A Review of WTA/WTP Studies

by

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1. Introduction

The difference between willingness to pay (WTP) and willingness to accept (WTA) has been widely studied both through theory (Hanemann, 1991, 1996; Randall and Stoll; Willig) and experiments. In a typical experiment, a subject is given some item, like a coffee mug, and then offered money to return it to the experimenter. The dollar amount the subject asks for is his WTA. Another subject is not given a mug and instead asked to pay for one. The amount he offers is his WTP. Previous authors have shown that WTA is usually substantially larger than WTP, and almost all have remarked that the WTA/WTP ratio is much higher than their economic intuition would predict (*e.g.*, Kahneman, Knetsch, and Thaler, hereafter KKT.)

The pervasiveness of high WTA/WTP ratios and the wide variety of goods that have been used in the experiments have combined to sustain interest in WTA *vs.* WTP for roughly thirty years. We analyze those studies here. Although large WTA/WTP ratios are well documented, the findings do not seem to have had much of a mark on either economic models or discussions of policy design, as Knetsch has noted. A wider role for these findings has been hampered, we believe, by two issues.

First, it has seemed possible that the high observed ratios are due to unsound experimental features such as hypothetical payments, student subjects, or elicitation questions that are not incentive-compatible. According to this argument, more realistic experiments, such as those with real money or incentive-compatible elicitation, will yield lower and more reasonable ratios. Likewise, we might suspect that repeating a given experiment with the same group of participants would cause the ratios to fall to closer-to-expected levels over time, as Coursey, Hovis, Schulze found in some of their experiments. These possibilities have not been investigated on a broad-

based scale. A finding that design features were playing a major role would make the typically high WTA/WTP ratios – which have been garnered from studies often with supposedly questionable design – of less concern.

The second issue that has blocked a wider role for the WTA/WTP findings is an absence of a rich set of behavioral patterns that any behavioral model might be expected to cover or explain. For which goods is a high ratio most likely to be found? A few studies have claimed that the ratio is smaller for goods that have close substitutes (Harless; Shogren *et al.*, 1996, hereafter SSHK), but the evidence about this, or other possible trends, has remained diffuse. In addition to providing a guide for future model-building, these patterns should make it easier to understand how the ratio might manifest itself in real-world economic behavior.

This paper uses a collection of WTA/WTP studies to address these issues. We found 45 studies that reported usable data. The studies draw on a remarkable range of goods: chocolates, pens, mugs, movie tickets, hunting licenses, visibility, nuclear waste repositories, nasty-tasting liquids, pathogen-contaminated sandwiches, and many others. This variety allows unique insight from the WTA/WTP ratio. To our knowledge, no other economic issue has been experimentally studied across such a wide variety of goods.

With regard to experiment design, we find that ratios in real experiments are not significantly different from hypothetical experiments, and that incentive compatible elicitation yields higher ratios, not lower. In other words, survey techniques that would be expected to yield a "truer" picture of preferences lead either to no change or to higher observed ratios. We also found that students tended to have lower, not higher ratios, than the general public, so moving the experiments out of the classroom seems not to lead to lower ratios. The evidence on the effects of repetition is mixed; there is not strong evidence that the ratio decreases through iteration.

Therefore, high WTA/WTP ratios are not the result of experimental design features that would be considered undesirable even apart from their WTA/WTP results.

With regard to stylized facts in the observed ratios, we find that, on average, the less the good is like an "ordinary market good," the higher is the ratio. The ratio is highest for public and non-market goods, next highest for ordinary private goods, and lowest for experiments involving forms of money. A generalization of this pattern holds even when we account for differences in survey design: Ordinary goods have lower ratios than non-ordinary ones. This pattern is the major result we discover.

This paper does *not* take up the issue of whether the WTA/WTP findings provide evidence against the neoclassical paradigm, even though that potential has been the theme of much of the literature we review. Rather, our goal is to uncover the most important influences on the WTA/WTP ratio without regard to the question of whether high values of this ratio are consistent with neoclassical preferences. It is possible to investigate these influences without addressing the neoclassical question. Further, steps toward resolving the consistency of WTA/WTP responses with the neoclassical paradigm do not necessarily help in understanding the determinants of the ratio.

We end this introduction with several examples of the importance of the WTA/WTP ratio. The ratio comes into play in the context of the assignment of property rights, since the difference between a WTA and WTP experiment is a difference in property rights over the item being valued. Therefore the WTA/WTP ratio measures the consequence of assigning a property right one way or the other.

Consider the case of preserving land from development. In our set of studies, the mean WTA/WTP ratio is approximately 7. This number suggests, roughly speaking, that the amount of land that would be preserved if development rights were held by the general public is *seven times*

higher than the amount that would be preserved if the rights were deeded to the landowner and must be purchased by the public.[1] This difference would substantially alter the balance of environmental preservation and urban development in the United States.

As Knetsch has pointed out, a large difference between WTA and WTP can have other potent effects on environmental policy. These occur when the appropriate welfare measure is willingness-to-accept (because, in most instances, environmental quality can only deteriorate) but policy analysts use willingness-to-pay as the measure of benefits. For marketed goods, predictions about volume of trade or gains from trade may be seriously flawed if they are derived without recognizing the frequently large difference between individuals' willingness-to-pay and their willingness-to-accept for goods (Borges and Knetsch.) Fischel reviews the arguments about whether the WTA/WTP ratio justifies compensating landowners by more than their property's market value when property is taken by eminent domain.

There seems to be a further lesson implied by these examples. If there will be relatively few transactions between buyers and sellers of some "right" (because sellers' willingness-to-accept tends to be much higher than buyers' willingness-to-pay), then one of the most economically consequential decisions will be the *initial* establishment of the property rights, especially for environmental and other public amenities for which property rights are unclear (Knetsch). Such a prediction runs contrary to economists' understanding of the role of property rights. It is with this possibility in mind that we undertake this review.

2. The Studies

The studies are listed in Tables 1 and 2. We have not re-analyzed the authors' analyses.

[1]This case presumes that for developers, willingness-to-pay for development rights equals willingness-to-sell.

For example, if the authors chose to remove outliers, we have accepted their decision and used the means and medians they reported. When multiple trials were conducted, we have used the authors' summary of the results. Table 1 contains the studies that are usable for our analysis. Table 2 contains studies that are not usable for our analysis.

Let $i = 1 \ldots 45$ index the individual studies in Table 1. Let $k = 1 \ldots K_i$ index the experiments within a study, where K_i is the number of experiments reported in study i. The pair *ik* is the unit of observation; that is, a line in Table 1, which we call an *experiment*. Let N_{ik} be the number of subjects in experiment k of study i. There are 208 observations in Table 1.

There are 6 experiments in 5 studies in Table 1 for which the authors did not report the number of subjects. These experiments are excluded from parts of the analysis.

Dependent Variable. The variable that is the focus of most of the analysis is mean-WTA/mean-WTP. This is labeled $RATIO_{ik}$. The highest $RATIO_{ik}$ is 2858, found by SSHK in the first round of a study of salmonella contamination. We drop this observation because it is 25 times larger than the next highest ratio, 113, found by Brookshire and Coursey. Therefore, there are 201 (208-6-1) observations in our main sample.

Aggregation: Individual Means *vs.* Group Means. Most studies report only mean-WTA/ mean-WTP, even when open-ended WTA and WTP values were collected from all individuals. Only two studies reported both the mean of individual WTA/WTP ratios and the ratio of mean-WTA/mean-WTP (Dubourg, Jones-Lee, and Loomes; Eisenberger and Weber). In those two experiments, the following result obtained:

$$(1) \qquad \sum_i \frac{WTA_i}{WTP_i} > \frac{\sum_i WTA_i}{\sum_i WTP_i}$$

where i represents the individual subject. The right hand side is our dependent variable. Therefore, the literature's high reported ratios are not due to using aggregated measures.

Kachelmeier and Shehata found a correlation between WTA and WTP of 0.35 and Borges and Knetsch found a correlation of 0.24. These relatively low correlations also suggest that little has been lost by the literature's concentrating on the ratio of means.

Median *vs.* Mean. There are 41 experiments that reported ratios of both means and medians. The ratio involving means was greater than the ratio involving medians in close to 80% of the experiments. An unweighted regression of the former on the latter yielded:

$$(2) \qquad \frac{Mean\ WTA}{Mean\ WTP} = \underset{(0.72)}{2.67} + \underset{(4.15)}{1.58}\ \frac{Median\ WTA}{Median\ WTP} \qquad n = 41$$

T-ratios are in parentheses. Studies 24 and 42 reported only ratios of medians. We predicted their ratio of means using equation (2) and used those ratios in our regressions. To test sensitivity to using the predicted ratio, we also estimated our main model without studies 24 and 42.

Mean WTP. This is the denominator of $RATIO_{ik}$. In the regressions, mean WTP is deflated to 1983 dollars using the Consumer Price Index.

Private/Public Good. We label as a public good any good for which a collective decision was being made, even if the good might actually be private (*e.g.*, hunting permits) or have both private (access) and public (quality) dimensions (*e.g.*, home postal delivery). The item types are discussed further in Section 3.

