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Implications of Bid Design and Willingness-To-Pay Distribution for Starting Point Bias in Double-Bounded Dichotomous Choice Contingent Valuation Surveys

Marcella Veronesi · Anna Alberini · Joseph C. Cooper

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Abstract We examine starting point bias in double-bounded dichotomous choice contingent valuation surveys. We investigate (1) the seriousness of the biases for the location and scale parameters of the willingness-to-pay (WTP) in the presence of starting point bias; (2) whether or not these biases depend on the distribution of WTP and on the bid design; 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. Monte Carlo simulations suggest that the effect of ignoring starting point bias depends on the bid design and on the true distribution of WTP. A well-balanced, symmetric bid design may result in very modest biases even when the anchoring mechanism is very strong. The power of bid set dummies in detecting starting point bias is low. They tend to account for misspecifications in the distribution assumed by the researcher for the latent WTP, rather than capturing the presence of starting point bias.

Keywords Anchoring · Bid design · Contingent valuation · Double-bounded dichotomous choice format · Monte Carlo simulations · Starting point bias · Willingness-to-pay distribution

The views expressed are the authors' and do not necessarily represent policies or views of their respective institutions.

M. Veronesi (✉)
Institute for Environmental Decisions, ETH Zurich, Universitaetstrasse 22, CHN K 76.1, 8092 Zurich, Switzerland
e-mail: marcella.veronesi@env.ethz.ch

A. Alberini
Department of Agricultural and Resource Economics, University of Maryland, College Park, USA

J. C. Cooper
Economic Research Service, USDA, Washington, DC, USA

Abbreviations

CV	Contingent valuation
DB	Double-bounded
DC	Dichotomous choice
WTP	Willingness-to-pay

1 Introduction

Many recent contingent valuation (CV) surveys elicit information about willingness-to-pay (WTP) by asking dichotomous choice (DC) questions. 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 costs \$X to the household. To refine information about WTP, it is possible to ask a dichotomous choice follow-up question, approach commonly dubbed "double-bounded" (DB) (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). Although many contingent valuation practitioners continue to implement surveys with dichotomous choice questions and follow-ups, and to fit double-bounded models, over the last decade researchers have examined this approach's potential for undesirable response effects (see Mitchell and Carson 1989; Hausman 1993; Bateman et al. 2002 among others).

In this paper, we focus on one such effect, namely starting point bias or anchoring bias (Tversky and Kahneman 1974). A number of papers in the economics and psychological literature find that respondents, when uncertain about their assessment of the good being valued, "anchor" their valuation to the available information, even if uninformative such as the last two digits of their social security number (Wilson et al. 1996; Ariely et al. 2003, and Bergman et al. 2010). In CV surveys where follow-up questions are used, respondents may "anchor" the value they place on a good on the bid amounts proposed to them in the initial and/or subsequent payment questions. This may happen, for example, because of a poor perception or description of the good being valued (Brookshire and Randall 1978), or when the uncertain respondent interprets the bid amount as an approximation of the good's true value, thus anchoring his WTP on the proposed bid to update priors in light of society's or experts' beliefs (Boyle et al. 1985; Mitchell and Carson 1989; Czajkowski 2009). In addition, Arana and Leon (2008) find that the emotional status of the individual can directly affect anchoring, and that the relationship is U-shaped: if emotional intensity increases, anchoring declines until it reaches a minimum at which the individual is not influenced by the first bid amount.[1]

In empirical work, a simple test for the presence of starting point bias consists of (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 coefficients on these dummies are jointly equal to zero (for example, Whittington et al. 1990; Cameron and Quiggin 1994; Chien et al. 2005).

[1] DeShazo (2002) considers iterative questions in ascending and descending sequence, and he finds that there is inconsistency of responses only in the ascending sequence due to framing effect rather than anchoring. Respondents who are assigned the ascending sequence will consider the lower value as a reference point while the follow-up question as a loss. By contrast, respondents who are assigned the descending sequence do not consider the initial value as a reference point. See also Green et al. (1998) for a review on anchoring effects.

