Nonparametric Bounds on Welfare with Measurement Error in Prices: Techniques for Non-Market Resource Valuation

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Nonparametric techniques are frequently applied in recreation demand studies when researchers are concerned that parametric utility specifications impart bias upon welfare estimates. A goal of this paper is to extend previous work on nonparametric bounds for welfare measures to allow for measurement errors in travel costs. Haab and McConnell (2002) state that issues in travel time valuation continue to be topical in the recreational demand literature. This paper introduces a bootstrap augmented nonparametric procedure to precisely bound welfare when price data contains measurement error. The technique can be extended and becomes more convenient relative to other approaches when more than two site visits are made by a single recreationist. These techniques are demonstrated in a Monte Carlo experiment.

Key Words: nonparametrics, welfare estimation, bootstrap, recreation demand, nonmarket valuation

Interest in nonparametric methods has arisen for several reasons in applied environmental studies. Among the reasons is the concern that researcher assumptions regarding model parameterizations impose potentially misspecified structure and value bias on the resulting welfare estimates. For example, nonmarket valuation methods typically require the analyst to specify a functional form (e.g., a demand, bid, utility, or hedonic price function). In these settings, the analyst can perform goodness-of-fit tests or use other tools to choose among functional forms. However, there is rarely a preponderance of statistical evidence that suggests the choice of one functional form over another. This implies that in some instances judgments made by the researcher will influence model estimates.

The literature has shown that the choice of functional form for the demand function has had significant impact on the magnitude of the resulting welfare estimates (Ziemer, Musser, and Hill 1980, Kling 1989, Ozuna, Jones, and Capps 1993). Given this sensitivity to functional form, it is natural to consider whether nonparametric methods such as those refined and developed by Varian (1982, 1983a, 1983b, 1984, and 1985) and Crooker and Kling (2000) might be of value in nonmarket welfare analysis.

Our goal in this research is to extend previous work on nonparametric bounds for welfare measures to allow for stochastic shock terms. For completeness, we mimic a procedure laid out in Crooker and Kling (2000) to construct upper and lower bounds on each consumer's compensating variation for an environmental improvement. The interesting feature of this procedure is that the bounds are derived using only observed quantity and prices of visits to recreation areas without resorting to any parametric assumptions on demand or utility. As Crooker and Kling (2000) demonstrate, there is potential for the bounds to be policy-relevant in a contingent behavior context, as we are able to tighten the bounds with each successive data point for each individual. However, this previous analysis ignores the consequences of measurement error in the formation of travel costs associated with the recreation site.

This is noteworthy because researchers assume that site visitors respond to changes in travel costs as they would to a change in admission price. This necessitates that the analyst be very accurate in measuring travel costs (Freeman 1993). In the applied setting, there are several issues that give

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rise to measurement errors. Freeman (1993) points out that researchers assume the following: that the sole purpose of a trip is to visit the site, that all visits entail the same amount of time spent at the site, that no utility or disutility can be derived from the trip itself, and that the wage rate is the relevant opportunity cost of time (or perhaps a fraction of it). Also, since costs incurred in traveling to visit the site are likely large portions of the total opportunity cost, calculating these costs are fundamental to estimating behavioral models in this setting (Hotelling 1949). However, the researcher rarely possesses direct methods to assess these costs, and much of these costs may be unobservable (Randall 1994). For these and related reasons, Englin and Shonkwiler (1995) formulate a recreation demand model that treats travel costs as a latent variable.

Further, early studies have suggested that travel time to the site be included as a component of the implicit price of attending a site, yet the rate at which to value this time is not obvious (Knetsch 1963, Scott 1965, Cesario and Knetsch 1970). Recently, Hagerty and Moeltner (2005) suggested that misperceptions associated with the automotive components of travel costs can generate misspecified implicit prices in recreation demand studies. The issue of travel time valuation continues to be topical in the recreation demand literature (Haab and McConnell 2002, Moons et al. 2001). These issues suggest that measurement error exists in the formulation of travel costs and that ignoring these errors will in general result in biased welfare values for either a parametric or nonparametric approach (we demonstrate this bias in the section "Nonparametric Bounds in the Presence of Measurement Error").

In this paper, we augment the bounds technique suggested by Crooker and Kling (2000) to incorporate the presence of measurement error. Further, with the aid of Monte Carlo simulations we find that we can still bound true welfare with a high level of precision. Our technique also allows the researcher to examine the trade-off between the level of uncertainty in the travel costs and the resulting width of the bounds, thus allowing the researcher to relate the policy relevance of the bounds to the precision of the data. In addition, formulation of the bounds allows us to assess the consistency of traditional parametric estimation techniques with these true bounds on welfare.

The remainder of this paper is divided into five sections. In the next section we give an overview of the methodology for deriving nonparametric bounds on willingness to pay (WTP) for a price change from observed data when there is no measurement error (i.e., the researcher has perfect knowledge as to the travel costs each recreationist perceives). In the section "Nonparametric Bounds in the Presence of Measurement Error," we analyze the impact of measurement error on the nonparametric bounds. Subsequently we present a methodology to augment the nonparametric bounds approach that is appropriate when measurement error is present. The final two sections of the paper report the results of a Monte Carlo study to assess the appropriateness of the bootstrap augmented nonparametric bounds approach and presents our conclusions.

Using Observed Data to Compute Bounds on Compensating Variation

Bounds Based on One Data Point for Each Individual

Crooker and Kling (2000) illustrate a technique proposed by Varian (1982) to construct nonparametric bounds on indifference curves. To ensure that bounds on compensating variation (CV) are truly nonparametric, these bounds must include the CV that would arise from any standard utility specification. As perfect complements and perfect substitutes are the bookends to the universe of acceptable specifications, these specifications are used to form the bounds on CV (Varian 1982).