Increments and Decrements. Most studies ask for willingness to pay to go from A to B and willingness to accept the change from B to A, which is the comparison that has been the subject of most theoretical analysis. A few studies ask about willingness to pay for the change from A to A + Δ and willingness to accept the change from A to A - Δ (Brookshire and Coursey;

Jones-Lee, Hammerton, and Philips; Thaler; Viscusi, Magat, and Huber); see Hanemann (1996) for discussion of the difference between this comparison and the standard one. The symbol $A \leftrightarrow B$ denotes the former and the symbol $A \pm \Delta$ denotes the latter.

Hypothetical, Real, and Random Real. A study is hypothetical if the valuation question was purely hypothetical, and real if it was carried out for real money and real exchange. A study is "random real" if several valuation questions were asked and (only) one of them was chosen at random and carried out for real money and real exchange. Random real experiments are classified as REAL = 1 in the regressions.

Elicitation Technique and Incentive Compatibility. The main techniques are the following: (1) A simple open-ended question such as "What is the maximum you would be willing to pay to obtain X?" These questions do not provide subjects with incentives to reveal their true maximum WTP, but strategic bias was typically considered unimportant when many of these studies were conducted (see Schulze, d'Arge, and Brookshire). (2) An incentive-compatible (IC) open-ended question. These use Vickrey auctions, Becker-deGroot-Marschak mechanisms, or something similar. (3) Payment card. A payment card has several values printed on it and the subject then circles his own WTP or WTA. These may be IC (*e.g.*, combined with a Vickrey auction) or not. (4) Single closed-ended, yes-no question. The individual is asked whether he would pay or accept some specified amount, call it c. The amount c varies across the sample but each individual answers only one valuation question. WTA and WTP means must be estimated from the group's responses. We used the authors' estimates unless otherwise noted. Simple closed-ended questions are incentive compatible. (5) Iterated closed-ended question. Each individual answers several closed-ended questions with different values of c. If c is varied a fixed number of times (usually twice), then WTA and WTP means must still be estimated from group responses. If c is

varied until the subject is roughly indifferent between yes and no, then each subject's individual value is eventually observed and no estimation is necessary. Iterated closed-ended questions are not incentive compatible.

Subjects. Many of the subjects were students. When a local issue was being studied with non-students, subjects are the "local public." When an issue without a clear location-specific connection was being studied with non-students, the subjects are the "public." For empirical purposes, we distinguish only between students and the rest of the subjects.

Number of Observations. If only a single number of observations is listed, then each of these N_{ik} subjects answered both a WTP and WTA question. If there were missing values for some subjects (but all subjects were asked both questions), we list the smaller number of responses, if available. If two numbers are listed, separate subject groups answered the WTA and WTP questions. The first number is WTA observations, the second is WTP observations.

Rules for Including Studies. We included every study we could find, including studies we do not have copies of but whose ratios were reported in other studies. There are 7 such studies in Table 1. Because these are frequently several years old and unpublished, we decided not to try to gather more information about them.

We exclude from the analysis three studies that used dichotomous choice but had only one offer price. The studies are listed in Table 2A. We estimated WTA and WTP from these studies using the Turnbull estimator, but such estimates are imprecise (Haab and McConnell).

A few studies elicited open-ended WTA values but dropped responses that said "I will not accept this trade under any condition." Therefore, their calculated mean WTA is a lower bound on true mean WTA. Of these, we include Dubourg, Jones-Lee, and Loomes (#13) and Viscusi, Magat, and Huber (#44) but drop Jones-Lee, Hammerton, and Philips (#49) because its

percentage of "never-accept" responses is quite large (81%). We further exclude one study from 1968 in which in mean WTP was negative but mean WTA was positive (#50). These last two exclusions are listed in Table 2B. All analysis is based on Table 1.

Weights. In calculating summary statistics in Table 3A, we weighted observations by $\text{sqrt}(N_{ik})/\text{sqrt}(K_i)$. The numerator gives higher weight to experiments with more subjects, but at a decreasing rate. The denominator treats different experiments within the same study as providing neither completely independent information (in which case the denominator would be 1) nor fully duplicated information (in which case the denominator would be K_i). There are 6 experiments in Table 1 that have no weights because authors did not report the number of subjects.

3. Type of Good

The mean of $RATIO_{ik}$ is 7.17 with standard error 0.93 (n=201). The median is 2.60. The data are summarized in Table 3. Statistics are weighted by $\text{sqrt}(N_{ik})/\text{sqrt}(K_i)$ in Tables 3A and 4.

The studies contain a wide variety of goods. We classify them in five broad categories: health and safety; lotteries; ordinary private goods; the time at which a good will be received or given up; and public or non-market goods not included in any of the other categories. We also subdivide the timing and public/non-market categories. Summary statistics for mean RATIO by good type are in Tables 4A, 4B, and 4C.

The main result is that the farther a good is from being an ordinary private good, the higher the ratio. The pattern is striking. See Table 4A. Ratios are highest for health/safety and public/non-market goods, next highest for ordinary private goods and lotteries, and lowest for surveys that involve the time at which a good is received. The health/safety and public/non-market ratios are almost identical. The latter finding is not surprising since health and safety are

themselves public/non-market goods.

The pattern continues: The closer the good comes to being actual money, the smaller the ratio. Lotteries, which were all based on money payments, have lower ratios on average than ordinary private goods. Timing studies that involve money have lower ratios than timing studies that involve goods (Table 4B). In this regard, it is reassuring (if a little self-evident) to note that no significant difference between WTA and WTP is observed when the good is money, as in experiments using tokens (KKT, p. 1328).

Timing studies that involve goods behave like ordinary private goods, although the sample size is small. Timing studies that involve money behave like lotteries. This latter finding is particularly striking given the close connection economists recognize between choice over time and choice under uncertainty (Prelec and Loewenstein, Quiggin and Horowitz).

In the public and non-market goods category (Table 4C), the lowest ratio is for tasting of sucrose octa-acetate, a bitter non-hazardous substance that is perhaps more like an ordinary private good than any of the other items in Table 4C. The elicited ratio is just slightly higher than for the ordinary private goods category. The pattern for other studies in the public and non-market goods category is similar but weaker, although the number of observations in many of the subcategories is low.

There are two anomalies, siting and hunting, that merit discussion because they pertain to issues that have been proposed to be resolved through issuing property rights.

Hunting permits are private goods and might therefore be expected to have lower ratios. But if subjects believed that their responses would also be used to make other wildlife policy decisions, then the hunting permit survey is not merely a question about a simple private good, it

is implicitly a question about wildlife management, a "non-market" good.[2]

For siting studies, the relatively low ratios may be explained by subjects believing that they *did* essentially hold a "property right." The fact that often only one suitable waste site is available in a community and that the siting survey may lead subjects to believe that any other siting-type decisions would also be subject to citizen review may make the siting problem more like a private good, *i.e.*, one that subjects had relatively clear control over; and this yields a lower ratio.[3] The sample size for this conclusion is small, however, and the explanation deserves further scrutiny.

4. Regression Analysis

This section uses a random effects model to look at the effects of type of good, survey design, mean WTP, and year. Three major survey design features are examined through regressions: hypothetical or real payoffs; elicitation technique; and student or non-student subjects. For type of good, we divided the sample into (a) ordinary goods and (b) all others. A more precise disentangling of good type and survey design is desirable, but it is nearly impossible because finer classifications of good types do not contain the full range of survey design features and so separate effects cannot be discerned.

Econometric Model

The econometric model we use is:

$$(3) \qquad RATIO_{ik} = \beta x_{ik} + u_i + \varepsilon_{ik}$$

[2]A subject's response may affect not just whether he gets a hunting permit but whether he will have to pay for permits in the future, whether hunting opportunities will be expanded/curtailed, or affect other wildlife programs.
[3]One community's responses might affect decisions made for another community. If subjects were concerned only about their own community, this would be sufficient for our explanation.

where u_i is a study-specific error and x_{ik} is a vector of experiment and study characteristics. We weight observations by sqrt(N_{ik}) to give experiments with more subjects greater weight.

This model has the structure of an unbalanced random effects model. The advantage of the random effects model is that it allows covariance among the experiments in a given study. In some studies, there is only one experiment, so a fixed effects model cannot be estimated.

The model can be estimated as a maximum likelihood model by fixing $\rho = \sigma_u^2 / (\sigma_u^2 + \sigma_\varepsilon^2)$, estimating β conditional on ρ, and then iterating on ρ until the likelihood function is maximized (Nerlove, Chapter 19). The value of ρ will be between 0 and 1, with $\rho = 0$ being the ordinary least squares value.

In the regressions, all values of ρ are less than or equal to 0.12, with two regressions having $\rho = 0$ (regressions 4 and 6). A low value of ρ indicates that between-experiment random variation is large relative to within-study random variation. This is a desirable finding; it suggests that quantifiable survey characteristics are capturing the greater part of the explainable variation.

The results of the weighted random effects model are in Table 5. The basic regression is the first column (regression 1). We also ran each of the Table 5 regressions using: (i) OLS with the observations weighted by sqrt(N_{ik})/sqrt(K_i), and (ii) random effects with unweighted observations. The regressions gave the same patterns of sign and significance as in Table 5. An unweighted OLS regression yielded different conclusions.

