Detecting Starting Point Bias in Dichotomous-Choice Contingent Valuation Surveys

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Summary

We examine starting point bias in CV surveys with dichotomous choice payment questions and follow-ups, and double-bounded models of the WTP responses. We wish to investigate (1) the seriousness of the biases for the location and scale parameters of WTP in the presence of starting point bias; (2) whether or not these biases depend on the distribution of WTP and on the bids used; and (3) how well a commonly used diagnostic for starting point bias—a test of the null that bid set dummies entered in the right-hand side of the WTP model are jointly equal to zero—performs under various circumstances. Because starting point bias cannot be separately identified in any reliable manner from biases caused by model specification, we use simulation approaches to address this issue. Our Monte Carlo simulations suggest that the effect of ignoring starting point bias is complex and depends on the true distribution of WTP. Bid set dummies tend to soak up misspecifications in the distribution assumed by the researcher for the latent WTP, rather than capturing the presence of starting point bias. Their power in detecting starting point bias is low.

Keywords: Anchoring, Dichotomous choice contingent valuation, Starting point bias, Double-bounded models, Estimation bias

JEL Classification: Q51

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I. Introduction and Motivation

Many recent high-quality contingent valuation surveys elicit information about Willingness to Pay (WTP) by asking dichotomous-choice (DC) questions.[4] Respondents are asked whether or not they would buy the good if its cost was $X, or whether they would vote in favor or against the proposed public program in a referendum on a ballot if implementing it cost $X to the household, usually in the form of higher taxes. In this way, the respondent's exact WTP amount is not directly observed, and all we do know is whether it is greater than the bid amount ("yes") or less than the bid amount ("no").

To refine information about WTP, it is possible to ask a dichotomous choice follow-up question (Hanemann et al., 1991). Specifically, respondents who answer "yes" ("no") to the initial payment question are asked whether they would be willing to pay if the cost was $Y, where Y>X (Y<X). The responses to the initial and follow-up questions are combined to form narrower intervals around the respondent's WTP, improving the efficiency of the estimates of WTP (Hanemann et al., 1991). Implicit in this approach—commonly dubbed "double-bounded" (DB)—is the assumption that an individual's responses to the initial and follow-up dichotomous-choice payment question are driven by *the same* WTP amount, which remains unobserved. WTP amounts are drawn from a distribution over the population and vary across individuals.

Although many contingent valuation (CV) practitioners continue to implement surveys with dichotomous choice questions and follow-ups, and to fit double-bounded

[4] Contingent valuation is a frequently used approach for placing a value on goods that are not traded in markets. Prominent examples of these goods include improvements in environmental quality, other public goods, ecosystem health, and risks to human health. In a contingent valuation study, individuals are asked to report information about their willingness to pay to obtain (or to avoid the loss of) the good to be valued. The good is specified in a hypothetical scenario, and no actual transaction takes place.

models, over the last decade researchers have examined this approach's potential for undesirable response effects (see section 2).

In this paper, we focus on one such effect, namely starting point bias. It is possible that when follow-up questions are used, respondents may "anchor" the value they place on the policy on the bid amounts proposed to them in the initial and/or subsequent payment questions. The latter problem is usually termed "starting-point bias" and a possible mechanism for it within a dichotomous-choice format is proposed by Herriges and Shogren (1996).[5] Specifically, Herriges and Shogren formulate a model where the WTP amount driving the response to the follow-up payment question is a weighted average of the first latent WTP and the initial bids. Variants on Herriges and Shogren include Aprahamian et al. (2004), who treat the anchoring parameter as a random coefficient drawn from a specified distribution, and Lechner *et al.* (2003), who assume that even the first WTP amount of the respondent is influenced by the initial bid. Most recently, the Herriges and Shogren mechanism has been combined with yea-saying by Chien et al. (2005), who represent the latter using an additional error term that follows the half-normal distribution and is folded with the regular econometric error into a compound distribution.[6]

In empirical work, it is common to test for the presence of starting point bias by (i) including in the right-hand side of the double-bounded model dummy variables for the bid set assigned to the respondent, and then (ii) testing the null hypothesis that the

[5] Starting point bias was suspected to affect responses to iterative bidding CV payment questions, which were first introduced by Randall et al. (1974). Boyle et al. (1985), for example, include the initial bid amount in the right-hand side of their WTP equation, where the dependent variable is the final WTP amount announced by the respondent. A significant coefficient on the initial bid variable is interpreted as evidence of starting point bias.
[6] See Johnson et al. (1994) for details about the half-normal distribution.

coefficients on these dummies are jointly equal to zero (Whittington et al., 1990; Green and Tunstall, 1991; Cameron and Quiggin, 1994, and most recently Chien et al., 2005).

In this paper, we examine four related issues pertaining to starting point bias. First, how serious are the biases of the location and scale parameters of WTP if starting point bias is present but ignored in the statistical model of the WTP responses? Second, what is the performance (measured in terms of nominal size and power) of the above mentioned diagnostic of starting point bias, namely the test on the coefficients on the bid set dummies? Third, how are the bias of the estimates and the performance of the diagnostic test affected when the distribution of WTP is misspecified? Fourth, how important is the bid design in all of the above?

To elaborate on the third question, we suspect that in some cases what has been interpreted by the researcher as evidence of anchoring to the initial bids is simply an artifact due to misspecification of the econometric model and/or the poor choice of distribution of WTP. In the case of the diagnostic test based on the use of bid set dummies, we suspect that the coefficients on these dummies may act as available free parameters, and absorb the effects of misspecifications of the econometric model or of the distribution of WTP, even though no starting point bias is present.

Because starting point bias cannot be separately identified in any reliable manner from biases caused by model specification, we use simulation approaches to address this issue. Hence, we conduct a series of Monte Carlo simulations to answer these questions. We generate the latent WTP amounts from various distributions using two alternative starting point bias mechanisms, and model the responses using double-bounded models, which ignore starting point bias.

