ESTIMATION USING CONTINGENT VALUATION DATA
FROM A "DICHOTOMOUS CHOICE
WITH FOLLOW-UP" QUESTIONNAIRE

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ABSTRACT

Dichotomous choice contingent valuation questions have gained popularity over the last several years due to their purported advantages for avoiding many of the biases known to be inherent in other value elicitation formats. However, this type of valuation question is highly inefficient in that a vastly larger number of observations is required to identify the distribution of values with any degree of accuracy. An alternative questioning strategy introduces a second value threshold which elicits a second discrete response. The size of the second offer depends on the answer to the first question. In analyzing data from this type of questionnaire, it is imperative to acknowledge the endogeneity of the second offered amount. This paper demonstrates the distortions in the final value estimates which can be introduced when this endogeneity is ignored or when inappropriate restrictions are placed upon the stochastic structure of the model.

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1. Introduction

Dichotomous choice contingent valuation questions have gained popularity over the last several years. This is due primarily to their purported advantages in avoiding many of the biases known to be inherent in other formats used in the contingent valuation method (CVM). Two standard references which discuss different CVM techniques are Cummings, Brookshire, and Schulze (1986) and Mitchell and Carson (1989). Whereas several varieties of bias may be minimized by dichotomous choice valuation questions, this elicitation method can be highly statistically inefficient in that vastly larger numbers of observations are required to identify the underlying distribution of resource values with any given degree of accuracy.

An alternative questioning strategy, intended to reduce this inefficiency, was first proposed and implemented by Carson, Hanemann, and Mitchell (1986). They advocate introducing a second offered threshold in a "follow-up" dichotomous choice CVM question which elicits a second discrete response. In practice, if a respondent indicates a willingness to pay the first offered amount, the new threshold is about double the first one. If the respondent is unwilling to pay the first offered amount, the second threshold is reduced to about half the original amount.

Carson and Mitchell (1987) employ survival analysis statistical techniques to analyze dichotomous choice with follow-up data. These methods were originally conceived to handle product failure data collected at

irregular intervals. Hanemann, Loomis, and Kanninen (1991) use maximum-likelihood models to analyze similar data. Both of these papers, however, maintain the hypothesis that the identical implicit true valuation drives respondents' answers to the two questions in this survey format. This paper proposes a more-general maintained hypothesis. Our estimation method allows the valuation information elicited at each of the two stages to be the same, or different, as the data dictate.

In analyzing data from a dichotomous-choice-with-follow-up (DCF) questionnaire, it is certainly important for the researcher to acknowledge explicitly the endogeneity of the second offered amount. Using a sample of data from an actual DCF survey, we examine the distortions in the final value estimates which can be introduced when this endogeneity is ignored, or is modelled incorrectly.

Separate distributional parameters for willingness to pay (for the two CVM questions), as well as the correlation across the two questions in the true underlying unobserved values, are estimated explicitly in our recommended specifications. Our models allow the researcher to test statistically for the equivalence of the implied valuation distributions across the original and follow-up questions. They also allow rigorous tests of a variety of restrictions that might be imposed upon both the distribution parameters and the error correlation.

We determine that the most common assumption--that identical value distributions are elicited by the first and follow-up questions--is implausible for our data set. This assumption also appears to compound the problem of the implicit distribution of value estimates being influenced by

the starting point (i.e. the first bid).

2. A General Model with Endogeneous Follow-up Offers

Throughout this analysis, we will be emphasizing the importance of plausible stochastic assumptions in the estimation of valuation models. If our objective in this paper was to ascertain the best possible estimate of the value of a specific environmental resource, we would employ covariates and go on to explore a vastly wider array of functional forms for the systematic portions of our valuation functions. The example employed here should be viewed simply as illustrative. Nevertheless, it succinctly conveys the importance of the issue we are highlighting.

As a point of departure for the model to be developed in this paper for DCF data, we rely upon the econometric framework developed in Cameron and James (1987) and in Cameron (1988). Those papers show how the data collected using a single discrete-choice CVM question can be employed in censored dependent variable models having a natural regression-like interpretation. These models can be estimated directly using general maximum likelihood optimization algorithms, or, can be calculated from the output of packaged probit algorithms.

However, much of the extant empirical work using discrete-choice CVM data without a follow-up has assumed, for expedience, that the underlying error distribution is logistic. This assumption leads to convenient closed-form integrals for the cumulative probability density functions which must be evaluated. For the present problem, however, we prefer normality of the errors since we with to model each participant's two discrete responses jointly. Bivariate normal probability density functions are the most

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familiar bivariate distributions employed commonly by statisticians and their properties are well-understood. Crucially, they allow for a non-zero correlation, whereas the standard logistic distribution does not.[1]

Hanemann (1991) does suggest a parametric test of the consistency between the responses to the second and first bids in his comments upon the work described in Imber, Stevenson and Wilks (1991). He recommends conducting

> "...a parametric, likelihood ratio test of the overall consistency of the responses to the first and second bids by estimating two models, one based exclusively on the responses to the first bid (e.g. a conventional logit model...) and the other based exclusively on the responses to the second bid (this involves maximizing the likelihood function framed in terms of the conditional probabilities for the response to the second bid), and then comparing the sum of the log-likelihoods with the log-likelihood function obtained from estimating the combined first and second bids (i.e. the model...actually estimated...)."

