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ECOSYSTEM VALUES AND SURFACE WATER PROTECTION: BASIC RESEARCH ON THE CONTINGENT VALUATION METHOD

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Executive Summary

Project Background

In 1981, President Ronald Reagan issued Executive Order 12291. This order required that significant federal regulations undergo cost-benefit analysis before approval by the Office of Management and Budget. Every President since Ronald Reagan has continued to follow this policy. This research is primarily motivated by two important questions regarding the estimation of benefits associated with public goods for use in cost-benefit analysis.

First, in evaluating the benefits of providing public goods, many studies, especially those based on contingent valuation, have shown substantial evidence of altruistic values. Thus, individuals indicate that they are willing to pay not only for the benefits to themselves from a particular public good, but also are willing to pay for the benefits that others receive from the public good. Considerable debate has ensued concerning the legitimacy of including such altruistic values in cost-benefit analysis. This debate has focused on two forms of altruism, non-paternalistic or pure altruism, and paternalistic or impure altruism. Pure altruism can be characterized by the statement: "What I want for you is whatever makes you happy." Paternalistic altruism can be characterized by the statement: "I want for you whatever I think is good for you, whether you like it or not." Pure altruism leads people to pay for others what others would pay for themselves and is generally viewed as leading to the double counting of benefits. In contrast, impure altruism motivates people to pay *in addition* to what others themselves would pay for a public good and is not viewed as double counting. Unfortunately, determining whether or not values for use in cost-benefit analysis incorporate altruism and what type of altruism is present is not easily accomplished. We address the empirical measurement of these forms of altruism in the first two papers of this report by developing and exploring the demand revelation characteristics of a new incentive compatible public goods elicitation mechanism we term the Random Price Voting Mechanism (RPVM) or the Voting Becker-DeGroot-Marshack (BDM) Mechanism.

The second issue we address is criterion validity in contingent valuation. In addition to providing a platform for examining the empirical components of altruism, the RPVM developed in this research offers a simulated market baseline for evaluating hypothetical bias in stated preference methods. The policy need for improved research in this area was highlighted by the Blue Ribbon Contingent Valuation Panel convened by National Oceanic and Atmospheric Administration (NOAA) in the wake of the Exxon Valdez oil spill and the consequent Oil Pollution Act of 1990.

Summary of Paper 1

The first paper develops and tests a new public goods value elicitation mechanism, referred to as the Random Price Voting Mechanism (RPVM or the Voting Becker-DeGroot-Marshack (BDM) Mechanism), in a series of laboratory economics experiments. The hope is that this mechanism will allow exploration of the nature of altruistic values as well as provide a tool for calibrating hypothetical value questions obtained through surveys (contingent values). Note that many experiments have been utilized to calibrate hypothetical values for <u>private</u> goods since several demand-revealing private good mechanisms exist but few studies have been conducted for <u>public</u> goods. The reason for this discrepancy is that all of the demand-revealing mechanisms available for public goods are cumbersome or confusing at best (these include the Smith Auction and the Groves-Ledyard Mechanism). Moreover, these mechanisms require multiple rounds and group interaction, and hence are not amenable to single-shot survey setting characterizing contingent valuation.

The experiments described use actual monetary payoffs and demonstrate that the RPVM is, in fact, demand revealing for both private and public goods. The data from these experiments further demonstrate that people do show non-paternalistic or pure altruism in voting on payoffs specified to each member of a voting group. In particular, voters with high payoffs from a program show concern for the cost/tax that voters with low payoffs will have to pay if the majority imposes a tax above the payoff for low payoff voters. Similarly, low payoff voters will vote for taxes above their payoffs so that high payoff voters will be better off. This behavior is consistent with pure altruism. Note that for public good settings in which voters all have identical payoffs, the voters tended to only vote yes if the payoff was greater than the cost. As such, for these baseline, homogeneous payoff cases, the mechanism is demand revealing.

Given that the RPVM is demand revealing and that it can reveal altruism in an induced value experimental setting, the second paper explores the possibility of paternalistic altruism in a risky environment. Such an extension is necessary because it is not possible to test for paternalistic altruism using certain monetary payoffs. Rather, it is possible that people will have paternalistic preferences with respect to the financial risks that others face. The third paper uses the mechanism to calibrate hypothetical values collected using contingent valuation techniques.

Summary of Paper 2

The purpose of this research is to develop a methodology to determine what types of altruism might be present in values obtained for cost-benefit analysis. The research utilizes the RPVM described in Paper 1. The methodology is tested for provision of group insurance to avoid a financial loss using laboratory economics experiments, more closely mimicking what is referred to in the experimental economics and contingent valuation literature as home grown values (as opposed to an induced value setting utilized in the first paper). Yet, the methods used here can be employed either in the laboratory or field for other commodities more likely to yield paternalistic altruism. The type of altruism present in values is important since non-paternalistic or purely altruistic values should usually be excluded from cost-benefit analysis of the provision of public goods, but paternalistic altruism generally should be incorporated. The possibility of paternalistic altruism can be examined because participants in a voting group who prefer that others in their group do not accept a risk of loss, but do not care about their overall payoff, will always value insurance for their group (public good) above the value of insurance against the same risk for themselves alone (private good). Thus, in the case of homogeneous risks, where all members of a voting group face the same probability of loss and financial loss, paternalistic altruists will value group insurance more than insurance for themselves alone. In contrast, pure altruists will value group insurance the same as insurance for themselves alone.

No evidence for paternalistic altruism is found in this study. The pattern of values when different group members face different risks is, however, consistent with non-paternalistic or pure altruism as shown for certain payoffs in Paper 1. It should not be concluded from this single study that paternalistic altruism does not exist. Rather, the purpose of this study is to develop methods that can be used to determine whether paternalistic altruism exists in a particular setting. This method could be extended in future experiments that utilize other commodities to see if convincing evidence for paternalistic altruism can be found.

A second finding of this paper is that non-paternalistic altruism can lead some people to be willing to pay less for public insurance than they would pay for private insurance. This underreporting for public goods challenges the maintained assumption often asserted in valuation research that altruism leads to artificial inflation in reported benefits.

Summary of Paper 3

This paper presents an analysis of hypothetical values for both private and public insurance obtained under identical circumstances to those described for Paper 2. WTP values are again using the RPVM. The focus of this research is to test three separate contingent valuation elicitation methods for the degree of hypothetical bias in comparison with the actual values. Parallel hypothetical data is elicited for both private and public insurance policies using two forms of the Multiple Bounded Discrete Choice (MBDC) method described by Welsh and Poe (1998), and using the standard PC method (Rowe et al. 1996). In the standard PC format (Rowe et al. 1996), subjects circle a value corresponding with their decision from a table of values. A second sample received an alternative Dichotomous Choice Payment Card (DC-PC), corresponding to that suggested in Boyle (2004) and employed by Bateman et al. (2005) amongst others. For the DC-PC, respondents indicate a vote of "Yes" or "No" to paying specified dollar amounts. As such, this elicitation method has similarities to the standard PC, but differs fundamentally in that respondents are asked to register a decision for each dollar value (rather than simply circling one value from a table). A third sample faced a MBDC version requiring respondents to indicate a level of certainty of WTP for each of a series of values. Certainty response options in the MBDC format are "Definitely Yes" (DY), "Probably Yes" (PY), "Unsure" (UN), "Probably No" (PN), and "Definitely No" (DN). Past research (Vossler et al. 2004) indicates that the multiple bounded "Probably Yes" (PY) model best predicts actual behavior in contribution settings. To the best of our knowledge, such comparisons have not been provided for DC-PC and PC formats.

Results suggest in this case that the PC and MBDC-PY are significantly different in both the private and public goods settings. We also find that the PC and DC-PC estimates significantly differ. However in all cases, the DC-PC and MBDC-PY estimates are nearly identical to the actual values. Nonparametric analysis shows that both the DC-PC elicitation format and the MBDC method, using a certainly level of Probably Yes, provide statistically similar estimates of WTP.

This finding indicates that calibration is possible to obtain accurate estimates of value using hypothetical questions in both private and public settings. A further implication of this research is that a simple DC-PC format may be preferred to the MBDC format in terms of predicting actual WTP, as well as being less cognitively taxing on the respondent. This experimental economics laboratory research lays a foundation for extending such research to the field.

ALTRUISM IN A COERCIVE TAX (REFERENDUM) SETTING: WTP AND WTA FOR A PUBLIC GOOD

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Abstract
This report introduces a new "Voting-BDM" mechanism, which combines the Becker-DeGroot Marschak (BDM) mechanism for private goods with a majority voting rule. We use the Voting-BDM mechanism in an induced value framework to test the effect of pure altruism on the provision of public goods in coercive willingness-to-pay (WTP) and willingness-to-accept (WTA) settings for both gains and losses. Laboratory experiments indicate that with homogeneous induced values, the Voting-BDM mechanism is demand-revealing in both WTP and WTA settings. Consistent with our theoretical model, however, non-paternalistic altruism or fairness concerns appear to influence behavior when induced values are heterogeneous. With induced gains, better-off subjects *under-report* their WTP and WTA and worse-off subjects *over-report* their WTP and WTA. The opposite holds true for induced losses.

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> "How selfish soever man may be supposed, there are evidently some principles in his nature, which interest him in the fortune of others, and render their happiness necessary to him, though he derives nothing from it except the pleasure of seeing it."
>
> Adam Smith, Section I, Chapter. I,
> *The Theory of Moral Sentiments*

1. Introduction

The experimental economics literature has provided increasing evidence that other-regarding behavior is a significant factor in individual contributions and deviations from the dominant, Nash equilibrium strategy of no contributions in voluntary public goods settings (see for instance Andreoni, 1995; Palfrey and Prisbey, 1997; Goeree, Holt and Laury 1999; and Ferraro, Rondeau and Poe*,* 2003). This is problematic from the perspective of social decision making because it has long been recognized that, at the margin, the optimal provision of public goods should be based solely on selfish preferences (Bergstrom, 1982; Jones-Lee, 1991, 1992; Milgrom, 1993; Johansson, 1994). Hence, some types of other-regarding behavior, such as pure (or non-paternalistic) altruism and impure altruism (i.e., the warm glow of giving - Andreoni, 1990) should not be included in social benefit-cost analyses for small projects evaluated close to a social welfare optimum. One implication for the valuation of public goods is that individual, and therefore aggregate, willingness to pay (WTP) should be conditional on everybody else paying so as to remain at their initial level of utility (Johansson, 1994).

