CONTINGENT VALUATION AND REVEALED PREFERENCE METHODOLOGIES: COMPARING THE ESTIMATES FOR QUASI-PUBLIC GOODS

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ABSTRACT

A literature search provides 83 studies from which 616 comparisons of contingent valuation (CV) to revealed preference (RP) estimates are made. Summary statistics of the CV/RP ratios are provided for the complete dataset, a 5% trimmed dataset, and a weighted dataset that gives equal weight to each study rather than each CV/RP comparison. For the complete dataset, the sample mean CV/RP ratio is 0.89 with a 95% confidence interval [0.81-0.96] and a median of 0.75. For the trimmed and weighted datasets, these summary statistics are (0.77; [0.74-0.81]; 0.75) and (0.92; [0.81-1.03]; 0.94), respectively. The Spearman rank correlation coefficients between the CV and RP estimates for the three datasets are 0.78, 0.88, and 0.92, respectively, with the Pearson correlations a bit larger. Non-parametric density estimates are provided, as well as the results of regressions of the observed CV/RP ratios on the basic RP technique used and the broad class of goods valued.

1. INTRODUCTION

Beginning with Knetsch and Davis (1966), the comparison of contingent valuation (CV) estimates for government-provided, quasi-public goods with estimates obtained from revealed preference (RP) techniques, such as travel cost analysis and hedonic pricing, has played a key role in assessing the validity and reliability of the contingent valuation method. In their assessment of the contingent valuation method twenty years later, Cummings, Brookshire and Schulze (1986) placed considerable emphasis on comparing estimates from eight studies that used both contingent valuation and revealed preference techniques for similar quasi-public goods.[1] The assemblage of studies in Cummings, Brookshire, and Schulze (1986) emphasized the shift away from treating revealed preference techniques as the "truth," toward the realization that revealed preference estimates are random variables which are sensitive to details such as commodity definition, the functional form used in estimation, and other technique-specific assumptions such as the value of time and the number of sites in a travel cost study. As a result of this shift, comparisons between contingent valuation and revealed preference estimates are generally assumed to represent tests of convergent validity rather than criterion validity.[2] Such comparisons can play a prominent role in discussions of whether there is a need to "calibrate" contingent valuation estimates up (Hoehn and Randall, 1987) or down (Diamond and Hausman, forthcoming) and issues such as whether contingent valuation estimates systematically vary with the good being valued.

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[1]The eight studies Cummings, Brookshire, and Schulze (1986) considered were Knetsch and Davis (1966), Bishop and Heberlein (1979), Thayer (1981), Brookshire *et al.* (1982), Desvousges, Smith and McGivney (1983), Sellar, Stoll and Chavas (1985), Brookshire *et al.* (1985), and Cummings *et al.* (1986).

[2]Tests of criterion validity are possible when comparing an estimate from a technique to a value known to be the truth. Tests of convergent validity are possible when two or more measurement techniques are potentially capable of measuring the desired quantity, but both techniques do so with error. See Mitchell and Carson (1989) for a discussion.

This paper presents the results of a meta-analysis that seeks to summarize the available information to provide readers with the broadest possible overview of how CV estimates for quasi-public goods correspond with estimates based on revealed preference techniques.[3] Through an extensive search of both the published and unpublished literature, we have located 83 studies that provide 616 comparisons of contingent valuation to revealed preference estimates. The studies considered provide value estimates for a wide variety of quasi-public goods.[4] We look at everything from the value of a recreational fishing day on the Blue Mesa Reservoir in Colorado to the value of a statistical life estimated from national occupational risk data. There is a substantial amount of variation between the goods considered, between the changes in the goods valued, and between the specific implementations of the valuation techniques used. There is also variation both across and within studies and in how closely the goods in different CV and RP comparisons actually match-up.

This variation is both a strength and a weakness. It allows for a meta-analysis that favors a "big-picture" view: if there is a strong signal that CV, as a general valuation approach, substantially under- or over- estimates quasi-public goods' values relative to revealed preference techniques, one is likely to see it in a sample as large as ours. Small effects and subtle interactions between particular types of goods and very specific aspects of valuation techniques used may, however, be missed.

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[3]For an overview of meta-analysis techniques, see Hedges and Olkin (1985) or Cooper and Hedges (1994).

[4]This meta-analysis differs from others that have appeared in the environmental economics literature. These have focused on either one valuation technique and one type of commodity (see, *e.g.*, Smith and Kaoru, 1990; Smith and Huang, 1993) or on comparing two techniques for one type of commodity (*see, e.g.*, Walsh, *et al.*, 1992).

2. STUDY INCLUSION CRITERIA

To help find studies that contain both CV and RP estimates, we systematically reviewed entries in the Carson *et al.* (1994) bibliography of over 1,600 contingent valuation papers. To be eligible for inclusion in our sample, a study must provide at least one contingent valuation estimate and one revealed preference estimate for essentially the same quasi-public good; thus, no studies of private goods (*e.g.*, Neill *et al.*, 1994) are included. The goods valued are various forms of recreation (mostly outdoor), changes in health risks, and changes in environmental amenities such as air pollution, noise pollution, water pollution, or parks. Consumers (individuals or households) had to have been interviewed to obtain contingent valuation estimates. Thus, we did not include studies where the respondents were not consumers such as Bohm's (1984) study of local governments' willingness to purchase statistical information. Furthermore, we considered only contingent valuation estimates of willingness to pay (WTP); we excluded estimates based on willingness to accept compensation or on contingent behavior responses.[5] Otherwise, we have tried to include all available study estimates.

The studies we examined span almost thirty years, 1966-1994. The earliest study is Knetsch and Davis' (1966) well-known contingent valuation-travel cost comparison of outdoor recreation in Maine. The latest study considered is Choe, Whittington, and Donald (1994) who value the opening of a polluted urban beach in the Philippines.

Due to well-known, potential biases in relying upon only the published literature to summarize research findings, we spent considerable effort searching the unpublished literature

[5]We do include CV estimates derived from willingness to drive questions if they were intended to be directly compared to a travel cost estimate. CV questions phrased in terms of willingness to give up other goods are not included. No comparisons between CV willingness to pay estimates and actual willingness to accept compensation (*e.g.*, Bishop and Heberlein, 1979) are used. However, our initial investigation suggested that CV/RP ratios in such comparisons are almost always substantially below 1.0.

including theses, dissertations, conference papers, and government reports.[6] We have also drawn upon the rapidly growing non-market valuation literature from studies conducted outside the United States.[7]

Multiple estimates from a single study are provided when the study valued multiple goods. This is common, for instance, in situations where respondents were interviewed at several recreational fishing locations and travel cost and contingent valuation estimates were made for each location (*e.g.*, Duffield and Allen, 1988) or where different levels of a good were valued (*e.g.*, Shechter, 1992). Multiple estimates are also provided when a study used different analytical assumptions (*e.g.*, Smith, Desvousges, and Fisher, 1986) in making the CV and/or RP estimates. In such cases, we considered all of the possible comparisons between the CV and RP estimates for the good in question. Studies often show a clear preference for a particular estimate and provide a rationale for the choice. However, the choice of a particular estimate is subjective, and when facing the same choices, different researchers may undoubtedly make different choices. To maintain as neutral a position as possible, we considered all available comparisons made explicitly in the study or which are easily inferred.