Since Hanemann's proposed method utilizes the stochastic assumptions of the usual logit specification, however, the error correlations in this model would not be estimable.

For our model with normally distributed errors, assume that each respondent has some unobserved true point valuation for the environmental resource in question at the moment the first dichotomous choice CVM question is posed. Let this unobserved value be y_{1i}. Let the first offered threshold, assigned arbitrarily to this individual, be denoted by t_{1i}. We will assume that the individual will state that they are willing to pay the offered amount ($I_{1i} = 1$) if $y_{1i} > t_{1i}$. They will be unwilling to pay this

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[1] For example, Hanemann, Loomis, and Kanninen (1991) employ models based upon logistic density functions. Consequently, their approach would not lead them to consider the presence of non-unitary correlations.

much ($I_{1i} = 0$) if $y_{1i} < t_{1i}$. Now let y_{1i} consist of a systematic component, $x_i'\beta_1$, which is a function of a vector of observable attributes of the respondent, x_i, plus an unobservable random component, ϵ_{1i} (distributed $N(0,\sigma_1^2)$), which absorbs all unmeasured determinants of the value of the resource to this individual.[2] The variable I_{1i} is the single endogenous (dependent) variable in this framework. Analysis of the first-stage responses is facilitated by the fact that the offered values in the first round, t_{1i}, have no possible correlation with the error terms, ϵ_{1i}.

So far, the development of the model mimics exactly the specifications used for single-threshold CVM data. But in the typical DCF framework, once an individual has been randomly assigned their initial offered value, the follow-up offer will take on one of two alternative predetermined values (one higher and one lower) depending upon the response to the first question. The probability of receiving the predetermined *higher* offer is just the probability of responding "yes" to the first WTP question. The probability of receiving the predetermined *lower* offer is the same as the probability of a "no" response to the first WTP question. The second offered threshold is clearly *not* independent of information which the respondent has revealed in answering the first WTP question.

In particular, it would be highly inappropriate to use just the set of second-round responses from a DCF questionnaire in a model which employed

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[2] The presence of regressors in a model would allow the researcher to explore any of a wide variety of alternative utility-theoretic specifications for WTP, which might be interpreted as the equivalent variation associated with the specified change in the resource. Linear-in-parameters models are the most popular (because they facilitate the use of packaged algorithms) but highly non-linear models are tenable when general function-optimizing algorithms are available.

the same assumptions as in the Cameron and James (1987) or Cameron (1988) papers. It would also be invalid to simply "pool" all of the thresholds and responses from the first and second questions in the estimation of a single valuation function. The perils of such a strategy are much the same as those awaiting labor economists who use panel data. We get two "readings" concerning resource values from each respondent. The individual-specific unobservable components in the underlying valuation function(s) which drive each individual's responses to this pair of questions will be correlated. This correlation must be explicitly accommodated in the estimation process.

Adopting similar notation for the data pertaining to the follow-up question, let y_{2i} be the respondent's implicit underlying point valuation of the resource at the moment the follow-up question is posed. This may or may not be identical to y_{1i}, depending upon the presence or absence of a strategic adjustment by the respondent. The indicator variable, I_{2i}, will be one if $y_{2i} > t_{2i}$ (where t_{2i} is explicitly endogenous), and zero if $y_{2i} < t_{2i}$. The point valuation will again be assumed to consist of a systematic component, $x_i'\beta_2$, and a random unobservable component, ϵ_{2i}, distributed $N(0,\sigma_2^2)$. Failure to acknowledge that ϵ_{2i} is correlated with ϵ_{1i} (and thus also with t_{2i}) will result in potentially serious endogeneity bias in all of the coefficients comprising the vector β_2.

A viable strategy for dealing with this endogeneity is to view the estimation problem as analogous to the more-common problem posed by a system of two equations with correlated errors. In linear systems, econometricians are familiar with recursive systems of two equations where the second equation contains the first equation's dependent variable on its right-hand

side. In that context, if the errors can be assumed to be independent, each equation can be estimated separately by ordinary least squares. If this assumption is not tenable, systems estimation methods must be employed.