However, public projects are rarely, if ever, financed under such conditions: most typically the funding for specific public projects imposes coercive costs that result in utility gains and losses. Moreover, projects evaluated tend to be discrete, and the initial allocation of public goods is inefficient. Under these conditions the extrapolation of Bergstrom's result for marginal changes at the optimum do not carry over to the "more modest problem [of benefit-cost analysis], determining whether a specific project can lead to a Pareto improvement" (Flores, 2002, p. 304; see also Johansson, 1993 and Johannesson, Johansson and O'Conor, 1996). Motives do matter.

Despite the fact that majority voting rules are increasingly used in ballot initiatives (referenda) to determine the provision of public programs that impose disproportionate costs and benefits on individuals, the effects of other-regarding behavior in involuntary, coercive settings are largely unexplored. To the extent that individuals exhibit other-regarding behavior, voting decisions are likely to be influenced by the perceived or actual impact on others (Deacon and Shapiro, 1975; Holmes, 1990). Understanding the motivation(s) and behavior of individuals who make personal choices affecting the public provision of public goods is therefore critical to benefit-cost analyses and the evaluation of the efficiency of referenda with coercive taxes.

To gain insights into individual behavior in coercive situations, we introduce a new public goods funding mechanism that combines the Becker-DeGroot-Marschak (BDM) (1964) mechanism for private goods with a majority voting rule. This we refer to as the Voting-BDM. For private goods, the BDM is theoretically incentive-compatible. Further, experimental evidence suggests that the BDM is a transparent mechanism with demand revealing properties (Boyce et al., 1992; Irwin et al., 1998). Laboratory experiments have further demonstrated the incentive compatibility and transparency of majority voting (see, for example, Plott and Levine 1978).

Whereas the BDM mechanism in a WTP setting has the subject purchase the good at a randomly determined cost whenever her maximum WTP is greater than or equal to the cost, the Voting-BDM implements the public good whenever a majority of subjects indicate a maximum WTP greater than or equal to a randomly selected cost, and where all subjects, regardless of their personal WTP, must pay this cost. In regards to a typical majority voting situation, note that the subject's bid is an indication of the highest price at which they would still vote yes. In a WTA setting, subjects indicate their minimum WTA to forego the public good and if the majority of offers are less than or equal to the randomly selected compensation, then all subjects, regardless of their personal WTA, must accept this amount.

There are several reasons to study the Voting-BDM in an induced value, continuous response setting. First, contrary to actual, "real-world" majority voting settings whereby a subject's yes or no vote implies a WTP or WTA interval, the Voting-BDM elicits a welfare point estimate[1]. Second, the Voting-BDM is less complex than incentive compatible public goods funding mechanisms such as the Smith (1980) Auction and the Groves-Ledyard (1977) mechanism, with the single round and the transparent nature facilitating the examination of treatment effects. Third, providing only monetary incentives in a laboratory setting excludes the possible influence of paternalistic altruism, in which the donor's utility is a function of particular consumption patterns of the beneficiary. Fourth, comparing Voting-BDM bids with induced values allows a direct test of warm glow. If no warm glow is evident in induced value monetary settings, any evidence of other-regarding behavior must be attributable to pure altruism.

The remainder of this report is organized as follows. The next section presents a theoretical model and derives testable hypotheses of the effects of pure altruism on the behavior of subjects in a coercive tax setting. Section 3 discusses the experimental design, which includes various treatments under WTP and WTA settings for both induced gains and induced losses. Results appear in Sections 4 and 5. We conclude with a discussion of trends that are evident in

[1] The submission of a bid is also more informative than a dichotomous "yes" or "no" response to a posted price which simply indicates that a subject's value is above or below the specified cutoff. Hence the use of a continuous response function offers a more efficient means of investigating anomalous behavior in voting settings. In ongoing NSF-funded research (Schulze et al., 2004) we demonstrate that the results reported here are also found in dichotomous choice referendum settings.

these experiments and suggest areas for future research with the Voting-BDM mechanism. In concluding this report, we note that some of the suggestions made herein are being pursued in a NSF project that builds upon the present research (Schulze et al., 2004).

2. A Theory of the Voting-BDM Mechanism and Testable Hypotheses

This section presents a theory of the Voting-BDM mechanism. For both clarity and brevity, we formulate this theory in the context of a "WTP-Gains" setting where individuals have a positive WTP for a public good. As we discuss below, the arguments in this setting are readily extendable to the other three possible welfare cases: willingness to pay to avoid a loss (WTP-Loss); willingness to accept compensation in lieu of a gain (WTA-Gain); and willingness to accept compensation to experience a loss (WTA-Loss). In the willingness-to-pay settings the reported values are referred to as "bids". We refer to willingness-to-accept values as "offers".

Consider a situation where *n* individuals are asked to express the maximum amount of money they would be prepared to pay for a particular program. A program is heretofore defined as a known vector of values $\prod = (\pi_1, \pi_2, ..., \pi_n)$, containing the payoff, π_i, for individual *i*, and payoffs, π_j, that all other individuals in the voting group stand to receive if a majority of subjects express a WTP equal or above a per-person (or average) cost *C*. The *i*th individual's revealed WTP amount or his bid is denoted B_i. The per person cost *C* is drawn from a uniform distribution in the interval $[0, C_{\max}]$. If a majority of bids are greater than or equal to *C,* individual *i* receives a monetary payoff $Y + \pi_i - C$ (which could be smaller than the subject's initial endowment *Y* if $\pi_i < C$).

Subjects are also postulated to have "social welfare" preferences consistent with the definition of Charness and Rabin (2002), according to which an individuals' utility is increasing (decreasing) in the gains (losses) of others. Thus, we posit that the total utility of individual i is given by $U_i = u\left(Y + \pi_i - C + \sum_{j \neq i} \left(\alpha_i \cdot (\pi_j - C) \right) \right)$, where $\alpha_i \geq 0$ is a parameter indicating the intensity with which individual i is affected by the gains and losses of others. This, in effect, adapts and extends the social welfare analysis of Charness and Rabin in three important dimensions. First, where Charness and Rabin are concerned with the case of two players, we allow for n players. Second, we do not restrict the analysis to linear utility functions. Third, and most importantly, since C is a stochastic variable, the game is one of incomplete information and the relevant framework of analysis will be expected utility with a Bayesian equilibrium concept. The term in the summation allows for the fact that subjects may have positive preferences for the benefits that others receive from the implementation of the program. The parameter α_i defines the extent to which the utility of individual i is affected by the potential gains (or losses) of others resulting from the implementation of the program. This formulation allows for heterogeneity across subjects, including the purely self-interested case where $\alpha_i = 0, \forall i \neq j$. Note further that α_i is symmetric – implying that gains and losses are treated equally. The α_i is also not recipient specific. Gains and losses of all j other individuals receive equal weight in i's utility function.[2]

To compute the Bayesian Nash Equilibrium bid of individual i, it is useful to rank order the bids of all n-1 other individuals. Define B_m as the bid that ranks as the $Integer\left((n+1)/2\right)^{\text{th}}$ largest of those bids (for n=3 this is the smallest of the other two bids). It is also useful to define B_k as the $Round((n-1)/2)^{\text{th}}$ largest bid (for n=3, this identifies the largest of the other two bids). The interval [B_m , B_k] defines the range over which a bid by voter i makes this individual have a

[2] The relaxation of these symmetries and individual independent assumptions is being pursued in current NSF-funded research on anomalous behavior in coercive public goods setting (Schulze et al., 2004).

marginal influence over the outcome of the game (in a probabilistic sense, it makes him the median voter).

To see this, consider the expected utility of individual i, where B_{-i} are the bids from all other individuals in the voting group:

$$\begin{aligned} EU_i\left(B_i, B_{-i}\right) = &\int_0^{B_m} p(C)U\left(Y + \pi_i - C + \sum_{j \neq i}\left(\alpha_i \cdot (\pi_j - C)\right)\right)dC \\ &+ \int_{B_m}^{B_i} p(C)U\left(Y + \pi_i - C + \sum_{j \neq i}\left(\alpha_i \cdot (\pi_j - C)\right)\right)dC \\ &+ \int_{B_i}^{B_k} p(C)U\left(Y\right)dC + \int_{B_k}^{C_{\max}} p(C)U\left(Y\right)dC \end{aligned} \tag{1}$$

The first term denotes the expected utility conditional on the randomly drawn cost being below B_m, with p(C) representing the probability that a certain cost is drawn. In this case, i's bid is completely irrelevant since there are a sufficient number of other voters who are willing to pay at least the drawn cost C to implement the program. The second and third terms represent the interval over which the bid of individual i will have a marginal effect. Here, B_i becomes the median bid. By increasing the bid amount individual i increases the probability that the program will be funded by increasing the range of costs that a plurality of voters is prepared to pay (second term), and decreasing the range over which the project is not implemented (third term). The last term is the interval of drawn costs for which, the program will not be implemented regardless of individual i's bid, as too few individuals are prepared to pay the random cost and the project is not implemented.

Exploring the family of affine strategies, individual i conjectures that individuals m and k choose bids of the form

$$B_m = \gamma_m\left(\pi_m + \sum_{j \neq m}\alpha_m\, \pi_j\right) \qquad \text{and} \qquad B_k = \gamma_k\left(\pi_k + \sum_{j \neq k}\alpha_k\, \pi_j\right).$$

Where γ_k and γ_m are positive constants, the exact value of which is defined by the equilibrium solution below. Substituting these expressions in Equation 1 and maximizing by choosing B_i yields the first order condition:

$$p(B_i)\left\{-U(Y)+U\left(Y+\pi_i-B_i+\sum_{j\neq i}\left(\alpha_i\cdot(\pi_j-B_i)\right)\right)\right\}=0$$

This equation has a degenerate solution $p(B_i)=0$ (where B_i is set equal to the lower support of the cost distribution). Assuming concavity of the utility function, there is also an interior maximum whereby individual *i* chooses his bid so as to equate the utility under the two alternative states of the world (the program is funded or it is not). This optimal bid is given by:

$$B_i^*=\frac{\pi_i+\sum_{j\neq i}\alpha_i\,\pi_j}{1+(n-1)\alpha_i} \qquad (2)$$

This optimal strategy has the same form as the prior of individual *i* regarding the bidding strategy of individuals *k* and *m* if one sets $\gamma_i = 1/\left(1+(n-1)\alpha_i\right)$. Therefore, a symmetric Bayesian Nash Equilibrium exists if all players adopt this linear strategy (including *k* and *m* of course). Note that an individual's optimal bid only depends on his own parameters and knowledge of the payoff vector.