Type of Good

Ordinary *vs.* Non-Ordinary Goods. Non-ordinary goods have significantly higher ratios – they are typically 6 to 8 points higher than ordinary goods. This effect occurs even when we take survey design features and mean WTP into account. The pattern that we uncovered in Section 3,

that the farther a good is from being an ordinary private good the higher the ratio, remains an prominent and robust feature of the observed WTA/WTP behavior.

Only "ordinary private goods" (see Table 4A) are counted as ordinary goods in these regressions. Lotteries and timing experiments are counted as non-ordinary goods. We adopt this division even though the previous analysis shows lotteries and timing to be like ordinary private goods in many ways. The reason for our classification is that *ex ante* – before any statistical analysis was conducted – we suspect that most economists would have proffered that experiments with lotteries or timing were more unusual than experiments with goods like mugs and pens; that they were more like non-market goods. We did not want the observed differences in the ratios to influence how we classified the goods for our analysis of survey design effects.

Survey Design

We next turn attention to survey design features. The main question that confronts us is whether high ratios can be said to reflect "true" preferences. Much has been written elsewhere about survey design for valuing non-market goods, and our compilation of studies provides some insight into those general survey design issues. Our main focus, however, is whether high WTA/WTP ratios are the result of questionable survey design and easily identifiable influences, or instead truly present a broad-based picture of preferences.

Hypothetical vs. Real Payoffs. Real experiments do not yield significantly different ratios from hypothetical experiments. In some instances, such as when we account for whether an experiment uses closed-ended elicitation (regression 3), the effect of realness essentially disappears entirely. Realness has its statistically strongest effect only when mean WTP is not included. This effect loses its significance when mean WTP is included.

The high values of the WTA/WTP ratio initially led some researchers to claim that hypothetical surveys were unsuitable for eliciting preferences. Our results show that real experiments do not yield significantly lower ratios. Thus, any claim about the suitability of hypothetical surveys must rest on evidence other than the size of the WTA/WTP ratio.

Elicitation Technique. Studies that are incentive compatible (IC) have significantly higher ratios. This result is unexpected. If high ratios were the result of "strategizing" by the subjects, rather than a feature of true preferences, then we would expect incentive compatible experiments to result in lower ratios. They do not.

We also looked at other categorizations of elicitation techniques. Open-ended studies, typically construed as not being incentive-compatible, had no statistically significant effect (regression 2). Closed-ended questions, which are considered incentive-compatible, do yield lower ratios than non-closed-ended questions (regression 3), although the statistical significance of this result is not as strong as for the other design features.

Unfortunately, intuition about the effect of elicitation method is complicated. Under some approaches, subjects might either overstate WTA or WTP (if they want a good to be provided and feel they will not have to pay full price) or under-report them, if they think they might thereby get the good cheaper. The overall effect that elicitation will have on the ratio is unknown. The observed effects of open-ended and closed-ended questions may provide insight on this topic.

Students vs. Non-Students. Students exhibit significantly and substantially lower ratios than non-students. This result is unexpected. Its main implication for our research is that high WTA/WTP ratios are exhibited by the general public, not just college undergraduates.

For non-ordinary goods, students exhibit lower ratios than non-students. For ordinary goods, students exhibit the same ratios as non-students, because in regressions 6 and 7 we cannot

reject the hypothesis that $\beta_{STUDENT} + \beta_{STUDENT \times ORDINARY} = 0$ (F = 0.17 with p-value of .68 for regression 6; and F = 0.06 with p-value of .79 for regression 7).

Ordinary Goods and Survey Features. We also checked whether these survey design features had the same effect on both ordinary and non-ordinary goods. The hypothesis that coefficients on the three survey features crossed with ORDINARY are jointly zero cannot be rejected in regression 6 (F = 1.96). In other words, with respect to realness, elicitation approach, and student subjects, the responses are the same for both ordinary and non-ordinary goods.

If we look at these effects individually, however, students do appear to behave a differently for ordinary and non-ordinary goods. For ordinary goods, students have the same ratios as non-students, since the coefficients on STUDENT and STUDENT×ORDINARY sum to approximately zero (as discussed above). For non-ordinary goods, students have lower ratios.

Likewise, the hypothesis that MEAN× ORDINARY is zero cannot be rejected (regression 4). In words, we find that the relationship between mean WTP and WTA/WTP is the same for ordinary and non-ordinary goods.

Other Findings

We next take up other survey design patterns and possible influences on the ratio. These yield less direct evidence about the "reliability" of the ratio, but are useful because they help form a broad picture of WTA/WTP behavior.

Mean WTP. This variable requires us to exclude 32 experiments that do not report mean WTP. We find that the higher is WTP, the lower is the WTA/WTP ratio. On average, a $200 increase in mean WTP causes the ratio to decrease by 1 point. The relationship between RATIO and mean WTP is the same for both ordinary goods and other goods (regression 4).

It is possible that mean WTP is endogenous. We performed a Hausman test for endogeneity of the right hand side mean WTP, both in an OLS model ($\rho = 0$) and for the model with the optimal value of ρ, for regressions 1-3 and 7-9. In all cases, the test statistic does not rise to a level that would lead to rejection of the null hypothesis of no endogeneity.

Year. There has been a slight increase in the ratio over the thirty years that it has been studied. This result is surprising because it does not have an obvious behavioral explanation. One explanation is that as the existence of a disparity has been established over the years, researchers have tended to study situations where it might arise.

When Benzion, Rapaport, and Yagil (BRY, Study 4) is excluded, YEAR has a small and statistically insignificant coefficient (regression 9). However, in the analog to regression 5 with BRY excluded (not shown), the YEAR coefficient is larger and statistically significant. In other words, the increase in the ratio over the years is a relatively robust finding that is only mildly sensitive to inclusion of BRY.

Income. Only a few studies have looked at the relationship between WTA/WTP and income. Therefore, we do not include income as an explanatory variable in the regressions. Adamowicz, Bhardwaj, and McNab found that WTA-WTP is decreasing in income, but the coefficient is not significant. Horowitz found no significant relationship between WTA/WTP and subjects' wealth.

Individual Studies. In regressions 8 and 9, we re-ran regression 1 without KKT and Thaler (Studies 24 and 42) and BRY (Study 4), respectively. Neither exclusion affects the results much. KKT and Thaler reported only ratios of medians, but their techniques were similar to the other studies, so it is not surprising that their results are consonant with the overall findings. BRY, however, differed substantially from the other studies. In BRY, each student subject was

given a 64 question survey that asked the subject, for example, to "state an amount of money $x so that he or she would be indifferent between paying $y t time periods from now or paying $x immediately" (p. 275). The sheer number of questions (*i.e.*, experiments) makes this study stand out. Also, for some responses it was necessary for us to convert a future WTA or WTP to its present value; we used the discount rate implicit in the question. Dropping this study has almost no effect on the coefficients but does slightly increase the standard errors of some estimates, as might be expected. We also ran the other regressions without BRY (not shown). There was little change. Regressions 1-7 therefore include all of the studies.

Sample Selection. The studies in Table 1 are not a random sample. They must be selected by editors or, for unpublished studies, by the other researchers that cite them and, at a minimum, by the authors themselves who felt the experiment worth conducting and the results worth writing up. We see three ways in which sample selection might affect our results. First, experiments in which the elicited WTA's were extremely high are less likely to enter our sample. The main consequence is that our observed mean RATIO is below the population mean. We suspect that this will be more of a problem for the types of goods that tend to exhibit high RATIOS.[4] Therefore, the coefficient on ORDINARY is probably biased upward. In other words, the difference between ordinary and non-ordinary goods may be greater than our regressions indicate.

Studies using non-incentive compatible techniques also likely have a lower probability of being published. Even in our own analysis, for example, we have in places excluded the BRY study because of the suspicious incentive structure. However, the fact that some elicitation methods are not included in the empirical analysis does not necessarily bias the coefficient on IC

[4]An example is Study 49, a health and safety study in which at least 80% of subjects implicitly claimed an infinite WTA. Another example is our own censoring of the largest SSHK observation, also a health and safety study.

as long as the relationship between the ratio and the elicitation method is monotonic and the exclusion does not remove all variation in IC.

5. Do Practice and Familiarity Lower the WTA/WTP Ratio?

Several authors have suggested that repeating an experiment for the same subjects might lower the ratio, primarily because WTA would be reduced as subjects realized they would be content to take home a smaller amount of real money than they first thought. KKT refer to the "conclusion reached in some other studies that the WTA-WTP discrepancy is greatly reduced by market experience" (p. 1335). We list the relevant studies in Tables 6A and 6B.

The evidence is mixed. Brookshire and Coursey found that the ratio decreased. Their study design is not readily generalizable because they told the subjects in their experiments the compensation fund that was available and then elicited WTA bids until either total WTA was less than the fund or five trials had been conducted; in their WTP experiments, they told subjects the cost of the item and elicited bids until they covered that cost or reached a maximum of five trials.