Immediately, our focus is on bounding an individual's willingness to pay to receive a decrease in the price of attending a recreation site—that is, the individual's CV. The individual's objective is to maximize utility subject to a budget constraint by choosing combinations of a recreation good (v) and a composite commodity (z). Suppose that at initial prices the individual satisfies this objective at $\mathbf{X}_0 = \{(z_0, v_0) : z_0, v_0 \in R_+\}$. Let M represent the individual's income level, and let $\mathbf{P}_0 = \{p_z^0, p_v^0\}$ and $\mathbf{P}_0 = \{p_z^N, p_v^N\}$ be the prices before and after the price change, respectively. For the cases examined below, we will consider only the case for a price decrease in the recreation good. This implies $p_z^0 = p_z^N$.

Allowing preferences to span the space of acceptable utility specifications results in welfare bounds being formed based on the extreme cases of perfect complements and perfect substitutes. That is, the true bounds on CV are

$$(1) \qquad B_{\mathrm{CV}}^{O} = \left\{ M - \mathbf{P}_N \cdot \mathbf{X}_0, M - p_v^N \left(\frac{M}{p_v^0} \right) \right\}.$$

The CV subscript on the term reflects the fact that this bound comes from bounds on CV, while the superscript *O* indicates that these bounds were formed only from observing the original commodity bundle.[1]

Bounds Based on Two Data Points for Each Individual

There are several different ways that a researcher may observe multiple data points for each individual at a particular recreation site. In the design of a survey instrument, contingent behavior questions may be posed to the respondent. These questions may elicit intended visits to a site at various prices. Price changes may take the form of access fees, licenses, or even additional access points (Burt and Brewer 1971). Bockstael and McConnell (1999) argue that respondents may more easily imagine facing different prices and other site characteristics than traditional contingent behavior scenarios. Moreover, data may be collected on site visitation over multiple seasons with different prices in each season.[2] As we presently explore, knowledge of additional data points allows us to tighten the welfare bounds.

An innovation presented in Crooker and Kling (2000) is the use of Hicksian welfare properties and knowledge of additional chosen commodity bundles at various prices to further tighten the bounds on CV. In particular, knowledge of a second data point, say $\mathbf{X}_N$ chosen at price vector $\mathbf{P}_N$, allows us to compute bounds on equivalent variation (EV) for a price change from $\mathbf{P}_O$ to $\mathbf{P}_N$. This is relevant to our task of bounding the individual's WTP for a price decrease because, as Crooker and Kling (2000) illustrate for a normal good, EV ≥ CV. Thus, a true upper bound on EV is also a true upper bound on CV. This second data point is an improvement if the upper bound on EV, call it $\bar{M}_{\mathrm{EV}}^{N}$, is closer to M than $p_v^N(M/p_v^O)$.

Equivalent variation for the price decrease is defined as

$$(2) \qquad \begin{aligned} \mathrm{EV} &= e(\mathbf{P}_0, U_N) = e(\mathbf{P}_N, U_N) \\ &= e(\mathbf{P}_0, U_N) - M, \end{aligned}$$

where $U_N \equiv U_N(z_N, v_N)$ (i.e., the optimal choice of the composite commodity and recreation visits at new prices). Again, following Crooker and Kling (2000), if we can bound the term $e(\mathbf{P}_0, U_N)$, we can bound equivalent variation for the price change. In other words, the term $e(\mathbf{P}_0, U_N)$ is the minimum expenditure necessary at price $\mathbf{P}_0$ to achieve utility level U_N. As argued above, the highest expenditure level necessary to maintain utility at U_N will occur if the two goods are perfect complements. Thus, the consumer would have to be guaranteed commodity bundle $\mathbf{X}_N$ at price $\mathbf{P}_0$ to keep utility at least at U_N. This implies $\bar{M}_{\mathrm{EV}}^{N} = \mathbf{P}_0 \cdot \mathbf{X}_N$. Using knowledge of both data points allows us to write the bounds on CV as

$$(3) \quad B_{\mathrm{CV}}^{ON} \equiv \left\{ M - p_v^N v_0, \;\; Min \begin{pmatrix} M - p_v^N \left(\dfrac{M}{p_v^O} \right), \\ \bar{M}_{\mathrm{EV}}^{N} - M \end{pmatrix} \right\}.$$

The superscripts on *B* indicate that both points are used in constructing the bounds. The only parametric assumption we have made to this point is that recreation goods are a normal good (i.e., the income effect is nonnegative).[3]

The upper bound on CV will be most concisely bounded by $\bar{M}_{\mathrm{EV}}^{N} - M$ in natural resource applications. To see this, note that the expression $M - p_v^N(M/p_v^0)$ may be equivalently expressed as

$$\frac{M}{p_v^0}(p_v^0 - p_v^N).$$

[1] Crooker and Kling (2000) formally discuss the formation of this bound.

[2] We would like to thank the anonymous reviewer who suggested this application. In this application, hypothetical bias is eliminated as we use actual trip data.

[3] See Crooker and Kling (2000) for a discussion on using the bounds with inferior goods.

Similarly, we may write $\overline{M}^{N}_{\text{EV}} - M$ as $p_v^0 v_N + p_z^0 z_N - p_v^N v_N - p_z^N z_N$. Given that only the price of the recreation good has changed, this term simplifies to $(p_v^0 - p_v^N)v_N$. If $M - p_v^N(M/p_v^0)$ is a more concise upper bound on CV, it must be that $M - p_v^N(M/p_v^0) < \overline{M}^{N}_{\text{EV}} - M$. However, this requires $M/p_v^0(p_v^0 - p_v^N) < (p_v^0 - p_v^N)v_N$. That is, $v_N > M/p_v^0$. The ratio of income to the original price of the recreation good represents the most recreation visits the consumer could afford if he or she devoted all income to recreation visits. The only way that $M - p_v^N(M/p_v^0)$ will be the tighter bound on CV is if, after the price decrease, the consumer takes a previously unaffordable level of trips. Again, in most recreation demand applications, this is certainly not the case, as most studies seem to point to small shares of income devoted to recreation resource use. In fact, this may suggest a method to check for unreliable observations.