The individually rational bid prediction for the standard BDM is nested in (2). In the absence of other players, the summation term vanishes and $n-1=0$, yielding the well-known incentive compatibility result, $B_i^*=\pi_i$. The model also predicts that if the payoffs are identical across subjects ($\pi_j=\pi_i$ for all j), the optimal strategy is for the individual to bid an amount equal to his payoff $(B_i^*=\pi_i)$. In this case, the subjects sensibly recognize that if all individuals were to bid above the common value, the resulting increase in the probability that the program will be funded is, in equilibrium, simply increasing the possibility that the program will be implemented at a cost above value, which will result in a net loss for all subjects, making bids above value irrational. Similar reasoning leads to the conclusion that bidding below the common value is also sub-optimal individually and collectively.

From (2), we can also predict how the optimal bid changes when payoffs are heterogeneous across subjects. This is best accomplished by considering a departure from the equal payoff situation ($\pi_i = \pi_j = \pi$), for which we have already established that $B^* = \pi$ for all players. Note that from (2) any changes in the payoffs of others that leave the sum of payoffs unchanged (any change that preserves the mean of the payoffs of others) should have no impact on B_i^*. Further inspection of (2) reveals that increasing (decreasing) the sum of other's payoffs increases (decreases) the optimal bid. This yields the additional prediction that individual *i* will increase his bid from his value when, from a situation of equal payoffs, all payoffs but his are increased (his remains constant). Similarly, going from equal payoffs to a vector where all but *i*'s payoffs are decreased decreases the optimal bid of individual *i* from his payoff.

The theory is easily extendable to a "WTA-Gains" situation where individuals have positive value for the public good and are asked to express the minimum amount of money they would be willing to accept to <u>not</u> implement the program. Let *C* now represent the randomly determined compensation to forego the good and let O_i denote individual *i*'s offer or revealed WTA amount. If a majority of offers are less than or equal to *C,* the program is not implemented and each receives compensation such that monetary payoff is $Y + C$; otherwise, the program is implemented and payoff for individual *i* is equal to $Y + \pi_i$. For this reinterpretation of model parameters and trivial modification of the implementation rule, it can be shown that Equation 2 analogously determines optimal offer strategies for the WTA-Gains setting.[3]

A number of testable hypotheses emerge for the WTP-Gains and WTA-Gains settings. For the case where n=1, the Voting-BDM operates just like the traditional BDM mechanism. In this situation, the null hypothesis is for the individual to submit a bid or offer equal to their payoff:

$$H_1^o: \quad B_i\,(or\ O_i) = \pi_i \qquad \text{if n=1.} \tag{3}$$

$$H_1^A: \quad B_i\,(or\ O_i) \neq \pi_i$$

When we move from the private or individual case to a homogenous public voting setting with n>1 and all individuals having equal payoffs, our theoretical framework predicts that an individual bids (or offers) an amount equal to his value regardless of other-regarding preferences:

$$H_2^o: \quad B_i\,(or\ O_i) = \pi_i \quad \text{if n>1 and } \pi_i = \pi_j, \forall j \tag{4}$$

$$H_2^A: \quad B_i\,(or\ O_i) \neq \pi_i$$

Note further that according to Hypothesis 1 and Hypothesis 2, a subject's WTP (WTA) in a private good setting should be the same as a subject's WTP (WTA) in a public good setting with homogeneous values, as both should equal the payoff amount. This finding would be strikingly different than behavior predicted and observed in other public good mechanisms, where subjects consistently report lower values in public good settings (Davis and Holt, 1993; Rondeau, Poe and Schulze, 2005). Because we do not formulate directional expectations, the null hypotheses are examined statistically using two sided significance tests.

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[3] The only difference in the WTP-Gains and WTA-Gains implementation rule is that a bid (offer) equal to *C* is considered as a yes vote in the WTP-Gains setting only. This does not change any of the theoretical results.

In the general case where n>1 and values are heterogeneous, Equation 2 suggests that strategies are a function of the relationship between individual *i*'s payoff and the average payoff over all *j* individuals. If individual *i*'s payoff is equal to the average payoff of other individuals, he should submit a bid (offer) equal to his value regardless of other-regarding preferences. That is,

$$H_3^o: \quad B_i(or\ O_i) = \pi_i \qquad \text{if n>1 and } \frac{\sum_{j \neq i} \pi_j}{n-1} = \pi_i \tag{5}$$

$$H_3^A: \quad B_i\ (or\ O_i) \neq \pi_i$$

We refer to such an individual as a "mean-value" subject. Two other logical cases arise when individual *i*'s payoff is either greater than or less than the average of the voting group. We refer to individuals in the two cases as being "better-off" and "worse-off", respectively. For the purely self-interested individual (i.e., $\alpha_i = 0$), theory predicts that the individual bids (offers) his value in either case. In the presence of other-regarding preferences, however, our theoretical model predicts that the better-off subjects will bid (offer) below value and that worse-off subjects will bid (offer) above value. Given the assumptions of our model, behavioral predictions for better-off and worse-off subjects, under the null of no other-regarding preferences, are summarized below as hypotheses 4 and 5, respectively:

$$H_4^o: \quad B_i(or\ O_i) = \pi_i \qquad \text{if n>1 and } \frac{\sum_{j \neq i} \pi_j}{n-1} < \pi_i \tag{6}$$

$$H_4^A: \quad B_i\ (or\ O_i) < \pi_i$$

and

$$H_5^o: \quad B_i(or\ O_i) = \pi_i \qquad \text{if n>1 and } \frac{\sum_{j \neq i} \pi_j}{n-1} > \pi_i \tag{7}$$

$$H_5^A: \quad B_i\,(or\ O_i) > \pi_i$$

Since behavioral predictions for these two situations in the presence of other-regarding behavior are unidirectional, this implies one-tailed hypothesis tests.

When payoffs are negative (losses), subjects can be construed as expressing their maximum WTP to avoid losing this amount or minimum WTA to accept this loss. It is thus appropriate to compare the bid with the absolute value of the (negative) payoff via Equation 2. In absolute value terms, this leads to the same set of hypotheses for both gains and losses. Note however, that in absolute terms better-off subjects are those with lower than average payoffs and worse-off subjects are those with higher than average payoffs. To emphasize this distinction, we rewrite hypotheses 4 and 5 for the case of better-off and worse-off subjects, respectively, for losses:

$$H_6^o: \quad B_i\ (or\ O_i) = |\pi_i| \qquad \text{if n>1 and } \frac{\sum_{j\neq i}|\pi_j|}{n-1} > |\pi_i| \tag{8}$$

$$H_6^A: \quad B_i\ (or\ O_i) > |\pi_i|$$

and

$$H_7^o: \quad B_i\ (or\ O_i) = |\pi_i| \qquad \text{if n>1 and } \frac{\sum_{j\neq i}|\pi_j|}{n-1} < |\pi_i| \tag{9}$$

$$H_7^A: \quad B_i\ (or\ O_i) < |\pi_i|$$

where the alternative hypotheses imply one-tailed statistical tests. In the sections that follow we discuss an experimental design and the results of implementation of this design.

3. Experimental Design:

All experiments were conducted in the Laboratory for Experimental Economics and Decision-Making Research at Cornell University. One hundred and eighty-six subjects volunteered for the experiments and were recruited from a variety of undergraduate economics courses. Each session consisted of either two WTP experiments: WTP-Gains and WTP-Losses (n=93) or two WTA experiments: WTA-Gains and WTA-Losses (n=93), thereby representing all four welfare settings.

All experiments use induced values (in absolute terms) of \$2, \$5 and \$8, and each experiment consists of two parts. In the first part, subjects gain experience with the traditional BDM by engaging in 10 rounds with low financial incentives. The induced values vary randomly across rounds. In the second part, subjects go through one round each of nine different treatments with high financial incentives. Three treatments involve a group size of 1, which we refer to as the "private treatments": (1) n=1, $|\pi_i|$ = \$2; (2) n=1, $|\pi_i|$ = \$5; and (3) n=1, $|\pi_i|$ = \$8. Three conditions involve a group size of 3 with homogeneous values, which we refer to as the "homogeneous treatments": (4) n=3, $|\pi_i| = |\pi_j| = \$2\ \forall j$; (5) n=3, $|\pi_i| = |\pi_j| = \$5\ \forall j$; and (6) n=3, $|\pi_i| = |\pi_j| = \$8\ \forall j$. The remaining three treatments involve a group size of 3 where the set of induced values for the group are \$2, \$5 and \$8, and which we refer to as the "heterogeneous treatments": (7) n=3, $|\pi_i| = \$2$, $|\pi_i| \neq |\pi_j|\ \forall j$; (8) n=3, $|\pi_i| = \$5$, $|\pi_i| \neq |\pi_j|\ \forall j$; and (9) n=3, $|\pi_i| = \$8$, $|\pi_i| \neq |\pi_j|\ \forall j$. We provide subjects with complete information about the payoff amounts of the other subjects in Voting-BDM rounds.

An example WTP session is as follows:

Part A: WTP-Losses, 10 low-incentive BDM rounds where the cost is determined and payoffs calculated at the end of each round.

Part B: WTP-Losses, nine high-incentive Voting-BDM rounds where the treatment and cost which result in earnings is determined at the end of the experiment.

Part C: WTP-Gains, 10 low-incentive BDM rounds where the cost is determined and payoffs calculated at the end of each round.

Part D: WTP-Gains, nine high-incentive Voting-BDM rounds where the treatment and cost which result in earnings is determined at the end of the experiment.