We coded the revealed preference techniques used in the papers into five broad categories. The first of these is single site travel cost models (TC1). The second is multiple

[6]Berg (1994, p. 401) underscores this position based on his study of publication bias by noting "If the meta-analysis is restricted to published studies, then there is a risk that it will lead to biased conclusions. This is especially problematic in that one of the major advantages of meta-analysis is that the aggregation of data can lead to effect size estimates with very small variance, giving the impression of conclusiveness in circumstances where the summary estimate is biased. That is, the resulting inferences may not only be wrong but appear convincing." In this case, *a priori* one might expect that publications would tend to favor the two extremes, that is, cases where the study resulted in either nearly identical CV and RP estimates or those where the estimates were highly divergent. A sensitivity analysis of this issue is presented in a later section of this paper.

[7]In addition to a sizeable number of non-U.S. studies available in English, we have also used several CV and RP comparisons from non-English language studies as summarized in Navrud (1992a).

site travel cost models (TC2). The third is hedonic pricing (HP). The fourth is averting behavior (AVERT) which includes expenditure and household production function models not already included in TC2. The last category includes the creation of simulated or actual markets (ACTUAL) for the good.[8] We excluded estimates from any technique which were not designed to capture net willingness to pay/consumer surplus such as actual trip expenditures. There are 295 TC1, 183 TC2, 62 HP, 28 AVERT, and 48 ACTUAL comparisons with CV estimates.

We have also coded the goods valued in the various studies into three broad classes. The first class, recreation (REC), includes studies that valued outdoor recreation such as sport fishing, hunting, and camping. The second class, environmental amenities (ENVAM), includes studies that valued changes in goods such as air and water quality. The third class, health risk (HEALTH), includes studies that valued small reductions in environmental or work-related health risks. There are 432 REC, 163 ENVAM and 21 HEALTH estimates. There is a considerable correspondence between the general class of good being valued and the RP technique used. This is particularly true of outdoor recreation where single (TC1) and multiple (TC2) site travel cost models are generally used.

3. COMPARISON STUDIES CONSIDERED

Table 1 displays the comparison studies used in our meta analysis. Within the table, the studies are grouped into five categories based on their revealed preference methodology: TC1, TC2, HP, AVERT, and ACTUAL. Within each revealed preference methodology, the studies are organized chronologically.

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[8]Several of these studies value goods which may have both direct and passive use values. Because many of the respondents surveyed were potential direct users of the valued goods, these studies are included for the sake of completeness. In some instances, the actual/simulated markets contained strong incentives for free-riding and hence, the CV/RP ratios from these studies may be biased upward.

4. SUMMARIZING THE CV/RP RATIOS

Table 2 summarizes the CV/RP ratios treating the dataset in three different ways. The complete sample uses each individual CV/RP ratio as an observation.[9] The trimmed sample uses the remaining data after trimming off the smallest 5% and largest 5% of the CV/RP ratios. The weighted sample uses the mean CV/RP ratio from each study as that study's observation.[10] For each of the three treatments, we have provided the mean, the standard error of the mean, the maximum and minimum observations, the median (the 50th percentile), a wide range of other percentiles of the sample distribution, and finally, the sample size.

Each of these treatments of the data has advantages and disadvantages. Statistics calculated from the complete sample effectively treat comparisons from the same study as being completely independent (*i.e.*, correlation equal to zero) with respect to the information they contain. In contrast, the weighted sample effectively treats the correlation between comparisons from the same study as one. Reality lies between these two extremes. Consequently, the confidence intervals for the complete sample will tend to be too small and the summary statistics will disproportionately be influenced by studies contributing a large number of comparisons. In contrast, the weighted sample's confidence intervals will tend to be too large and the summary statistics will give equal influence to each study. The trimmed sample is based on the concept

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[9] As much of the discussion pertaining to calibration is cast in percentage terms, the CV/RP ratio is used as the dependent variable in much of the analysis that follows. Looking at the ratios also has the advantage that the ratios are not sensitive to the scale of the data. We do examine whether the conclusions of our analysis change if the RP/CV ratio or the difference between the two estimates is used as the variable of interest instead.

[10] The differences between the estimates from this treatment of the data and the complete and trimmed samples are due largely to the weighting (using the mean of each study's ratios) which reduces the influence of studies that provide multiple estimates. Adamowicz (1988) accounts for 72 comparisons; Desvousges, Smith and McGivney (1983) combined with estimates from Smith, Desvousges, and Fisher (1986) account for 48 comparisons (both use the same data); McCollum, Bishop, and Welsh (1988) and Wegge, Hanemann, and Strand (1985) both account for 42 comparisons; and White (1989) accounts for 24 comparisons. Twelve other studies provide between 10 and 17 comparisons. Because we are considering ratios which are bounded below by zero and unbounded above, averaging is understandably sensitive to large ratios within studies.

of an α-trimmed mean which is the most common univariate statistical procedure used to deal with the possibility of gross outliers which may have an arbitrarily large influence on the estimate of the mean CV/RP ratio (see, e.g., Barnett and Lewis, 1984; Bickel and Doksum, 1977).

For the complete sample the estimate of the mean CV/RP ratio is 0.890 with a 95% confidence interval [0.813-0.960] and a median ratio of 0.747. For the trimmed sample, the estimate of the mean CV/RP ratio is 0.774 with a 95% confidence interval [0.736-0.811][11] and a median of 0.747. For the weighted sample the mean CV/RP ratio is 0.922 with a 95% confidence interval [0.811-1.034] and a median of 0.936.[12]

Figure 1 depicts a non-parametric density estimate of the complete sample using a simple kernel density estimator first proposed by Wegman (1972; see also Silverman, 1986 and Statistical Sciences, Inc., 1993) with a width parameter of 0.5. Almost all of the density falls below a CV/RP value of 2.0 with almost 70% of the mass to the left of a CV/RP ratio of 1.0.