Our simulations suggest that the effect of ignoring starting point bias is complex, and depends on the true distribution of WTP and on the WTP statistic being estimated (mean WTP or the variance of WTP). We find that bid set dummies, which are used by many researchers to detect starting point bias, have only very modest power in detecting starting point bias. We find that the coefficients on these dummies tend to soak up misspecifications in the distribution assumed by the researcher for the latent WTP, so that the diagnostic test rejects the null too frequently, falsely pointing to starting point bias when the real problem is a poor distributional assumption.

The remainder of the paper is organized as follows. Section II discusses undesirable response effects that are possible when follow-up questions are used. In section III we present the starting point bias mechanism developed by Herriges and Shogren (1996) and a plausible variant on this model. In section IV, we present a commonly used test for the presence of starting point bias. We present the simulation study design in section V, and its results in section VI. Section VII concludes.

II. Possible Response Biases In Double-Bounded Models.

Cameron and Quiggin (1994) relax the assumption that the response to the initial and follow-up payments are driven by the same amount. They estimate alternative models that assume distinct, but correlated, WTP amounts for each DC payment question. To detect the presence of starting point bias they include dummy variables for the initial bids, concluding that constraining the distributional parameters to be identical and the correlation to be unity exacerbates starting point effects.

Alberini et al. (1997) apply a random effects model to DB contingent valuation data allowing for differing mean WTP across the initial and follow-up questions because respondents may become "confused about how much they will have to pay or what they will actually get" (p. 311) as the survey proceeds. They reason that follow-up questions may induce respondents to effectively substitute the program or policy described in the scenario with another program or policy package that has different characteristics, and to form a new, systematically different WTP value that reflects the attributes of the new program. Using data from the San Joaquin Valley wetlands study (Hanemann et al., 1991), the study on the Kakadu Conservation Zone in Australia (Carson et al., 1994) and the Alaska survey to estimate the loss of passive-use values for Prince William sound resulting from the Exxon-Valdez oil spill of 1989 (Carson et al., 1992), they find that follow-up questions resulted in a systematic downward shift in median WTP in the Alaska study, while in the other studies the structural shift is negative but not statistically significant at the conventional levels.

DeShazo (2002) considers alternative mental models—such as prospect theory (Kahneman and Tversky, 1979, and Tversky and Kahneman, 1991), which implies forming a reference point—and starting point bias, and predicts the probability of "yes"/"no" responses to the follow-up questions implied by these models. For example, prospect theory implies that the probability of a respondent answering "yes" to a follow-up question from an ascending sequence is less than the probability of a respondent answering "yes" to the same value presented in an initial valuation question. By contrast, if anchoring occurs, respondents who are assigned the ascending sequence will anchor on the lower value, while respondents who are assigned the descending sequence will anchor on the higher value. (By ascending sequence, we mean a follow-up dollar amount that is greater than the initial bid because the respondent answered "yes" to the first payment question. The term "descending sequence" refers to the opposite situation.)

Carson et al. (2000) examine response effects that result in violations of the assumption that the responses to all payment questions are driven by the same WTP amount. Respondents, Carson et al. argue, may (i) take the second price as the expected price but consider the cost of the program to be somewhat uncertain, (ii) take a weighted average between the two prices, (iii) adjust the quantity of the good to match the change in price,[7] or (iv) enter in a bargaining mode. Burton et al. (2003) use experiments to empirically discriminate between hypotheses (i) and (ii).

III. Models of Starting Point Bias

Dichotomous-choice contingent valuation assumes that the "yes" or "no" responses to the payment questions are determined by comparing the respondent's WTP amount with the bids assigned to him. In DC CV surveys with a DC follow-up question, the responses to the payment questions are used to construct an interval around each respondent's unobserved WTP amount. Assuming, for example, that respondent i's WTP is normally distributed with mean $\mathbf{x}_i\boldsymbol{\beta}$ and variance σ^2, this respondent's contribution to the likelihood function is:

(1) $$\Phi\left(\frac{WTP_i^H - \mathbf{x}_i\boldsymbol{\beta}}{\sigma}\right) - \Phi\left(\frac{WTP_i^L - \mathbf{x}_i\boldsymbol{\beta}}{\sigma}\right),$$

[7] Evidence from focus groups suggests that people that answer "yes" to the initial payment question expect the government to be capable of providing the public program at the cost stated to them in the initial question. Higher cost amounts, therefore, are sometimes interpreted to imply government waste. Likewise, people who initially answered "no" may suspect that in the follow-up question the public program being valued is a scaled down version of the initially described program.

where WTP_i^L and WTP_i^U are the lower and upper bound, respectively, of the interval around the respondent's unobserved true WTP amount.

The log likelihood function is

(2) $$\log L = \sum_i \ln\left[\Phi\left(\frac{WTP_i^H - \mathbf{x}_i\boldsymbol{\beta}}{\sigma}\right) - \Phi\left(\frac{WTP_i^L - \mathbf{x}_i\boldsymbol{\beta}}{\sigma}\right)\right]$$

and the parameters are estimated by the method of maximum likelihood.[8]

If starting point bias is present, the bid amounts influence the response to a payment question in two ways: (i) by affecting directly WTP, and (ii) through the comparison between WTP (which is already affected by the bid) and the bid.

Herriges and Shogren (1996) propose the following mechanism for starting point bias. Assume that when first faced with a dichotomous-choice question, an individual compares the initial bid, B_1, with his WTP amount, WTP_{1i}. The latter is a draw from the population distribution of WTP, and the answer to the payment question is "yes" ("in favor") if WTP_{1i} exceeds B_1, and "no" ("against") otherwise.

Now suppose that the individual is asked a dichotomous-choice follow-up question where he is queried about B_2. Herriges and Shogren argue that the initial bid may provide a "focal point or anchor for the uncertain respondent."[9] This may happen when the uncertain respondent interprets the bid amount as an approximation of the good's true value, thus anchoring his or her WTP on the proposed bid to update priors in light of society's or experts' beliefs. They further propose that the response to the second payment question is driven by a different amount, WTP_{2i}, which is a weighted average of WTP_{1i} and the initial bid, B_1. Formally,

[8] This log likelihood function is easily amended to accommodate for other distributions. See, for example, Alberini, et al. (forthcoming).