To control for potential order effects, the experiment order (gains or losses) varies across sessions. Further, Part B and Part D vary the order of the nine treatments across subjects. To prevent deterioration of other-regarding behavior that can occur in voluntary public good mechanisms (Davis and Holt 1993), subjects submit bids for the nine Voting-BDM treatments without feedback. At the end of the experiment, one of the nine Voting-BDM programs is implemented (and payoffs calculated) by having the subjects draw from a bag of marked poker chips. The exchange rate for the low-incentive BDM rounds is fifteen experimental dollars for one US dollar, while the exchange rate for the implemented Voting-BDM rounds is one experimental dollar for one US dollar. The experiment lasted approximately two hours and the average payoff was $35.

Subjects receive written instructions (Appendix A and B). As part of the verbal protocol, they are permitted to ask questions at the beginning of each part of the experiment. The instructions use language parallel to that found in surveys for referendum voting settings (Carson et al., 2000). The WTP instructions direct each subject to *vote* whether to *fund* a *program* by submitting a *bid* that represents the "highest amount that you would pay and still vote for the program." The WTA instructions direct each subject to *vote* whether to *implement* a program by submitting an *offer* that represents the "lowest amount of compensation that you would accept and still vote against the program." Each subject was seated at an individual computer and an Excel spreadsheet was used to record bids and to calculate payoffs. Written voting sheets are collected after each round to determine the outcomes and to prevent subjects from revising their decisions.

Subjects are assigned to voting groups of varying size of either one or three. Obviously voting group of one corresponds to the private good situation where the single individual constitutes the median voter and the majority. For the groups of three, the administrators announced the groups and asked each group member to raise their hand so that they could be identified by other members of their group. This ensured that subjects were aware of who was in their voting group for all treatments. However, subjects did not know which of the other members of the group received which payoff in the heterogeneous cases. As such, it is reasonable to assume that the α_i's are not recipient specific. No communication was allowed and subjects in the same group size of three were not seated next to each other.

The cost amount (for WTP experiments) or compensation amount (for WTA experiments) is determined by using a random numbers table with values from zero to nine. The first random number from the table represented the dollars amount, the second number the dimes amount, and the third number the pennies amount. For example, if the first random number was a four, the second was a nine, and the third a two, the determined cost (compensation) would have been $4.92. Consequently, the cost (compensation) is uniformly distributed between $0.00 and $9.99 with discrete intervals of $0.01.

A. WTP-Gains Experiment

In each round, subjects start with an initial balance of $10 and are assigned an induced value of $2, $5 or $8. For n=3 public treatments with heterogeneous values, an individual with an induced value of $2 is "worse-off", a subject with $5 has "mean-value", and an individual with a value of $8 is "better-off". Subjects submit bids, ranging from zero to the entire initial balance, equal to the maximum amount for which they would still vote *for* the program. After the subjects submit their bids, the experiment administrator determines the random cost for the program. If the majority of the bids are *greater than or equal to* the randomly determined cost, then the program is funded. In this case, all of the subjects in the voting group receive their personal payoff amount in addition to the initial balance, but also have to pay the determined cost. If the majority of bids are *less than* the randomly determined cost, then the program is not funded. In this case, all of the subjects in the voting group neither receive their personal payoff amount nor pay the cost, and thus, the subjects receive only their initial balance.

B. WTA-Gains Experiment

In each round, subjects start with an initial balance of $5 and are assigned an induced value of $2, $5 or $8. This lower initial balance makes the expected earnings in the WTA setting equivalent to the WTP setting, thus allowing us to avoid possible income effects. For n=3 public treatments with heterogeneous values, an individual with an induced value of $2 is "worse-off", a subject with $5 has "mean-value", and an individual with a value of $8 is "better-off". Subjects submit offers, ranging from zero to $10, which reflects their minimum WTA compensation to forego the public good. After the subjects submit their offers, the experiment administrator determines the randomly determined compensation for the program. If the majority of the offers are *less than or equal to* the random compensation, then the program is not implemented and all group members receive the compensation in addition to their initial balance. If the majority of offers are *greater than* the random compensation, the program is implemented and group members receive their induced value in addition to their initial balance.

C. WTP-Losses Experiment

In each round, subjects start with an initial balance of $10 and are assigned an induced value of -$2, -$5 or -$8 such that the program is a public bad. For n=3 public treatments with heterogeneous values, an individual facing a loss of $2 is "better-off", a subject facing a loss of $5 has "mean-value", and an individual facing a loss of $8 is "worse-off". Subjects submit bids, ranging from zero to the entire initial balance, equal to their maximum WTP to fund a program to forego the public bad. After subjects submit their bids, the experiment administrator determines the random cost for the program. If the majority of bids are *greater than or equal to* the determined cost, the program is funded, and all group members pay the cost from their initial balance of $10 but do not experience the public bad. If a majority of the bids is *less than* the random cost, the program is not funded. Consequently, all group members receive their initial balance plus the value of the public bad.

D. WTA-Losses Experiment

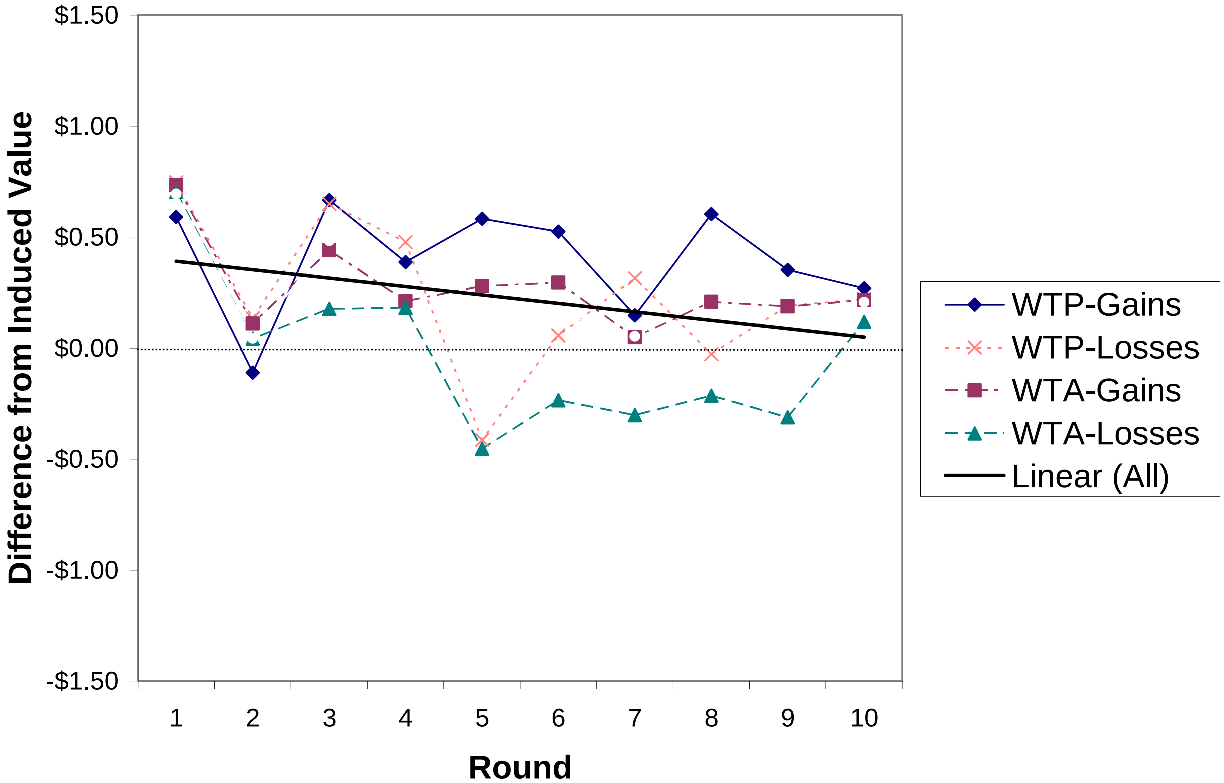
In each round, subjects start with an initial balance of $5 and are assigned an induced value of -$2, -$5 or -$8. For n=3 public treatments with heterogeneous values, an individual with an induced value of -$2 is "better-off", a subject with -$5 has "mean-value", and an individual with a value of -$8 is "worse-off". Subjects submit an offer, ranging from zero to $10, which reflects their minimum WTA compensation to accept the public bad. After the subjects submit their offers, the experiment administrator determines the randomly determined compensation for the program. If the majority of the offers are *less than or equal to* the random compensation, then the program is not implemented and all group members receive the compensation, their induced value and their initial balance. If the majority of offers are *greater than* the random compensation, the program is implemented and group members receive their initial balance only.

4. Results

A. Practice Rounds with Low Incentives

Similar to other studies using the BDM mechanism (Boyce et al., 1992; Irwin et al., 1998), the goal of the low-incentive rounds is to give subjects an opportunity to gain experience with the mechanism before introducing additional complexities to the decision environment. Repeated low-incentive BDM rounds provide subjects an opportunity to receive feedback on how their bids and offers affect payoffs. Over ten practice rounds, subjects bids/offers improved on average by 70% from \$0.69 above induced value in the first round to only \$0.21 above induced value in the tenth round (Figure 1). For the WTP practice rounds, the average bid decreased from \$0.67 to \$0.25 above induced value (an improvement of 63%). For the WTA practice rounds, the average bid decreased from \$0.72 to \$0.17 above induced value (an improvement of 76%). Overall, while learning was obviously taking place during practice rounds, by the last practice round subjects were submitting bids that are statistically indistinguishable from their induced values at the α=0.10 in all four welfare settings.

Figure 1. Low-Incentive Practice Rounds: Difference from Induced Value.