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[11]Some of the most extreme CV/RP ratios come from a small number of studies and are subject to several qualifications: Smith, Desvousges, and Fisher (1986) (4 of the 10 largest ratios and 7 of the 10 smallest ratios) whose purpose was to pick assumptions which demonstrated how an analyst's judgement plays a very important role in the development of both CV and TC estimates; Shechter (1992) (2 of the largest 10 ratios) who used an RP estimate, which was one-tenth and one-twentieth the size of two other RP estimates for the same change, to compare with different CV estimates; Sellar, Stoll, and Chavas (1985) (2 of 10 smallest ratios) who obtained two negative net willingness to pay values; and the ECO Northwest study (1984) where the two CV estimates were 5 times higher than one of the two RP estimates, but one-half the size of the other RP estimate. The other large CV/RP ratios that are excluded from the trimmed sample come from Adam (1988), Bishop and Heberlein (1990), Duffield (1984), Duffield and Paterson (1990), Eubanks and Brookshire (1981), Hanley (1989), Johnson (1989), Kealy, Dovidio, and Rockel (1986), Loomis, Creel, and Park (1991), Milon (1986), Navrud (1991b), Sutherland (1983), and Wegge, Hanemann, and Strand (1995); and the other small CV/RP ratios from Desvousges, Smith, and McGivney (1983), Hanley (1989), Harris (1983), Haspel and Johnson (1982), White (1989) and Wegge, Hanemann, and Strand (1985.)

[12]An alternative weighting scheme which is more robust to large outliers and also avoids giving disproportionate influence to studies with multiple estimates is to use the median ratio from each study (rather than the mean). Doing this results in a N=83 dataset of CV/RP ratios with mean 0.820 with a 95% confidence interval [0.729-0.912] and a median of 0.858. There are also 7 pairs of studies which have substantial overlap in the data analyzed (*e.g.*, Desvousges, Smith, and McGivney, 1983; Smith, Desvousges, and Fisher, 1986). Treating these pairs as individual studies (N=76) results in only a small change in the summary statistics for the weighted sample (a mean CV/RP ratio of 0.936 with a 95% confidence interval of [0.819 - 1.052] and a median of 0.938).

This figure also shows a fairly long, but very shallow, right tail that would be even longer (to just past 10) if we had not cut it off at 6, which is the first time the density estimate has a relative frequency of zero. Figure 2 depicts the non-parametric density estimate for the trimmed sample. Because the maximum CV/RP ratio is slightly greater than 2.0, one can see that almost all of the density lies to the left of 1.5 with over 80% to the left of 1.25. Figure 3 depicts the nonparametric density estimate for the weighted sample. This figure shows a very pronounced peak at about 1.0, with over half the density to the left and a thicker, but much shorter, right tail than Figure 1.

The analysis provided is not invariant to whether the CV estimate is chosen as the numerator of the ratio (as above) or as the denominator. One could instead look at the ratio of the RP to CV estimates. For the complete dataset, one gets a mean value of 5.671 with a 95% confidence interval of [4.189-7.153] and a median estimate of 1.338. This estimate, which suggests that the RP estimates are on average over five times the CV estimates, is driven by the several large outliers noted earlier. Using the trimmed dataset, we estimate a smaller, but still large, mean RP/CV ratio of 2.626 with a 95% confidence interval [2.351-2.902]. For the weighted sample, the mean RP/CV ratio is 3.542 with a 95% confidence interval of [2.029-5.057] and a median of 1.416. Thus, looking at the RP/CV ratios suggests that RP estimates are on average considerably larger than their CV counterparts.

We can also directly test whether the quantity (CV - RP) is different from zero. Doing so rejects the null hypothesis in favor of the alternative that the difference is negative with t-statistics of -7.31, -6.19, and -2.58, respectively, for the complete, trimmed, and weighted datasets. One can also conduct a traditional vote-counting analysis (see, *e.g.*, Bushman, 1994), which ignores the magnitude of the difference by assigning a value of 1 for those comparisons where the CV estimate is greater than the RP estimate and a value of 0 otherwise. For all three treatments, the null hypothesis that the vote-count is equal to zero can be rejected using a sign test in favor of the alternative that the average vote is less than zero (z-statistics=17.13, 15.57, and 5.44 for complete, trimmed, and weighted samples, respectively). All three samples suggest that there is almost a 70 percent chance that, for a randomly drawn comparison, the CV/RP ratio will be less than one.

5. VARIATION WITH RP TECHNIQUE, CLASS OF GOOD, PUBLICATION AND TIME

We regressed the CV/RP ratios from the trimmed dataset on a set of dummy variables representing the RP technique used with the single site travel cost models (TC1) as the omitted category. These results are shown in Table 3 with the t-statistics reported based on the White (1980) heteroskedasticity-consistent covariance matrix. They suggest that the CV estimates run about 20% lower than the TC1 counterparts, about 30% lower than their TC2 counterparts, a little less than 40% lower than their HP counterparts, about 20% lower than their AVERT counterparts, and are, on average, indistinguishable from their ACTUAL counterparts.[13]

We also regressed the CV/RP ratios from the trimmed dataset on a set of dummy variables for the broad class of goods valued. These results are shown in Table 4 with the t-statistics similarly calculated. They suggest that the HEALTH goods may have CV/RP ratios

[13]It is possible to use the parameters in Tables 3 and 4 to assess the influence of the inclusion or exclusion of a particular RP technique or type of good. This can be done by noting that the mean CV/RP estimate is simply the sum of the intercept and the weighted parameter estimates where the weights are the percent of the sample in each category. To recalculate the weights from dropping one or more categories, the only additional information needed is the original number of observations in each category (*i.e.*, TC1=272, TC2=152, HP=62, AVERT=23, ACTUAL=46; REC=400, ENVAM=134, HEALTH=21). For instance, one may want to drop the comparisons with HP studies because the assumptions necessary to identify consumer surplus in hedonic models are often questionable (doing so changes the original mean CV/RP estimate from 0.775 to 0.795), or to drop the ACTUAL comparisons because some of these RP estimates came from situations which had strong incentives for free-riding (mean CV/RP ratio goes from 0.775 to 0.752), or to drop the health studies because of frequent difficulties with either perceived or conveyed health impacts (mean CV/RP goes from 0.775 to 0.770).

closer to 1.0 relative to the other two categories of goods, although this conclusion should be tempered by the smaller number of CV/RP estimates in the HEALTH category and the marginally significant t-statistic.[14]

We also regressed the CV/RP ratios on a dummy variable representing those studies that had been published in an academic journal or as a chapter in an edited volume (PUBLISH). The results for the three treatments are summarized in Table 5 with the t-statistics based on the White standard errors. The results suggest that the CV/RP ratios from studies that are published are closer to 1.0 than those from studies that are not published.[15] Consistent with our expectations based on the publication bias literature (Berg, 1994), for the complete dataset the variance for the published studies is much larger than that for the unpublished set (p-value for Bartlett's test for equal variance < 0.001).

Another question which we were able to examine is whether CV/RP ratios exhibited any notable fluctuation over time.[16] Using the complete sample, we regressed a set of dummy variables representing studies published (or, if unpublished, dated) prior to 1984 (99 observations; 17 studies; P1YEAR), those published between 1984 and 1989 (363 observations; 36 studies; P2YEAR), and those published after 1989 (154 observations; 30 studies; P3YEAR) with P1YEAR as the omitted category. The results of the regression, shown below, suggest that

[14]Results based on the complete dataset are quite similar in both relative and absolute magnitude for the various RP techniques with the exceptions: TC1 has an intercept term of 0.9392, AVERT has a significant positive coefficient, and ACTUAL has an insignificant positive coefficient. Neither the HEALTH nor ENVAM dummies are even marginally significant in the regression equation using the complete dataset.