CHS also showed a decrease in WTA but their sample size was small and, like Brookshire and Coursey, they repeated their experiment with an explicit goal, namely until a consensus was reached, with a maximum of ten trials. In a reassessment, Gregory and Furby emphasized the smallness of CHS's samples and claimed that the paper's reported convergence "depends upon inclusion of ... suspicious outlying groups" (p. 285). There appears to be no convergence in CHS's initial repeated hypothetical rounds, only in the final real rounds.

Knez *et al.* looked at the number of instances in which an individual subject's WTP exceeded his WTA and concluded that this number fell when the experiment was repeated. Under

this result, the WTA/WTP ratio would likely have been *rising* with repetition. They did not report any ratios, so actual results are unknown.

The strongest evidence of a falling ratio comes from SSHK, who showed a significant decrease in the ratio between their first and middle rounds, for both contaminated sandwiches and mugs. This was observed in five separate experiments with a total of roughly 60 subjects. In a subsequent experiment, Shogren *et al.* (1996, cited in Shogren and Hayes) showed the ratio fell in a Vickrey auction but not in a Becker-DeGroot-Marschak auction, and argue that the former is more like a market (Shogren and Hayes).

Studies that explicitly claim that WTA/WTP did not fall include KKT and Morrison.

In summary, the idea that the ratio will fall as subjects become familiar with an experiment may be intuitively compelling, but the evidence is weak. Some experimental techniques appear to aid convergence, such as repeating the experiment to aim for a goal and having an outside market for the good, as KKT did for their last mug experiment. In many cases, even when the ratio does fall, it falls to levels that still seem high.

Last, we should note that even if the ratio falls with practice, the implications are somewhat limited because the ratio has its most important economic role in environmental and public policy decisions for which familiarity and practice are likely to be absent.

6. Concluding Comments

Our research investigates a body of empirical work that has appeared to challenge two important strands of economics: Neoclassical consumer theory and the role of property rights. Before this challenge can stand it has been necessary to establish two related points: (i) The high observed WTA/WTP ratios appear to represent "true" preferences. Our claim is based on the

findings that hypothetical or non-incentive compatible experiments do not yield statistically significantly higher ratios; that high ratios are exhibited by a broad-based (*i.e.*, non-student) population; and that familiarity with the experiments does not demonstrably lead to lower ratios.

(ii) A robust and economically useful response pattern exists. We find that the farther a good is from being an "ordinary private good," the higher the ratio. The pattern prevails, in a coarser version, even when we account for possible differences in survey designs. The extensive body of literature on the WTA/WTP ratio provides sturdy evidence for these claims.

We leave for future papers the two major unanswered questions that this research raises. First, and we believe most important: To what extent can a disparity between WTA and WTP, or anticipation of it, be observed in real world economic choices? Are there "remedies" and, if so, are they desirable? Second, does the WTA/WTP disparity provide evidence sufficiently broad and deep against the neoclassical model? Does that evidence warrant substantially modifying that model, at least in some situations for which economists' expertise might be called upon?

7. References

(A separate list contains the bibliography of studies.)

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Table 3A. SUMMARY STATISTICS (Weighted)

	N	Mean	Standard Error
$RATIO_{ik}$	201	7.17	0.93
$RATIO_{ik}$ (excludes estimated RATIOs)[a]	175	7.18	1.02
$RATIO_{ik}$ (excludes Study 4)	169	7.86	1.07
Median WTA/ Median WTP	66	5.52	1.03
MEAN WTP ($1983)	169	$175	22.40
REAL (Real = 1, hypothetical = 0)	201	0.22	0.03
IC (Incentive compatible = 1)	201	0.25	0.03
STUDENT (Student subjects = 1)	201	0.35	0.03
OPEN-ENDED (Open-ended = 1)	201	0.56	0.04

[a]Excludes 25 experiments in Study 24 and 1 experiment in #42 in which only ratios of medians were reported.

Table 3B. SUMMARY STATISTICS (Unweighted) – QUANTILES

	Minimum	10%	25%	50%	75%	90%	Maximum
$RATIO_{ik}$ (n = 206)	0.74	1.16	1.66	2.60	6.12	10.52	112.67
Median WTA/Median WTP (n = 67)	1.00	1.33	1.67	2.33	3.00	7.28	42.94
Mean WTP ($1983) (n = 173)	$0.12	$0.34	$0.91	$3.73	$34.51	$280	$5847

Table 4A. RATIO BY TYPE OF GOOD

	Mean RATIO	Standard Error	Number of Expts.
Public or non-market goods	10.41	2.53	46
Health and safety	10.06	2.28	32
Ordinary private goods	2.92	0.30	59
Lotteries	2.10	0.20	25
Timing	1.95	0.17	39
All goods	7.17	0.93	201
Unknown number of subjects	6.71	(not calculated)	6

Public or non-market: Studies 2, 5, 6, 8, 9, 12, 17, 18, 20, 26, 31, 35, 36, 37, 39, 43, 45.
Health and safety: 13, 16, 32, parts of 38, 44.
Ordinary private goods: 1, 3, 7, 24, 29, 30, 34, parts of 38, 41.
Lotteries: 11, 15, 19, 23, 25, 27, 40.
Timing: 4, 21, 28, 42.
Unknown number of subjects: 10, 14, 22, last observation of 24, 33.

Table 4B. RATIO BY TYPE OF GOOD: TIMING STUDIES

Type of Good	RATIO		
	Mean	S.E.	n
Timing of receipt of private goods (Study 28)	2.82	0.39	5
Timing of receipt of money (4, 21, 42)	1.84	0.18	34

Table 4C. RATIO BY TYPE OF GOOD: PUBLIC/NON-MARKET GOODS

Type of Good	RATIO		
	Mean	S.E.	n
Misc. public and non-market goods (2, 6, 8, 18, 31, 39, 43, 45)	27.57	7.50	19
Hunting (5, 9, 17, 20)	10.47	5.29	8
Visibility (35, 36)	7.40	2.31	7
Siting (26, 37)	4.14	1.83	4
Sucrose octa-acetate (12)	3.99	0.47	8

TABLE 5. REGRESSION RESULTS. DEPENDENT VARIABLE = RATIO.

	#1	#2	#3	#4	#5	#6	#7	#8[b]	#9[c]
INTERCEPT	6.08	4.71	2.90	8.01	2.94	4.90	7.44	5.16	18.59
	(1.12)	(1.01)	(0.59)	(1.37)	(0.77)	(1.05)	(1.04)	(0.79)	(1.96)
ORDINARY	-6.12	-6.32	-5.92	-8.14	-5.02	-8.22	-16.44	-6.73	-7.84
(Ordinary = 1)[a]	(1.78)	(1.77)	(1.70)	(1.98)	(1.62)	(1.14)	(2.86)	(1.81)	(2.13)
REAL	-7.09	1.17	-0.27	-6.10	-6.75	-5.54	-6.83	-7.79	-8.02
(Real = 1)	(1.47)	(0.34)	(0.08)	(1.23)	(1.78)	(1.31)	(1.43)	(1.51)	(1.56)
IC	10.59	--	--	11.14	9.34	11.18	11.95	10.87	6.93
(Inc. Comp. = 1)	(2.45)			(2.55)	(2.56)	(2.90)	(2.81)	(2.39)	(1.44)
STUDENT	-13.24	-11.71	-12.50	-14.49	-10.04	-13.44	-15.71	-13.05	-15.28
(Student = 1)	(3.75)	(3.16)	(3.47)	(3.81)	(3.20)	(3.73)	(4.41)	(3.46)	(3.97)
MEAN WTP	-0.005	-0.005	-0.005	-0.005	--	--	-0.005	-0.005	-0.03
(Deflated to $1983)	(1.72)	(1.80)	(1.95)	(1.71)			(1.81)	(1.63)	(3.83)
OPEN-ENDED	--	-0.54	--	--	--	--	--	--	--
(Open-ended = 1)		(0.21)							
CLOSED-ENDED	--	--	-5.17	--	--	--	--	--	--
(Closed-ended = 1)			(1.73)						
MEAN WTP ×	--	--	--	0.34	--	--	--	--	--
ORDINARY				(0.89)					
REAL ×	--	--	--	--	--	-0.26	--	--	--
ORDINARY, β_5						(0.02)			
IC ×	--	--	--	--	--	-6.99	--	--	--
ORDINARY, β_6						(0.55)			
STUDENT×	--	--	--	--	--	11.53	14.13	--	--
ORDINARY, β_7						(1.86)	(2.23)		
YEAR	0.07	0.10	0.15	0.05	0.09	0.07	0.07	0.07	0.02
	(1.06)	(1.60)	(2.11)	(0.65)	(1.65)	(1.06)	(0.82)	(0.98)	(0.19)
ρ	0.08	0.11	0.10	0[d]	0.12	0[d]	0.05	0.07	0.07
R^2	0.20	0.16	0.18	0.20	0.14	0.17	0.23	0.20	0.23
N	169	169	169	169	201	201	169	152	137

T-statistics are in parentheses. [a]Ordinary goods: #1, 3, 7, 24, 29, 30, 34, parts of 38, and 41. [b]Excludes most of #24 (KKT) and all of 42, which reported only ratio of medians. [c]Excludes #4 (BRY). [d]Equations estimated by OLS.