Nonparametric Bounds in the Presence of Measurement Error

In applied scenarios, we will have collected site visitation data as well as socio-demographic information, which includes the price of attending a recreation site and income data. The nonparametric bounds, however, are not functions of income. This feature is demonstrated as follows. From the lower bound given in equation (3), note that

$$(4) \quad M - \mathbf{P}_N \cdot \mathbf{X}_0 = \mathbf{P}_0 \cdot \mathbf{X}_0 - \mathbf{P}_N \cdot \mathbf{X}_0 = (\mathbf{P}_0 - \mathbf{P}_N) \cdot \mathbf{X}_0 .$$

We can rewrite this expression as

$$(5) \qquad \left[\begin{pmatrix} p_{v,i}^0 \\ p_{z,i}^0 \end{pmatrix} - \begin{pmatrix} p_{v,i}^N \\ p_{z,i}^N \end{pmatrix}\right] \cdot \begin{pmatrix} v_i^0 \\ z_i^0 \end{pmatrix}.$$

In the scenario we are investigating, there is no change in the price of the composite good. Thus, the lower bound for an individual may be simply stated as

$$(6) \qquad LB_i = -(p_{v,i}^N - p_{v,i}^O)v_i^O = dp_i \cdot v_i^O ,$$

where the subscript i denotes individual i in the sample, dp_i is the individual's price change proposed in the contingent behavior survey, and v_i^O is the number of times the individual visited the site before the price change. Similar to the restatement of the lower bound, we may also restate the upper bound given in equation (3) as

$$(7) \qquad UB_i = \left[\begin{pmatrix} p_{v,i}^O \\ p_{z,i}^O \end{pmatrix} - \begin{pmatrix} p_{v,i}^N \\ p_{z,i}^N \end{pmatrix}\right] \cdot \begin{pmatrix} v_i^N \\ z_i^N \end{pmatrix} = dp_i v_i^N ,$$

where v_i^N is the individual's site visitation after the price change, assuming that the share of income devoted to site visitation is not exceptionally large.

The nonparametric bounds will be valid bounds on compensating variation as long as the data on each individual is perfectly accurate, including the analyst's calculation of travel costs. However, if the calculated implicit price of attending the site differs from the perceived costs to the recreationist, the nonparametric bounds based on the data may be inaccurate. There are many reasons to believe that the individual does not respond exactly to the researcher's calculated implicit price. Individuals may enjoy the drive to the recreation site (scenery, wildlife viewing, interaction with other recreationists, etc.). They may have other motivations for attending the site or may visit multiple sites. Also, the individual may not weigh the opportunity cost of time as the researcher has calculated (perhaps employment opportunities after 40 hours per week are not equal to or a fraction of the individual's regular wage rate).[4] This implies that we may model calculated travel costs to be of the following form:

$$(8) \qquad p_{v,i}^O = \tilde{p}_{v,i}^O + \varepsilon_i^O \text{ and } p_{v,i}^N = \tilde{p}_{v,i}^N + \varepsilon_i^N ,$$

where $\tilde{p}_{v,i}^o$ and $\tilde{p}_{v,i}^N$ are the actual travel costs the recreationist responds to; however, they are unknown to the analyst. Again, $p_{v,i}^o$ and $p_{v,i}^N$ are the calculated travel costs the researcher forms based on the collected data set, and they contain the noise terms ε_i^O and ε_i^N, which represent the errors in implicit price calculations. If the researcher

[4] Randall (1994) and McKean, Johnson, and Walsh (1995) have questioned the appropriateness of using a fraction of the wage rate as the relevant opportunity cost of time despite this approach being a common approach in recreation demand modeling (Freeman 1993).

constructs the nonparametric bounds ignoring the impact of any error, the bounds become

$$(9) \qquad \left\{ d\tilde{p}_i v_i^O + \left(\varepsilon_i^O - \varepsilon_i^N\right) v_i^O,\ d\tilde{p}_i v_i^N + \left(\varepsilon_i^O - \varepsilon_i^N\right) v_i^N \right\}.$$

If $(\varepsilon_i^O - \varepsilon_i^N) > 0$ (and we are considering a price decrease), then the lower bound is mistakenly constructed too high by the amount $(\varepsilon_i^O - \varepsilon_i^N)v_i^O$. If $(\varepsilon_i^O - \varepsilon_i^N) < 0$, then the upper bound is mistakenly constructed too low by the amount $-(\varepsilon_i^O - \varepsilon_i^N)v_i^N$. Thus, the incorporation of measurement error into the nonparametric bounds methodology implies that the bounds as Crooker and Kling (2000) have constructed them and as we have presented them above are not necessarily valid bounds on welfare.[5] As we expect that even under a best-case scenario our data will contain noise, this is a troubling implication.

Our approach to account for this noise is to adopt a procedure laid out by Varian (1985) to ensure that our nonparametric bounds retain satisfactory coverage properties. This treatment we prescribe is noteworthy for several reasons. Many studies have explored the dilemmas inherent in calculating implicit prices and expressed concern for probable errors (Knetsch 1963, Scott 1965, Cesario and Knetsch 1970, Cesario 1976, McConnell and Strand 1981, Bockstael, Strand, and Hanemann 1987, Freeman 1993, Randall 1994, Englin and Shonkwiler 1995); however, these studies have not formally addressed the effects of these errors on the resulting welfare estimates. Moreover, the development of the nonparametric bounds in the literature does not account for the likely errors in travel cost formations.