[15]This same results holds, although not quite as strongly if the definition of a published study is expanded to include government reports (t-statistics on PUBLISH for complete, trimmed, and weighted samples are 1.72, 0.66, and 1.17, respectively). We can also directly test whether the quantity (CV/RP for the published studies - CV/RP for unpublished) is different from zero. Doing so using the complete sample results in t-statistics of -1.90 for the more limited definition of published studies and a t-statistic of -1.80 for the expanded definition.

[16]This analysis should be interpreted as there is often a lag between when a study is actually conducted and the year in which the study results are published.

the CV/RP ratios do not exhibit any statistically significant difference between these three time periods:

$$\underset{}{CV\_RP} = \underset{(9.562)}{0.8995} + \underset{(0.040)}{0.0042*P2YEAR} - \underset{(-0.521)}{0.0628*P3YEAR}$$

A similar conclusion results using both the trimmed and weighted samples. We can also regress the year of the study on the CV/RP ratio. Using the complete sample suggests that the date of the study does not significantly impact the ratios (t-statistic=0.494) and this is also true using both the trimmed and weighted datasets (t-statistic=-0.087 and -0.266, respectively).

An obvious next step is to conduct a more detailed analysis of this data using additional variables which show the specific details of the contingent valuation implementation, a finer partitioning of the RP techniques, and potential indicators of reliability such as sample size and standard errors. Our efforts to conduct this analysis, however, have been greatly hindered by the curse suffered by other meta-analyses of non-market data (*e.g.*, Smith and Kaoru, 1990): incomplete reporting of the necessary details. With rapidly declining sample sizes due to missing data and a large set of dummy variables, we found we were soon identifying individual studies with particularly large or small CV/RP ratios. However, some general observations may be warranted which are along the lines of the meta-analyses of contingent valuation, travel cost analysis, and hedonic pricing which have previously been performed (Smith and Kaoru, 1990; Walsh, Johnson, and McKean, 1992; Smith and Huang, 1993; Smith and Osborne, 1994). The single-site travel cost models produce higher CV/RP ratios on average than do the multiple-site models. This is largely because many TC1 models do not include any value of travel time while most TC2 models make some allowance for travel time cost. TC2 models also tend to be more elaborate with some visitors coming from long distances to one or more of the sites examined. Estimates from the TC2 models are often presented using different functional forms, some of which produce quite large RP numbers. Hedonic pricing and averting/household production models are quite sensitive to the particular functional form and attributes used, and can generate a wide range of RP estimates from the same dataset. The CV estimates vary with the treatment of outliers and protest responses, the functional form used with discrete choice CV data, and the payment mechanism used. CV estimates are undoubtedly sensitive to how well the good is described and whether the respondents believe the good can be provided (Mitchell and Carson, 1989). RP estimates are undoubtedly sensitive to the researcher's assumptions about a good's input costs (Randall, 1994) and characteristics (Freeman, 1993).[17]

6. CORRELATION BETWEEN CV AND RP ESTIMATES

The average CV/RP ratio does not directly address whether CV and RP estimates tend to move together. The convergent validity of the two measurement techniques is closely tied to the presence of a significant correlation between the estimates derived using the different techniques, although how large such a correlation should be is an open question. A correlation framework in this case can also be linked to a measurement error model where neither of two available measurements is error free and the two techniques may measure the desired quantity in different units such as gallons and liters.[18]

We provide two measures of correlation, the Pearson correlation coefficient and the Spearman rank-order correlation coefficient. The Pearson correlation coefficient is the ratio of covariance of the two measures to the square root of the product of the variances of the two

[17] For instance, recreationists' costs of travel may differ greatly from the researcher's assigned costs or lake users may be unaware of an invisible toxin known to the researcher. In both cases, there is a divergence between the researchers's assumptions and the consumer's perceptions.

[18] It is possible to have an average CV/RP ratio of 0.5 or 2.0 and to have the correlation between the two estimates equal 1.0. It is also possible to have an average CV/RP ratio of 1.0 and a correlation coefficient of zero.

measures. The Spearman correlation coefficient is a non-parametric measure which first individually rank orders the values obtained from the two measurement approaches and then calculates the Pearson measure using the ranks as the data. It tends to be less sensitive to outliers and differences in scale than the Pearson measure.[19]

For the complete sample, the Pearson coefficient is 0.81 and the Spearman coefficient is 0.78. For the trimmed sample, these two measures are 0.91 and 0.88, respectively, while for the weighted sample they are 0.98 and 0.92, respectively. As expected, both of these datasets show higher correlation than the complete dataset since in the trimmed dataset, the most divergent observations have been dropped and in the weighted dataset, CV and RP estimates which were divergent in one direction have often been averaged with those divergent in the opposite direction. In all three datasets, both the Pearson and Spearman correlation coefficient are significantly different from zero ($p < 0.001$).

In any finite sample, estimated correlation coefficients maybe sensitive to the scale of the data.[20] The largest estimate in the sample is a RP estimate of 5920 (CV estimate 4650) from the Brookshire *et al.* study (1982) on the increased value of a house due to being outside rather

[19]Note that the CV/RP ratios are not sensitive to the scale of the data. For the purpose of calculating the CV/RP ratio it does not matter whether the CV estimate is in 1972 dollars or 1994 dollars, or for that matter, pounds or kroner, as long as the RP estimate is in the same units. Similarly, it does not matter whether individual or aggregate estimates are used.

[20]In an earlier version of this paper, we reported Pearson correlation coefficients in the 0.4 to 0.7 range. While there has been a substantial increase in the number of comparisons since that version, the principal change has been placing all of the estimates at the individual consumer level rather than the aggregate level. Originally, aggregate CV and RP estimates had been entered into our database for a small number of studies, because it was not immediately obvious how to obtain the preferred consumer level estimate from the aggregate estimate in those studies. The CV/RP ratios from these studies tend to be quite erratic relative to the studies with more complete reporting. These large and highly variable aggregate estimates had a very large influence on the magnitude of the estimated Pearson correlation coefficient. We have devoted considerable effort to extracting the appropriate individual agent level estimate from the aggregate estimate in these studies. In only one instance, the early Darling (1973) CV/HP comparison, is it impossible to determine the exact rule, or a close approximation, for going from the aggregate to the individual level estimate. As the Darling estimates are in millions of dollars, we divided these estimates by 1 million dollars to make them consistent with the scale of most of the other estimates.

than inside an earthquake zone in the greater Los Angeles area. There are six comparisons with CV or RP estimates above 2000, four valuing housing characteristics and two valuing big game hunting. Dropping these comparisons reduces the correlations a small amount, and dropping the much larger number of comparisons (N=53) with CV or RP estimates above 1000 reduces them a bit further (*i.e.*, Pearson [0.81, 0.85, 0.92] and Spearman [0.77, 0.85, 0.91], for the three samples, respectively). Dropping the 106 comparisons with a CV or RP estimate above 500 results in a sizeable reduction in the Pearson correlation coefficients for the full and trimmed samples, but not for the weighted sample (0.60, 0.64, 0.90). The Spearman correlation coefficients, which are less sensitive to scale, remain largely unchanged (0.72, 0.81, 0.90). All of these Pearson and Spearman correlation estimates are significantly different from zero ($p < 0.001$).

