no over-correction with the cheap talk script, but their test was at the $10 payment level.

well for contingent valuation, because hypothetical bias in dichotomous choice contingent valuation of public goods tends to remain substantial at higher payment levels (see Brown et al., 1996 for supporting data).

Turning now to the dual referenda version, our result of no significant effect of the prior hypothetical referendum on voting in the subsequent real payment referendum is in concert with what several other studies found for private goods. Hypothetical willingness to pay responses had no aggregate effect on subsequent real cash responses in dichotomous choice bidding for common consumer items (Cummings et al., 1995), in open-ended bidding in an auction for an art print (Loomis et al., 1996); and at one of two payment levels in dichotomous choice bids for a consumer good (Blumenschein et al., 1998). Our result is tentative because the difference in voting, though not significant, was substantial. A larger sample size might produce a significant difference. But based on the available evidence, we are unable to conclude that votes in a referendum are any different from dichotomous choice responses about private goods regarding the effect of hypothetical bidding on subsequent real cash bidding. If the finding holds, it offers an opportunity for exploring the reasons for hypothetical bias with individual respondents (by, or example, asking respondents to explain the difference).

Understanding how payment amount or other factors alter the effectiveness of the cheap talk script will require a better understanding of its workings at the individual level, for people may differ in both the correction required and the effect of the script. An understanding at the individual level could show to what extent the cheap talk script causes all respondents to correct by an equal amount regardless of their level of bias (thus causing over-correction in some people and under-correction in others), versus causing each individual to remove the bias whatever its magnitude. Some insight into this issue is offered by further examination of the data collected by Loomis et al. (1996) in a study, mentioned above, that used a short version of cheap talk, called "reminder," with a first-price auction for an art print. That examination, presented in Appendix B, shows that respondents largely fell into two groups, those who removed their bias and those who remained unaffected by the script. Over-correction was almost non-existent. We cannot know how the effect of the cheap talk script on individual bias differs from the effect of the milder reminder used in the art print study. The differences in script and in response mode

(referendum versus first price auction) confound conclusive comparisons. But the art print data at least does not dampen hopes that the cheap talk script works as intended, by individually removing bias without causing over-correction.

The Cummings and Taylor script is long, and would thus be difficult to use on the phone or in the mail. A much shorter version of the entreaty mentioned above (used by Loomis et al., 1996), which did not specifically educate respondents about hypothetical bias, helped reduce hypothetical bias but under-corrected on average. Although we lack a definitive test of the effect of the difference between the Loomis et al. and the Cummings and Taylor scripts, it appears that the entreaty has a greater impact if it not only instructs participants to act as if the response were for real, but also explains that responses to a hypothetical question are often very different from those to a real payment question. Only further research will determine whether the script can be shortened without loss of impact.

If further tests continue to show that the cheap talk script helps remove bias at all but the trivially small (e.g., $1) payment amounts, and works extremely well at higher payment amounts, it would be unduly prudent to delay its application to dichotomous choice contingent valuation—especially if additional research shows that the script can be shortened in length without loss of effect.

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Appendix A. Experimental Material

Figure A1. Description of Referendum ($8 payment version)
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        The University of Massachusetts at Amherst receives applications from a broad range of prospective students. Many of these applicants come from poor families who cannot afford the tuition, fees, room and board, and other expenses of attending the University. A 1998 survey conducted for the State Board of Higher Education found that as many as 23% of all high school graduates fail to go on to college, or drop out of college, for financial reasons. The State of Massachusetts is heavily dependent on an educated workforce for its high-tech industries and cannot afford to lose this potential source of qualified workers.

        As a State University, UMass–Amherst is committed to providing accessible higher education to every qualified resident. Not only does this help to prepare a well trained work force, it is also an essential tool for social mobility and equity. To achieve these goals, UMass–Amherst tries to provide full or partial scholarships to as many needy and deserving students as possible. Although the University receives some State funds for scholarships, the amount available is woefully inadequate. Consequently, UMass–Amherst maintains a general scholarship fund and is actively soliciting donations to increase the number of scholarships it can offer.

        If everyone in this room were to contribute $8.00, these moneys would be added to the UMass–Amherst Scholarship Fund and would be used to help deserving students attend the University.

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Figure A2. Description of Voting Procedure Outcomes (hypothetical $8 payment version)
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1. If more than 50% of you were to vote YES on this proposition, all of you would pay $8.00 – I would collect $8.00 from each of you – and we would give this money to the University of Massachusetts – Amherst with instructions that the money is to be credited to the University Scholarship Fund.

   We wouldn't send cash. We would take your cash, write a check for the total amount of money, and the check would be mailed to the University Development Office which manages all donations. I would put the check in this stamped envelope addressed to the Development Office. <HOLD UP ENVELOPE> I would ask one of you to put the envelope in the mail box on the third floor. When I received a receipt for the money from the Development Office, I would make it available for your inspection on the bulletin board near Room 623 in Tobin Hall.