In the present situation, however, we are not dealing with the familiar case of continuous dependent variables, but the two discrete responses, I_{1i} and I_{2i}. The offered threshold entering into the second "equation" reflects the probabilities associated with the discrete outcome I_{1i}. We must therefore develop the model in the context of the joint distribution of (y_{1i}, y_{2i}). We will assume a bivariate normal distribution, $BVN(x'\beta_1, x'\beta_2, \sigma_1^2, \sigma_2^2, \rho)$ for these two implicit valuations. There are four possible pairs of responses to these questions: (I_{1i}, I_{2i}) = (1,1), (1,0), (0,0) and (0,1). Dropping the i subscripts for ease of exposition, recall that I_1 = 1 implies $y_1 > t_1$. Using y_1 = $x'\beta_1 + \epsilon_1$, this condition can be expressed equivalently as $(\epsilon_1/\sigma_1) > (t_1 - x'\beta_1)/\sigma_1$, where ϵ_1/σ_1 is a standard normal random variable. The analogous transformation can be applied to y_2 and t_2 in determining the formula for the probability function for outcome I_2.

Denote the standardized normal error ϵ_1/σ_1 as z_1 and denote ϵ_2/σ_2 as z_2. The analysis can proceed in terms of the probabilities associated with regions in the domain of a standard bivariate normal distribution where the pair (z_1, z_2) is distributed $BVN(0,0,1,1,\rho)$. To simplify the notation in preparation for writing the log-likelihood function for this model, let $g(z_1, z_2)$ be the bivariate standard normal density function. This density takes the explicit form:

(1) $$g(z_1, z_2) = (1/2\pi) \exp \{ -(2-2\rho^2)^{-1} [z_1^2 - 2\rho z_1 z_2 + z_2^2] \}$$

The log-likelihood function for the model then takes the following form.

$$
\begin{aligned}
(2) \quad \text{LogL} = \Sigma_i \{ \ & (I_1 I_2) \log \left[\int_{(t_1 - x'\beta_1)/\sigma_1}^{\infty} \int_{(t_2 - x'\beta_2)/\sigma_2}^{\infty} g(z_1, z_2) \, dz_2 \, dz_1 \right] \\[2ex]
+ \ & (1 - I_1)(I_2) \log \left[\int_{-\infty}^{(t_1 - x'\beta_1)/\sigma_1} \int_{(t_2 - x'\beta_2)/\sigma_2}^{\infty} g(z_1, z_2) \, dz_2 \, dz_1 \right] \\[2ex]
+ \ & (1 - I_1)(1 - I_2) \log \left[\int_{-\infty}^{(t_1 - x'\beta_1)/\sigma_1} \int_{-\infty}^{(t_2 - x'\beta_2)/\sigma_2} g(z_1, z_2) \, dz_2 \, dz_1 \right] \\[2ex]
+ \ & (I_1)(1 - I_2) \log \left[\int_{(t_1 - x'\beta_1)/\sigma_1}^{\infty} \int_{-\infty}^{(t_2 - x'\beta_2)/\sigma_2} g(z_1, z_2) \, dz_2 \, dz_1 \right] \} .
\end{aligned}
$$

Note that most of the parameters to be estimated, β_1, β_2, σ_1, and σ_2, appear in the limits to the integrals. The remaining correlation parameter, ρ, is embedded in the $g(z_1, z_2)$ terms.

This general model can be readily estimated using standard packaged bivariate probit algorithms such as those offered in the programs LIMDEP or SST. Recall that models for *single*-threshold dichotomous choice can be estimated using conventional maximum likelihood probit algorithms. Exploiting the invariance property of maximum likelihood, the resulting standard probit parameter point estimates can then be transformed to yield an associated regression-like relationship for the dichotomous choice model. To obtain the variance-covariance matrix corresponding to the transformed parameters, one can take advantage of a formula offered in Lehmann (1983), pointed out by Patterson and Duffield (1991). Analogous transformations

exist for the bivariate case.

3. A Brief Description of the Data

The literature on dichotomous choice contingent valuation methods is now quite substantial. However, the first published application to a major issue of political contention appears to be a study undertaken by the Australian Resource Assessment Commission (RAC) in 1990 (Imber, Wilks and Stevenson). The study was part of an evaluation of alternative proposals for the management of the Kakadu region of the Northern Territory (NT) in Australia. Kakadu is an important wilderness area, but also contains significant mineral deposits. The establishment of a proposed National Park was divided into three stages. At the time of the study, the first two stages had been declared as National Park, with the excision of an existing uranium mine. The debate concerned the proposed third stage, which included a promising site for gold and uranium mining at Coronation Hill.

Conservationists supported the declaration of the entire third stage, while mining interests supported exploration and development at Coronation Hill. The interests directly at stake are not exceptionally large. The present value of the mining prospect has been estimated at $A82m, or $5 for each Australian citizen (ABARE, 1990). As far as conservation values are concerned, Stage 3 contains largely peripheral areas valued primarily as a buffer for Stages 1 and 2, and in order to permit the preservation of a complete river basin.