[9] Accordingly, in this paper the terms "starting point bias" and "anchoring" are used interchangeably.

(3) $$WTP_{1i} = \mu + \varepsilon_i ,$$

where μ is mean WTP and ε is (normally distributed) error term with variance σ^2, and

(4) $$WTP_{2i} = WTP_{1i}(1-\gamma) + \gamma \cdot B_1 ,$$

where $0 \leq \gamma \leq 1$ is the weight placed on the initial bid. (Clearly, this notation assumes that mean WTP is the same for all respondents. This common mean replaces the individual-specific expectation $\mathbf{x}_i\boldsymbol{\beta}$ used in equations (1) and (2).)

If γ=0, there is no anchoring, and $WTP_{2i} = WTP_{1i}$, as is routinely assumed in double-bounded models. If γ=1, no memory of the original WTP amount is retained in the follow-up question, and WTP_{2i} is equal to the second bid amount.

Conventional double-bounded models of WTP assume that the responses to both the initial and the follow-up payment questions are driven by the same underlying WTP amount, and are thus misspecified in this situation. Herriges and Shogren show that the mechanism described by equations (1) and (2) effectively widens the boundaries placed on WTP by the follow-up question. The greater the weight γ, the wider these boundaries, and the less information about the original WTP is contained in response to the follow-up payment question. In addition, with this anchoring mechanism the WTP amount driving the response to the follow-up payment question has, by construction, a smaller variance than the original WTP, WTP_1.

If one fits a conventional double-bounded model in this situation, are the estimated coefficients biased, and, if so, how severely? Herriges and Shogren conduct simulations, showing that in the presence of starting point bias the estimates of mean WTP, μ, are unbiased, but σ, the standard deviation of WTP, is systematically underestimated.[10] They point out that "The starting point bias squeezes the distribution tightly around the mean, but does not bias the estimated mean WTP" (Herriges and Shogren, 1996, p. 121).

Their first claim follows from the fact that multiplying WTP_1 by $(1-\gamma)$ shrinks the variance, a reduction that cannot be offset by the addition of B_1. (If WTP follows the normal or any other distribution defined between $-\infty$, or 0 and ∞, the bids will usually cover a much smaller range.) Their second claim rests on the fact that in their study (i) the distribution of WTP is symmetric, and (ii) the average of the bid amounts is about equal to mean WTP. Their anchoring mechanism implies that individuals simply compute a weighted average of WTP_1 and B_1, so if the average initial bid is roughly equal to mean WTP_1, mean WTP_2 is roughly equal to mean WTP_1, and so is the weighted average of these two means, which the double-bounded estimator tends to.

Based on these considerations, we would expect conventional double-bounded models to produce biased estimates of WTP if the average of the initial bids is different from mean WTP. We would also expect them to underestimate the variance of WTP, since they will tend to capture an average of the variances of WTP_1 and WTP_2, and the latter is less than the former. Because the variance of WTP enters in the computation of the standard errors around the estimate of mean WTP, this has potentially important implications for statistical inference about WTP and its use in policy contexts.

In this paper, we generate data following the Herriges and Shogren mechanism, but we estimate double-bounded models (which ignore the presence of anchoring), and examine the consequences of doing so on the estimates of mean WTP and variance of

[10] By contrast, in the presence of omitted starting point bias the one-way up and the one-way down approaches produce biased estimates of both mean WTP and the standard deviation of WTP.

WTP. Our work differs from earlier studies in that (i) when using the Herriges-Shogren approach, we consider WTP distributions other than the normal, (ii) we examine the effects of using different bid sets, and (iii) we check the size and power of a commonly used diagnostic test for anchoring.

In addition, (iv), we study (ii) and (iii) after introducing an amendment to the Herriges and Shogren that, in our opinion, reflects a realistic response effect induced by the follow-up payment question. We reason that while respondents might treat the initial bid as providing information about the value of the policy—as suggested by Herriges and Shogren—the follow-up question may end up confusing them. In practice, this is one possible representation for the uncertainty about the cost of the program effect discussed in Carson et al. (2000). We therefore amend equation (4) to obtain

(5) $$WTP_{2i} = WTP_{1i}(1-\gamma) + \gamma \cdot B_1 + e_i \, ,$$

where the error term e captures the possible uncertainty/confusion associated with the follow-up question.

IV. Detecting Starting Point Bias.

Whittington et al. (1991), Green and Tunstall (1991), Cameron and Quiggin (1994) and Chien et al. (2005) include bid set dummies among the regressors of the double-bounded model to capture starting point effects.[11] This approach is an extension to dichotomous-data model of an approach previously used with WTP responses on a continuous scale elicited through open-ended questions (Boyle et al.,1985).

[11] By bid set dummies, we mean a set of dummies where the first takes on a value of one if the respondent was assigned to the first bid set used in the survey and 0 otherwise, etc.

Letting δ be the vector of coefficients on the bid set dummies, one tests the null hypothesis that $\delta=0$ (no anchoring) against the alternative that at least one of the elements in δ is different from 0. Rejection of the null is interpreted as evidence of anchoring. Because the parameters of the model are estimated using the method of maximum likelihood, any one of the three classical tests—the Wald, likelihood ratio, or score test—can be used. Under relatively mild regularity assumptions, under the null the three statistics are each distributed as a chi square with $m=\dim(\delta)$ degrees of freedom, and are thus asymptotically equivalent.

In this paper, we use the Wald statistic, which is calculated as

$$w = \hat{\boldsymbol{\delta}}'\mathbf{V}^{-1}\hat{\boldsymbol{\delta}} \ , \tag{6}$$

where $\hat{\boldsymbol{\delta}}$ is the vector of coefficients on the bid set dummies estimated from the augmented double-bounded model, and **V** is the block of the information matrix for all parameters corresponding to the coefficients on the bid set dummies. **V** is, therefore, an $m\times m$ matrix. As mentioned, for large sample size and under the null, the test statistic w is distributed as a chi square with m degrees of freedom. Failure to reject the null implies that there is no evidence of anchoring on the bid amounts.