Crooker and Kling (2000) present welfare bounds based on three data points for each individual in the sample and suggest that with additional data points, welfare bounds may be further tightened. Our purpose in this analysis is to highlight the implications when errors exist in prices. The addition of more data points simply imparts additional stochastic terms in the bound formulations. This suggests that the stochastic terms, if correlated, will adversely influence welfare bounds when errors are not modeled. In these instances, the techniques described here remain valid. For this reason and interest in brevity, we do not consider the implications of three data points in the body of the paper. However, an analysis for three data points is included in Appendix A.[6]

Bootstrap Augmented Nonparametric Technique

Varian (1985) suggests that the presence of measurement error is one possible explanation for detecting violations in the Weak Axiom of Cost Minimization (WACM) in observed firm data. The author develops a methodology to test the hypothesis that the firm data is consistent with WACM subject to a measurement error. The researcher can then assess the level of variability in the observed data necessary to satisfy the proposition that firms are actually satisfying WACM. This is the spirit of our analysis with a few departures. In this paper, we are not interested at this point in testing the hypothesis that consumers minimize their expenditures subject to some standard of utility (though this is a useful exercise). Instead, we are interested in the effects of measurement error on the width of our nonparametric bounds. As we have seen above, the presence of measurement error in the calculation of travel costs will imply that these bounds as currently formed are not always accurate.

We will now present a statistical methodology that will allow us to increase the coverage of the true bounds on welfare. This methodology allows us to maintain our claim that this technique is nonparametric. That is, our statistical formulation does not rely upon any parametric specification of utility structure. In a closely related production setting, Varian (1985) suggests taking draws from a normal distribution. He argues that these draws do not diminish the nonparametric specification of the firm's production function. In traditional econometric demand applications, researchers make a parametric assumption regarding the structure of demand (e.g., linear) and a parametric assumption regarding the structure of the stochastic error term (e.g., normal). Studies that fo-

[5] As suggested by an anonymous reviewer, the Crooker and Kling (2000) constructed bounds are entirely appropriate if the error terms are identical. That is, if the measurement error in prices is equivalent across collected data sets, the difference in stochastic components falls to zero.

[6] Appendix A is available online at http://faculty.cmsu.edu/crooker/ARER.

cus on relaxing the parametric imposition on the nonstochastic portion of the model while retaining the parametric stochastic assumptions are often termed nonparametric (e.g., Varian 1985). Still other studies may focus on relaxing the parametric imposition on the stochastic portion of the model while retaining parametric restrictions on the nonstochastic component of the model, and term these investigations nonparametric. Our approach is consistent with the Varian (1985) nonparametric investigation.

We suggest extending Varian's technique to this recreation demand setting. In the present context, this means the researcher employs a parametric distributional assumption concerning the measurement error terms ε_i^O and ε_i^N. We assume the following:

$$(10) \qquad \left(\varepsilon_i^O, \varepsilon_i^N\right) \sim BVN\left(\begin{pmatrix}0\\0\end{pmatrix}, \begin{pmatrix}\sigma_O^2 & \sigma_{ON}\\ \sigma_{ON} & \sigma_N^2\end{pmatrix}\right).$$

To perform the bootstrap, the researcher will need to specify a

$$\Sigma = \begin{pmatrix}\sigma_O^2 & \sigma_{ON}\\ \sigma_{ON} & \sigma_N^2\end{pmatrix}.$$

However, we do not believe this specification will be prohibitively problematic for the applied researcher. This is because the researcher can use a range of various parameterizations of Σ. The bootstrap technique we present can speedily calculate the new bounds even when more than two data points are gathered for some recreationists. The analytical derivation of the bounds becomes untractable beyond two data points, as demonstrated in Appendix B.[7] Upon examining the resulting nonparametric bounds generated by this procedure, the researcher would need to make a determination as to whether or not the parameterization of Σ that makes the bounds no longer policy-relevant is a realistic possibility in the application at hand [Varian's (1985) work was concerned with the parameterization of Σ that results in production decisions being consistent with the Weak Axiom of Cost Minimization]. We should also point out that the researcher could make the parameterization of Σ individual-specific. This is relevant because attitudinal questions asked in the survey instrument may reveal additional insights into how the recreationist reacts to implicit prices. Researchers may begin to formally investigate these attitudes as well. Seminal work in travel cost models has explored the proper calculation of the opportunity cost of time (Cesario 1976, Bockstael, Strand, and Hanemann 1987, and McConnell and Strand 1981). This suggests that the researcher could parameterize Σ_i on the basis of these attitudes. To the extent that the researcher could do this accurately, it is reasonable to expect that the resulting nonparametric bounds will be further refined.

Succinctly, we are suggesting that the researcher formulate Σ_i based on sample estimates. Next, the researcher may take draws from the parametric distribution expressed in equation (10) above. This allows the researcher to construct computer-simulated replicate samples. In the statistics literature, this process is known as the parametric bootstrap (Efron and Tibshirani 1993). For each sample replication, the researcher calculates the statistics of interest (in our application, this will be the average lower bound and upper bound). Doing this for many computer replications allows us to construct a distribution of the statistics of interest. Provided that our parametric distributional assumption is accurate and that we create a sufficiently large number of replicated samples, our simulated distribution will resemble the unobserved true distribution of welfare. We will now formally present the parametric bootstrap procedure step by step.

The bootstrap procedure is as follows:[8]

- Draw $\left(\eta_i^{(b),O}, \eta_i^{(b),N}\right) \sim BNV\left(\begin{pmatrix}0\\0\end{pmatrix}, \Sigma_i\right)$ for $i = 1, 2, \ldots, n.$
- Form $p_i^{(b),O} = p_i^O + \eta_i^{(b),O}$ and $p_i^{(b),N} = p_i^N + \eta_i^{(b),N}$.

[7] Appendix B is available online at http://faculty.cmsu.edu/crooker/ARER.

[8] For background on the bootstrap, see Efron and Tibshirani (1993).

- Construct $LB_i^{(b)} = dp_i^{(b)} v_i^O$ and $UB_i^{(b)} = dp_i^{(b)} v_i^N$.

- Calculate $\overline{LB}^{(b)} = \frac{1}{n}\sum_{i=1}^{n} LB_i^{(b)}$ and $\overline{UB}^{(b)} = \frac{1}{n}\sum_{i=1}^{n} UB_i^{(b)}$.

- Repeat the first four steps B times, storing the average bootstrap sample lower and upper bounds.