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 wonder whether in some cases what has been interpreted by the researcher as evidence of anchoring to the initial bids was simply an artifact due to misspecification of the econometric model and/or the poor choice of the 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 and use the widely adopted anchoring mechanism proposed by Herriges and Shogren (1996) to answer these questions. 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.[2] 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, on the bid design, and on the WTP statistic being estimated (mean WTP or the variance of WTP). We find that bid set dummies and a test of the null that their coefficients are jointly zero have only very modest power in detecting starting point bias. The coefficients on these dummies tend to account for misspecifications in the distribution assumed by the researcher for the latent WTP, rather capturing the presence of starting point bias.

The remainder of the paper is organized as follows. In Section 2, we present the starting point bias mechanism developed by Herriges and Shogren (1996) and a plausible variant on this model. In Section 3, we present a commonly used test for the presence of starting point bias. We present the simulation study design in Section 4, and its results in Section 5. Section 6 concludes.

[2] Herriges and Shogren (1996) model combines WTP and initial bids in linear or as weighted average specifications. Extensions of Herriges and Shogren (1996) and alternative models of starting point bias have been proposed. Whitehead (2002, 2004) examine incentive compatibility and starting point bias by including the structural shifts by Alberini et al. (1997). Leon and Leon (2003) assume that even the first WTP amount is influenced by the initial bid. Chien et al. (2005) combine the Herriges and Shogren's mechanism with yea-saying. Most recently, Flachaire and Hollard (2006) present models of starting point bias in ascending iterative questions, and show that models that combine framing effects with anchoring and/or shifts effects provide efficiency gains in DB CV surveys. Flachaire and Hollard (2007) develop a model that deals with respondents' uncertainty and starting point bias in DC CV surveys. Aprahamian et al. (2007, 2008) provide evidence in support of heterogeneous anchoring, and in particular, Aprahamian et al. (2008) find that the shift effects in iterative valuation surveys can be the result of a mistaken assumption of homogenous anchoring. Finally, Czajkowski (2009) develop a model of Bayesian updating behavior that can be considered as an extension of traditional starting point bias models.

2 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 stated WTP amount with the bids assigned to him. In dichotomous choice CV surveys with a dichotomous choice follow-up question, the responses to the payment questions are used to construct an interval around each respondent's unobserved WTP amount.[3]

If starting point bias is present, the bid amounts may influence the response to a payment question in two ways: (i) by affecting underlying WTP directly if respondents use the bid information to update their true 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 (1996) argue that the initial bid may provide a "focal point or anchor for the uncertain respondent."[4] 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,

$$WTP_{1i} = \mu + \varepsilon_1, \tag{1}$$

where μ is mean WTP and ε_1 is (normally distributed) error term with variance σ_1^2, and

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

where $0 \leq \gamma \leq 1$ is the weight placed on the initial bid.[5] If $\gamma = 0$, there is no anchoring, and $WTP_{2i} = WTP_{1i}$, as is routinely assumed in double-bounded models. If $\gamma = 1$, no memory of the original WTP amount is retained in the follow-up question, and WTP_{2i} is equal to the first 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 (1996) show that the anchoring mechanism described by Eqs. (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_{1i}.

If one fits a conventional double-bounded model in this situation, are the estimated coefficients biased, and, if so, how severely? Herriges and Shogren (1996) conduct Monte Carlo

[3] Notice that for respondents who give two "yes" responses, the upper bound of WTP may be infinity, or the respondent's income; for respondents who give two "no" responses, the lower bound is either zero (if the distribution of WTP admits only non-negative values) or negative infinity (if the distribution of WTP is a normal or a logistic), In addition, see Alberini et al. (2006) for details on the log-likelihood functions when the WTP follows, respectively, the normal, lognormal or Weibull distributions.