In the Imber et al. study, only two percent of respondents spontaneously mentioned preservation of Kakadu as one of Australia's most important environmental problems. The issue cannot be viewed in isolation,

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however. First, it has been treated both by conservationists and miners as a test case of the principle of permitting mining in national parks. Second, the mining of uranium has been the subject of prolonged and bitter controversy because of its role in the nuclear industry. Third, the region is located in land traditionally owned by members of Australia's Aboriginal population. Policies permitting Aborigines to reclaim their traditional lands have also been subject to controversy, frequently involving mining companies. Thus, opinion on the question is sharply polarized.

The RAC was commissioned in 1990 to report to the Australian Government on the appropriate policy response. As part of this effort, Imber, Wilks and Stevenson (1990) undertook a contingent valuation of the preservation option. A total of 2034 adult Australians were interviewed. As discussed below, the estimates yielded by the contingent valuation procedure have been the subject of considerable criticism, and this has included some criticism of the data collection methods. However, there appears to be a consensus among experts on contingent valuation that the procedure was applied in an exemplary fashion. A leading polling agency was employed to conduct the interviews. The questionnaire was subject to extensive pre-testing, and careful attempts were made to cope with all of the sources of bias discussed in the literature.

Eight different "treatments" were used in the CVM questionnaire, arising from four payment levels and two different scenarios (minor damage and major damage) describing possible damage to conservation values arising from mining. In each treatment, respondents were asked some introductory questions, then asked to nominate areas of major environmental concern. As

noted, only a small proportion (2 per cent) specifically named Kakadu at this stage.

Respondents were then asked about their knowledge of Kakadu, and were presented with photographs and maps describing the conservation zone. Each respondent was then presented with one of the two scenarios and asked two willingness-to-pay questions. If the first question was answered affirmatively, the amount was increased (approximately doubled), otherwise it was decreased (approximately halved). The payment vehicle was an increase in taxes.

After answering the WTP questions, subjects were asked for reasons why they were (or were not) willing to pay the amounts in question. They were then presented with a number of questions eliciting attitudes to environmental issues and a range of questions on socio-economic variables (age, sex, education, income, national origin, occupation).

As already stated, the issue in question was politically contentious and a large proportion of respondents had strongly held views on the subject. Supporters of mining were unlikely to state any positive willingness to pay (and might well have indicated negative willingness to pay if asked). The modal explanation among subjects who answered "No" was a statement of the form "Support mining/good for the country." A smaller proportion of subjects gave explanations the form "Too much money/not worth it to me," which would indicate a willingness to pay a positive amount less than the threshold asked. Similarly, committed supporters of preservation were likely to answer "Yes" to questions involving even very high thresholds.

There has been vigorous debate (Quiggin, Rose and Chambers, 1992, ABARE, 1991) over whether these responses should be interpreted in WTP terms (implying a large proportion of respondents with WTP outside the range $0-$250) or in terms of a 'voting' model, in which respondents with a precommitted position disregard the stated threshold, and answer "Yes" or "No" in line with their policy position. No simple statistical test can distinguish a person with a well-defined WTP lying outside the range of the questioning procedure from one who is simply voting for or against a policy position. Rather, it is necessary to explore the implications of both models in detail, in order to see which best fits the observed data.

With these caveats, the object of the present paper is to develop, in detail, the implications of the WTP interpretation for dichotomous choice with follow-up questions. The data used in the present paper are drawn from the published aggregate responses in Imber, Wilks and Stevenson (1991). Attention was confined to the 1013 respondents presented with the "minor damage" scenario, which was considered by Imber, Wilks and Stevenson to reflect majority scientific judgment of the likely impact of the mine. The responses are summarized in Table 1.

4. Empirical Results under the General Model

If the researcher is merely trying to quantify the location and scale of the current distribution of valuations in a particular sample, additional covariates may not be required. The $x'\beta_1$ and $x'\beta_2$ terms will be simple intercept terms represented by the scalars β_1 and β_2. Of course, if the model is intended to be used for forecasting, simulation, or benefits transfer, the use of any available regressors will probably be advisable.

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Regressors are also crucial if one is attempting to ascertain the marginal value of changes in amenity levels associated with particular resources.

To emphasize the possible distortions when the endogeneity of t_2 is ignored or modelled incorrectly, it is sufficient to simplify the model until it involves only the means, the variances, and the correlation of the assumed bivariate normal WTP values elicited by the initial and follow-up dichotomous choice CVM questions on our survey. (All of the procedures we utilize can be readily adapted to include covariates.)

Our specifications can also be modified to employ a variety of transformations of the threshold variables. Here, we focus on estimating only the marginal mean and variance of the implicit WTP variable. With normality, however, the admissible range contains the negative portion of the real line. Especially when regressors are employed, it will occasionally be the case that certain individuals will exhibit negative fitted values for their WTP. If it is deemed important to preclude negative fitted values, a logarithmic transformation of the thresholds, t_{1i} and t_{2i}, can be employed before the model is estimated. Likewise, the more general Box-Cox transformation is also viable and potentially very useful. Extensions of our general model which incorporate additional regressors, transformations of the thresholds, and further generalizations appear in a related paper, Cameron and Quiggin (1992).