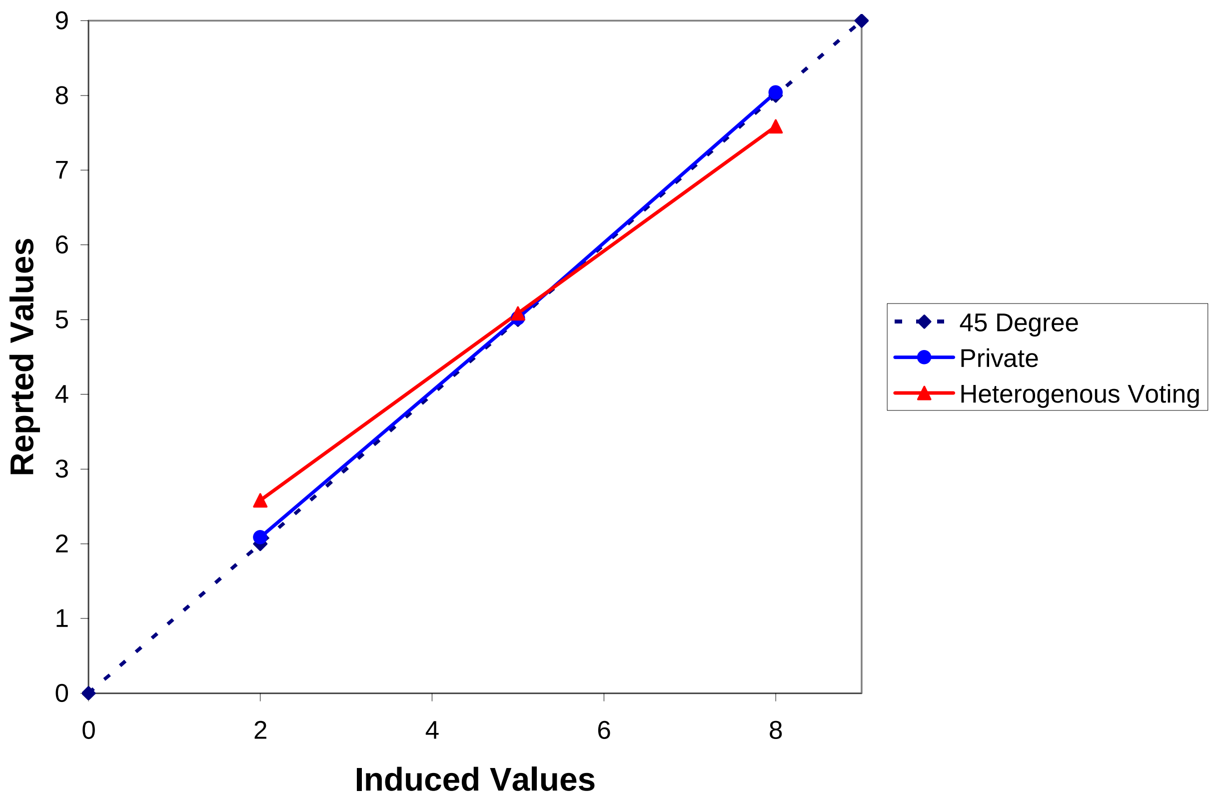


B. Voting-BDM

The broad results from the four sets of voting BDM experiments can be most easily understood in the context of Figure 2 below. This figure provides aggregated willingness-to-pay and willingness-to-accept values for private and heterogeneous gains and losses as a function of induced values. Absolute values are used to enable pooling of losses and gains. The willingness-to-pay and willingness-to-accept values for public homogenous voting settings are not provided in the figure as they are virtually indistinguishable from those for the private valuation settings[4].

[4] The averaged private, public homogeneous, and public heterogeneous for induced values of $2, $5 and $8 are: $2.09, $5.02, and $8.04; $2.09, $4.99 and $8.02; and $2.58, $5.08 and $7.59 respectively.

Figure 2: Aggregated Private and Heterogeneous Public Good Values Across Gains, Losses, WTP, WTA



The close correspondence of private bid and offers with the 45 degree line equating the vertical and horizontal axis measures is evident in Figure 2. In contrast, average heterogeneous values intersect the 45 degree line from above. That is: at induced values of $2, the reported values exceed $2; at $5, induced and reported values are proximate; and at $8; reported values fall below induced values. These general trends are consistent with the other-regarding behavior hypotheses expressed above.

This general, intuitive presentation is reflected in the results of individual hypothesis tests. Our specific analyses of each of the voting-BDM hypotheses makes use of graphical presentations as well as formal econometric analyses. The graphical presentations provide the basis for a more intuitive understanding of the nature of the shifts in valuation functions that occur across treatments. For the econometric analysis, we treat the set of decisions from each individual across high-incentive rounds as a panel data set and analyze the data using a two-factor fixed effects model. Indicator variables capture the differences across the (i=1,…,93) individuals, D_i, as well as the (t=1,…,9) treatment conditions, I_t. The individual fixed effects capture the unobserved heterogeneity across individuals, such as differences in altruism. Specifically, for each of the four experiments we estimate the model:

$$B_{it}\ (or\ O_{it}) - \pi_{it} = \zeta + D_i + I_t + \varepsilon_{it} \tag{10}$$

where the dependent variable is the difference between individual i 's bid and his induced value for treatment t; ζ is an overall constant term; the D_i and I_t are estimable fixed-effects parameters; and ε_{it} is a mean-zero random error term. Note that we do not have to worry about subject-specific autocorrelation here, as subjects do not receive any feedback until the end of the experiment (i.e., there are no learning effects). Also, no ordering effects are present since the sequence of treatments was randomized across individuals. The model is obtained by the inclusion of a full set of individual and treatment-specific indicator variables. The problem of perfect collinearity – the treatment and individual indicator variables both sum to one – is avoided by imposing the restrictions that the set of individual and treatment fixed effects independently sum to zero via a restricted least squares estimator.

We use the estimated models to test the hypothesized relationships between bids and induced values under the different treatment conditions and welfare settings as described in the Experimental Design section. The results of the hypothesis tests for the WTP-Gains, WTA-Gains, WTP-Losses, and WTP-Losses, respectively, are presented in Tables 1 through 4. The reported values correspond to specific treatment conditions: different induced values (in absolute terms) ($2, $5, and $8), different voting group sizes (one or three) and the distribution of induced values (homogeneous or heterogeneous). For brevity we omit the estimated equations in the text, providing the regression output in Appendix D. For all statistical tests below we employ a 5% significance level.

Hypothesis 1: Private Treatments. As demonstrated in Figure 3 and conjectured in the theoretical section, the distributions of bids and offers for the Voting-BDM with n=1 (which is equivalent to the conventional, private BDM) closely track induced values. The percent of optimal bids, defined as $4.99-$5.00 for the WTP treatments, ranged from 47% (WTP-Losses, $2) to 69% (WTP-Losses, $5). Similarly the range of optimal offers, defined as $5.00-$5.01 for the WTA treatments, ranged from 52% (WTA-Gains, $2) to 75% (WTA-Gains, $5). If each of these optimal ranges was increased to include induced value +/- 0.01 (e.g., $1.99-$2.01, $4.99-5.01, and $7.99-8.01), over 59% of the values would be deemed optimal in each of the treatments.

Table 1. WTP-Gains

		Group Size = 1		Group Size = 3			
		Private	*P-value*	**Homogeneous**	*P-value*	**Heterogeneous**	*P-value*
Low Value	Mean	**$2.10**		**$2.06**		**$2.64** †	
$2	Median	$2.00		$2.00		$2.00	
(Worst-off)							
	Difference from Induced Value	$0.10	*0.463*	$0.06	*0.694*	$0.64	*0.000*
Middle Value	Mean	**$5.09**		**$5.11**		**$5.19**	
$5	Median	$5.00		$5.00		$5.00	
	Difference from Induced Value	$0.09	*0.506*	$0.11	*0.427*	$0.19	*0.179*
High Value	Mean	**$8.11**		**$8.14**		**$7.78** †	
$8	Median	$8.00		$8.00		$8.00	
(Best-off)							
	Difference from Induced Value	$0.11	*0.435*	$0.14	*0.314*	-$0.22	*0.061*

n=93

† one-tailed test

Table 2. WTA-Gains

		Group Size = 1		Group Size = 3			
		Private	*P-value*	**Homogeneous**	*P-value*	**Heterogeneous**	*P-value*
Low Value	Mean	**$1.96**		**$2.06**		**$2.47**	†
$2	Median	$2.00		$2.00		$2.00	
(Worst-off)							
	Difference from Induced Value	-$0.04	*0.764*	$0.06	*0.646*	$0.47	*0.000*
Middle Value	Mean	**$5.12**		**$5.03**		**$5.06**	
$5	Median	$5.00		$5.00		$5.00	
	Difference from Induced Value	$0.12	*0.392*	$0.03	*0.806*	$0.06	*0.657*
High Value	Mean	**$8.15**		**$8.18**		**$7.59**	†
$8	Median	$8.00		$8.00		$8.00	
(Best-off)							
	Difference from Induced Value	$0.15	*0.293*	$0.18	*0.180*	-$0.41	*0.002*
	Difference from Private, N=1			$0.04	*0.839*	-$0.55	*0.002*
	Difference from Homogeneous, N=3					-$0.59	*0.001*

n=93

† one-tailed test

Table 3. WTP-Losses

		Group Size = 1		Group Size = 3			
		Private	*P-value*	**Homogeneous**	*P-value*	**Heterogeneous**	*P-value*
Low Value	Mean	**$2.23**		**$2.14**		**$2.67** †	
-$2	Median	$2.00		$2.00		$2.00	
(Best-off)							
	Difference from Induced Value	$0.23	*0.143*	$0.14	*0.368*	$0.67	*0.000*
Middle Value	Mean	**$5.19**		**$5.00**		**$5.38**	
-$5	Median	$5.00		$5.00		$5.00	
	Difference from Induced Value	$0.19	*0.241*	$0.00	*0.992*	$0.38	*0.017*
High Value	Mean	**$7.99**		**$7.80**		**$7.68** †	
-$8	Median	$8.00		$8.00		$8.00	
(Worst-off)							
	Difference from Induced Value	-$0.01	*0.928*	-$0.20	*0.201*	-$0.32	*0.020*