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

[5] Clearly, this notation assumes that mean WTP is the same for all respondents. This common mean replaces the individual-specific expectation $\mathbf{x_i \beta}$.

simulations assuming a normal distribution and varying the degree of anchoring. They show 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. 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.[6] 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_{1i} and B_1, so if the average initial bid is roughly equal to mean WTP_{1i}, mean WTP_{2i} is roughly equal to mean WTP_{1i}, and so is the weighted average of these two means, which the double-bounded estimator tends to.

In this paper, we generate data following the Herriges and Shogren (1996) 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 WTP. Our work differs from earlier studies in that: (i) when using the Herriges and Shogren (1996) 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, we study (ii) and (iii) after introducing an amendment to the Herriges and Shogren (1996) 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 (1996)—the follow-up question may end up confusing them. We therefore, amend Eq. (2) to obtain

$$\mathrm{WTP}_{2i} = \mathrm{WTP}_{1i}(1-\gamma) + \gamma \cdot B_1 + \varepsilon_2 \tag{3}$$

that is (see Appendix A)

$$\mathrm{WTP}_{2i} = (1-\gamma)\mu + \gamma B_1 + \left[\varepsilon_1(1-\gamma) + \varepsilon_2\right], \tag{4}$$

where the error term ε_2 represents either the uncertainty by the respondent or by the researcher. For instance, it can capture the possible uncertainty/confusion associated with the follow-up question, and possibly to some extent, the misspecification by the researcher of the anchoring effect.

3 Detecting Starting Point Bias

A number of papers include bid set dummies among the regressors of the double-bounded model to capture starting point effects (for example, Whittington et al. 1990; Cameron and Quiggin 1994; Chien et al. 2005).[7] This approach is similar to the test implemented by Boyle et al. (1985) in iterative bidding games where they estimate a linear regression between starting bids and final bids, and test for significant coefficients on the starting bids. A significant coefficient on the initial bid variable is interpreted as evidence of starting point bias.

[6] If WTP follows the normal or any other distribution defined between $-\infty$, or 0 and ∞, the bids will usually cover a much smaller range.

[7] 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 starting point bias. In this paper, we use the Wald statistic, which is calculated as

$$w = \hat{\delta}' \mathbf{V}^{-1} \hat{\delta}, \tag{5}$$

where $\hat{\delta}$ is the vector of coefficients on the bid set dummies estimated from the augmented double-bounded model, and $\mathbf{V}$ is the block of the information matrix for all parameters corresponding to the coefficients on the bid set dummies. $\mathbf{V}$ is an m $\times$ m matrix, where m = dim (δ). For large sample size and under the null, the test statistic w is distributed as a chi square with m degrees of freedom.

4 Study Design

To answer our research questions, we conducted a series of Monte Carlo simulations.[8] 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).[9] In each experiment, the number of replications is 1,000 and in each replication the sample size is 1,000. Our study design is summarized in Table 1. All simulations fit a normal likelihood function, but we assume different distributions (normal, Weibull, and lognormal) for the true WTP (column (B) of Table 1).We generate draws from the assumed distribution, and each draw is assigned at random to one of the possible bid designs (column (E) of Table 1). 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.

In addition, Table 1 (column D) shows that simulation sets I, III and IV adopt the Herriges and Shogren (1996) anchoring mechanism (Eqs. (1) and (2)), while simulation set II adopts our amendment to the Herriges-Shogren (1996)'s model (Eqs. (3) and (4)). In simulation set II we assume that the true distribution is normal and that ε_1 and ε_2 are uncorrelated, so that we can compare the results of simulation set II with those of simulation set I.[10] Simulation set II is repeated under two alternative values for σ_2, where $\sigma_2^2 = \text{Var}(\varepsilon_2)$, 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.

We use a total of three bid designs ("base", "upper tail", and "lower tail"). Each is comprised of 5 initial bid amounts and their corresponding high and low follow-up bids (Table 2). The follow-up amounts are double or half of the initial amount. It is important that the bid amounts are 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 3. (This means that the actual bid amounts differ across simulation sets to mirror the different distributions

8 Monte Carlo simulations are often used in CV literature to investigate the bias derived by model misspecification, or for example, to build confidence intervals for welfare measures. See Baiocchi (2005) for a review of the literature on Monte Carlo methods and their use in CV literature, and for guidelines on how to implement a Monte Carlo study.