Table 6A. STUDIES IN WHICH HYPOTHETICAL ELICITATION(S) PRECEDED REAL ELICITATION(S)

Study	Procedure	Sequence of Mean WTA/Mean WTP
7. Boyce *et al.*, 1992	10 practice rounds[a]	Not reported
	1 binding round	1.66 (no-kill), 2.36 (kill)
12. CHS, 1987	1 practice round[a]	3.79
	Iterated practice rounds[a] – First & final bids	5.26, 3.80
	4 practice rounds[b]	3.95, 6.13, 3.90, 3.49
	Maximum of 6 rounds until no subject objected[b]	1.59
24. KKT, 1990	3 practice rounds[c] (pens)	6.00, 6.00, 5.00
	1 binding round	5.00

[a]No information on other bids was announced after a round. [b]Other subjects' bids were announced after each round. [c]The market-clearing bid was announced after each round.

Table 6B. STUDIES WITH REPEATED, RANDOM-REAL ELICITATIONS

Study	Repetitions	After each round: - No information on other bids announced or - Mean or market-clearing bid announced	Sequence of Mean WTA/Mean WTP
24. KKT	4 (mug)	Market-clearing bid announced	1.91, 2.33, 2.33, 2.33
	4 (pen)	Market-clearing bid announced	3.33, 2.33, 3.00, 2.33
	4 (mug)	Market-clearing bid announced	2.71, 2.11, 2.11, 1.89
	4 (binocs.)	Market-clearing bid announced	1.67, 1.67, 2.33, 2.33
	5 (mug)	Market-clearing bid announced	3.8, 2.8, 2.2, 1.8, 1.8
34. Morrison	5 (chocolates)	No information on others' bids announced	0.99, 1.09, 1.09, 1.13, 1.13
	5 (mug)	No information on others' bids announced	2.01, 2.22, 2.42, 2.29, 2.19
38. SSHK	5 (candy)	High bidder & reigning price announced	1.28, 1.16, 0.98, 0.93, 0.93
	20 (sandwich) (Rounds 1, 7-10, & 17-20)	High bidder & reigning price announced (?)	8.74, 2.11, 2.60 (pathogen 1) 2858, 3.39, 2.20 (pathogen 2) 4.00, 3.22, 3.66 (pathogen 3) 16.09, 6.42, 6.61 (pathogen 4) 34.04, 3.05, 4.65 (pathogen 5)
	10 (mug)	High bidder & reigning price announced (?)	2.76, 1.74, 1.10, 1.05, 1.07, 1.45, 1.29, 1.24, 1.16, 0.74[a] 2.74, 1.98, 1.27, 1.03, 1.21, 1.31, 0.97, 1.19, 1.23, 0.80[a]
8. Brookshire and Coursey	Repeated until WTP covered cost or WTA did not exceed fund. Max of 5 trials.	Sum of WTP or WTA announced	3.92, 3.63, 2.90, 2.28 (25 tree) 8.08, 8.19, 11.16, 8.28, 7.39 (50 tree)

[a]WTA was elicited two ways but only one set of WTP's was elicited. Thus, the denominators are the same in the two sequences. In the first WTA set, an identical mug was for sale just outside the experiment. In the second set, no mug was available. See SSHK for details.

Table 1. MAJOR STUDIES

Study	Med. WTA/ Med. WTP	Mean WTA/ Mean WTP	Mean WTP	Private/ public good	Hyp./real	Elicitation technique	Subjects	N
1. Adamowicz, Bhardwaj, and McNab, 1993	-	1.95	$4.76	private (movie ticket)	hyp.	open	students	157
	1.85	1.70	$28.50	private (hockey ticket w/ subst.)	hyp.	closed	students	150
	1.87	1.91	$36.60	private (hockey tkt w/o subst.)	hyp.	closed	students	150
2. Banford, Knetsch, and Mauser, 1979/80	2.60	2.78	$43.10	public (ocean pier)	hyp.	closed, iterated	local public	71
	4.40	4.24	$21.97	public (home postal deliv.)	hyp.	closed, iterated	local public	71
3. Bateman *et al.*, 1997	3.95	2.81	£0.78	private (10 chocolates)	random real	open; IC	students	89, 96
	2.00	2.09	8.7 chocs.	private (money -- £2.00)	random real	open; IC	students	96, 89
	1.30	2.53	£0.60	private (4 cans of Coke)	random real	open; IC	students	89, 96
	2.00	2.00	2.5 cans	private (money -- £0.80)	random real	open; IC	students	96, 89
(Pounds sterling were converted at £1 = $1.55. To calculate mean WTP for treatments 2 and 4, we used the prices implied by treatments 1 and 3.)								
4. Benzion, Rapoport, and Yagil, 1989	-	1.76	$4.90	private ($40 in 0 ↔ 6 mos.)	hyp.	open	students	204
	-	1.92	$6.80	private ($40 in 0 ↔1 yr.)	hyp.	open	students	204
	-	1.96	$9.80	private ($40 in 0 ↔2 yrs.)	hyp.	open	students	204
	-	3.35	$13.40	private ($40 in 0 ↔4 yrs.)	hyp.	open	students	204
	-	1.74	$19.20	private ($200 in 0 ↔ 6 mos.)	hyp.	open	students	204
	-	1.71	$26.00	private ($200 in 0 ↔1yr.)	hyp.	open	students	204
	-	2.22	$40.00	private ($200 in 0 ↔ 2 yrs.)	hyp.	open	students	204
	-	2.46	$67.50	private ($200 in 0 ↔ 4 yrs.)	hyp.	open	students	204
	-	2.00	$80.20	private ($1000 in 0 ↔ 6 mos.)	hyp.	open	students	204
	-	2.07	$117.20	private ($1000 in 0 ↔ 1 yr.)	hyp.	open	students	204
	-	2.21	$176.10	private ($1000 in 0 ↔ 2 yrs.)	hyp.	open	students	204
	-	2.38	$319.70	private ($1000 in 0 ↔ 4 yrs.)	hyp.	open	students	204
	-	1.24	$321.10	private ($5000 in 0 ↔ 6 mos.)	hyp.	open	students	204
	-	1.35	$535.80	private ($5000 in 0 ↔ 1 yr.)	hyp.	open	students	204
	-	1.97	$728.20	private ($5000 in 0 ↔ 2 yrs.)	hyp.	open	students	204
	-	1.77	$1366.00	private ($5000 in 0 ↔ 4 yrs.)	hyp.	open	students	204
	-	1.27	$4.90	private (-$40 in 0 ↔ 6 mos.)	hyp.	open	students	204
	-	1.12	$7.06	private (-$40 in 0 ↔ 1 yr.)	hyp.	open	students	204
	-	1.01	$12.38	private (-$40 in 0 ↔ 2 yrs.)	hyp.	open	students	204
	-	1.00	$17.62	private (-$40 in 0 ↔ 4 yrs.)	hyp.	open	students	204
	-	1.03	$20.96	private (-$200 in 0 ↔ 6 mos.)	hyp.	open	students	204
	-	1.18	$27.97	private (-$200 in 0 ↔ 1 yr.)	hyp.	open	students	204

	-	1.03	$53.07	private (-$200 in 0 ↔ 2 yrs.)	hyp.	open	students	204
	-	0.88	$88.12	private (-$200 in 0 ↔ 4 yrs.)	hyp.	open	students	204
	-	1.21	$88.59	private (-$1000 in 0 ↔ 6 mos.)	hyp.	open	students	204
	-	1.23	$130.37	private (-$1000 in 0 ↔ 1 yr.)	hyp.	open	students	204
	-	0.81	$269.49	private (-$1000 in 0 ↔ 2 yrs.)	hyp.	open	students	204
	-	0.86	$445.08	private (-$1000 in 0 ↔ 4 yrs.)	hyp.	open	students	204
	-	1.38	$324.35	private (-$5000 in 0 ↔ 6 mos.)	hyp.	open	students	204
	-	1.72	$408.26	private (-$5000 in 0 ↔ 1 yr.)	hyp.	open	students	204
	-	1.27	$791.97	private (-$5000 in 0 ↔ 2 yrs.)	hyp.	open	students	204
	-	1.28	$1337.73	private (-$5000 in 0 ↔ 4 yrs.)	hyp.	open	students	204
5. Bishop, Heberlein,	-	4.81	$21.00	private (goose hunting permit)	hyp.	closed	hunters	306
and Kealy, 1979 and 1983	-	6.18	$11.00	private (goose hunting permit)	hyp.	open	hunters	306
6. Bowker and MacDonald, 1993	8.44	6.98	$105.31	public (factory odor)	hyp.	payment cards	local public	32, 34
7. Boyce, Brown,	-	1.66	$4.81	private (small tree)	real	open; IC	staff	30, 26
McClelland,	-	2.36	$7.81	private (tree killed if not bought/sold)	real	open; IC	staff	29, 30
Peterson, and Schulze, 1992. (WTA responses above $40 were set equal to $40 in the analysis.)								
8. Brookshire	20.81	61.11	$14.00	public (tree density ± 25)	hyp.	paym. cards	local public	45
and Coursey, 1987	42.94	89.40	$19.40	public (tree density ± 50)	hyp.	paym. cards	local public	45
	2.57	56.06	$14.40	public (tree density ± 25)	hyp.	paym. cards?; IC	local public	47
	7.28	112.67	$15.40	public (tree density ± 50)	hyp.	paym. cards?; IC	local public	47
	1.61	3.92	$7.31	public (tree density ± 25)	hyp.	paym. cards?; IC	local public	12, 17
	8.00	8.08	$8.33	public (tree density ± 50)	hyp.	paym. cards?; IC	local public	13, 12
	1.42	2.42	$7.31	public (tree density ± 25)	real	paym. cards?; IC	local public	12, 17
	2.49	7.39	$12.92	public (tree density ± 50)	real	paym. cards?; IC	local public	13, 12
(Entries 3 and 4 used a Smith auction, probably with payment cards. This auction is "almost" incentive-compatible. Entries 5-8 are from a repeated Smith auction, probably with payment cards; initial and final round data are reported. Last round counted for real money, although not for the decision described in the survey.)								
9. Brookshire, Randall,	-	1.57	$43.64	public (elk encounters 0.1 ↔ 1)	hyp.	closed, iterated	hunters	10, 11
and Stoll, 1980	-	2.64	$54.06	public (elk encounters 1 ↔ 5)	hyp.	closed, iterated	hunters	12, 16
	-	6.47	$32.00	public (elk encounters 5 ↔ 10)	hyp.	closed, iterated	hunters	9, 15
10. Casey, 1990, reported in	-	4.00	--	private (lottery; E.V. = $100)	?	not IC	unknown	unknown