2. If 50% or fewer of you were to vote YES on this proposition, no one would pay $8.00, and we would not send a check to the UMass Scholarship Fund.

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Figure A3. Cheap Talk Script ($8 payment version)

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Before we have our vote, I want to talk to you about a problem that we have in studies like this one. As I told you a minute ago, this is a hypothetical referendum – not a real one. No one will actually pay money at the end of the vote. But I also asked you to respond to the vote as though the result of your vote could involve a real cash payment by you.

And that's the problem. In most studies of this kind, people seem to have a hard time doing this. They vote differently in a hypothetical referendum, where they don't really have to pay money, than they do in a real referendum, where they really could have to pay money. For example, in a recent study, several different groups of people voted on a referendum just like the one you are about to vote on. Payment was hypothetical for these groups, as it will be for you. No one had to pay money if the referendum passed. <SHOW OVERHEAD OF SURVEY RESULTS> The results of these studies were that on average, across the groups, 45% of them voted YES. With another set of groups with similar people voting on the same referendum as you will vote on here, but where payment was real and people really did have to pay money if the referendum passed, the results on average, across the groups were that 27% voted YES. That's quite a difference, isn't it?

We call this a "hypothetical bias". "Hypothetical bias" is the difference that we continually see in the way people respond to hypothetical referenda as compared to real referenda. In the real referendum, where people knew they would have to pay money if the referendum passed, 27% voted yes and 73% voted no. When payment was hypothetical and people knew they would not pay anything if the referendum passed, just like your vote today, 45% voted yes and 55% voted no.

How can we get people to think about their vote in a hypothetical referendum like they think in a real referendum, where if enough people vote YES, they'll really have to pay money? How do we get them to think about what it means to really dig into their pocket and pay money, if in fact they really aren't going to have to do it?

Let me tell you why I think that we continually see this hypothetical bias, why people behave differently in a hypothetical referendum than they do when the referendum is real. I think that when we hear about a referendum that involves doing something that is basically good – helping people, improving environmental quality, or anything else – our basic reaction in a hypothetical situation is to think: sure, I would do this. I really would vote YES to spend the money – I really, really, think I would. What our YES vote really means in this case is that we are basically good people, and that we would like to see good things happen.

But when the referendum is real, and we would actually have to spend our money if it passes, we think a different way. We basically still would like to see good things happen, but when we are faced with the possibility of having to spend money, we think about our options: if I spend money on this, that's money I don't have to spend on other things. If I spend money to help needy students, that's money I don't have to spend on groceries, go to a movie, or perhaps give to some environmental organization. So when the payment is real if the referendum passes, we vote in a way that takes into account the limited amount of money we have. We vote realizing that we just don't have enough money to do everything we might like to do. This is just an opinion, of course, but it's what may be going on in hypothetical referenda.

In any case, the only way that we know to get people like you to vote in our hypothetical referendum just like you'd vote if the referendum was real is to simply ask you: in the vote that we're going to take in a few minutes, please do the following:

● Think about what you're voting on. If this were real—if more than 50% of you voted YES, you would actually have to dig into your pocket and pay $8.00 right now—do you really want to support the UMass-Amherst Scholarship Fund enough that you would be willing to spend the money?

● Also, let me make clear that the $10.00 participation fee that you were paid today is your money. You've spent your time helping us in our research, and you've earned it! You were told that the money is yours, believe it! So, if I were in your shoes, and I was asked to vote YES or NO on this proposition that requires all of us to pay $8.00, I would think about how I feel about spending my money this way. When I got ready to vote, I would ask myself: if this were a real referendum, and I had to pay $8.00 if the referendum passed, do I really want to spend my money this way. If I really did, I would vote YES; if I didn't, I would vote NO – I wouldn't throw my money around. That's just my opinion, of course. You must do whatever you want to do.

● In any case, I ask you to vote just exactly as you would vote if you were really going to face the consequences of your vote: which is to pay money if the proposition passes.

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Appendix B. Further Evidence on Effect of Entreaties to Avoid Hypothetical Bias

In the art print study (Loomis et al., 1996), the hypothetical auctions, one with a "reminder" and another without, were followed by real payment auctions. The reminder encouraged participants to bid as if the auction were for real. Participants in these two hypothetical auctions did not know that a real payment auction would follow. A third group participated only in a real payment auction. Sample sizes of the three groups varied from 32 to 35 participants. Mean bids of the three real payment auctions varied from $12 to $14, suggesting that the prior hypothetical auctions held with two of the groups did not affect bidding in the subsequent real payment auctions. As previously mentioned, the mean hypothetical bids were $42 without the reminder and $26 with the reminder. Of particular interest here is the distribution of hypothetical bias (i.e., the difference between hypothetical and real payment bid) across participants in the two groups that bid in both kinds of auctions (Figure B1).