9 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.

10 Appendix A shows that WTP_{1i} and WTP_{2i} are correlated, since they both contain ε_1.

Table 1 Summary of the simulation experiment design

(A)	(B)	(C)	(D)	(E)
Simulation set	True WTP distribution	Parameters of true WTP distribution	Anchoring mechanism	Bid design
I	Normal	$\mu = 10$ (mean WTP) $\sigma = 10$ (standard deviation WTP)	Herriges and Shogren (1996) with $\gamma = 0$ (no anchoring), 0.3, 0.5, 0.7, 0.9	Base Upper tail Lower tail
II	Normal	$\mu = 10$ (mean WTP_1) $\sigma_1 = 10$ (standard deviation WTP_1) $\sigma_2 = 3$ or 20 (standard deviation ε_2)	Anchoring + error term Eq. (3) with $\gamma = 0$ (no anchoring), 0.3, 0.5, 0.7, 0.9	Base Upper tail Lower tail
III	Weibull	Scale parameter $\sigma = 10$ Shape parameter $\theta = 1$	Herriges and Shogren (1996) with $\gamma = 0$ (no anchoring), 0.3, 0.5, 0.7, 0.9	Base Upper tail Lower tail
IV	Lognormal	$\mu = 1.956$ (mean of log WTP) $\sigma = 0.693$ (standard deviation of log WTP)	Herriges and Shogren (1996) with $\gamma = 0$ (no anchoring), 0.3, 0.5, 0.7, 0.9	Base Upper tail Lower tail

we assume for WTP. We remind the reader that the percentile is 1 minus the probability of answering "yes" to that bid amount).[11]

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.[12] 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.[13] 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.

We use a total of five 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 relatively 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

[11] In the base bid design, the initial bid values cover the 18–69th percentile. The bid design labeled "upper tail" covers the 18–93th percentiles, while the bid design 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 (Table 3).

[12] 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).

[13] 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.

Table 2 Bid designs and 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	2.03	3.69	6.93	8.66	11.76
	Upper tail	2.03	3.69	6.93	11.76	27.06
	Lower tail	2.03	2.77	3.21	3.69	4.81
Lognormal	Base	3.34	4.66	7.07	8.35	10.72
	Upper tail	3.34	4.66	7.07	10.72	24.65
	Lower tail	3.34	3.95	4.29	4.66	5.51

Table 3 Percentiles corresponding to the bid amounts in the simulations

Bid design	Percentile				
Base bid design	0.18	0.31	0.50	0.58	0.69
Upper tail bid design	0.18	0.31	0.50	0.69	0.93
Lower tail bid design	0.18	0.24	0.27	0.31	0.38

to establish the seriousness of the biases (if any) of the estimates of mean and variance WTP. In the second double-bounded model, the likelihood function is amended to include dummies for the bid set.[14] Since there are a total of five bid sets, we include four bid set dummies, and we compute the Wald statistics (Eq. (5)) for the null that the coefficients on the bid dummies are all equal to zero.

5 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).

[14] For example, in simulation set I, when the base bid design is used, the bid set dummies are D1 = 1 if the initial bid is 1, and 0 otherwise; D2 = 1 if the initial bid assigned to this observation is 5, and 0 otherwise, etc.