Eisenberger and Weber, 1995								
11. Coombs, Bezembinder, and Goode, 1967	-	3.21	$1.25	private (lottery)	hyp.	open	students, public	40
12. Coursey, Hovis, and Schulze, 1987	2.85	3.79	$2.50	private (taste SOA)	hyp.	open	students	32, 32
	3.37	5.26	$2.20	private (taste SOA)	hyp.	open	students	32, 32
	-	3.80	$2.58	private (taste SOA)	hyp.	open, iterated	students	32, 32
	-	3.95	$2.50	private (taste SOA)	hyp.	open; IC	students	32, 32
	-	6.13	$1.92	private (taste SOA)	hyp.	open; IC	students	32, 32
	-	3.90	$2.70	private (taste SOA)	hyp.	open; IC	students	32, 32
	-	3.49	$2.88	private (taste SOA)	hyp.	open; IC	students	32, 32
	2.64	1.59	$2.94	private (taste SOA)	real	open; IC	students	32, 32

(i) Ratios of medians were reported in Harless (also Kahneman, Knetsch, and Thaler), not in CHS. (ii) All subjects tasted SOA at the beginning of the second treatment. Because the subjects were then familiar with SOA, the subsequent experiments were called 'semi-hypothetical.' Subjects were unfamiliar with SOA in the first treatment. (iii) A Vickrey auction was used but unanimity among the "winners" was required; otherwise, another elicitation took place. Unanimity took between 2 and 5 rounds to achieve in the real-money experiment. Only the final, unanimous round is reported.

13. DuBourg, Jones-Lee, and Loomes, 1994	-	1.95	-	private (slight, perm. disability)	hyp.	closed, iterated	public	26
	-	2.40	-	private (injury req. 1-3 yr. recovery)	hyp.	closed, iterated	public	24
	-	2.14	-	private (slight, perm. disability)	hyp.	closed, iterated	public	27
	-	2.07	-	private (injury req. 1-3 yr. recovery)	hyp.	closed, iterated	public	27
	-	3.75	-	private (sligt, perm. disability)	hyp.	payment cards	public	20
	-	4.63	-	private (injury req. 1-3 yr. recovery)	hyp.	paym. cards	public	19
	-	3.07	-	private (slight, perm. disability)	hyp.	paym. cards	public	20
	-	3.15	-	private (injury req. 1-3 yr. recovery)	hyp.	paym. cards	public	20
	-	5.13^m	-	private (slight, perm. disability)	hyp.	paym. cards	public	19
	-	4.99^m	-	private (injury req. 1-3 yr. recovery)	hyp.	paym. cards	public	21
	-	6.16^m	-	private (slight, perm. disability)	hyp.	paym. cards	public	20
	-	4.55^m	-	private (injury req. 1-3 yr. recovery)	hyp.	paym. cards	public	20

(Superscript m denotes mean WTA/WTP. For the first 8 treatments, mean WTA/WTP was 2.40, 4.13, 2.71, 3.08, 4.43, 4.72, 4.45, and 6.11. Mean WTA/mean WTP was not reported for the last 4 treatments.)

14. Eby, 1975, reported in Meyer, 1979	-	3.50	-	unknown (fishing in a park)	hyp.	unknown	unknown	unknown

15. Eisenberger and Weber, 1995	-	1.44	DM4.23	private (lottery DM10, 0.5)	real	open; IC	students	54
	-	1.58	DM3.51	private (lottery DM10, ambig.)	real	open; IC	students	54
	-	1.50	DM4.79	private (lottery -DM10, 0.5)	real	open; IC	students	54
	-	1.50	DM5.21	private (lottery -DM10, ambig.)	real	open; IC	students	54
	-	1.45	DM3.52	private (lottery w/ gain; ambig.)	real	open; IC	students	25
	-	1.34	DM4.62	private (lottery w/ gain; less ambig.)	real	open; IC	students	25
	-	1.58	DM4.88	private (lottery w/ loss; ambig.)	real	open; IC	students	25
	-	1.38	DM5.86	private (lottery w/ loss; less ambig.)	real	open; IC	students	25
(Deutsche marks were converted at DM1 = $0.67.)								
16. Gerking, de Haan, and Schulze, 1988	-	2.56	$665.00	private (fatal job accident, ± 1 step)	hyp.	payment cards	public	871
17. Hammack and Brown, 1974 (pp. 26-7)	-	4.23	$247.00	private (duck hunt. permit)	hyp.	open	hunters	2455
18. Hanemann, Hoehn, and Loomis, reported in Mansfield, 1994; and Hoehn and Loomis, 1993	-	5.07	$28.76	public (wetlands maintenance)	hyp.	closed	public	540, 521
(Hoehn and Loomis included only WTP results. Mansfield estimated WTA & WTP equations from original data and calculated mean WTA & mean WTP.)								
19. Harless, 1989	1.33^m	1.70^m	-	private (lottery $2, 0.24)	real	open; IC	unknown	17
	1.50^m	3.46^m	-	private (lottery $6, 0.04)	real	open; IC	unknown	17
	1.83^m	2.20^m	-	private (lottery $4, 0.03)	real	open; IC	unknown	17
	1.33^m	2.25^m	-	private (lottery -$4, 0.12)	real	open; IC	unknown	17
	1.50^m	2.51^m	-	private (lottery -$1, 0.24)	real	open; IC	unknown	17
	1.00^m	4.27^m	-	private (lottery -$6, 0.02)	real	open; IC	unknown	17
(Superscript m denotes median WTA/WTP or mean WTA/WTP.)								
20. Heberlein and Bishop, 1986 (pp. 129-31)	-	20.83	$40.00	private (deer hunt. permit)	hyp.	open; IC	hunters	70, 272
	-	51.48	$23.00	private (deer hunt. permit)	real	open; IC	hunters	70, 270
21. Horowitz, 1991	1.60	2.44	$30.64	private ($50 received 1 mo. later)	real	open; IC	students	52
22. Hueth, Voorhees, and Cosagrande, 1981, reported in Bergstrom, 1990	-	1.12	$25.00	unknown (moth damage prev.)	hyp.	unknown	unknown	unknown