- Order the average bootstrap sample lower bounds from smallest to largest and order the average bootstrap sample upper bounds from smallest to largest.

- Use the $0.025B^{\text{th}}$-ordered average bootstrap sample lower bound as the Bootstrap Augmented Nonparametric Lower Bound on Welfare (call it $\overline{LB}_{(0.025B)}$). Similarly, use the $0.975B^{\text{th}}$-ordered average bootstrap sample upper bound as the Bootstrap Augmented Nonparametric Upper Bound on Welfare (call it $\overline{UB}_{(0.975B)}$).

Thus, so long as our assumption regarding the statistical distribution of the measurement error is an adequate representation and we choose B large enough, the resulting bootstrap augmented nonparametric bounds on welfare will be accurate.[9] We will now investigate how accurate we may expect this bootstrap procedure to be in practice when implicit price calculations do contain errors. To analyze this situation completely, we must have a controlled environment. Thus, we propose the Monte Carlo study in the next section.

We should also point out that given the parametric bootstrap we have presented in this section, closed-form analytical percentiles of the sample average lower and upper bounds can be derived. We present these derivations in Appendix C.[10] Despite the existence of this closed-form analytical result, we find the bootstrap approach important for several reasons. First, we desire to establish that the bootstrap procedure will generate an accurate confidence interval. The best way to demonstrate these good properties is to compare the bootstrap results with analytical results when the analytical results are readily available. We make this comparison in Appendix C. As the number of data points available to the researcher for a collection of individuals in the sample exceeds two, analytical results are not readily available. This implies that the approach of our bootstrap procedure is warranted. In fact, the modification to the bootstrap procedure when three data points are available, which we present in Appendix A, is very slight.

Monte Carlo Study

Design of the Study

The Monte Carlo experiment is designed with the following three questions in mind: (i) How accurate are the sample nonparametric bounds when the data contains analyst specification error? (ii) Will the use of a parametric bootstrap improve the accuracy of our welfare bounds when the data contains errors? (iii) How robust are the bootstrap augmented nonparametric bounds across data-generation mechanisms? To progress towards answering these questions, we assume that the applied researcher has access to a data set containing a revealed preference and a stated preference data point for each individual in the sample. We have in mind that the researcher may have undertaken a contingent behavior survey to collect such data. Here we assess how well the researcher could do with such a data set in accurately bounding CV using the nonparametric bounds techniques.

For each sample, we calculate the actual sample nonparametric bounds on compensating variation. While this is information that the applied researcher does not have available, we will use this information to learn about the accuracy of the bounds technique when the data contains errors in implicit price calculations. Next, we calculate the nonparametric bounds the researcher would construct if he or she assumes that the data contains no errors (call these bounds the "sample nonparametric" bounds). Thus, comparing the actual nonparametric bounds to the sample nonparametric

[9] Shao and Tu (1995) discuss the convergence properties of bootstrap techniques.

[10] Appendix C is available online at http://faculty.cmsu.edu/crooker/ARER.

bounds allows us to assess the accuracy of the bounds technique when errors in implicit price calculations are ignored.

Finally, for each sample, we will calculate the nonparametric bounds the researcher would generate when employing the bootstrap procedure discussed above to account for the errors in travel costs. Comparing these sample bootstrap nonparametric bounds with the actual nonparametric bounds again allows us to assess the accuracy of this bootstrap augmented nonparametric bounds technique. Further, comparing the bounds to the simple sample nonparametric bounds allows us to investigate the increased width due to correcting for errors in travel costs.

We will also calculate the true compensating variation for each sample as we know precisely the form of recreation demand. Again, the researcher would not have access to this information, yet it is interesting to note it here, in judging the proximity of the various bounds techniques to the true welfare values.

To learn about the robustness of the techniques we present, we will use three different data-generating mechanisms. Each of the mechanisms are standard parametric demand functions. They are

(11) log-linear: $\ln(v_i) = \alpha + \beta \ln(\tilde{p}_{v,i}) + \gamma \ln(M_i)$,

semi-log: $\ln(v_i) = \alpha + \beta \cdot \tilde{p}_{v,i} + \gamma \cdot M_i$, and

linear: $v_i = \alpha + \beta \cdot \tilde{p}_{v,i} + \gamma \cdot M_i$,

where the Greek letters correspond to parameters. These demand functions were chosen because of their common use in recreation demand modeling. The variables $\tilde{p}_{v,i}$, M_i, and v_i are the perceived price of site visits to recreationist *i* (i.e., the price that recreationist *i* is actually reacting to in making decisions), recreationist *i*'s income, and recreationist *i*'s site visits, respectively. Prices and income differ across individuals in the sample. Actual implicit price was selected at random according to a uniform distribution over the range of (5, 55). Income was selected at random according to a uniform distribution for each individual over the range of (5000, 85,000). Note that this structure implies that preferences are analogous across individuals. The only reason that site visitations vary across individuals is that the values of price and income differ.[11]

In the linear specification, the parameters α, β, and γ are 6.72, -0.004, and 0.00002, respectively. The semi-log model parameters α, β, and γ are 1.234903, -0.004, and 0.00002, respectively. Finally, the log-linear model parameters α, β, and γ are 2.028294, -0.004, and 0.00002.

To mimic the information that the researcher will have available, we form $p_{v,i} = \tilde{p}_{v,i} + \varepsilon_i$. This stochastic price reflects the portion of implicit price calculated in error by the researcher. The researcher observes only $p^O_{v,i}$, $p^N_{v,i}$, M_i, v^O_i, and v^N_i. The superscripts *O* and *N* reflect that the researcher has knowledge of two data points from each individual. Each data point is generated in the manner stated above.