Table 4 Percent bias mean willingness to pay

	Anchoring level (γ)	Base bid design	Upper tail bid design	Lower tail bid design
(a) Simulation set I				
(Herriges-Shogren, 1996's model)	0	−0.20	−0.10	0.18
True WTP: normal distribution	0.3	−1.50	6.54	−16.20
Likelihood function: normal	0.5	−1.72	12.85	−27.22
	0.7	−1.74	14.89	−39.30
	0.9	−7.00	7.98	−49.70
(b) Simulation set II	0	−0.02	−0.12	−0.15
True WTP: normal distribution	0.3	−2.03	5.22	−17.23
Likelihood function: normal	0.5	−3.23	10.21	−27.30
$\sigma_2 = 3$	0.7	−4.49	12.08	−37.57
	0.9	−7.80	7.60	−46.73
(c) Simulation set II	0	−0.02	−0.12	−0.15
True WTP: normal distribution	0.3	0.69	3.13	−27.60
Likelihood function: normal	0.5	−0.50	3.84	−30.66
$\sigma_2 = 20$	0.7	−1.73	4.20	−33.57
	0.9	−2.96	4.80	−36.02
(d) Simulation set III	0	−19.03	−16.64	−30.73
True WTP: Weibull distribution	0.3	−18.99	−16.71	−30.62
Likelihood function: normal	0.5	−18.78	−16.68	−30.74
	0.7	−19.94	−16.72	−30.83
	0.9	−19.12	−16.86	−30.67
(e) Simulation set IV	0	−17.32	−15.45	−24.68
True WTP: Weibull distribution	0.3	−17.31	−15.49	−24.60
Likelihood function: normal	0.5	−17.40	−15.43	−24.71
	0.7	−17.43	−15.50	−24.67
	0.9	−17.44	−15.62	−24.64

5.1 Bias of the Welfare Estimates

Table 4 displays the relative bias of mean WTP for the three bid designs and the four simulation sets. 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., $\gamma = 0$), 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 $\gamma = 0.3$ to −7% for $\gamma = 0.9$), finding consistent with Herriges and Shogren (1996)'s results.

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 ($\gamma = 0.9$) results in an underestimate of mean WTP by at least 50%.

Panel (b) displays 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 is 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.

Table 5 presents 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, result consistent with Herriges and Shogren (1996)'s results. 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 $\gamma = 0.9$. 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 Eq. (3) 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% .

5.2 Diagnostic Test

Table 6 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 $\alpha = 0.05$.

Table 6 shows clearly that in simulation set I, where the correct distribution (the normal) is assumed for WTP, and no anchoring is present ($\gamma = 0$), 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 the anchoring parameter γ, but this expectation is not borne out in the results: rejections occur in 5–6% 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.

Table 5 Percent bias standard deviation willingness to pay

	Anchoring level (γ)	Base bid design	Upper tail bid design	Lower tail bid design
(a) Simulation set I				
(Herriges-Shogren (1996)'s model)	0	0.01	−0.01	0.31
True WTP: normal distribution	0.3	−24.20	−17.81	−27.37
Likelihood function: normal	0.5	−38.78	−28.13	−45.68
	0.7	−49.65	−36.52	−64.62
	0.9	−51.60	−38.68	−76.79
(b) Simulation set II	0	0.05	−0.01	0.04
True WTP: normal distribution	0.3	−20.80	−15.81	−27.78
Likelihood function: normal	0.5	−34.32	−25.06	−43.36
$\sigma_2 = 3$	0.7	−44.11	−31.88	−57.91
	0.9	−48.10	−35.13	−68.92
(c) Simulation set II	0	0.05	−0.01	0.04
True WTP: normal distribution	0.3	2.30	1.51	−38.77
Likelihood function: normal	0.5	−2.70	−2.65	−42.58
$\sigma_2 = 20$	0.7	−7.57	−6.40	−46.08
	0.9	−11.84	−9.59	−49.22
(d) Simulation set III	0	−13.57	−6.19	−45.64
True WTP: Weibull distribution	0.3	−13.82	−6.24	−45.52
Likelihood function: normal	0.5	−13.45	−6.04	−45.46
	0.7	−13.55	−6.10	−45.62
	0.9	−13.68	−6.67	−45.57
(e) Simulation set IV	0	−35.32	−26.24	−56.50
True WTP: Weibull distribution	0.3	−35.28	−26.61	−56.54
Likelihood function: normal	0.5	−35.18	−26.42	−56.53
	0.7	−35.50	−26.55	−56.54
	0.9	−35.32	−26.69	−56.41