For our sample of 1013 respondents, Table 2 gives the results for a model without covariates under a range of different assumptions about β_1, β_2, σ_1, σ_2, and ρ. Model 1 is the most general, with all five parameters free to take on any value. The point estimate of the mean willingness to

pay (WTP) for the first question is $128.77, while the point estimate for the second question is $146.06. The dispersion parameters for the fitted normal distributions are quite large, at $339.51 for the first question and $510.62 for the second. The error correlation is very precisely estimated at 0.9509.

Model 2 constrains the mean WTP underlying each response to be identical ($\beta_1 - \beta_2$). This "cross-equation" parameter restriction precludes estimation of this model using the packaged bivariate probit algorithms that could be used to produce the estimates shown for Model 1 (after an appropriate transformation). A likelihood ratio test with a value of only 0.48 indicates that this single parameter restriction cannot be rejected.

Model 3 constrains not only the means to be identical, but also the variances ($\beta_1 - \beta_2$, $\sigma_1 - \sigma_2$). This additional restriction is rejected by the likelihood ratio test statistic (which takes a value of 5.46, exceeding the critical value of 3.84). While the mean values elicited by the two questions appear to be statistically indistinguishable, there is significantly greater "noise" in the second-round responses. Model 2 will therefore be considered the preferred specification among those considered in this paper.

The next four rows of the Table 2 illustrate the effects of restrictions on ρ under different assumptions about β_1, β_2, σ_1, and σ_2. Model 4 assumes that ρ is exactly equal to 0, while $\beta_1 - \beta_2$ and $\sigma_1 - \sigma_2$. This model is equivalent to a specification where we pool the first-round and second-round data to produce a sample of (2*1013) - 2026 *independent* responses to each individual dichotomous choice questions, ignoring the

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information that pairs of responses are elicited from the same person. The underlying valuations are therefore assumed to be independent. Nonsensical results are produced because of unobserved individual-specific effects and the endogeneity of the second offered threshold.

The specification of Model 4 is distinct from Models 5 and 6, where we estimate completely separate models for the 1013 responses to each of the two questions taken alone. Model 4 restricts $\beta_1 = \beta_2$ and $\sigma_1 = \sigma_2$; Models 5 and 6 produce distinct estimates of β_1 and β_2, and of σ_1 and σ_2. In Table 2, the Model 5 results are for the first valuation question, taken by itself, and Model 6 is for the second question, taken by itself. The respective maximized log-likelihoods for this pair of sub-models may be summed for comparability with all other models.

Model 5 (for the first question) appear to gives sensible results, since the offered threshold is truly exogenously determined for the first question. The point estimates of both β_1 and σ_1 are very similar to those pertaining to the first valuation question in the specification in Model 1. On the other hand, Model 6 produces estimates so severely affected by endogeneity bias that (like Model 4) they actually suggest that demand for this environmental commodity slopes upwards.[3]

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[3] In the censored regression interpretation of standard probit results, the coefficient on the offered threshold is $-1/\sigma$. If the thresholds are truly exogenous, one expects that the probability of the respondents being willing to pay a given threshold offer will decrease as the threshold is made larger. A positive value for the coefficient $-1/\sigma$ (implying a negative estimate of σ itself) is a common artifact of incorrectly assigning the indicator for the discrete responses. For example, if I=1 means the respondent is *not* willing to pay the offered amount, and I=0 indicates that the amount would be paid, a "wrong" sign on σ will result. Here, it is the severe endogeneity of the second threshold that produces a similar result.

The intuition behind the emergence of an upward sloping demand curve is simple. A respondent is only offered a higher threshold if he/she has already stated a willingness to pay the first amount. Chances are, he/she will also be willing to pay the new higher amount. (Only a very small proportion of the sample changed their responses from a "yes" to a "no". See Table 1.) Similarly, a respondent is only asked to consider a lower threshold if he/she was unwilling to pay the first offered amount. An unwillingness also to pay the lower amount is not surprising. This tendency makes it seem, for the second question by itself, that the higher the offered amount, the more likely people are to be willing to pay it, and the lower the threshold, the less likely people are to be willing to pay. This is the opposite of what one would expect with exogenously assigned thresholds.

For this data set, the endogeneity bias is sufficiently severe so as to produce absurd results which serve to cue the researcher that something is amiss. For other data sets, the bias may be present to a much lesser extent. In some cases, the distortion from assuming a zero correlation may go unnoticed.