One can also regress the RP estimate on the CV estimate. Depending on the sample used, the coefficient on the CV estimate ranges from 0.9 to 1.4 and is always highly significant. The intercept term is always positive and tends to be reasonably large and quite significant for treatments where the coefficient on the CV estimate is near or below 1.0. One of the more interesting and best fitting regression models was found by taking the average RP and CV estimates from the 83 studies as the observations when the averaging is performed using the trimmed dataset rather than complete dataset.[21] The resulting regression equation is given by:

$$\text{RP\_ESTIMATE} = \underset{(0.117)}{0.8995} + \underset{(64.609)}{1.2652}{*}\text{CV\_ESTIMATE},$$

where the White t-statistics are in parentheses and the adjusted R^2 is 0.98. The high R^2 suggests that after eliminating *a fraction* of the *between* studies variance by trimming off the overall

[21]This procedure still results in one observations per study because no study has CV/RP comparisons where all of the study ratios are in the largest or smallest 5% of the 612 ratios contained in the complete data set.

smallest and largest 5% of the CV/RP ratios and eliminating *all within* study variance by averaging, the CV and RP estimates are very closely linked. Furthermore, the reciprocal of the coefficient on the CV estimate (0.79) is almost identical to the mean CV/RP ratio (0.78) from the trimmed data.

7. OTHER COMPARISON APPROACHES

Comparing WTP estimates from contingent valuation to estimates from RP methodologies is certainly the most popular way of comparing the two approaches, but it is not the only one. Another approach is to compare estimates of the fraction of a particular population who say that they will undertake a given activity with the fraction who actually undertake the activity. For example, Carson, Hanemann, and Mitchell (1987) look at the correspondence between the estimate of the percent who say in a survey that they will vote for a water quality bond issue (70-75%) and the percent actually voting in favor of it (73%). Kealy, Montgomery and Dovidio (1990) find that 72% of those who said they would donate money to the New York Department of Environmental Conservation to reduce acid rain in the Adirondacks actually did so several weeks later. This percentage increased to 92% in a subsample in which they strongly stressed the future payment obligation.[22] In contrast, Seip and Strand (1992), using members of a Norwegian environmental group as interviewers, found that only 10% of respondents who indicated they would be willing to pay a specified membership fee for the group actually did so when solicited a month later. Navrud (1992b) conducted a similar exercise, but this time sampling people who had sent in a reply coupon from a full page World Wildlife Federation (WWF) newspaper ad in Norway "contributing their vote as a WWF friend." While Navrud's

[22]The number of subjects who declined to donate after earlier saying they would was only slightly larger than the number of subjects who said they would not donate but who actually did so.

study showed the percentage joining the environmental group as several times that of Seip and Strand's study, Navrud emphasizes the difficulty in drawing a close correspondence between a vague initial request which potentially includes ideological support for the environmental group's public goals and the actual private good purchase of membership in the group.[23]

Analysts may also be interested in price and substitution elasticities. For example, Cummings *et al.* (1986) estimated the elasticity of substitution between wages and municipal infrastructures in western boomtowns to be -0.35 using a hedonic wage equation estimated on data from 29 towns and -0.037 to -0.042 using CV surveys done in three boomtowns. Thomas and Syme (1988) used a contingent valuation study in Perth, Australia to estimate the residential water demand price elasticity since there had been little prior variation in water rates. The authors estimated the price elasticity to be -0.20 using the data from their CV study, whereas econometric models estimated from actual demand observed after water rate changes had been put in place resulted in price elasticity estimates ranging from -0.10 to -0.43. In the public finance literature, tax price elasticities for a particular good estimated from survey data tend to be similar to those estimated from aggregate voting data and governmental provision decisions (Bergstrom, Rubinfeld, and Shapiro, 1982; Gramlich and Rubinfeld, 1982).

A different approach is to compare the utility of different choices from stated preference (SP) and RP models using the suggestions of Louviere and Timmermans (1990) for recreational modeling.[24] In some instances, it may also be possible to compare parameters estimated from

[23]It can also be shown that the incentive structure of the two-step mechanism used in Seip and Strand (1992) and Navrud (1992b) should lead to over-pledging in the survey market and free-riding in the actual market.

[24]Utilities from choice models estimated from RP and SP data cannot be directly compared unless one takes account of the possibility of different latent scale parameters underlying the choice models (Morikawa, 1989). A number of comparisons in the literature which were previously thought to be divergent have been shown to be consistent once differences in scale (which is related to reliability) are taken into account.

different models. Hensher, *et al.* (1989) use this approach to show the similarity of the value of travel time estimates from the two types of models in the transportation literature. With adequate and similar information on the variables underlying the choice process, one can directly test for the statistical equivalence of the estimated contingent valuation and revealed preference choice models. Mu (1988) shows this for the choice problem of where to obtain household water in Brazil.[25] A less structured approach based on the non-parametric consumer preference framework of Varian (1983) has been applied to contingent valuation and travel cost data for big horn sheep hunting in Canada by Adamowicz and Graham-Tomasi (1991). They show that most of their data from both approaches is consistent with the basic set of theoretical restrictions on demand, with the contingent valuation data showing fewer violations.

If one is prepared to say that neither CV nor RP data is inherently superior to the other, an obvious thing to do is combine them in some fashion. This approach has recently been applied in the marketing and transportation literatures (Ben-Akiva and Morikawa, 1990; Hensher and Bradley, 1993; Swait and Louviere, 1993), and has seen some initial applications (Adamowicz, Louviere, and Williams, 1994; Cameron, 1992; Hanemann, Chapman and Kanninen, 1993) in the recreational demand literature. Cameron (1992) proposes a procedure for jointly estimating a recreational demand equation and a CV valuation function in a utility-consistent framework.

[25]It is difficult to test whether the CV and RP data were generated by the same utility function without making strong structural assumptions about the choice process. It is particularly difficult unless one has obtained the key variables underlying that process for both the RP and CV samples. See Larson (1990) for an application and discussion of problems with this approach.