n=93

† one-tailed test

Table 4. WTA-Losses

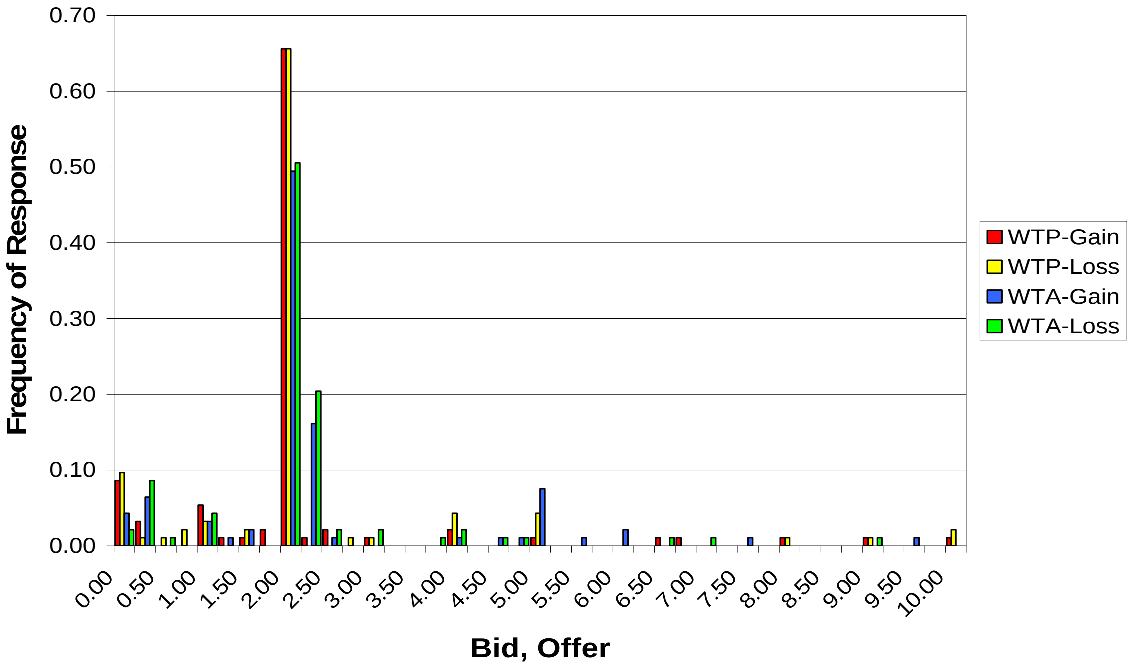
		Group Size = 1		Group Size = 3			
		Private	*P-value*	**Homogeneous**	*P-value*	**Heterogeneous**	*P-value*
Low Value	Mean	**$2.06**		**$2.11**		**$2.54** †	
-$2	Median	$2.00		$2.00		$2.00	
(Best-off)							
	Difference from Induced Value	$0.06	*0.695*	$0.11	*0.495*	$0.54	*0.000*
Middle Value	Mean	**$4.68**		**$4.82**		**$4.70**	
-$5	Median	$5.00		$5.00		$5.00	
	Difference from Induced Value	-$0.32	*0.039*	-$0.18	*0.246*	-$0.30	*0.056*
High Value	Mean	**$7.91**		**$7.94**		**$7.29** †	
-$8	Median	$8.00		$8.00		$8.00	
(Worst-off)							
	Difference from Induced Value	-$0.09	*0.579*	-$0.06	*0.705*	-$0.71	*0.000*

n=93

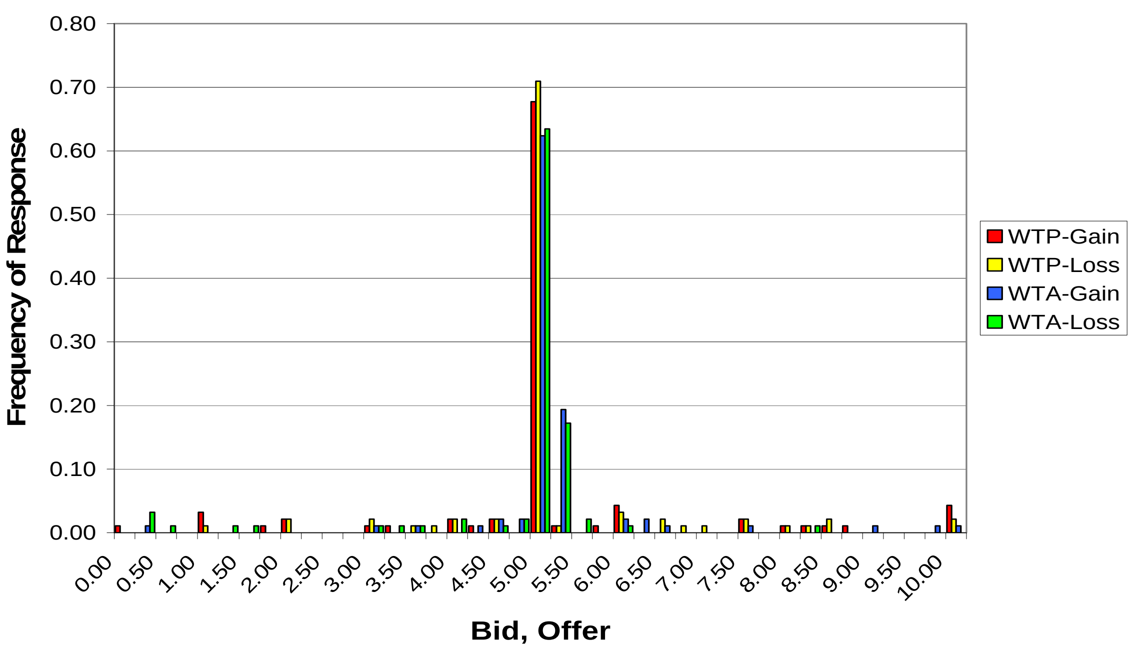
† one-tailed test

Figure 3: Distribution of Private Bids and Offers for Each Induced Value (Absolute) in $0.25 Increments

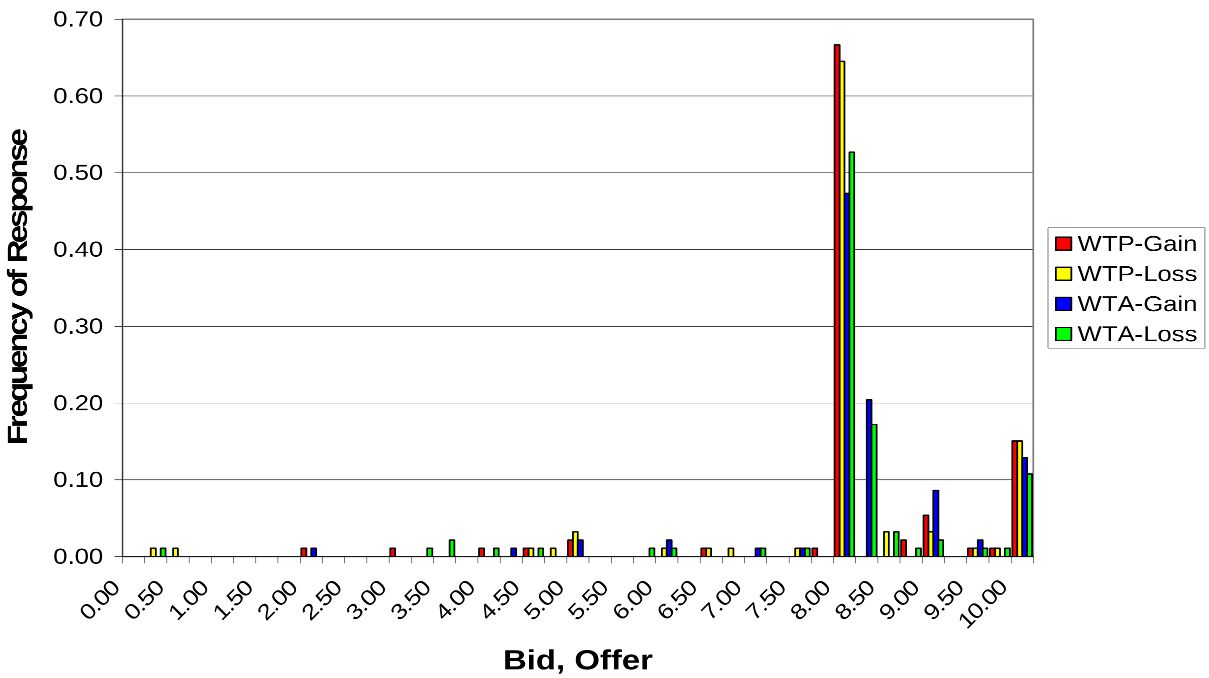
a. $2



b. $5



c. $8



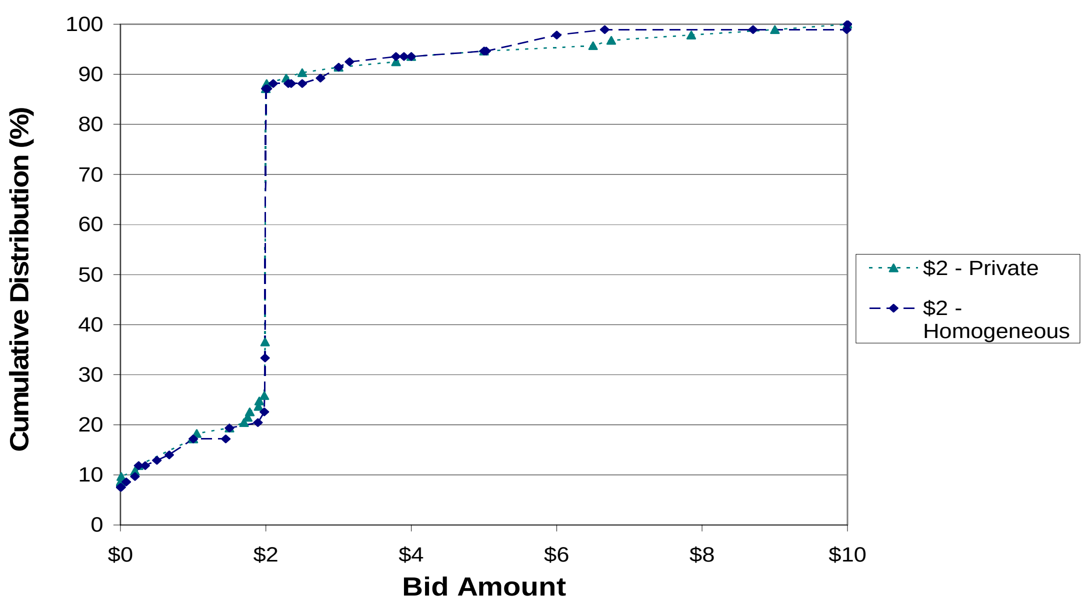
Such results are consistent with Irwin et. al (1998), who report that 62% and 67% of bids and offers, respectively, fell in the optimal range in a private BDM setting. These modal responses in the optimum range are reflected in measures of central tendency. In each of the private good cases, the median value equals the induced value (indeed, this result holds for all the treatments). Of the 12 private treatment scenarios (i.e., 3 treatments by 4 experiments), the null hypothesis is never rejected.