V. Study Design

To answer our research questions, we conducted a series of Monte Carlo simulations. We ran a total of four sets of simulations. Each simulation set is comprised of 15 experiments (5 values of $\gamma \times 3$ bid designs).[12] In each experiment, the number of

[12] In simulation set II, we have a total of 30 experiments, because we also change the variance of one of the error terms in the model. See table 1.

replications is 1000 and in each replication the sample size is 1000. Our study design is summarized in table 1.

Table 1. Summary of the simulation experiment design

(A) Simulation set	(B) True WTP distribution	(C) Parameters of true WTP distribution	(D) Anchoring mechanism	(E) Bid sets
I	Normal	μ=10 σ=10	Herriges and Shogren with γ=0 (no anchoring), 0.3, 0.5, 0.7, 0.9	--base --upper tail --lower tail
II	Normal	μ=10 σ_1=10 σ_2=3 or 20	Anchoring + error term with γ=0 (no anchoring), 0.3, 0.5, 0.7, 0.9	--base --upper tail --lower tail
III	Weibull	Scale parameter σ=10 Shape parameter θ=1	Herriges and Shogren with γ=0 (no anchoring), 0.3, 0.5, 0.7, 0.9	--base --upper tail --lower tail
IV	Lognormal	μ=1.956012 (mean of log WTP) σ=0.693147 (standard deviation of log WTP)	Herriges and Shogren with γ=0 (no anchoring), 0.3, 0.5, 0.7, 0.9	--base --upper tail --lower tail

We generate draws from the assumed distribution, shown in column (B) of table 1. Each draw is assigned at random to one of the possible bid sets (reported in table 3), and binary indicators corresponding to “yes” or “no” responses to the payment questions are created by comparing the draw with its assigned bid value and appropriate follow-up bid amount.

All simulations fit normal likelihood function, but we assume different distributions (normal, Weibull, and lognormal) for true WTP in different simulation sets. Simulation set I, III and IV adopt the Herriges-Shogren anchoring mechanism (equations (1) and (4)). By contrast, in simulation set II we use our amendment to the Herriges-Shogren model (equation (5)), but assume that true distribution is normal, so that we can compare the results of these runs with those of simulation set I. In simulation set II, we assume that ε and *e* are uncorrelated; however, it is easily shown that WTP_1 and WTP_2 are correlated, since they both contain ε. In sum, WTP_1 and WTP_2 are jointly normally distributed.

Simulation set II is repeated under two alternative values for σ_2, where $\sigma_2^2 = Var(e)$, namely 3 and 20, where the latter signifies a situation where respondent confusion is more pronounced.

To make all simulation sets comparable as we vary the distribution of WTP, we choose the parameters of the distribution of WTP so that its expected value (mean WTP) is 10 and its variance 100.[13]

We use a total of three bid sets. Each is comprised of 5 initial bid amounts and their corresponding high and low follow-up bids. As before, it is important that the bid

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[13] If Y denotes a Weibull random variate, its cdf is $1-\exp\left[-(y/\sigma)^{\theta}\right]$, its mean is $\sigma \cdot \Gamma(1/\theta+1)$, and its median is $\sigma[-\ln 0.5]^{1/\theta}$. If Y is a lognormal, the density is $\frac{1}{\sigma y\sqrt{2\pi}}\exp\left\{-\frac{1}{2}\left(\frac{\ln y-\mu}{\sigma}\right)^2\right\}$, mean is $\exp(0.5\sigma^2+\mu)$ and median is exp(μ).

amounts be comparable across different WTP distributions, so we choose our bid sets to correspond to specified percentiles of the distribution of WTP, as shown in table 2. (This means that the actual bid amounts differ across simulation sets to mirror the different distributions we assume for WTP. We remind the reader that the percentile is 1 minus the probability of answering "yes" to that bid amount.)

Table 2. Percentiles corresponding to the bid amounts in the simulations.

Design	*Percentile*				
Base design	0.184	0.310	0.500	0.580	0.692
Upper tail design	0.184	0.310	0.500	0.692	0.933
Lower tail design	0.184	0.242	0.274	0.310	0.382

Table 3. Initial bid amounts.

Distribution	**Bid Design**	*1st initial bid*	*2nd initial bid*	*3rd initial bid*	*4th initial bid*	*5th initial bid*
Normal	*Base*	1	5	10	12	15
	Upper tail	1	5	10	15	25
	Lower tail	1	3	4	5	7
Weibull	*Base*	2.034	3.689	6.931	8.657	11.759
	Upper tail	2.034	3.689	6.931	11.759	27.059
	Lower tail	2.034	2.770	3.206	3.689	4.814
Lognormal	*Base*	3.342	4.663	7.071	8.352	10.722
	Upper tail	3.342	4.663	7.071	10.723	24.652
	Lower tail	3.342	3.948	4.291	4.663	5.508

The artificial draws from the WTP distribution are evenly divided among the five possible bid sets. In the base bid set, the initial bid values cover the 18th-69th percentile. The bid set labeled "upper tail" covers the 18th to 93th percentiles, while the bid set labeled "lower tail" is skewed towards the lower tail of the distribution of WTP and fails to cover the right tail of the distribution of WTP. When the distribution of WTP is a normal, the average of the initial bids for the base, upper tail, and lower tail designs is 8.6, 11.2 and 4, respectively.

Earlier research (Alberini, 1995; Kanninen, 1991, and Cooper, 1993) shows that when the distribution of WTP is symmetric, an unbalanced bid design (i.e., one that places more bids and/or respondents on side of the distribution, or farther away from the mean) tends to result in inefficient, but unbiased, estimates of mean WTP.[14] However, with right-skewed distributions of WTP the estimate of mean WTP depends crucially on "nailing down" the upper tail of distribution, a task that can be accomplished only by querying respondents about their willingness to pay relatively large bid amounts. At such large bid amounts, a large fraction of the respondents are expected to answer "no" to the payment question.[15] These considerations suggest that with right-skewed distributions we would expect the "upper tail" design to perform best, and the "lower tail" design to result in less efficient, and potentially unstable, estimates of mean WTP. The follow-up amounts are double or half of the initial amount.