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 ($\gamma = 0$), 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 is 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 $\gamma = 0$, 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% against a nominal size of 5%. Little change is seen when γ increases for a given bid design. We conclude that in this

Table 6 Empirical size and power of the test of starting point bias

Distribution	Anchoring present?	Double-bounded log likelihood	Percent rejection of null Wald test (Base bid design)	Percent rejection of null Wald test (Upper tail bid design)	Percent rejection of null Wald test (Lower tail bid design)
Normal (Simulation set I)	No	Normal	5.50	4.70	7.51
	Yes, $\gamma = 0.3$		6.00	5.80	5.53
	Yes, $\gamma = 0.5$		5.90	6.70	5.68
	Yes, $\gamma = 0.7$		3.40	6.90	4.47
	Yes, $\gamma = 0.9$		4.70	6.40	5.82
Normal ($\sigma_2 = 3$) (Simulation set II)	No	Normal	5.11	7.26	6.06
	Yes, $\gamma = 0.3$		6.30	7.66	3.31
	Yes, $\gamma = 0.5$		4.02	2.47	6.20
	Yes, $\gamma = 0.7$		7.80	3.69	5.56
	Yes, $\gamma = 0.9$		6.69	6.45	4.66
Normal ($\sigma_2 = 20$) (Simulation set II)	No	Normal	5.11	7.26	6.06
	Yes, $\gamma = 0.3$		5.70	6.27	5.74
	Yes, $\gamma = 0.5$		5.20	5.27	6.38
	Yes, $\gamma = 0.7$		5.50	5.90	4.21
	Yes, $\gamma = 0.9$		5.80	4.97	6.29
Weibull (Simulation set III)	No	Normal	10.88	26.04	13.65
	Yes, $\gamma = 0.3$		13.29	21.80	12.78
	Yes, $\gamma = 0.5$		12.98	23.01	13.01
	Yes, $\gamma = 0.7$		12.18	23.57	14.13
	Yes, $\gamma = 0.9$		13.31	21.92	12.77
Lognormal (Simulation set IV)	No	Normal	7.06	12.84	14.44
	Yes, $\gamma = 0.3$		7.43	12.23	13.02
	Yes, $\gamma = 0.5$		5.30	11.58	14.45
	Yes, $\gamma = 0.7$		8.17	10.97	16.30
	Yes, $\gamma = 0.9$		5.78	15.37	15.82

simulation set the Wald test exhibited limited power in picking up either anchoring or the poor distributional assumption.

6 Conclusions

In this paper, we focus on starting point bias (anchoring) in dichotomous choice contingent valuation surveys with a dichotomous choice follow-up question. In particular, we examine the role of the bid design and the underlying distribution of WTP on the impact of starting point bias. We use Monte Carlo simulations and generate data using the anchoring mechanism described in Herriges and Shogren (1996), which is frequently adopted in the literature. We then investigate 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.

In addition, 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.

Finally, we also investigate 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. Our results show that this test is not a powerful test, and thus not a useful tool to test for anchoring. When the true distribution of WTP is a normal and the econometric model of the responses to the payment questions is a double-bounded model that assumes a normal distribution, 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.

Based on our findings, we caution researchers that the consequences of starting point biases are complex and depend on the underlying distribution of WTP, and on the bid design. We also caution researchers 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 since they may "absorb" deviations from the assigned distribution of the latent WTP. We find 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 in writing out the double-bounded models.