In all, although the presentation of the private BDM has changed by the addition of voting terminology in the instructions, the results from the voting–BDM, n=1 experiments are consistent with previous research demonstrating positive results for demand revelation characteristics of willingness-to-pay and willingness-to-accept values in private BDMs.

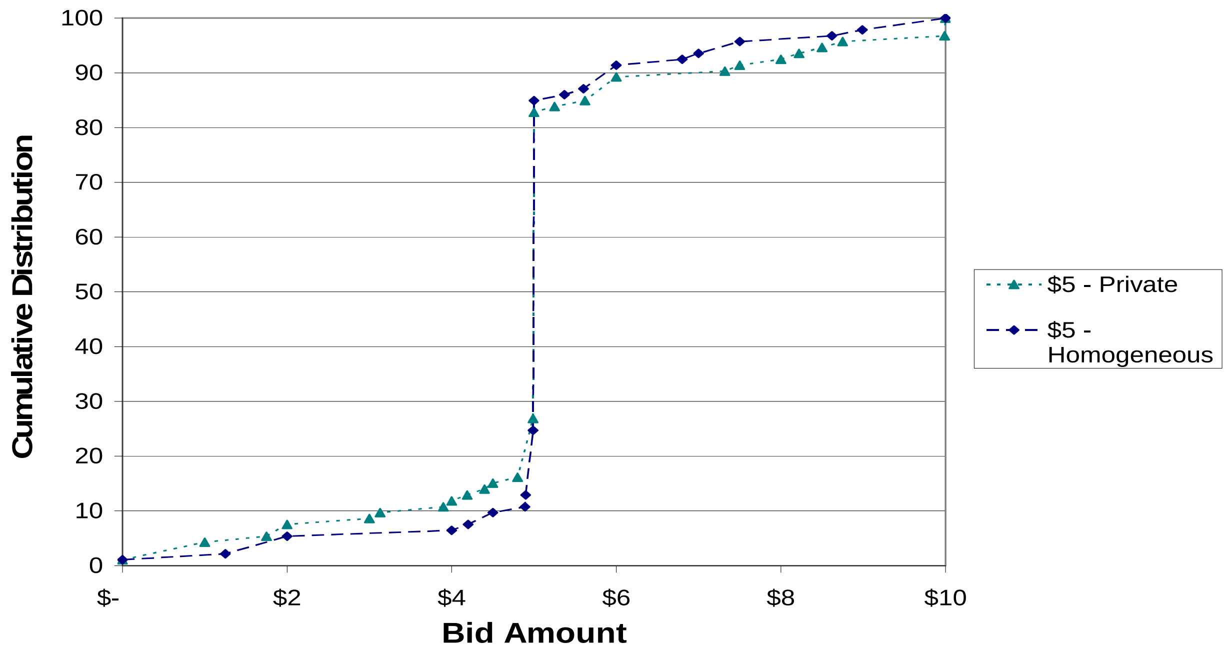
Hypothesis 2: Homogeneous Treatments. As might be expected from the aggregated mean WTP bids across treatments reported in Footnote 6 and presented in Figure 2, homogeneous WTP and WTA distributions closely correspond with the respective distributions of private bids and offers as well as of induced values. As a representative example, we provide the private and homogenous distributions for WTP-Gains for induced values of $2, $5 and $8 in Figure 4. The correspondence between the private distributions for other treatment/induced values can be gleaned from Figures 5-9. These graphical impressions are reflected in the econometric analysis. In public treatments with homogeneous values, average voting-BDM bids or options are not statistically different from induced values, such that we fail to reject Hypothesis 2 in all treatments in all experiments. These results are summarized in Tables 1 – 4 and porivde in detail in Appendix D.

Figure 4: Private and Homogenous WTP-Gains Bids

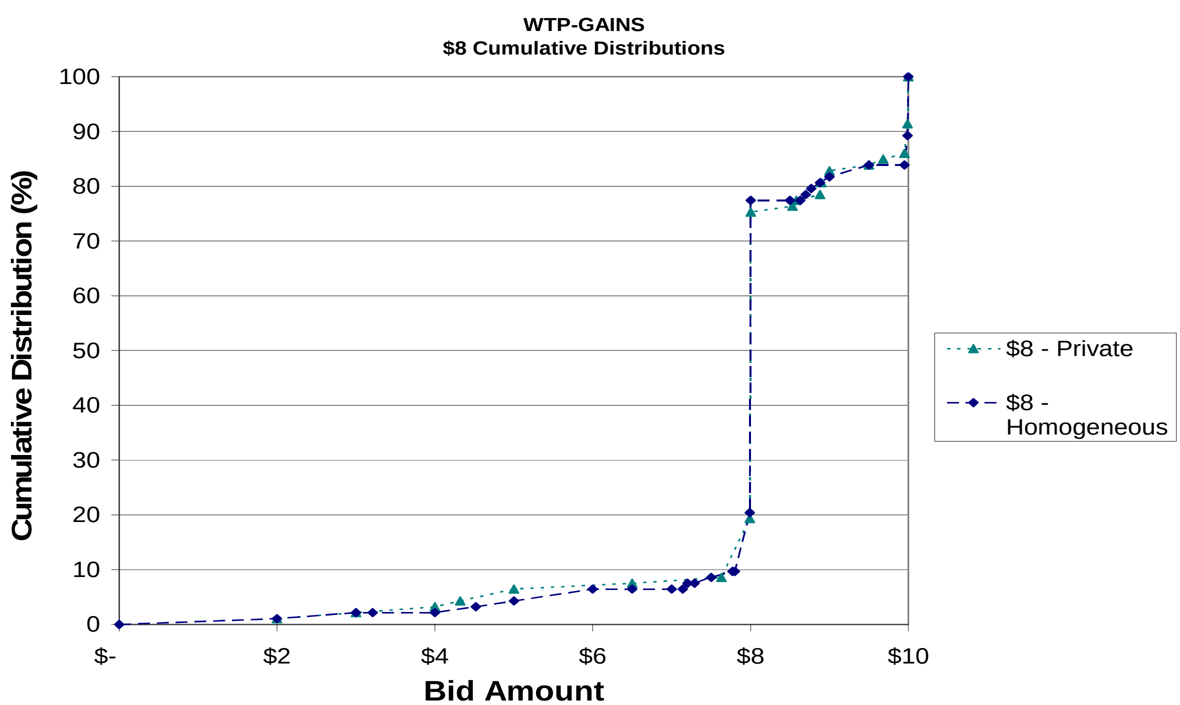
a. $2



b. $5


100
90
80
70
60
50
40
30
20
10
0
$-
$2
$4
$6
$8
$10
Cumulative Distribution
Bid Amount
$5 - Private
$5 - Homogeneous

c. $8


WTP-GAINS
$8 Cumulative Distributions
100
90
80
70
60
50
40
30
20
10
0
$-
$2
$4
$6
$8
$10
Cumulative Distribution (%)
Bid Amount
$8 - Private
$8 - Homogeneous

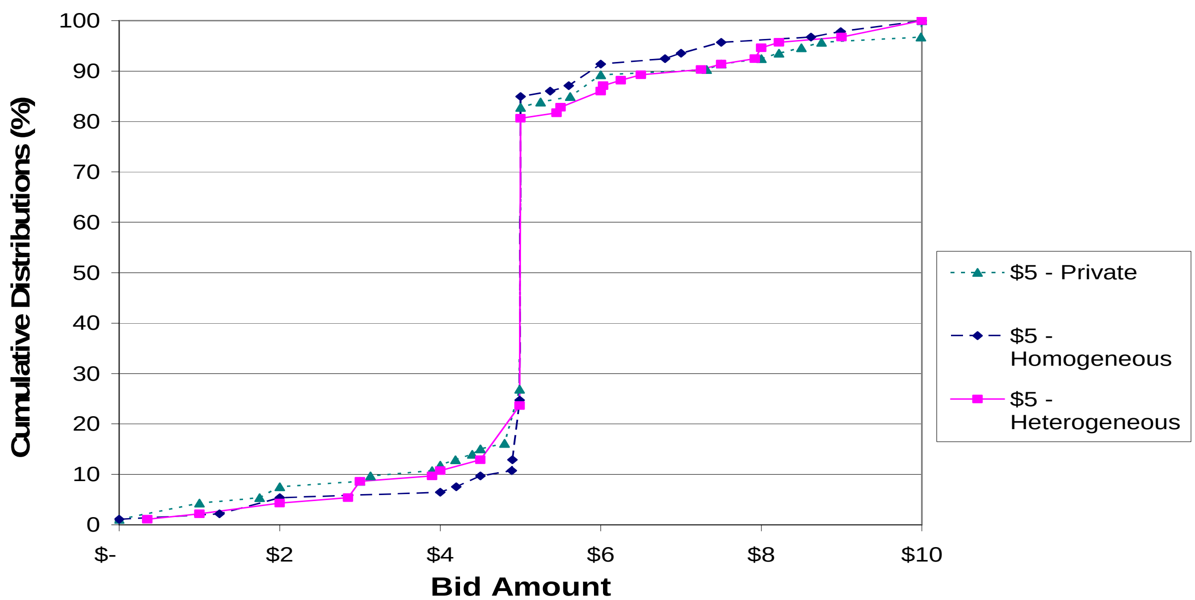
The statistical similarity between induced and reported values in the homogeneous voting setting contrasts with other popular single-shot public goods mechanisms, such as the Voluntary Contributions Mechanism or Provision Point Mechanism, where subjects with homogeneous values systematically bid below value for induced gains in the range of values studied here. Note that if warm-glow were a significant factor here then we would expect significant deviations from induced value, namely positive deviations in the WTP experiments (Rondeau, Poe and Schulze, 2005). Since this is clearly not the case, it appears that impure altruism/warm glow does not contaminate the results from the public heterogeneous treatments. Hence, any differences from induced values in the heterogeneous settings may be attributable to pure altruism in this study. We turn to this issue now.