We use a total 5 values for γ, the anchoring parameter: 0, which means that there is no anchoring, then 0.3, 0.5, 0.7, and 0.9, which imply levels of anchoring ranging from mild to severe. For each artificial data generation, we fit two double-bounded interval-data likelihood functions, both of which assume that WTP is a normal variate. The first is the regular double-bounded model (with no individual characteristics), which is used to establish the seriousness of the biases (if any) of the estimates of mean and variance

[14] Efficiency goals with respect to estimating mean WTP are sometimes in conflict with doing a good job estimating the variance of WTP: a compromise can be reached when choosing the bid amounts, for example, by adopting the d-optimality design criterion (Kanninen, 1991).

[15] This is again a situation where statistical estimation needs may be in conflict with a realistic scenario. If the bid amount is perceived to be unrealistically large for the good described in the questionnaire, the respondent may question the credibility of the exercise and provide unreliable responses.

WTP. In the second double-bounded model, the likelihood function is amended to include dummies for the bid set.[16] Since there are a total of five bid sets, we include four bid set dummies, and we compute the Wald statistics for the null that the coefficients on the bid dummies are all equal to zero.

VI. Results

We use two criteria to examine the performance of double-bounded models in the presence of starting point bias. The first is the relative bias of mean WTP, and the second is the relative bias of the standard deviation of WTP, σ(WTP). (The relative bias is the bias divided by the true value of the WTP statistic.) Regarding the diagnostic test, i.e., the Wald test of the null that the coefficients of the bid dummies are jointly equal to zero, we examine the percentage of times that the test rejects the null hypothesis for a given significance level. Clearly, if $\gamma=0$, this percentage is the empirical size of the test, i.e., the frequency with which the null is falsely rejected. If γ is different from zero, this percentage is the empirical power of the test. We expect the power of the test to increase with γ. We do not have any prior expectation of the empirical size of the test when there is no starting point bias and the true WTP distribution is not normal (but the likelihood function assumes that it is).

A. Bias of the welfare estimates.

Figure 1 displays the relative bias of mean WTP for the three bid designs and the four simulation sets.

[16] For example, in simulation set I, when the base bid design is used, the bid set dummies are A1=1 if the initial bid is 1, and 0 otherwise; A2=1 if the initial bid assigned to this observation is 5, and 0 otherwise, etc.

Panel (a) refers to the situation where true WTP is normal and one fits the double-bounded model that assumes a normal distribution (and ignores the presence of starting point bias). When there is no starting point bias (i.e., γ=0), this is the correct model, and the estimates of mean WTP are virtually unbiased. The relative bias—which is computed as the average mean WTP over the replications minus the true mean WTP, and then divided by the true mean WTP—is only -0.20 to -0.18%. With the base bid design, the bias of mean WTP does not change much, even when anchoring is more pronounced (-1.50% for γ=0.3 to -7% for γ=0.9).

The upper tail design does not fare as well, but the biases resulting from this design never exceed 15% of the true mean WTP. It is interesting that—against our expectations—the bias is non-monotonic in γ. The lower tail design is the worst of the three. Even a moderate degree of anchoring produces a bias of -16%, and extreme anchoring (γ=0.9) results in an underestimate of mean WTP by at least 50%.

Panel (b) display the results when we use our amendment to the Herriges-Shogren model when the variance of the error term in the follow-up question is small. Clearly, the results are very similar to those of panel (a) because the variance of the additional error term is too small to offset the variance shrinkage due to the anchoring on the first bid. As shown in panel (c), the biases are of similar magnitude (but slightly smaller) when the variance of the additional error term is larger.

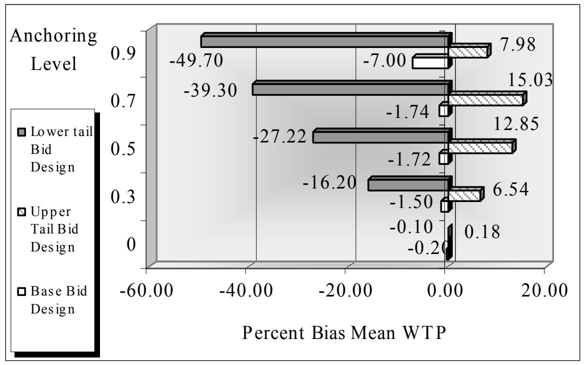
Panel (d) shows that assuming the wrong distribution results in biased estimates of mean WTP. What's interesting is that the bias of mean WTP varies with the bid design used, but for a given bid design does *not* vary with the severity of the anchoring. This is a somewhat surprising result. As we expected, the design that fares the best is the upper tail design, which underestimates mean WTP by about 16%. This design barely outperforms the base design, which on average underestimates mean WTP by 19%. The worst is the lower tail design, which underestimates mean WTP by about 30%. Panel (e) shows similar effects of fitting a normal double-bounded model to lognormal WTP data in the presence of varying degrees of anchoring.

Figure 2 present similar summary statistics of the simulations for the standard deviation of WTP, σ(WTP). Panel (a) shows that the double-bounded model underestimates true σ(WTP), an effect that becomes more pronounced as anchoring becomes stronger. As before, the best behaved design is the base design. The one that results in the most severe biases is the lower tail design, which underestimates true σ(WTP) by up to 76% for γ=0.9. Panel (b) shows similar biases when only a small error term is added to the anchoring mechanism. As shown in panel (c), the biases are reduced somewhat when the variance of the error term in equation (5) is larger, thus partially offsetting the shrinkage of WTP due to the anchoring.