It is difficult to come up with alternative approaches for detecting and correcting for anchoring unless the correct distribution of WTP is assumed, and one is prepared to make specific assumptions about the form of the anchoring. (See Leon and Leon (2003) for the development of a Bayesian approach to testing competing models, which relies on the prior distributions for the parameters in the double-bounded model.) In addition, semi-parametric, and nonparametric models, which alleviate the need for making assumptions regarding the distribution, cannot separately identify response biases from other forms of bias.[15]

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 of the interval around the respondent's unobserved true WTP amount 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 conclusion, we suggest estimating the anchoring parameter directly, rather than testing whether coefficients of bid set dummies jointly equal zero. In addition, we recommend that

[15] See Cooper (2002); Crooker and Herriges (2004), and more recently, Huang et al. (2008) and Watanabe (2010) for a review of the literature on non-parametric or semi-parametric approaches.

researchers: (i) pay close attention to the bid design chosen; and (ii) examine the sensitivity of their results to alternative underlying distribution of WTP. For instance, it is not uncommon to postulate some rather complicated behavioral model and then assume that WTP is (log) normally distributed to make the estimated model computationally tractable. The results of this paper suggest that this is a poor strategy due to the strong possibility of confounding the systematic and error component parts of the model.[16]

Appendix A: Model of the Responses Corresponding to Equations (3) and (4)

This appendix shows (i) how we derived Eq. (4); (ii) how we handled the new error term $\left[\varepsilon_1(1-\gamma)+\varepsilon_2\right]$ in the estimation; and (iii) that WTP_{1i} and WTP_{2i} are correlated, since they both contain ε_1.

Let WTP_1 be the WTP to the first bid, WTP_2 the WTP to the follow-up bid, B_1 the first bid, B_2 the follow-up bid, μ the mean WTP, and γ the anchoring parameter.

$$\mathrm{WTP}_1 = W^* = \mu + \varepsilon_1 \tag{A1}$$
$$\mathrm{WTP}_2 = \tilde{W}^* = W^*(1-\gamma) + \gamma B_1 + \varepsilon_2 \tag{A2}$$

with

$$\begin{pmatrix}\varepsilon_1\\ \varepsilon_2\end{pmatrix} \sim N\left[\begin{pmatrix}0\\0\end{pmatrix},\begin{pmatrix}\sigma_1^2 & \sigma_{12}\\ \sigma_{21} & \sigma_2^2\end{pmatrix}\right]$$

and

$$\begin{aligned} W &= 1 \quad \text{if} \quad W^* \geq B_1\\ &= 0 \quad \text{if} \quad W^* < B_1\\ \tilde{W} &= 1 \quad \text{if} \quad \tilde{W}^* \geq B_2\\ &= 0 \quad \text{if} \quad \tilde{W}^* < B_2 \end{aligned}$$

If $\gamma = 0$, i.e., no anchoring, then $\mathrm{WTP}_2 = W^* + \varepsilon_2$; if $\gamma = 1$, then $\mathrm{WTP}_2 = B_1 + \varepsilon_2$. From (A1) and (A2) WTP_2 becomes

$$\begin{aligned} \mathrm{WTP}_2 = \tilde{W}^* &= (1-\gamma)(\mu+\varepsilon_1) + \gamma B_1 + \varepsilon_2\\ &= (1-\gamma)\mu + \gamma B_1 + \left[\varepsilon_1(1-\gamma)+\varepsilon_2\right], \end{aligned} \tag{A3}$$

which corresponds to Eq. (4), where $\left[\varepsilon_1(1-\gamma)+\varepsilon_2\right]$ is the new error term, and

$$\begin{pmatrix}\varepsilon_1\\ \left[\varepsilon_1(1-\gamma)+\varepsilon_2\right]\end{pmatrix} \sim N\left[\begin{pmatrix}0\\0\end{pmatrix},\begin{pmatrix}\sigma_1^2 & \omega_{12}\\ \omega_{21} & \omega^2\end{pmatrix}\right],$$

where

$$\omega^2 = V\left[\varepsilon_2 + \varepsilon_1(1-\gamma) + \varepsilon_2\right] = \sigma_2^2 + (1-\gamma)^2\sigma_1^2 + 2(1-\gamma)\sigma_{12},$$

$\mathrm{Cov}\left(\varepsilon_1, \left[\varepsilon_1(1-\gamma)+\varepsilon_2\right]\right) = \omega_{12} = \sigma_{12} + (1-\gamma)\sigma_1^2$. The correlation term between the new error term of WTP_2 and the error term of WTP_1 is defined as $\rho = \omega_{12}\big/\sigma_1\omega$.