The ε_i represents the calculation error. The distribution of the measurement error was set to be

$$(12) \qquad \begin{pmatrix} \varepsilon^O_i \\ \varepsilon^N_i \end{pmatrix} \sim BVN\left(\begin{pmatrix} 0 \\ 0 \end{pmatrix}, \Sigma\right)$$

for all $i = 1, 2, \ldots, n$. The *n* represents the number of observations in the sample and was set at 1,000 throughout this experiment. Also, the variance-covariance matrix was specified as

$$\Sigma_i = \begin{bmatrix} \sigma^2_i & \frac{\sigma^2_i}{4} \\ \frac{\sigma^2_i}{4} & \frac{\sigma^2_i}{2} \end{bmatrix} \tilde{p}^2_{v,i}.$$

To explore the consequences of the level of variability in the measurement error on the nonparametric bounds, we used three different levels for

$$\sigma_i \quad \left(\frac{1}{40}, \frac{1}{20}, \frac{1}{10}\right).$$

This first setting, for example, ensures us that the observed implicit price (or implicit price calculated by the researcher) is within 7.5 percent of

[11] A methodology that is consistent with neoclassical utility theory and allows for differing preferences across individuals within the same utility structure would be a random parameters model. For simplicity we do not explore the consequences of random parameters on the nonparametric bounds.

the actual price (i.e., the implicit price the individual actually responds to) for 99 percent of the simulated observations (according to the 3-sigma rule). The highest setting ensures us that the observed implicit price is within 30 percent of the actual price for 99 percent of the simulated observations. Note that in all cases, the correlation between individual specific errors in travel costs was set at 0.5. We generated 1,000 samples based on a population with these characteristics. These settings give us the sample data that the researcher has available. However, the researcher observes neither the parameters nor the ε_i's and does not know the structure of the model that generates the data set.

The results of the Monte Carlo experiment are presented in Tables 1 through 3. The columns of the tables report the true parameterization of the distribution on the measurement error. The columns headed by an "L" contain information pertaining to the lower bound, while the columns headed by a "U" refer to upper bounds. The rows titled "Actual nonparametric bounds of sampled individuals" are the nonparametric bounds we would derive if we observed the individual data without measurement error. The rows labeled "Sample nonparametric bounds" list the bounds derived with the nonparametric technique if we ignore the presence of errors in implicit price formations. The "Bootstrap Technique" section of the tables report the resulting bounds after employing the bootstrap augmented technique described above. The rows in this portion of the table refer to the parameterization of the bootstrap errors [see equation (10) above].

Before we begin analyzing the "accuracy" of the nonparametric bounds techniques, there are a few points that should be made. Though the true data-generating mechanisms range over the possibilities of linear, log-linear, and semi-log demand specifications, in all cases we compare the nonparametric bounds techniques to the true nonparametric bounds. That is, the range of possible welfare values from each parametric specification is a proper subset of the true nonparametric bounds. Thus, comparing the sample nonparametric bounds technique to the true nonparametric bounds is setting a high standard (in fact, it is the highest standard, as it is sufficiently general to cover all possible utility structures even if preferences are not uniform across the population). The reason we feel that this is appropriate is because in typical applications of these techniques, the researcher will not know the true underlying parameterization of demand. This implies that the actual welfare coverage of the nonparametric techniques are guaranteed to be at least as high as the performance reported here.

In Tables 1 through 3, we find that the bootstrap technique does result in a widening of the nonparametric bounds formed when measurement error is ignored. In fact, we see that ignoring measurement error results in bounds that do not accurately reflect the actual bounds on welfare. The bootstrap technique results in accurate bounds being formed when the bootstrapped error variability is at least as high as the actual variability in errors. These findings hold in all three demand specifications considered, and we expect this to be the case in general. In this Monte Carlo study, the cost of ignoring errors may not seem large, as the sample nonparametric bounds are "close" to the actual nonparametric bounds. However, notice that as the magnitude of the error increases, the sample nonparametric bounds ignoring errors do worse. As indicated in the design of the Monte Carlo section above, price ranged from $5 to $55 for all individuals in the sample. In settings that have a high degree of uncertainty in prices, we would anticipate poorer performance when errors are ignored. The appealing feature is that as we model these errors in the bootstrap augmented technique, we are able to construct accurate welfare bounds.

How Accurate Are the Nonparametric Bounds When Measurement Error Is Ignored?

Tables 4 through 6 list the percentage of times the sample and bootstrapped sample bounds correctly contain the true bounds on CV. The row labeled "Sample nonparametric bounds" contains information pertaining to the accuracy of the sample nonparametric bounds when measurement error is present but ignored. The columns of the tables report the true dispersion in recreation site visits. The best the sample bounds do in terms of accurately bounding true CV is a 0.3 percent accuracy performance for the case of the semi-log demand setting. In all the log-linear and the two lower variance cases in the linear formulations, the sample bounds fail to contain the true bounds in

Table 1. Linear Demand and Bounds Value

		Actual dispersion coefficient (σ_i)					
		$\sigma_i = (1/40)\tilde{p}_{v,i}$		$\sigma_i = (1/20)\tilde{p}_{v,i}$		$\sigma_i = (1/10)\tilde{p}_{v,i}$	
		L	U	L	U	L	U
Actual nonparametric bounds of sampled individuals		120.66	129.71	126.06	135.53	125.04	134.26
Sample nonparametric bounds		120.65	129.70	126.05	135.53	125.08	134.30
Bootstrap Technique							
$\sigma_i = (1/40)p_{v,i}$	$\rho = 0.1$	119.17	131.28	124.49	137.21	123.52	135.97
	$\rho = 0.5$	119.45	130.98	124.79	136.88	123.82	135.65
	$\rho = 0.9$	119.83	130.57	125.19	136.45	124.22	135.23
$\sigma_i = (1/20)p_{v,i}$	$\rho = 0.1$	117.70	132.87	122.93	138.89	121.96	137.65
	$\rho = 0.5$	118.26	132.26	123.53	138.24	122.56	137.00
	$\rho = 0.9$	119.02	131.44	124.33	137.38	123.36	136.15
$\sigma_i = (1/10)p_{v,i}$	$\rho = 0.1$	114.75	136.05	119.80	142.24	118.84	141.00
	$\rho = 0.5$	115.88	134.82	121.01	140.96	120.04	139.71
	$\rho = 0.9$	117.39	133.20	122.60	139.23	121.64	137.99