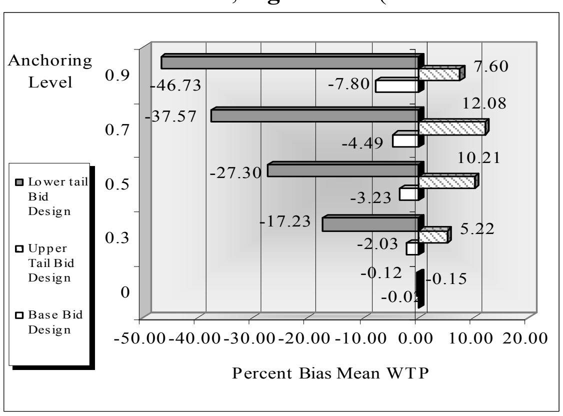
Panels (d) and (e) confirm that when the wrong distribution is used, and anchoring is present but ignored, the estimates of σ(WTP) are biased. As before, the biases depend on the bid design, but for a given bid design they do *not* depend on the severity of the anchoring. The biases can be very pronounced: in our examples, the true σ(WTP) may be underestimated by over 50%.

Figure 1 – Percent Bias Mean WTP - Anchoring Present but Ignored

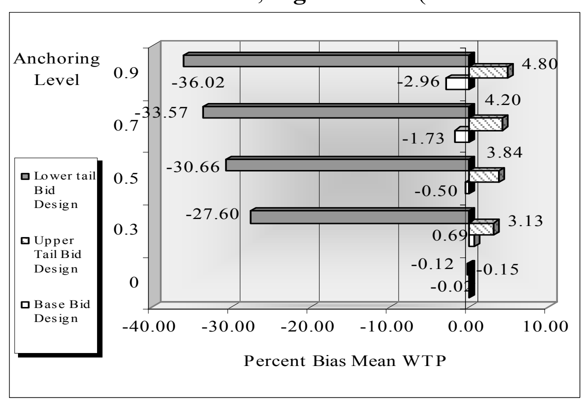
a) True WTP: Normal Distribution; Likelihood Function: Normal (Simulation Set I)



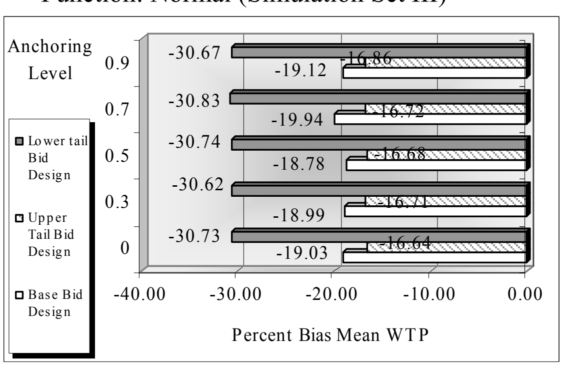
b) True WTP: Normal Distribution; Likelihood Function: Normal, **Sigma 2 =3** (Simulation Set II)



c) True WTP: Normal Distribution; Likelihood Function: Normal, **Sigma 2 =20** (Simulation Set II)



d) True WTP: Weibull Distribution; Likelihood Function: Normal (Simulation Set III)



e) True WTP: LogNormal Distribution; Likelihood Function: Normal (Simulation Set IV)

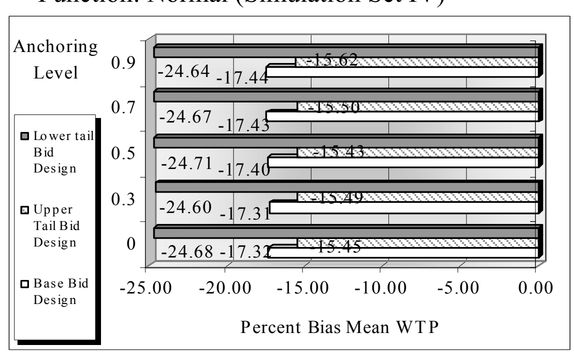
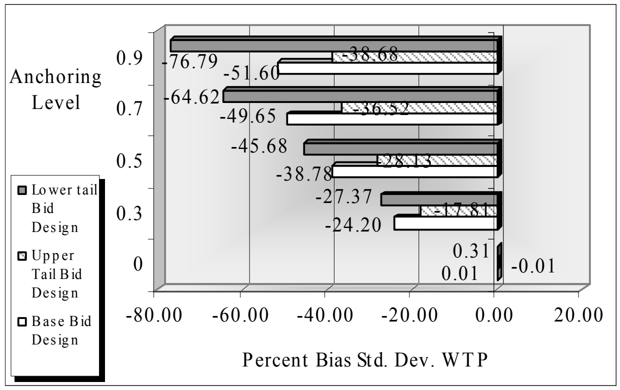
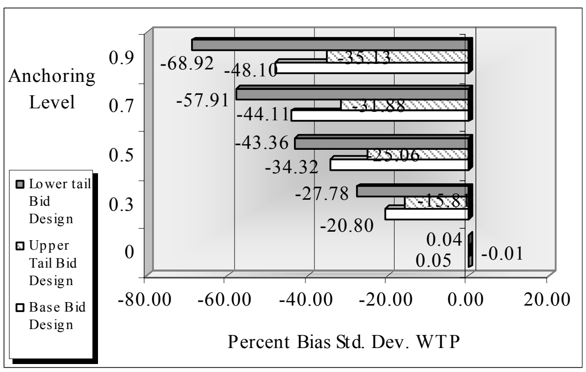


Figure 2 – Percent Bias Std. Dev. WTP - Anchoring Present but Ignored

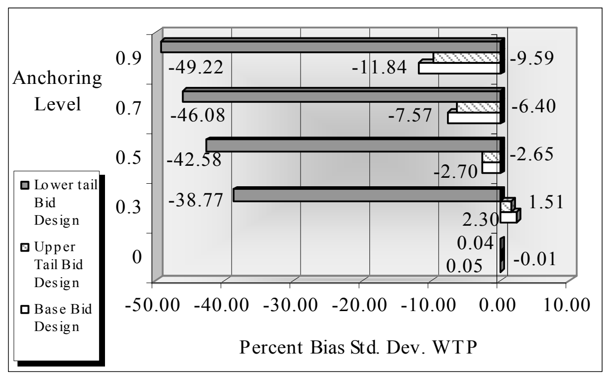
a) True WTP: Normal Distribution; Likelihood Function: Normal (Simulation Set I)