23. Kachelmeier and Shehata, 1992	1.67	1.83	$6.07	private (lottery $20, 0.5)	real	open; IC	students	15
	2.00	2.07	$5.12	private (lottery $20, 0.5)	real	open; IC	students	13
24. Kahneman, Knetsch, and Thaler, 1990	1.91	-	$2.75	private (mug)	random real	paym. card; IC	students	22, 22
	2.33	-	$2.25	private (mug)	random real	paym. card; IC	students	22, 22
	2.33	-	$2.25	private (mug)	random real	paym. card; IC	students	22, 22
	2.33	-	$2.25	private (mug)	random real	paym. card; IC	students	22, 22
	3.33	-	$0.75	private (pen)	random real	paym. card; IC	students	22, 22
	2.33	-	$0.75	private (pen)	random real	paym. card; IC	students	22, 22
	3.00	-	$0.75	private (pen)	random real	paym. card; IC	students	22, 22
	2.33	-	$0.75	private (pen)	random real	paym. card; IC	students	22, 22
	2.71	-	$1.75	private (mug)	random real	paym. card; IC	students	19, 19
	2.11	-	$2.25	private (mug)	random real	paym. card; IC	students	19, 19
	2.11	-	$2.25	private (mug)	random real	paym. card; IC	students	19, 19
	1.89	-	$2.25	private (mug)	random real	paym. card; IC	students	19, 19
	1.67	-	$0.75	private (binoculars)	random real	paym. card; IC	students	19, 19
	1.67	-	$0.75	private (binoculars)	random real	paym. card; IC	students	19, 19
	2.33	-	$0.75	private (binoculars)	random real	paym. card; IC	students	19, 19
	2.33	-	$0.75	private (binoculars)	random real	paym. card; IC	students	19, 19
	6.00	-	-	private (pen)	hyp.	open; IC	students	13, 13
	6.00	-	-	private (pen)	hyp.	open; IC	students	13, 13
	5.00	-	-	private (pen)	hyp.	open; IC	students	13, 13
	5.00	-	-	private (pen)	real	open; IC	students	13, 13
	3.80	-	-	private (mug)	random real	open; IC	students	37, 37
	2.80	-	-	private (mug)	random real	open; IC	students	37, 37
	2.20	-	-	private (mug)	random real	open; IC	students	36, 36
	1.80	-	-	private (mug)	random real	open; IC	students	36, 37
	1.80	-	-	private (mug)	random real	open; IC	students	37, 37
	2.56	2.62	$2.21	private (mug)	real	paym. card; IC	students	30, 29
	2.80	3.18	$1.25	private (choc. bar)	unknown	open; IC	students	unknown

(These data do not include the experiments run with induced values. The payment card used a response form like the following (for sellers): "At a price of $8.75 I will sell ___ I will not sell ____." Prices in the range $0.50 to $8.75 were listed. A simulated market was used to determine the price, therefore the technique is IC.)

25. Knetsch and Sinden, 1984	2.85	4.05	$1.28	private (lottery)	real	closed	students	64, 64

(Estimated medians were reported in Harless, 1989.)

26. Kunreuther	-	2.59	$150.00	public (site nuclear waste)	hyp.	closed; double-bound	local public	498
and Easterling, 1992	-	1.22	$187.70	public (site nuclear waste)	hyp.	closed; double-bound	natl. public	603
(Estimated means were calculated by us.)								
27. Lichtenstein	-	1.20	$2.08	private (lottery; high P of winning low $)	hyp.	open; IC	students	173, 74
and Slovic, 1971	-	2.49	$2.49	private (lottery; low P of winning high $)	hyp.	open; IC	students	173, 74
(The authors report only mean WTA and mean WTP in relation to lotteries' expected values. We converted these to WTA/WTP using the expected value over 6 lotteries. We also calculated WTA/WTP for each of the 6 lotteries, then averaged, but the ratios were roughly the same as above).								
28. Loewenstein, 1988	-	2.33	$54.00	private (VCR in 0 ↔ 1 yr.)	hyp.	open	students	33, 33
	-	2.35	$10.17	private (meal in 0 ↔ 6 mos.)	hyp.	open	students	58, 58
	-	4.36	$0.25	private (record in 1 ↔ 4 wks.)	real	open; IC	HS students	≈17, 17
	-	2.27	$0.37	private (record in 4 ↔ 8 wks.)	real	open; IC	HS students	≈17, 17
	-	3.38	$0.52	private (record in 1 ↔ 8 wks.)	real	open; IC	HS students	≈17, 17
29. Loewenstein and Adler, 1995	-	1.47	$4.05	private (mug)	real	pay. cards; IC	students	24, 29
30. Loomis, Peterson, Brown, Champ, and Lucero, 1996	1.53	2.23	$28.00	private (art print)	hyp.	closed; but randomly iterated for WTA	staff	79, 55
(Two estimation methods were used to derive WTA from dichotomous choices. Mean and median WTA are the average of the two estimates. Ratios of means were 2.11 and 2.35. Ratios of medians were 1.86 and 1.21.)								
31. Mantymaa, 1996	-	42.66	F455.00	public (change in right-of-access)	hyp.	closed	public[1]	317, 227
	-	46.79	F178.00	public (change in right-of-access)	hyp.	closed	public[2]	172, 124
	-	28.49	F356.00	public (change in right-of-access)	hyp.	open	public[1]	72, 92
	-	31.67	F203.00	public (change in right-of-access)	hyp.	open	public[2]	54, 62
(Finnish currency was converted at F1 = $0.18. Superscript 1 denotes subjects who identified themselves as finding the two rights-of-access to have low substitutability. Superscript 2 denotes subjects who identified themselves as finding the rights to have high substitutability. These are groups $R_{4,5}$ and $R_{1,2,3}$.)								
32. McDaniels, 1992	-	3.59	$308.50	private (auto safety feature)	hyp.	open	public, students	43
33. Meyer 1976, reported	-	19.07	$1099.00	public (saltwater recreation, use value)	hyp.	open	local public	unknown
in Hyman and Stiftel,	-	9.36	$2894.00	public (use + option value)	hyp.	open	local public	unknown
1988 (See also Meyer, 1979)								

34. Morrison, 1997	-	0.99	$0.45	private (chocolate bar)	random real	paym. card; IC	students	10, 10
	-	1.09	$0.43	private (chocolate bar)	random real	paym. card; IC	students	10, 10
	-	1.09	$0.47	private (chocolate bar)	random real	paym. card; IC	students	10, 10
	-	1.13	$0.43	private (chocolate bar)	random real	paym. card; IC	students	10, 10
	-	1.13	$0.44	private (chocolate bar)	random real	paym. card; IC	students	10, 10
	-	2.01	$1.69	private (mug)	random real	paym. card; IC	students	10, 10
	-	2.22	$1.56	private (mug)	random real	paym. card; IC	students	10, 10
	-	2.42	$1.40	private (mug)	random real	paym. card; IC	students	10, 10
	-	2.29	$1.48	private (mug)	random real	paym. card; IC	students	10, 10
	-	2.19	$1.57	private (mug)	random real	paym. card; IC	students	10, 10

(Pounds sterling were converted at £1 = $1.55.)

35. Rae, Hausman, and Wickham, 1982	-	2.06	$521.00	public (visibility; median ± 1 step)	hyp.	closed; rank ordered	local public	296
	-	2.00	$150.00	public (existing qual. ± 13% clear)	hyp.	closed; rank ordered	local public	313
	-	1.83	$240.00	public (existing qual. ± (+50%, -19%))	hyp.	closed; rank ordered	local public	313

(Visibility in Cincinnati was evaluated. Existing air quality was 27% clear days. Last treatment had uncertainty.)

36. Rowe, d'Arge, and Brookshire, 1980	-	5.15	$4.75	public (visibility 50 miles ↔ 75 miles)	hyp.	closed, iterated	local public	45, 93
	-	10.92	$6.54	public (visibility 25 ↔ 75)	hyp.	closed, iterated	local public	35, 93
	-	13.21	$3.53	public (visibility 25 ↔ 50)	hyp.	closed, iterated	local public	36, 93
	-	16.60	$6.85	public (25 & visible power plant ↔ 75)	hyp.	closed, iterated	local public	31, 93

(Visibility at the Grand Canyon was evaluated.)

37. Schulze, McClelland, Hurd, and Smith, 1985, reported in Fisher, McClelland, and Schulze, 1988	30.00	9.49	$88.02	public (closure of oil landfill)	hyp.	open	local public	161, 226
	20.00	3.39	$221.73	public (presence of oil landfill)	hyp.	open	local public	196, 246

38. Shogren, Shin, Hayes, and Kliebenstein, 1994	-	1.28	$0.40	private (candy upgrade)	random real	open; IC	students	74, 68
	-	1.16	$0.38	private (candy upgrade)	random real	open; IC	students	74, 68
	-	0.98	$0.40	private (candy upgrade)	random real	open; IC	students	74, 68
	-	0.93	$0.40	private (candy upgrade)	random real	open; IC	students	74, 68
	-	0.95	$0.39	private (candy upgrade)	random real	open; IC	students	74, 68
	-	8.74	$0.53	private (campylobacter)	random real	open; IC; no info.	students	12, 13
	-	2.11	$0.71	private (campylobacter)	random real	open; IC; no info.	students	12, 13
	-	2.60	$0.88	private (campylobacter)	random real	open; IC; informed	students	12, 13
	-	2858.18	$0.55	private (salmonella)	random real	open; IC; no info.	students	13, 13
	-	3.39	$0.49	private (salmonella)	random real	open; IC; no info.	students	13, 13
	-	2.20	$0.56	private (salmonella)	random real	open; IC; informed	students	13, 13
	-	4.00	$1.02	private (staphylococcus)	random real	open; IC; no info.	students	13, 10
	-	3.22	$0.97	private (staphylococcus)	random real	open; IC; no info.	students	13, 10
	-	3.66	$0.91	private (staphylococcus)	random real	open; IC; informed	students	13, 10
	-	16.09	$0.44	private (trichinella)	random real	open; IC; no info.	students	13, 11
	-	6.42	$0.69	private (trichinella)	random real	open; IC; no info.	students	13, 11
	-	6.61	$0.82	private (trichinella)	random real	open; IC; informed	students	13, 11
	-	34.04	$0.57	private (clostridium)	random real	open; IC; no info.	students	13, 11
	-	3.05	$0.60	private (clostridium)	random real	open; IC; no info.	students	13, 11
	-	4.65	$0.43	private (clostridium)	random real	open; IC; informed	students	13, 11
	-	2.76	$2.37	private (mug upgrade)	random real?	open; IC[A]	students	15, 15
	-	1.26[1]	$2.72	private (mug upgrade)	random real?	open; IC[A]	students	15, 15
	-	0.74	$3.09	private (mug upgrade)	random real?	open; IC[A]	students	15, 15
	-	2.74	$2.37	private (mug upgrade)	random real?	open; IC[B]	students	15, 15
	-	1.27	$2.72	private (mug upgrade)	random real?	open; IC[B]	students	15, 15
	-	0.81	$3.09	private (mug upgrade)	random real?	open; IC[B]	students	15, 15

(i) Superscript 1 denotes a "mean of means" for trials 2-9. The mean WTAs were $3.42 for A and $4.03 for B. (ii) Treatment B: Identical mugs were available at the experiment so that WTA-sellers who sold a mug could immediately buy one with their earnings for the market price. Treatment A: No such opportunity was available at the experiment. (iii) Twenty trials were conducted for the pathogens. Mean ratios for trial 1, trials 7-10, and trials 17-20 are reported by the authors. Trials 11-16 are not reported separately or included with other numbers, but they appear in the Figures.