Table 2. **Log-Linear Demand and Bounds Value**

		Actual dispersion coefficient (σ_i)					
		$\sigma_i = (1/40)\tilde{p}_{v,i}$		$\sigma_i = (1/20)\tilde{p}_{v,i}$		$\sigma_i = (1/10)\tilde{p}_{v,i}$	
		L	U	L	U	L	U
Actual nonparametric bounds of sampled individuals		119.64	119.97	114.86	115.18	108.58	108.88
Sample nonparametric bounds		119.63	119.96	114.88	115.20	108.58	108.88
Bootstrap Technique							
$\sigma_i = (1/40)p_{v,i}$	$\rho = 0.1$	118.22	121.35	113.55	116.53	107.30	110.16
	$\rho = 0.5$	118.49	121.08	113.81	116.27	107.55	109.91
	$\rho = 0.9$	118.85	120.73	114.15	115.93	107.87	109.58
$\sigma_i = (1/20)p_{v,i}$	$\rho = 0.1$	116.82	122.75	112.22	117.86	106.02	111.43
	$\rho = 0.5$	117.36	122.21	112.73	117.34	106.51	110.94
	$\rho = 0.9$	118.08	121.50	113.41	116.67	107.17	110.29
$\sigma_i = (1/10)p_{v,i}$	$\rho = 0.1$	114.01	125.54	109.54	120.51	103.45	113.96
	$\rho = 0.5$	115.09	124.46	110.57	119.49	104.44	113.00
	$\rho = 0.9$	116.5	123.03	111.93	118.13	105.76	111.70

all 1,000 simulations. Clearly, this illustrates that errors in travel costs may have a serious impact on the accuracy of the sample nonparametric bounds. This suggests that methods to account for this error are an appropriate avenue of investigation.

Table 3. Semi-Log Demand and Bounds Value

		Actual dispersion coefficient (σ_i)					
		$\sigma_i = (1/40)\tilde{p}_{v,i}$		$\sigma_i = (1/20)\tilde{p}_{v,i}$		$\sigma_i = (1/10)\tilde{p}_{v,i}$	
		L	U	L	U	L	U
Actual nonparametric bounds of sampled individuals		142.03	153.45	132.17	141.81	120.65	130.08
Sample nonparametric bounds		142.01	153.44	132.14	141.90	120.69	134.71
Bootstrap Technique							
$\sigma_i = (1/40)p_{v,i}$	$\rho = 0.1$	138.50	156.92	128.88	145.12	117.33	138.04
	$\rho = 0.5$	138.50	156.93	128.88	145.12	117.33	138.04
	$\rho = 0.9$	138.50	156.93	128.88	145.12	117.33	138.03
$\sigma_i = (1/20)p_{v,i}$	$\rho = 0.1$	134.99	160.41	125.63	148.34	113.96	141.39
	$\rho = 0.5$	134.99	160.40	125.63	148.35	113.96	141.38
	$\rho = 0.9$	134.99	160.42	125.63	148.35	113.96	141.37
$\sigma_i = (1/10)p_{v,i}$	$\rho = 0.1$	124.44	170.86	115.86	158.00	103.87	151.40
	$\rho = 0.5$	124.44	170.87	115.86	158.02	103.87	151.34
	$\rho = 0.9$	124.44	170.87	115.86	158.04	103.87	151.38

Table 4. Linear Demand and Bounds Accuracy

		Actual dispersion (σ_i)		
		$\sigma_i = (1/40)\tilde{p}_{v,i}$	$\sigma_i = (1/20)\tilde{p}_{v,i}$	$\sigma_i = (1/10)\tilde{p}_{v,i}$
		% Correct	% Correct	% Correct
Sample nonparametric bounds		0.0	0.0	0.1
Bootstrap Technique				
$\sigma_i = (1/40)p_{v,i}$	$\rho = 0.1$	98.3	76.3	46.3
	$\rho = 0.5$	95.0	65.6	38.6
	$\rho = 0.9$	82.0	50.8	25.9
$\sigma_i = (1/20)p_{v,i}$	$\rho = 0.1$	100.0	98.0	76.4
	$\rho = 0.5$	100.0	94.4	66.3
	$\rho = 0.9$	99.1	80.7	49.3
$\sigma_i = (1/10)p_{v,i}$	$\rho = 0.1$	100.0	100.0	97.7
	$\rho = 0.5$	100.0	100.0	94.2
	$\rho = 0.9$	100.0	98.9	81.9

Does the Bootstrap Augmented Nonparametric Bounds Technique Improve the Accuracy and at What Cost?

Tables 4 through 6 also contain the performance of the Bootstrap Augmented Nonparametric Bounds Technique (BANBT). The rows in the Bootstrap Technique section contain the specification of the Σ matrix regarding the parametrization of (ε_i^O, ε_i^N). That is,

$$(13) \qquad \left(\varepsilon_i^O, \varepsilon_i^N\right) \sim BVN\left(\begin{pmatrix}0\\0\end{pmatrix}, \Sigma\right),$$

Table 5. Log-Linear Demand and Bounds Accuracy

		Actual dispersion (σ_i)		
		$\sigma_i = (1/40)\tilde{p}_{v,i}$	$\sigma_i = (1/20)\tilde{p}_{v,i}$	$\sigma_i = (1/10)\tilde{p}_{v,i}$
		% Correct	% Correct	% Correct
Sample nonparametric bounds		0.0	0.0	0.0
Bootstrap Technique				
$\sigma_i = (1/40)p_{v,i}$	$\rho = 0.1$	99.1	77.4	46.6
	$\rho = 0.5$	96.6	66.6	38.0
	$\rho = 0.9$	83.9	48.9	26.9
$\sigma_i = (1/20)p_{v,i}$	$\rho = 0.1$	100.0	98.6	78.6
	$\rho = 0.5$	100.0	94.8	69.2
	$\rho = 0.9$	99.4	81.3	49.7
$\sigma_i = (1/10)p_{v,i}$	$\rho = 0.1$	100.0	100.0	98.6
	$\rho = 0.5$	100.0	100.0	95.0
	$\rho = 0.9$	100.0	99.2	83.0