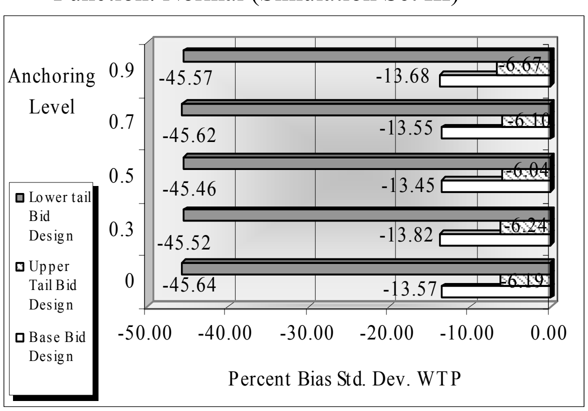
b) True WTP: Normal Distribution; Likelihood Function: Normal, **Sigma 2 =3** (Simulation Set II)



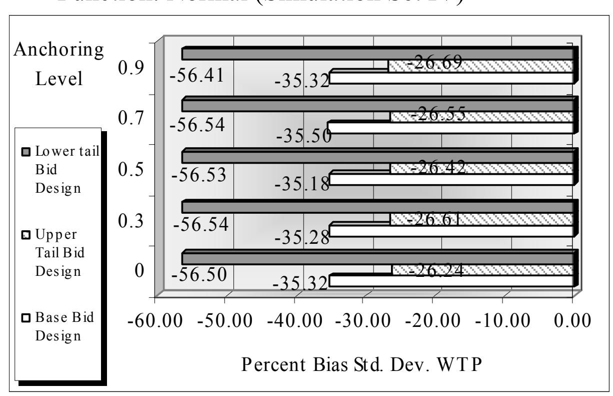
c) True WTP: Normal Distribution; Likelihood Function: Normal, **Sigma 2 =20** (Simulation Set II)



d) True WTP: Weibull Distribution; Likelihood Function: Normal (Simulation Set III)



e) True WTP: LogNormal Distribution; Likelihood Function: Normal (Simulation Set IV)



B. Diagnostic test

Table 4 summarizes the relative frequencies of rejection of the null hypothesis that the bid set dummies are jointly equal to zero for all experiments and simulations sets. The table was constructed assuming that the significance level (or nominal size of the test) is α=0.05.

Table 4 shows clearly that in simulation set I, where the correct distribution (the normal) is assumed for WTP, and no anchoring is present (γ=0), the percentage of rejections of the null is similar to the nominal size of the test, although it slightly exceeds it if the upper tail bid design is used. We had expected the relative frequency of rejections to increase with γ, but this expectation is not borne out in the results: rejections occur in 5-6 percent of the replications, regardless of the value of γ, and do not appear to depend in any predictable way on the bid design. We believe that this is due to the fact that the estimate of μ adjusts accordingly. We did not detect any particular patterns in the estimated coefficients on the bid dummies.

The results are similar when we introduce an error term to capture respondent confusion, as we do in simulation set II. Changing the variance of this term does not change much the percentage of rejections.

In simulation set III, the true distribution is a Weibull, but we fit a normal double-bounded model and ignore anchoring. If anchoring is absent (γ=0), the relative frequency of the rejections does vary with the bid design used, and ranges from 11 to 26%. This means that the diagnostic test must be picking up the effect of a poor distributional assumption. We note three interesting findings at this point. First, the most frequent rejections occur with the bid design that tracks the upper tail of the distribution. Second, the percentage of rejections are insensitive to the value of γ, the anchoring parameter, in the sense that they do not exhibit a clear trend as γ increases. Third, the power of the test when γ is greater than zero is rather modest, as it never exceeds 24%.

Results for the lognormal distribution (simulation set IV) are qualitatively similar to those for the Weibull. When γ =0, the empirical size of the Wald test slightly exceeds the nominal size of the test for all designs, especially the upper tail and lower tail designs. In these cases, the empirical frequency of rejection of the null is 7-15 percent against a nominal size of 5 percent. Little change is seen when γ increases for a given bid design. We conclude that in this simulation set the Wald test exhibited limited power in picking up either anchoring or the poor distributional assumption.

Table 4. Empirical Size and Power of the Test of Starting Point Bias.

DGP	Anchoring Present?	DB Log Likelihood	Percent Rejection of Null Wald Test (Base Bid Set)	Percent Rejection of Null Wald Test (Upper Tail Bid Set)	Percent Rejection of Null Wald Test (Lower Tail Bid Set)
Normal (simulation set I)	**No**	Normal	5.50	4.70	7.51
	Yes, □ = 0.3		**6.00**	**5.80**	**5.53**
	Yes, □ = 0.5		**5.90**	**6.70**	**5.68**
	Yes, □ = 0.7		**3.40**	**6.90**	**4.47**
	Yes, □ = 0.9		**4.70**	**6.40**	**5.82**
Normal (σ_2 = 3) (simulation set II)	**No**	Normal	5.11	7.26	6.06
	Yes, □ = 0.3		**6.30**	**7.66**	**3.31**
	Yes, □ = 0.5		**4.02**	**2.47**	**6.20**
	Yes, □ = 0.7		**7.80**	**3.69**	**5.56**
	Yes, □ = 0.9		**6.69**	**6.45**	**4.66**
Normal (σ_2 = 20) (simulation set II)	**No**	Normal	5.11	7.26	6.06
	Yes, □ = 0.3		**5.70**	**6.27**	**5.74**
	Yes, □ = 0.5		**5.20**	**5.27**	**6.38**
	Yes, □ = 0.7		**5.50**	**5.90**	**4.21**
	Yes, □ = 0.9		**5.80**	**4.97**	**6.29**
Weibull (simulation set III)	**No**	Normal	**10.88**	**26.04**	**13.65**
	Yes, □ = 0.3		13.29	21.80	12.78
	Yes, □ = 0.5		12.98	23.01	13.01
	Yes, □ = 0.7		12.18	23.57	14.13
	Yes, □ = 0.9		13.31	21.92	12.77
Lognormal (simulation set IV)	**No**	Normal	**7.06**	**12.84**	**14.44**
	Yes, □ = 0.3		7.43	12.23	13.02
	Yes, □ = 0.5		5.30	11.58	14.45
	Yes, □ = 0.7		8.17	10.97	16.30
	Yes, □ = 0.9		5.78	15.37	15.82

VII. Conclusions

In this paper, we have focused on starting point bias (anchoring) in the dichotomous choice contingent valuation surveys with a dichotomous choice follow-up question. We have considered a mechanism that generates anchoring first developed by

Herriges and Shogren and frequently adopted in the literature, and have examined the effect of ignoring starting point bias and fitting double-bounded models.