Figure B1 shows three findings of interest. First, bias was less common with the reminder. Eighty percent of participants in the group without the reminder exhibited a bias, whereas only 52% in the group with the reminder had a positive bias. Second, over-correction with the reminder was minimal. Only two participants (numbers 1 and 2 in the bottom graph of Figure B1) bid more in the real payment auction than in the hypothetical auction, and the differences were within $2. Third, among participants with a positive bias, the average magnitude of the bias did not differ much between treatments. Ignoring a $400 bid in the without-reminder treatment (which does not show in the upper right corner of the top graph), the mean positive bias was $26 without the reminder and $25 with the reminder (the medians are $20 and $24, respectively).

Further evidence is offered by an additional treatment in the Loomis et al. (1996) study, one identical to the hypothetical with-reminder treatment of Figure B1 except that a dichotomous choice response preceded the two open-ended responses of interest here (see Loomis, Brown, Lucero, & Peterson, 1997). In this treatment of 54 participants, mean open-ended willingness to pay was $23 in the hypothetical auction and $12 in the real auction, only one with-reminder participant over-corrected (by $1), 44 percent of the participants had a positive bias, and positive bias averaged $24. These results are very close to those of the with-reminder treatment shown in Figure B1.

Figure B1. Open-ended Bids for an Art Print, with Participants Sorted in Order of Increasing Bias
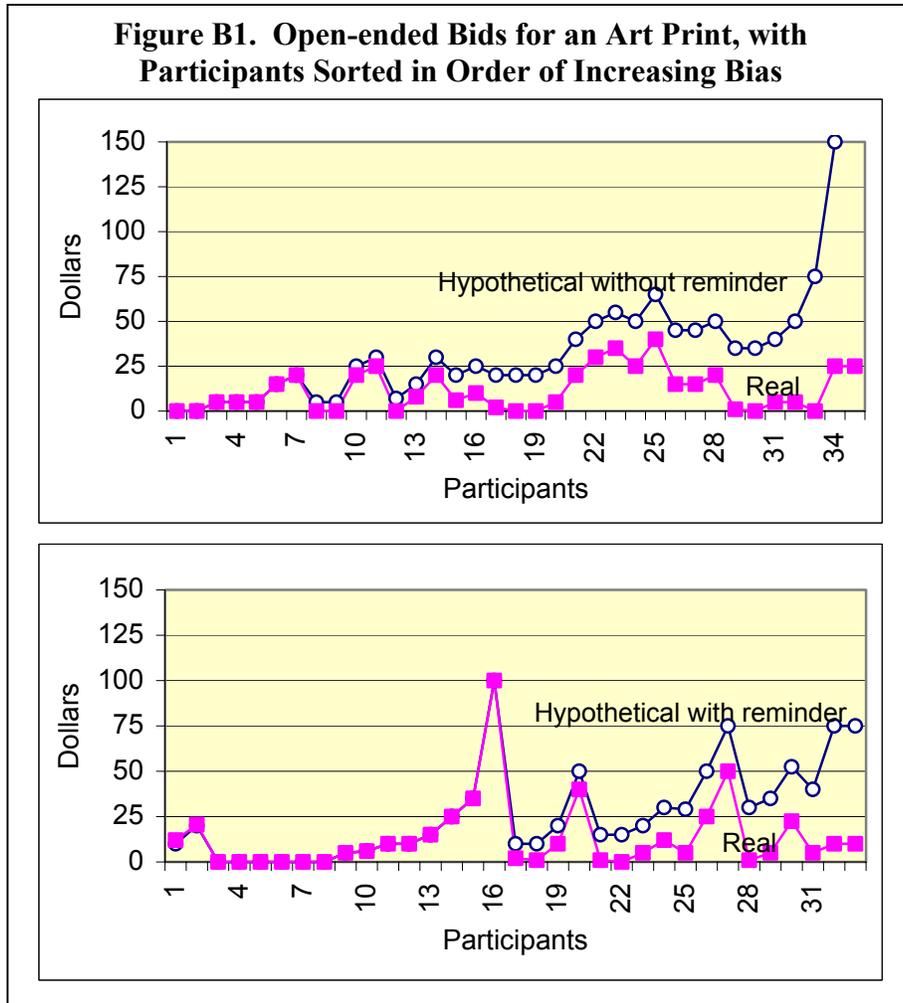
Thus, the principal effect of the reminder appears to have been to decrease the number of participants with a bias. With the exception of the possible effect on extreme hypothetical bids (which involves only one participant in the without-reminder treatment), the reminder left the average positive bias unchanged, suggesting that the size of participants' hypothetical bias played no role in their susceptibility to the reminder.