Table 6. Semi-Log Demand and Bounds Accuracy

		Actual dispersion (σ_i)		
		$\sigma_i = (1/40)\tilde{p}_{v,i}$	$\sigma_i = (1/20)\tilde{p}_{v,i}$	$\sigma_i = (1/10)\tilde{p}_{v,i}$
		% Correct	% Correct	% Correct
Sample nonparametric bounds		0.3	0.2	0.1
Bootstrap Technique				
$\sigma_i = (1/40)p_{v,i}$	$\rho = 0.1$	98.7	76.4	44.6
	$\rho = 0.5$	95.6	67.7	35.7
	$\rho = 0.9$	84.7	52.3	24.4
$\sigma_i = (1/20)p_{v,i}$	$\rho = 0.1$	100.0	98.0	77.2
	$\rho = 0.5$	100.0	93.1	66.9
	$\rho = 0.9$	99.6	80.7	49.0
$\sigma_i = (1/10)p_{v,i}$	$\rho = 0.1$	100.0	100.0	98.3
	$\rho = 0.5$	100.0	100.0	94.2
	$\rho = 0.9$	100.0	98.9	82.2

where

$$\Sigma = \begin{pmatrix} \sigma_O^2 & \rho\sigma_O\sigma_N \\ \rho\sigma_O\sigma_N & \sigma_N^2 \end{pmatrix}.$$

In over 50 percent of the cases examined, the BANBT contained actual sample CV upper and lower bounds at least at the desired 95 percent rate. Further, in all cases the BANBT more precisely bounded the CV bounds than the nonparametric sample bounds. Also, in every instance where the BANBT used a level of variance that was greater than the true level of variability, the BANBT accurately bounded the true CV bounds in every simulated sample. This performance suggests that the technique is reliable. However, the increased accuracy in bounding the true

CV bounds comes at some cost. The cost of improved accuracy is increased width in the bounds.

Examining the tables, we notice that as the researcher allows for greater correlation between errors, the bounds on CV narrow. If we reconsider equation (9), we see that the bounds are affected by the difference in error terms. Hence, as the correlation increases, the variability in the bounds actually decreases. Thus, allowing for greater correlation is equivalent to imposing smaller variability on our augmented bounds.

We also calculated the percentage reduction in the lower bound and percentage increase in the upper bound from the actual CV bounds.[12] We found that when the BANBT supposes a relatively small level of site visitation variability, the bounds are off at most by 2.8 percent of the true levels. However, if site visitation variability is relatively high, the lower bounds are reduced by up to 13.9 percent, while the upper bound is raised by up to 12.4 percent (these extremes were observed in the semi-log formulation). Ultimately, the expense of the BANBT approach hinges on the dollar value of policy relevance. However, this analysis suggests that the researcher could create bounds based on several parameterizations of Σ. Thus, the trade-off between precision and uncertainty in the data can be formally stated. If the data suggest that a relatively high degree of error is possible or that some peculiar aspects exist to specifying the implicit prices, one may choose a parameterization that conveys a higher degree of uncertainty.

How Robust Are the Nonparametric Bounds to Various Data-Generating Mechanisms?

The Monte Carlo study above suggests that the BANBT does well in all the parameterizations we explored. This suggests that there is potential for the BANBT to be quite robust. Further, this result is not surprising. The BANBT methodology is based on theoretical foundations that include all possible utility structures. The bootstrap augmentation ensures that the resulting bounds include the potential for error to be present in the data.

[12] In the interest of space, tables showing this are not included in this manuscript but are available from the author.

Conclusions

Our focus in this study is to investigate the likely usefulness of nonparametric bounds techniques in an applied setting. The Crooker and Kling (2000) study suggests that there is potential for the technique to be useful. However, this previous work did not explore the consequences of errors in the calculation of travel costs, which are sure to plague almost every applied travel cost estimation study (Randall 1994). Thus, the contribution of this investigation is twofold. First, we laid out the effects of the error theoretically and concluded that the nonparametric techniques developed in the literature are not necessarily correct. More significantly, we presented a methodology to augment the nonparametric approach that regains the accuracy of the nonparametric bounds techniques.

An interesting feature of this methodology is that these bounds are accurate regardless of the true underlying demand behavior. Further, it is not necessary for the researcher to assume that people are identical in *any* sense. As the results of the Monte Carlo study also suggest, these features imply that the techniques are likely to be quite robust.

The findings are generally positive concerning the techniques explored. We do see that an implication of the presence of the miscalculation error is wider bounds on CV. Thus, there is increased potential for the bounds to be uninformative from a policy perspective. However, the researcher is able to formally state the relationship between the precision of the welfare bounds and the level of uncertainty in the data. These findings suggest that applying the nonparametric techniques to empirical data sets would be fruitful.

Further, an interesting direction for new research would be to scrutinize the performance of parametric demand specifications and consistency with the nonparametric bounds on welfare. Another topic of investigation is to explore the impact of measurement error in site visitations. These errors could be due to recall error, etc. Additionally, the bootstrap methodology presented above is well-equipped to explore the implications of correlation across individuals in the sample. For example, Larson and Shaikh (2004) develop a methodology for estimating the marginal value of time. They suggest that some individuals are able to vary work time at the margin while others lack this opportunity. They also find that empirical evidence exists that suggests recreation demand models must treat these distinct groups of workers differently in the calculation of implicit prices. This suggests that implicit prices are likely to be correlated across individuals in these subgroups. There is potential that the bootstrap augmented nonparametric bounds could be further refined to account for this correlation.

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