39. Sinclair, 1976, reported in Banford, Knetsch, and Mauser; Gordon and Knetsch; and Kahneman, Knetsch, and Thaler.	2.86	2.33	$300.00 (medn. WTP = $35.00)	public (fishing site)	hyp.	unknown	anglers	785

40. Singh, 1991	1.50	1.57	-	private (lottery $1, 0.5; $2, 0.5)	hyp.	open	civil servants	42?
	1.12	1.20	-	private (lottery $1, 0.5; $2, 0.5)	real	open; market	civil servants	42?
	1.07	1.17	-	private (lottery $1, 0.5; $2, 0.5)	hyp.	open	students	54?
	1.03	1.10	-	private (lottery $1, 0.5; $2, 0.5)	real	open; market	students	54?
	1.01	1.29	-	private (lottery $1, 0.5; $2, 0.5)	hyp.	open; advice	unknown	46

(i) Income endowment was not the same for WTA question as for WTP question. (ii) For treatments 2 and 4, the item was first traded in a market. After the market had cleared, subjects were asked WTP and WTA. (iii) It is not clear how a subject's WTA the loss of 1 unit was elicited from subjects who were endowed with zero units (as were some subjects for entries 1 and 3) or who ended up with zero units (as were some subjects for entries 2 and 4). (iv) For treatment 5, subjects from a separate sample were asked to advise a hypothetical client about buying or selling the item.

41. Smith, 1986 (pp. 199-201)	-	1.41	$1.39	private (risky asset)	hyp.	open	unknown	9
42. Thaler, 1981	1.88	-	$16.00	private (± $15 received in 3 mos.)	hyp.	open	students	20
43. Van Kooten and Schmitz, 1992	-	6.87	$3.90	private (perm. to drain wetland)	hyp.	open, iterated	farmers	44
44. Viscusi, Magat and Huber, 1987	-	13.75	$0.21	private (hhold product, ± 10^{-4} risk)	hyp.	open	public w/o young chil.	672
	-	8.67	$0.37	private (hhold product, ± 10^{-4} risk)	hyp.	open	public w/ young children	113
	-	42.46	$0.13	private (hhold product, ± 10^{-4} risk)	hyp.	open	public w/o young chil.	551
	-	6.46	$0.20	private (hhold product, ± 10^{-4} risk)	hyp.	open	public w/ young children	183
45. Welle, 1986 (p. 232 and 237).	-	4.96	$369.00	public (acid rain; severe effects)	hyp.	closed	public	276, 589

(**WTA data are only for subjects who who agreed to accept payment.* Only 13%, 32%, 39%, and 26% of the subjects agreed to accept the riskier product.)

Table 2A. One-Price Dichotomous Choice Surveys

These studies used dichotomous choice questions and a single payment amount for all subjects, and reported the percent of subjects willing to make that payment or willing to accept it. If a subject says yes, he will pay $25, then $25 is a lower bound estimate of his WTP. If a subject says no, she will not accept $25, then $25 is a lower bound estimate of her WTA. In this Table, we multiply the percent of yes for WTP questions by the payment amount to estimate WTP. We multiply the percent of no for WTA questions to estimate WTA. *These data are not used in our analysis.*

Study	Est. mean WTA/ est. mean WTP	Estimated mean WTP	Private/ public good	Hyp./real	Payment	Subjects	N
46. Gregory, 1986	1.26	$18.25	public (environmental quality)	hyp.	$25.00	students	52, 56
	1.23	$15.50	public (environmental quality)	hyp.	$25.00	students	58, 73
	1.40	$14.50	public (environmental quality)	hyp.	$25.00	students	32, 33
	1.81	$9.25	public (environmental quality)	hyp.	$25.00	students	54, 30
	2.10	$60.00	public (environmental quality)	hyp.	$200.00	students	28, 30
	1.53	$0.20	private (lottery for calculator)	hyp.	$0.60	students	46, 47
	1.85	$0.27	private (lottery for calculator)	hyp.	$1.00	students	38, 30
	2.70	$0.10	private (lottery for calculator)	hyp.	$1.00	students	48, 49
	9.40[O]	$0.05	private (lottery for calculator)	hyp.	$1.00	students	36, 37
	1.84[B]	$0.32	private (lottery for calculator)	hyp.	$1.00	students	37, 31
	1.78[B]	$0.40	private (lottery for calculator)	hyp.	$1.00	students	41, 35
	2.15	$0.16	private (lottery for calculator)	real	$0.60	students	31, 33
	1.32[B]	$0.34	private (lottery for calculator)	real	$0.60	students	54, 48
	1.93	0.15	private (probability of champagne?)	real	0.50	students	34, 31
	1.73	$5.20	private (entrance to museum)	hyp.	$10.00	museum visitors	30, 33
	1.85	$5.20	private (entrance to museum)	hyp.	$10.00	museum visitors	45, 33
	1.36	$11.00	public (air quality)	hyp.	$25.00	students	45, 41
	1.71	$13.00	public (air quality)	hyp.	$25.00	students	35, 46
	1.32	$63.00	public (air quality)	hyp.	$100.00	students	35, 43
	1.33	$60.00	public (air quality)	hyp.	$100.00	students	49, 43
	1.40	$62.00	public (air quality)	hyp.	$100.00	students	39, 50
(Superscript O is open-ended elicitation, but only closed-ended results reported. Superscript B is bidding elicitation, but only closed-ended results reported.)							
47. Marshall, Knetsch, and Sinden, 1986	1.73	$0.44	private (lottery for shoes)	hyp.	$1.00	unknown	25, 25
	1.11[A]	$0.72	private (lottery for shoes)	hyp.	$1.00	unknown	25, 25
	2.71	$0.61	private (lottery for cash or books)	hyp.(?)	$2.00	students	22, 23
	0.92[A]	$1.09	private (lottery for cash or books)	hyp.	$2.00	students	23, 22
	2.83	$0.55	private (lottery for cash or books)	hyp.	$2.00	students	23, 22
	1.31[A]	$1.18	private (lottery for cash or books)	hyp.	$2.00	students	22, 22
(Superscript A indicates that subjects were asked to advise a friend about buying or selling the item.)							

48. McDaniels, 1992	1.80	$321.00	private (auto safety feature)	hyp.	$700.00	auto customers	46, 48
	0.73	$420.00	private (auto safety feature)	hyp.	$700.00	auto customers	50, 50

(In Treatment 2, subjects were told that "alternative vehicles" have or do not have the feature. In other words, by buying (in WTP) or selling (in WTA) the item, the subject will be exhibiting preferences like other people in the market [our interpretation].)

Table 2B. WTA/WTP Studies Whose Data are not used in our Analysis

Study	Med. WTA/ Med. WTP	Mean WTA/ Mean WTP	Mean WTP	Private/ public good	Hyp./real	Elicitation technique	Subjects	N
49. Jones-Lee,	0.51	0.49	£1.60×10^6	private (trip safety; 2× or □)	hyp.	open	public	981
Hammerton,	0.29	0.21	£1.39×10^6	private (trip safety; 8× or □)	hyp.	open	public	999

and Philips, 1985

(*WTA data are only for subjects who agreed to accept payment.* Only 19% and 13% of the subjects agreed to accept the riskier trip. Untrimmed WTP means were £3.42×10^6 and £2.22×10^6.)

50. Slovic and	-	6.53	-$0.63	private (lottery)	hyp.	open; IC	students	40, 40

Lichtenstein, 1968

(Mean WTA and WTP were reported relative to lotteries' expected value. Expected value of the 27 lotteries was - 0.04. On average, subjects bid -0.59 below the expected value. On average, therefore, the lotteries were undesirable and subjects were willing to pay a negative amount (had to be compensated) to play. Mean WTA was $3.56 above expected value.)

Bibliography of Studies

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