8. CONCLUDING REMARKS

Our examination of 83 studies containing 616 CV/RP comparisons for quasi-public goods finds that CV estimates are smaller, but not grossly smaller, than their RP counterparts. For the complete dataset, 1.0 is just outside the upper-end of the 95% confidence interval [0.81-0.96] for the mean CV/RP ratio (0.89).[26] For the trimmed dataset, one can clearly reject the hypothesis that the mean CV/RP ratio (0.77) is 1.0 in favor of the alternate hypothesis that it is less than one. For the weighted dataset, the mean CV/RP ratio (0.92) is not significantly different from 1.0 using a 5% two-sided t-test. The median CV/RP ratios range between 0.75 and 0.94 depending upon the treatment of the sample. Most of the density lies in the range of CV/RP ratios of 0.25 to 1.25. The Pearson correlation coefficient between the CV and RP estimates varies between 0.60 and 0.98, depending on the sample considered; the Spearman rank correlation coefficient varies between 0.72 and 0.92. In every case, the correlation coefficient estimates are significant at $p < 0.001$, thus providing support for the convergent validity of the two basic approaches to non-market valuation of quasi-public goods.

Some CV estimates clearly exceed their revealed preference counterparts, and therefore one should not conclude that CV estimates are always smaller than revealed preference estimates. Nonetheless, based on the available CV/RP comparisons, arbitrarily discounting CV estimates by a factor of two or more, as some have proposed, appears to be unwarranted given that CV/RP ratios of greater than 2.0 comprise only 5% of our complete sample and only 3% of our weighted sample. Indeed, applying a discount factor of 2.0 or greater to the CV

[26]By carefully selecting a small subset of study estimates, one could argue either that the CV/RP ratio was almost always 1.0 or that it was almost always substantially larger or smaller than 1.0. Of course, any such selection should be carefully justified.

estimates used in our analysis would result in "adjusted" CV estimates that, in almost all cases, *diverge* from the estimates obtained from observable behavior, rather than *converge*.

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TABLE 1
COMPARISON STUDIES

Author	Good Valued	Number of Comparisons	RP Technique
Knetsch & Davis (1966)	Outdoor recreation of a forest area in northern Maine	2	TC1
Beardsley (1971)	Recreation on Cache la Pandre River, Colorado	2	TC1
Shechter, Enis & Baron (1974)	Preservation of Israel's Mt. Carmel National Park from limestone quarry expansion	2	TC1
Bishop & Heberlein (1979)	Goose hunting in Wisconsin's Horicon Zone	3	TC1
Smith (1980)	Outdoor recreation at Cullahy Lake in Oregon	1	TC1
Thayer (1981)	Prevention of geothermal development in Santa Fe National Forest	6	TC1
Haspel & Johnson (1982)	The impact of proposed surface mining to be located near Utah's Bryce Canyon National Park	8	TC1
Johnson & Haspel (1983)	The impact of proposed surface mining to be located near Utah's Bryce Canyon National Park	2	TC1
Duffield (1984)	Kootenai Falls recreation in Montana	4	TC1
Bojö (1985)	Preservation of a nature reserve in Vaalaa Valley, Sweden from forest harvesting	1	TC1
Michaelson & Smathers (1985)	Recreation usage of public campgrounds in the Sawtooth National Recreation Area	3	TC1
O'Neil (1985)	Recreation on the West Branch of the Penobscot River and the Saco River	16	TC1
Loomis, Sorg, & Donnelly (1986)	Cold-water fishing in Idaho	1	TC1
Smith, Desvousges, & Fisher (1986)	Water quality improvements in the Monongahela River basin in Western Pennsylvania	12	TC1

Author	Good Valued	Number of Comparisons	RP Technique
Farber & Costanza (1987)	Recreation at Terrebonne Parish wetland system in South Louisiana	3	TC1
Hanley & Common (1987)	Recreational in Queen Elizabeth Forest Park in Scotland	1	TC1
Adamowicz (1988)	Bighorn sheep hunting in Alberta, Canada	72	TC1
Duffield & Allen (1988)	Trout fishing on seventeen Montana rivers	17	TC1
McCollum, Bishop, & Welsh (1988)	Wisconsin Sandhill Deer hunting permits	42	TC1
Navrud (1988)	Freshwater fishing, River Vikedalselv, Norway	4	TC1
Ralston (1988)	Recreation at Reelfoot Lake, Tennessee	1	TC1
Schelbert *et al.* (1988)	Recreation in Zurichberg forest, Switzerland	1	TC1
Bockstael, McConnell, & Strand (1989)	Chesapeake Bay water quality improvement	2	TC1
Brown & Henry (1989)	Viewing of elephants on wildlife safari tours in Kenya	8	TC1
Hanley (1989)	Recreation in Queen Elizabeth Forest, Scotland	8	TC1
Harley & Hanley (1989)	Recreation at three U.K. nature reserves: Island of Handa, Loch Garten, and Blacktoft Sands	6	TC1
Huppert (1989)	Salmon and striped bass fishing in California	4	TC1
Johnson (1989)	Recreational fishing at Blue Mesa Reservoir and the Poudre River, Colorado	4	TC1
White (1989)	Recreation at Belmar Beach in New Jersey	24	TC1
Navrud (1990)	Salmon and sea trout fishing, River Audna, Norway	4	TC1

Author	Good Valued	Number of Comparisons	RP Technique
Rolfsen (1990)	Salmon and sea trout fishing in the Gaula River, Norway	2	TC1
Loomis, Creel, & Park (1991)	Deer hunting in California	2	TC1
Navrud (1991a)	Brown trout fishing, Lauvann and Gjerstadskog Lakes, Norway	8	TC1
Navrud (1991b)	Salmon and sea trout fishing, River Audna, Norway	4	TC1
Sievänen, Pouta, & Ovaskainen (1991)	Recreation at a regional recreational area near Helsinki	5	TC1
Mungatana & Navrud (1993)	Wildlife viewing in Lake Nakuru National Park in Kenya	6	TC1
Choe, Whittington, & Donald (1994)	Recreation at an urban beach which had been closed, Davao, Philippines	4	TC1
Binkley & Hanemann (1978)	Beach recreation in Boston	2	TC2
Vaughan & Russell (1982b)	National freshwater fishing	4	TC2
Desvousges, Smith, & McGivney (1983)	Water quality improvements in the Monongahela River basin in Western Pennsylvania	12	TC2
Harris (1983)	Recreational fishing in Colorado	8	TC2
Sutherland (1983)	Water-based recreation in the Pacific Northwest	10	TC2
ECO Northwest (1984)	Recreational fishing of three different sites in the Swan River drainage basin	12	TC2
Devlin (1985)	Recreation associated with firewood collection in Colorado National forests	2	TC2
Donnelly *et al.* (1985)	Steelhead fishing trips in Idaho	1	TC2
Sellar, Stoll, & Chavas (1985)	Recreational boating on four lakes in East Texas	10	TC2