5. Estimates under the Standard Assumptions

Previous studies using dichotomous choice with follow-up CVM questions have assumed that $y_{1i} = y_{2i} = y_i$, so that $\beta_1 = \beta_2$, $\sigma_1 = \sigma_2$, and $\rho = 1$. The likelihood for the general model in (2) above is undefined if $\rho = 1$, so we must construct a new likelihood function appropriate to this case. For each respondent, the two offered thresholds, t_{1i} and t_{2i}, then serve to divide the range of y_i into three regions. The two discrete responses, I_{1i} and

I_{2i}, can be viewed as identifying which of these three regions contains the implicit valuation of the respondent.[4]

Define R_{1i}, R_{2i}, R_{3i}, $T_{\ell i}$ and T_{ui} as follows:

(3) $\qquad R_{1i} = 1$ and $\qquad T_{ui} = t_{2i}$ if ($I_{1i} = 0$ and $I_{2i} = 0$);

$\qquad R_{2i} = 1$ and $\qquad \begin{aligned} T_{ui} &= t_{2i} \\ T_{\ell i} &= t_{1i} \end{aligned}$ if ($I_{1i} = 1$ and $I_{2i} = 0$), or

$\qquad R_{2i} = 1$ and $\qquad \begin{aligned} T_{ui} &= t_{1i} \\ T_{\ell i} &= t_{2i} \end{aligned}$ if ($I_{1i} = 0$ and $I_{2i} = 1$);

$\qquad R_{3i} = 1$ and $\qquad T_{\ell i} = t_{2i}$ if ($I_{1i} = 1$ and $I_{2i} = 1$),

where each of the variables R_{ji}, $j=1,2,3$, is zero otherwise.

The likelihood function required to fit such a model can then be expressed as:

(4) $\quad$ Log L $= \Sigma_i$ (R_1 log [$\Phi((T_\ell - x'\beta)/\sigma)$]

$\qquad\qquad\qquad + R_2$ log [$\Phi((T_u - x'\beta)/\sigma) - \Phi((T_\ell - x'\beta)/\sigma)$]

$\qquad\qquad\qquad + R_3$ log [$1 - \Phi((T_u - x'\beta)/\sigma)$])

It may appear superficially that the $\rho = 1$ assumption with identical means and variances produces responses that can be analyzed like payment card interval data. If everyone received the identical two thresholds, packaged software for interval data could readily be employed (e.g. LIMDEP).

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[4] Hanemann, Loomis, and Kanninen (1991) utilize this assumption in their analysis of willingness to pay for wetlands in the San Joaquin Valley of California. They use an alternative but equivalent formulation of the likelihood function that preserves the four possible pairs of responses (YY,YN,NY,NN) for each observation. We opt first to identify the three effective intervals facing each individual. This highlights the correspondence between these models and payment-card interval data models with fixed thresholds across respondents.

Each pair of thresholds does indeed divide the range of the implicit

underlying valuation variable. However, most packaged software programs

require that everyone face the identical payment card. Here, there are many

different "cards." At present, only the SURVIVAL subroutine in the SYSTAT

package analysis package appears to be able to analyze interval data in

which the intervals are individual-specific. But general function-

optimizing software can always be used.[5]

In Table 2, the row for Model 7 shows the parameter estimates when the

underlying valuations y_1 and y_2 are assumed to be identical. Clearly, this

assumption vastly biases both the implied mean value and the dispersion

estimate. It also causes a sharp decrease in the maximized value of the

log-likelihood function. These results are striking because the assumption

underlying them--that identical implicit valuations are being elicited by

the two questions--would seem a perfectly plausible working hypothesis in

many applications. Such an hypothesis is implicit in the analyses by Carson

and Mitchell (1987) and by Hanemann, Loomis, and Kanninen (1991).

For this particular data set, it is *very* clear that the error

correlation across the two implicit valuations is very strongly

significantly different from both zero and one. For Model 1, the 95%

confidence interval for our estimate of ρ is (0.9262, 0.9756). Table 2 also

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[5] In Cameron and Huppert (1991), discrete-choice CVM with followup response data were simulated from actual payment card data to illustrate the potential efficiency gains from appending the followup question to a single discrete choice question in the absence of other distortions or biases. It was assumed that the true distribution of the unobserved valuation y was identical across all simulated formats: payment card, single dichotomous choice, and dichotomous choice with followup. The alternative specifications examined in this paper clearly require actual data.

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shows how drastically the value of the maximized log-likelihood function is compromised by imposing either $\rho = 0$ or $\rho = 1$. Given the distortions in the estimates of the location and scale parameters when either of these assumptions is imposed, it seems clearly necessary to estimate ρ explicitly before contemplating the imposition of either restriction.

It is interesting to explore the sources of the bias in the valuation distribution point estimates due to Model 7. We suspect that the starting point (i.e. the first offered value) may be unduly influential when this model is assumed. In a more standard context, starting point bias can be interpreted as the effect of suggested values acting as cues for respondents who have no implicit agenda and are motivated only to provide a "correct" answer to a survey question. In a problem of environmental valuation, however, subjects can potentially be sending much more complex messages in their valuation responses. For example, "I'm in favor of environmental preservation but not if it's 'too' expensive." This message can be contained in a dichotomous yes/no response to a CVM question. Similarly, a message like "Environmental issues are above monetary concerns" can be sent by expressing WTP in excess of income.