16 We thank the reviewers for pointing this out.

We denote the corresponding response probabilities as

$$
\begin{aligned}
P(\text{No}, \text{No}) &= P(\text{WTP}_1 \leq B_1, \text{WTP}_2 < B_2) \\
&= P\left[(\mu + \varepsilon_1 \leq B_1); \left(\mu(1-\gamma) + \gamma B_1 + \left[\varepsilon_1(1-\gamma) + \varepsilon_2\right]\right) < B_2\right] \\
&= P\left[\frac{\varepsilon_1}{\sigma_1} \leq \frac{(B_1 - \mu)}{\sigma_1}; \frac{\left[\varepsilon_1(1-\gamma) + \varepsilon_2\right]}{\omega} < \frac{(B_2 - \mu(1-\gamma) - \gamma B_1)}{\omega}\right] \\
&= \int_{-\infty}^{\frac{(B_2 - \mu(1-\gamma) - \gamma B_1)}{\omega}} \int_{-\infty}^{\frac{(B_1 - \mu)}{\sigma_1}} \varphi(z_1, z_2, \rho) dz_1 dz_2 \\
&= \Phi\left[\frac{(B_1 - \mu)}{\sigma_1}, \frac{(B_2 - \mu(1-\gamma) - \gamma B_1)}{\omega}, \rho\right]
\end{aligned}
$$

that is the cdf of a bivariate normal.

$$
\begin{aligned}
P(\text{Yes}, \text{No}) &= P(., \text{No}) - P(\text{No}, \text{No}) \\
&= P(\text{WTP}_2 < B_2) - P(\text{No}, \text{No}) \\
&= \Phi\left[\frac{(B_2 - \mu(1-\gamma) - \gamma B_1)}{\omega}\right] - P(\text{No}, \text{No}) \\
P(\text{No}, \text{Yes}) &= P(\text{No}, .) - P(\text{No}, \text{No}) \\
&= P(\text{WTP}_1 < B_1) - P(\text{No}, \text{No}) \\
&= \Phi\left[\frac{(B_1 - \mu)}{\omega_1}\right] - P(\text{No}, \text{No}) \\
P(\text{Yes}, \text{Yes}) &= 1 - P(\text{No}, \text{No}) - P(\text{Yes}, \text{No}) - P(\text{No}, \text{Yes}) \\
&= 1 - P(\text{No}, \text{No}) - \Phi\left[\frac{(B_1 - \mu)}{\sigma_1}\right] + P(\text{No}, \text{No}) \\
&\quad - \Phi\left[\frac{(B_2 - \mu(1-\gamma) - \gamma B_1)}{\omega}\right] + P(\text{No}, \text{No}) \\
&= 1 - \Phi\left[\frac{(B_1 - \mu)}{\sigma_1}\right] - \Phi\left[\frac{(B_2 - \mu(1-\gamma) - \gamma B_1)}{\omega}\right] + P(\text{No}, \text{No})
\end{aligned}
$$

The log-likelihood function is

$$
\begin{aligned}
\log L = \sum \big[& d_{yy} \log P(\text{Yes}, \text{Yes}) \\
&+ d_{yn} \log P(\text{Yes}, \text{No}) + d_{ny} \log P(\text{No}, \text{Yes}) + d_{nn} \log P(\text{No}, \text{No})\big]
\end{aligned}
$$

where $d_{nn} = 1$ if the starting bid is B_1, $B_1 > B_2$ and the response is (No, No), and 0 otherwise; $d_{yn} = 1$ if the starting bid is B_1, $B_1 < B_2$ and the response is (Yes, No); $d_{ny} = 1$ if the starting bid is B_1, $B_1 > B_2$ and the response is (No, Yes), and 0 otherwise; and $d_{yy} = 1$ if the starting bid is B_1, $B_1 < B_2$ and the response is (Yes, Yes). This log likelihood function is easily amended to accommodate for other distributions. See Alberini et al. (2006) for more details.

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