Author	Good Valued	Number of Comparisons	RP Technique
Walsh, Sanders, & Loomis (1985)	Recreation on eleven Colorado rivers recommended for protection under the Wild and Scenic Rivers Act and on a second group of rivers in the state	4	TC2
Wegge, Hanemann, & Strand (1985)	Marine recreational fishing in Southern California	42	TC2
Loomis, Sorg, & Donnelly (1986)	Cold-water fishing in Idaho	2	TC2
Milon (1986)	Artificial reef in South Florida	15	TC2
Mitchell and Carson (1986)	Change in national amount of fishable quality water	1	TC2
Smith, Desvousges, & Fisher (1986)	Water quality improvements in the Monongahela River basin in Western Pennsylvania	24	TC2
Sorg & Nelson (1986)	Elk hunting in Idaho	4	TC2
Young *et al.* (1987)	Small game hunting in Idaho	4	TC2
Walsh, Ward, & Olienyk (1989)	Effect of tree density on recreational demand for six recreational sites in Colorado	8	TC2
Duffield & Neher (1990)	Deer hunting in Montana	1	TC2
Richards *et al.* (1990)	Recreation at national forest campgrounds in Northern Arizona	10	TC2
Walsh, Sanders, & McKean (1990)	Pleasure driving/sightseeing along eleven rivers in the Colorado Rocky Mountains	3	TC2
Willis & Garrod (1990)	Open-access recreation on inland waterways in the United Kingdom	2	TC2
Duffield (1992)	Sportfishing in South Central Alaska	2	TC2
Darling (1973)	Amenities at three urban lakes in California	6	HP
Loehman, Boldt, & Chaikin (1981)	Changes in air quality in Los Angeles and the San Francisco Bay	7	HP

Author	Good Valued	Number of Comparisons	RP Technique
Brookshire *et al.* (1982)	Improvements in Los Angeles air quality	11	HP
Blomquist (1984)	Lake and high-rise views, Chicago	14	HP
Gegax (1984)	Job-related risk reduction	1	HP
Brookshire *et al.* (1985)	Housing locations inside and outside Los Angeles County's special earthquake study zones	1	HP
Gegax, Gerking, & Schulze (1985)	Job-related risk reduction	2	HP
Blomquist (1988)	Lake and high-rise views, Chicago	4	HP
IADB (1988)	Three types of housing structures	6	HP
Pommerehne (1988)	Road and aircraft noise in Basle, Switzerland	2	HP
d'Arge & Shogren (1989)	Water quality in the Okoboji Lakes region of Iowa	3	HP
Randall & Kriesel (1990)	25 percent reductions in both air and water pollution in the United States	1	HP
Shechter (1992)	Air pollution in the Haifa area, Israel	4	HP
Eubanks & Brookshire (1981)	Elk hunting in Wyoming	3	AVERT
Hill (1988)	Reduction of risk of breast cancer mortality	12	AVERT
John, Walsh, & Moore (1992)	Mosquito abatement program, Jefferson County, Texas	1	AVERT
Shechter (1992)	Air pollution in the Haifa area, Israel	12	AVERT
Bohm (1972)	Public television program in Sweden	10	ACTUAL
Kealy, Dovidio, & Rockel (1986)	Preventing additional damages from acid rain to the Adirondack region's aquatic system	2	ACTUAL
Hoehn & Fishelson (1988)	Visibility levels at the Hancock Tower Observatory in Chicago	3	ACTUAL
Sinden (1988)	Soil and forest conservation in Australia	17	ACTUAL

Author	Good Valued	Number of Comparisons	RP Technique
Boyce *et al.* (1989)	Preventing destruction of a Norfolk pine tree	1	ACTUAL
Bishop & Heberlein (1990)	Wisconsin Sandhill Deer hunting permits	3	ACTUAL
Hoehn (1990)	Visibility levels at the Hancock Tower Observatory in Chicago	3	ACTUAL
Duffield & Patterson (1991)	Purchasing water rights for Big and Swamp Creeks in Montana	8	ACTUAL
Essenburg (1991)	Water system in Philippine village	1	ACTUAL

TABLE 2
CV/RP ESTIMATES FOR THREE SAMPLE TREATMENTS

Percentile	Complete Sample	Trimmed Sample	Weighted Sample
Mean	0.886	0.774	0.922
Standard Error	0.038	0.019	0.057
Maximum	10.269	2.071	3.512
99%	5.584	1.948	3.512
95%	2.071	1.593	1.780
90%	1.524	1.345	1.447
80%	1.201	1.144	1.153
75%	1.122	1.090	1.111
70%	1.037	1.007	1.066
60%	0.908	0.886	0.990
50%	0.747	0.747	0.936
40%	0.610	0.624	0.809
30%	0.467	0.502	0.640
25%	0.376	0.432	0.585
20%	0.294	0.358	0.568
10%	0.094	0.132	0.349
5%	0.043	0.092	0.201
1%	0.011	0.063	0.079
Minimum	0.005	0.054	0.079
N	616	555	83

TABLE 3
REGRESSION OF CV/RP on RP TECHNIQUE USED

Parameter	Estimate	t-Statistic
Intercept	0.8014	28.55
TC2	-0.1039	-2.21
HP	-0.1813	-3.18
AVERT	0.0335	0.51
ACTUAL	0.2348	3.91
N=555	R^2=.051	

TABLE 4
REGRESSION OF CV/RP on TYPE OF GOOD VALUED

Parameter	Estimate	t-Statistic
Intercept	0.7706	34.06
ENVAM	-0.0107	-0.23
HEALTH	0.1450	1.64
N=555	R^2=.004	

TABLE 5
REGRESSION OF CV/RP on PUBLISH

	Complete [N=616]	Trimmed [N=555]	Weighted [N=83]
Parameter	Estimate (t-statistic)	Estimate (t-statistic)	Estimate (t-statistic)
Intercept	0.8272 (18.15)	0.7555 (33.18)	0.8731 (11.70)
PUBLISH	0.1838 (2.29)	0.0632 (1.48)	0.1168 (1.02)
	R^2=0.008	R^2=0.004	R^2=0.013