To explore the influence of starting points in Model 7, as opposed to Models 1 and 2, we can include dummy variables in each specification for first bids of $20, $50, and $100. The omitted category will be the $5 first bid. Table 3 displays the results of these specifications. The footnotes to the table detail how the results for Model 1' show that the two sets of starting point dummies have no statistically significant joint effect upon the fitted means implied for either the initial or the followup valuation

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questions. The findings for Model 2' are similar, although only one set of dummy variables is involved because $\beta_1 = \beta_2$ in this specification. While none of the mean-shifting terms is statistically significant in these models, there is weak evidence in the point values suggesting that the implied mean of the value distribution may vary inversely with the magnitude of the starting bid.[6]

In contrast, Model 7' demonstrates that starting point effects are definitely present under the standard assumptions of $\beta_1 = \beta_2$, $\sigma_1 = \sigma_2$, and $\rho = 1$. The fitted mean WTP decreases from \$745 to \$584 to \$430 to \$256 as the first offered value increases from \$5 to \$20 to \$50 to \$100 respectively.[7] These differences are surprisingly large and the null hypothesis of no difference in mean WTP across starting points is firmly rejected by a likelihood ratio test.

6. Implications of our Estimated Models

We have restricted the analysis in this paper merely to determination of the location and scale of the implicit univariate marginal distributions of (a variable we label as) WTP at the instant of the initial, and then the

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[6] If, in Model 2', we restrict the starting point to have a *linear* effect on the fitted mean WTP, the coefficient on the initial offer bears an estimated coefficient of -0.39 (with a t-ratio of -1.37). Higher starting values have a small and insignificant effect upon fitted mean WTP. Note that Model 1' cannot be converted to a model with a linear starting point effect, since the coefficient on the starting point variable (in the model for the mean at the instant of the first question) would not be identified.

[7] If we impose upon Model 7' the assumption that the fitted WTP is linearly related to the starting value, the starting value bears a coefficient of -4.84 (with a t-ratio of -3.22). On average, for each dollar higher is the initial offered value, the fitted WTP is lower by \$4.84. This is a substantial and statistically significant effect.

follow-up, contingent valuation questions. We have assumed normality for the distributions of the true underlying values. For this particular data set, we are unable to reject the hypothesis that the mean WTP elicited by the first and second questions is the same. However, we can reject the assumption that the variances are identical. There appears to be greater dispersion in WTP values for the second question. It would seem natural to attribute this greater dispersion to the possibility that at least some portion of the sample is disconcerted by the follow-up question, or that opportunities for strategic responses have been perceived. We explore possible interpretations in much greater depth in Cameron and Quiggin (1992).

Model 2 appears to be preferred among the simple specifications (without regressors) examined in this exercise. With the large estimated values for the dispersion parameters, however, it is clear that many negative values of WTP are implied. While negative values of WTP might actually exist for some members of the population, policy makers may wish to place a minimum bound of zero on WTP in calculating a "mean WTP" for the environmental good in question.

We can take models where the fitted normal distributions imply some negative values and calculate the revised marginal mean if implied negative values are arbitrarily converted to zeros. This requires formulas for the expected value of a truncated normal distribution. If X is $N(0,1)$, and we limit the domain of X to $X \geq c$, then $E(X)$ is $\phi(c)/(1-\Phi(c))$. From Model 2, for the first question, the mean of WTP over *only* the group with positive values is $380.53. However, the location and scale parameters imply that

35.98% of WTP values are negative. If the values for the group with negative WTP are set equal to zero, the overall mean of WTP from this question (across the two groups) is $243.61. A similar strategy can be employed for the fitted distribution of WTP from the second question. The mean of WTP over *just* the group with positive values is $475.07 (due to the significantly larger variance in this distribution). If the 39.11% of negative values are converted to zeros, however, the overall mean across the positive and zero groups is $289.28.

7. Conclusions

It is critically important, when analyzing responses from dichotomous choice with follow-up contingent valuation survey, to acknowledge the imperfect correlation between the responses to the first and second valuation questions. This simple illustrative example has highlighted the fact that serious biases can be introduced into valuation estimates by erroneously constraining the correlation to be either zero or unity. Furthermore, constraining the correlation to unity also appears to exacerbate starting point bias in our example.

We have not entertained more-general models which introduce the complexity of additional covariates. When covariates are available, they frequently contribute substantially to the explanation of systematic variation in fitted valuations across individuals. Covariates can be especially useful because their presence reduces the *conditional* dispersion of the unobserved true valuation (σ in our specifications). In other work, (Cameron and Quiggin, 1992) we generalize the models we have introduced in this paper to accommodate covariates and a variety of alternative functional

forms.