Our results suggest that normally distributed double-bounded models *may* produce biased estimates of mean WTP and the standard deviation of WTP when anchoring is present, that these biases are more severe the stronger the anchoring is, and that the severity of the biases varies with the bid design used. A well-balanced, symmetric bid design may result in very modest biases even when the anchoring mechanism is very strong.

When the true WTP is not a normal variate, but a normal double-bounded model is estimated, the biases do *not* vary with the severity of the anchoring, and seem to depend primarily on the misspecification of the distribution. As before, the biases do depend on the bid design.

We also investigated the empirical size and power of a commonly used test for detecting the presence of starting point bias. This test consists of including bid set dummies in the right-hand side of the double-bounded model, and of testing the null that all bid set coefficients are equal to zero. We used a Wald test to test this hypothesis, but the other two classical tests (the likelihood ratio and score test) can be used interchangeably, since they are asymptotically equivalent to the Wald test.

We found that when the true distribution of WTP is a normal and the econometric model of the responses to the payment questions is a normal double-bounded, the test has very little power against the alternative even when the anchoring parameter is very high. When the true distribution of WTP is a Weibull or a lognormal, but one fits a normal double-bounded model, depending on the bid design used, one may tend to reject the null hypothesis of no anchoring too frequently when anchoring is not present. The power of the Wald test is modest, and does not change much with the anchoring parameter γ.

Based on our findings, we caution researchers that the consequences of starting point biases are complex and depend on the underlying distribution of WTP. We also caution them that simple to implement diagnostic tests, such as the inclusion of bid set dummies in the right-hand side of double-bounded models of WTP, may be misleading. We have found that tests of the null that the coefficients on these dummies are equal to zero may fail to reject the null when they should, or may tend to reject it even if no starting point bias is present, simply because the researcher did not use the correct distribution of WTP or the correct random utility model (RUM) in writing out the double-bounded models.

Unfortunately, it is difficult to come up with alternative approaches for detecting and correcting for anchoring unless the correct distribution of WTP or the correct RUM model are assumed, and one is prepared to make specific assumptions about the form of the anchoring. Semi-parametric, semi-nonparametric, and nonparametric models (reviewed in Cooper, 2002), which alleviate the need for making assumptions regarding the distribution and/or the functional form of the RUM, cannot separately identify response biases from other forms of bias.

In principle, one can compare the relative frequency of "yes" or "no" responses to the same bid amount in groups of respondents that were assigned different bid sets. If the probability of a yes to $X as a starting bid is statistically the same as a probability of a yes to $X in the follow-ups (after converting the follow-up probability from a conditional to an unconditional probability), then the null hypothesis that there is no response bias cannot be rejected. However, even if bias is present, this approach cannot identify its form nor know which bound is associated with the most severe bias in the responses: all we can surmise when using such an approach is that the responses to the bid values are not consistent across the bounds.

In sum, unless one is prepared to make assumptions about the form of the bias, it cannot be corrected for. As we have suggested, without additional information beyond the responses to the bids themselves, econometric approaches to identifying and correcting for response bias do not appear to be fruitful. An alternative may be to use follow-up questions specifically pertaining to the respondent's views on being asked follow-up questions. Another approach that we deem worth investigating is to openly tell respondents in advance that there will be multiple bids to respond to, and that multiple bid response questions will be asked simply to get a more precise assessment of willingness to pay. We believe that this is a potentially promising area for future research.

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(lxv) This paper was presented at the EuroConference on “Auctions and Market Design: Theory, Evidence and Applications” organised by Fondazione Eni Enrico Mattei and sponsored by the EU, Milan, September 25-27, 2003

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(lxvii) This paper has been presented at the international conference on “Tourism and Sustainable Economic Development – Macro and Micro Economic Issues” jointly organised by CRENoS (Università di Cagliari e Sassari, Italy) and Fondazione Eni Enrico Mattei, and supported by the World Bank, Sardinia, September 19-20, 2003

(lxviii) This paper was presented at the ENGIME Workshop on “Governance and Policies in Multicultural Cities”, Rome, June 5-6, 2003

(lxix) This paper was presented at the Fourth EEP Plenary Workshop and EEP Conference “The Future of Climate Policy”, Cagliari, Italy, 27-28 March 2003

(lxx) This paper was presented at the 9th Coalition Theory Workshop on "Collective Decisions and Institutional Design" organised by the Universitat Autònoma de Barcelona and held in Barcelona, Spain, January 30-31, 2004

(lxxi) This paper was presented at the EuroConference on “Auctions and Market Design: Theory, Evidence and Applications”, organised by Fondazione Eni Enrico Mattei and Consip and sponsored by the EU, Rome, September 23-25, 2004

(lxxii) This paper was presented at the 10th Coalition Theory Network Workshop held in Paris, France on 28-29 January 2005 and organised by EUREQua.

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(lxxiv) This paper was presented at the ENGIME Workshop on “Trust and social capital in multicultural cities” Athens, January 19-20, 2004

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(lxxvii) This paper was presented at the Workshop on Infectious Diseases: Ecological and Economic Approaches held in Trieste on 13-15 April 2005 and organised by the Ecological and Environmental Economics - EEE Programme, a joint three-year programme of ICTP - The Abdus Salam International Centre for Theoretical Physics, FEEM - Fondazione Eni Enrico Mattei, and The Beijer International Institute of Ecological Economics.

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