FIGURE 1
COMPLETE DATASET: NONPARAMETRIC DENSITY ESTIMATE

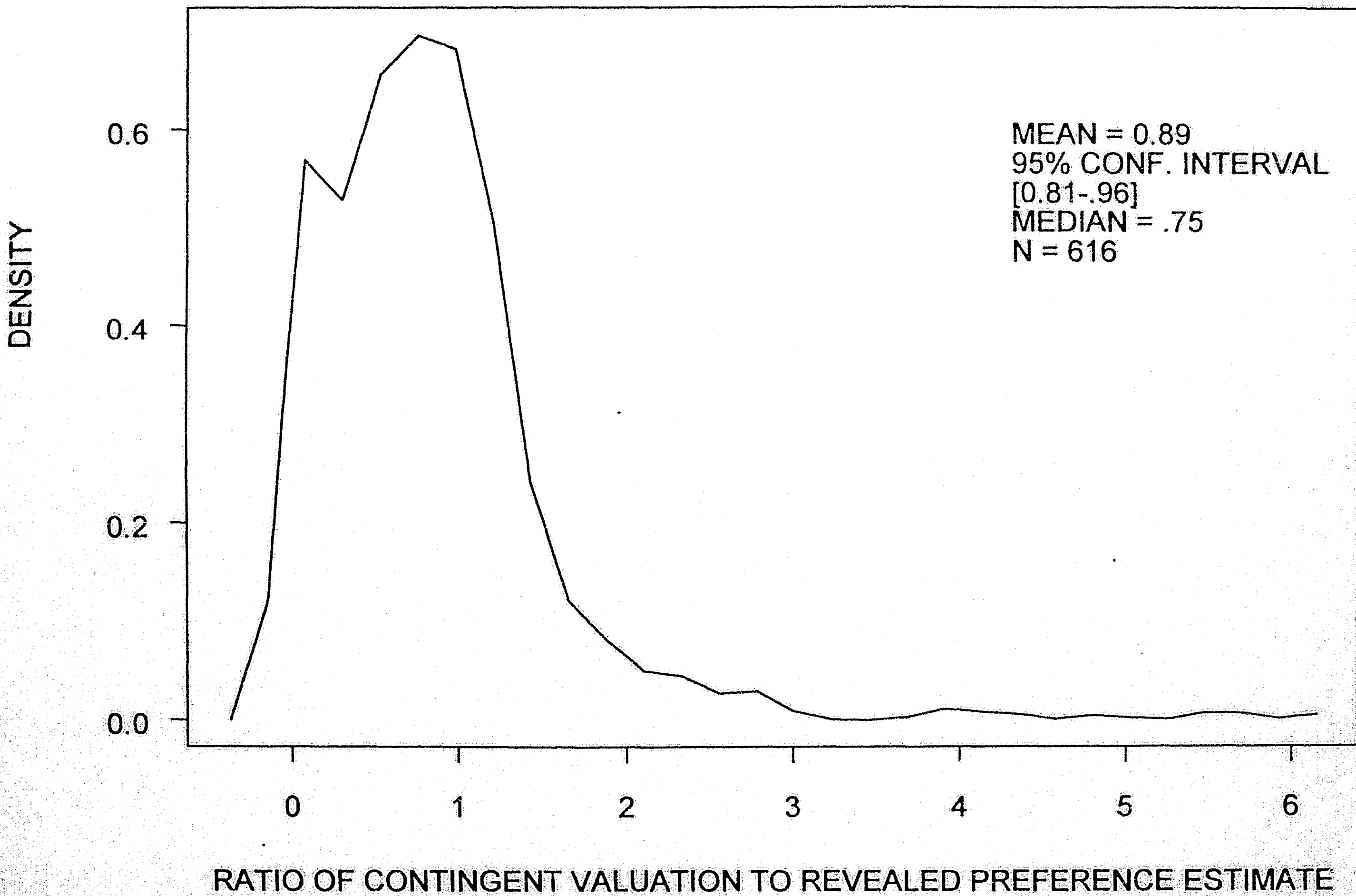

FIGURE 2

TRIMMED DATASET: NONPARAMETRIC DENSITY ESTIMATE

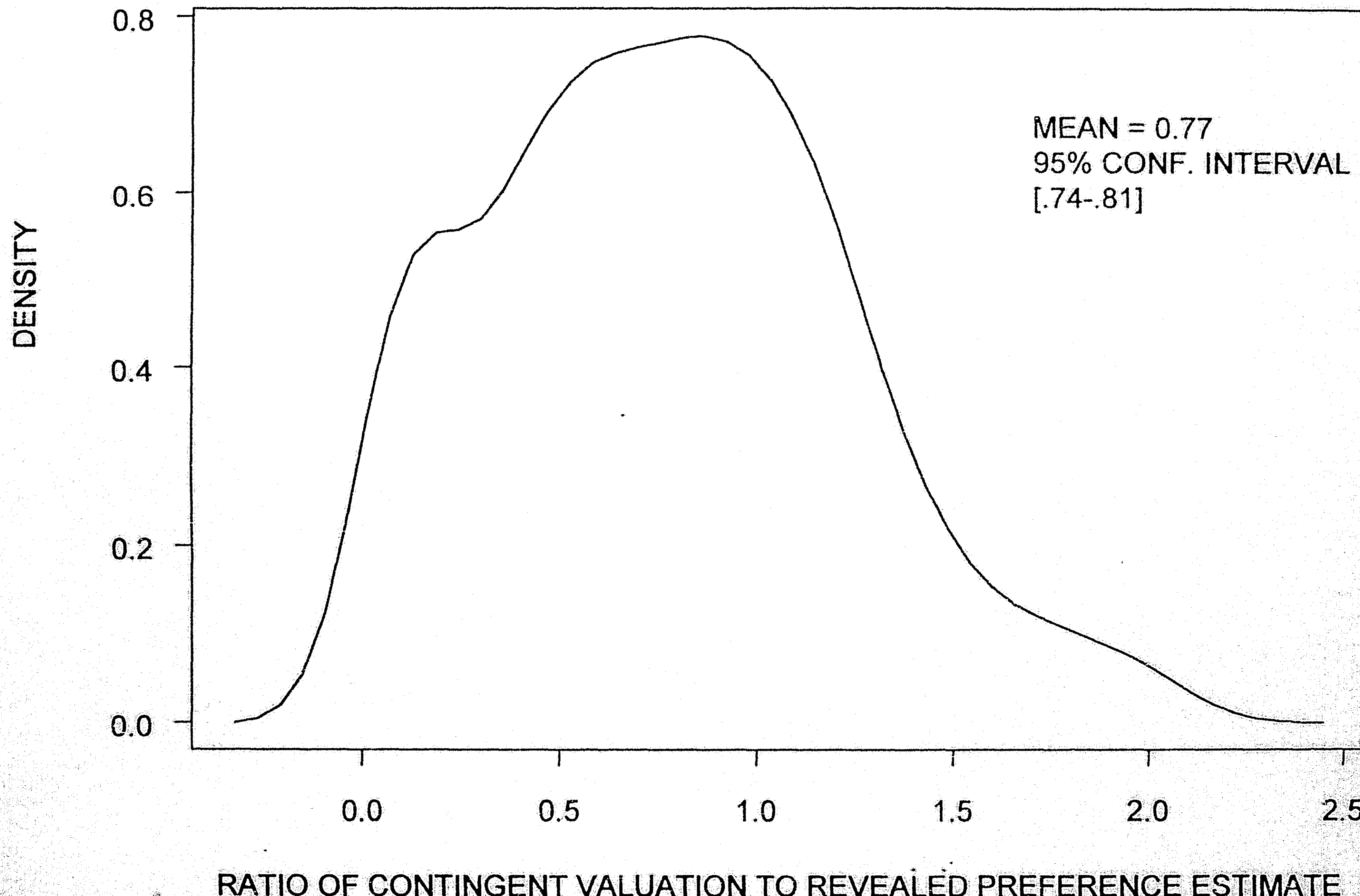

FIGURE 3
WEIGHTED DATASET: NONPARAMETRIC DENSITY ESTIMATE