While our example is only illustrative, it successfully demonstrates how researchers can examine the stability of the implicit valuation distribution across the two stages of questioning in a dichotomous choice with follow-up survey. The results will vary with the precise wording of the questionnaire, and with myriad other factors. We find in this case that mean WTP does not appear to vary significantly across the two questions, but the dispersion of valuations increases for the second question. Clearly, something is happening as a consequence of the posing of the second question. We hypothesize the difference to be an artifact of some sort of strategic behavior, but its sources should certainly be the subject of future inquiry.

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TABLE 1

Descriptive Statistics
(n - 1013)

Acronym	Description	Mean	Std. Dev.
t_1	Exogenous threshold for first question	$43.79	$36.29
t_2	Endogenous threshold for second question	$67.62	$77.85
I_1	Discrete response to first question (1 - yes, WTP amount; 0 - no, not WTP)	0.5892	
I_2	Discrete response to second question (1 - yes, WTP amount; 0 - no, not WTP)	0.5578	

Joint frequencies of discrete responses:

$I_1 - 1$ and $I_2 - 1$	0.5084
$I_1 - 1$ and $I_2 - 0$	0.08983
$I_1 - 0$ and $I_2 - 0$	0.3520
$I_1 - 0$ and $I_2 - 1$	0.04936

Responses:		YY NY	YN NN	%YY %NY	%YN %NN
Thresholds: 1st(2nd)	n				
$5(20/2)	253	150 7	17 79	59.3% 2.8%	6.7% 31.2%
$20(50/5)	252	136 11	20 85	54.0% 4.4%	7.9% 33.7%
$50(100/20)	255	124 15	23 93	48.6% 5.9%	9.0% 36.5%
$100(250/50)	253	105 17	31 100	41.5% 6.7%	12.3% 39.5%

TABLE 2

Estimation Results for General and Restricted Models
(n - 1013)

Statistic	β_1	β_2	σ_1	σ_2	ρ[a]	LogL
Model 1	$128.77 (6.88)	$146.06 (7.08)	$339.51 (5.07)	$510.62 (6.09)	0.9509 (77.12)	-1080.86
Model 2	144.82 (6.94)	-	403.42 (6.65)	523.72 (5.60)	0.9493 (76.04)	-1081.10
Model 3	150.22 (6.31)	-	472.42 (5.88)	-	0.9520 (78.22)	-1083.83
Model 4	-5.34 (-0.44)	-	-300.74 (-6.80)	-	0	-1355.20
Model 5	123.16 (4.08)	-	317.14 (2.87)	-	0	-678.35[b]
Model 6	-	36.28 (4.53)	-	-186.49 (-9.19)	0	-648.36
Model 7	501.10 (8.74)	-	1437.81 (12.69)	-	1	-1364.55

[a] It may be useful in some applications to have the algorithm estimate $\rho^* = (1-exp(-\rho))/(1+exp(-\rho))$, in order to constrain the estimated value of ρ to lie strictly within the (-1,+1) interval. Here, we had no problem estimating ρ itself.

[b] The sum of the separate likelihoods from Models 5 and 6 is -1326.71. This sum is comparable to the maximized log-likelihood values for models 1 through 4 and model 7.

TABLE 3

Starting Point Effects in Models 1, 2, and 7

	Model 1	Model 2	Model 7
β_1	$133.90 (0.18)	$187.60 (4.50)	$745.28 (6.46)
$\Delta\beta_1$ ($20)	-20.54 (-0.10)	-35.37 (-0.56)	-161.39 (-1.05)
$\Delta\beta_1$ ($50)	-25.02 (-0.06)	-54.23 (-1.48)	-314.75 (-2.05)
$\Delta\beta_1$ ($100)	-44.22 (-0.01)	-45.82 (-1.50)	-488.71 (-3.15)
β_2	$178.29 (2.71)	-	-
$\Delta\beta_2$ ($20)	-32.71 (-0.53)	-	-
$\Delta\beta_2$ ($50)	-50.21 (-0.74)	-	-
$\Delta\beta_2$ ($100)	-29.61 (-0.37)	-	-
σ_1	309.22 (0.17)	437.53 (4.25)	1427.91 (12.69)
σ_2	535.89 (3.31)	565.38 (4.19)	-
ρ	0.9491 (62.04)	0.9470 (71.42)	1
Log L	-1080.44[a]	-1080.48[b]	-1358.78[c]

[a] Maximized log-likelihood for Model 1 without starting point dummy variables wwas -1080.86. Chi-squared test statistic for the hypothesis that all six of the dummy coefficients are zero is only 0.84, so this hypothesis cannot be rejected.

[b] Maximized log-likelihood for Model 2 without these three starting point dummy variables was -1081.10. Chi-squared test statistic is only 1.24. Starting point effects are insignificant.

[c] Maximized log-likelihood for Model 7 without starting point dummies was -1364.55. Chi-squared test statistic for restricting these three dummies to have zero coefficients is 11.54. Critical value is 7.81, so starting point effects are indeed statistically significant in this model.