Hypothesis 3: Heterogeneous Treatments - Mean-value Subjects. The behavior of mean-value subjects in public heterogeneous treatments does not follow a consistent pattern relative to the private and public homogenous distributions, and may vary depending upon the framing of the situation. This lack of pattern is evident in Figure 5, which compares the private, public homogenous and public heterogeneous distributions of bids and offers for the $5 induced values in each of the four treatments. Indeterminacy in directional effects carries over to the econometric estimates, with some of the mean differences between reported and induced values being positive and others being negative. Nevertheless, although there appear to be some movement in distributions, these effects are not significant in the econometric model for any of the treatments.

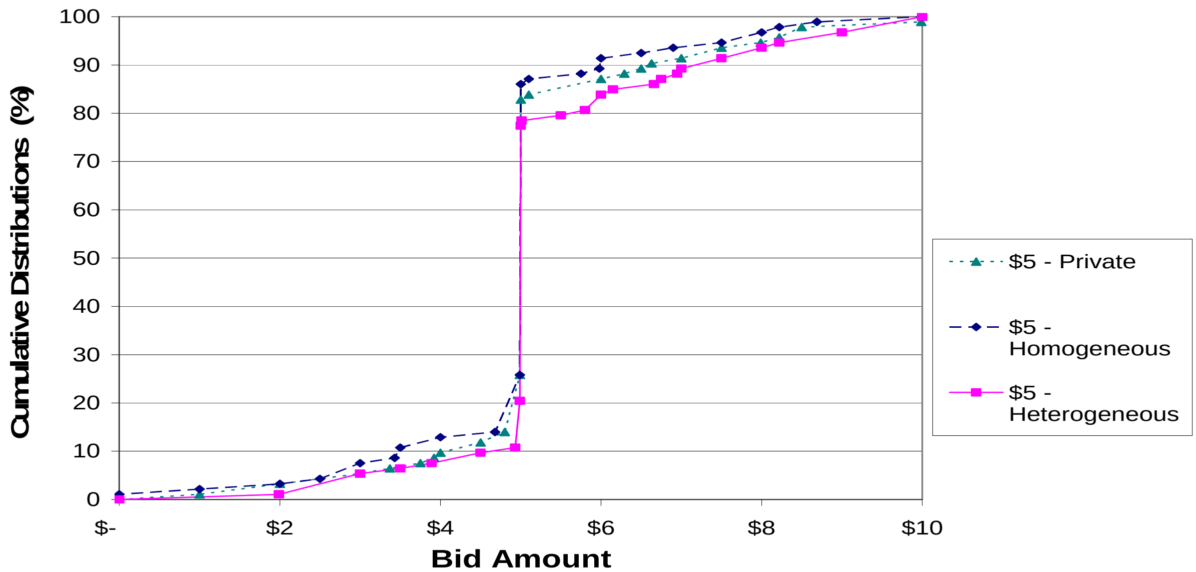
Hence, Hypothesis 3 cannot be rejected, lending some tentative support to the conclusion that gains and losses across other individuals are judged symmetrically.

Figure 5: Private, Public Homogeneous, and Public Heterogeneous Bid Distributions: Mean Value Subjects $5, -$5

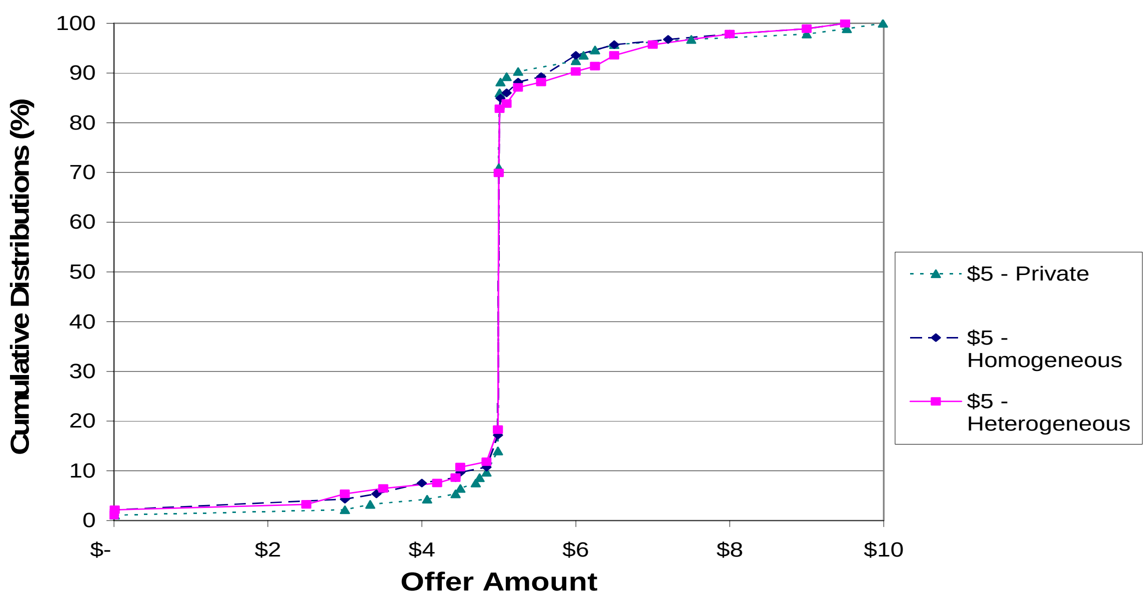
a. WTP-Gains



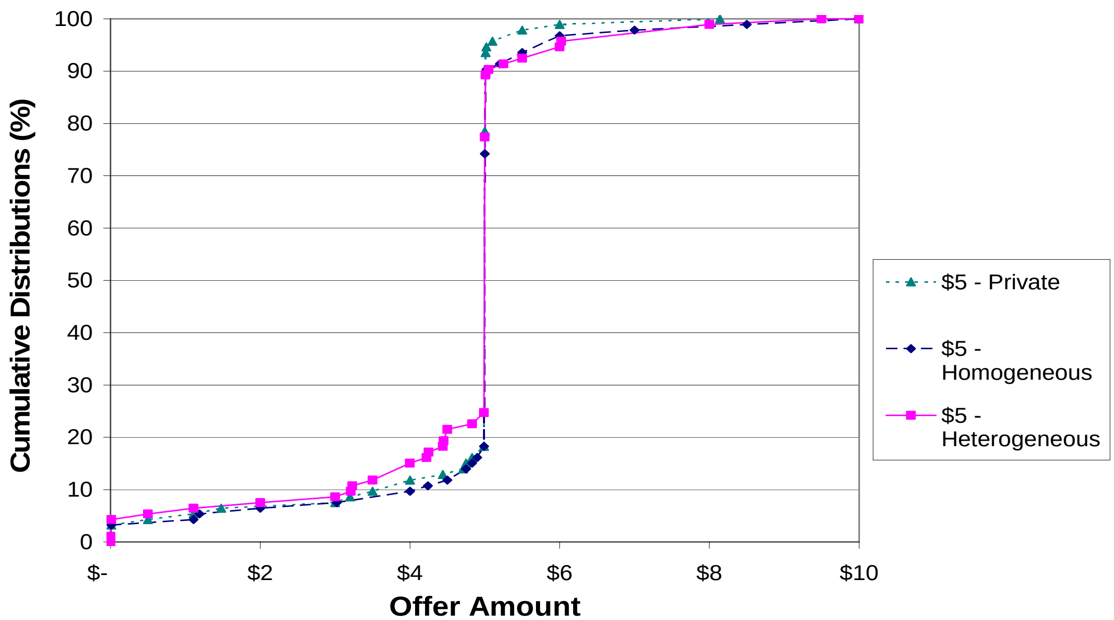
b. WTP-Losses



c. WTA-Gains



d. WTA-Losses



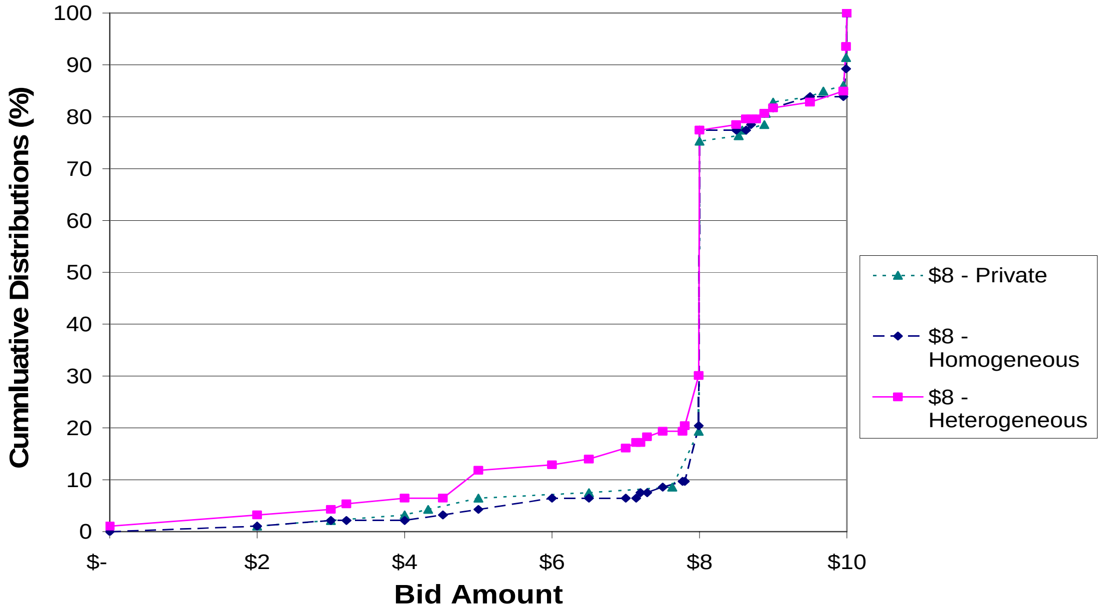
Hypothesis 4: Heterogeneous Treatments - Better-Off Subjects, Gains. In contrast to the mean-value subjects, there is a notable directional effect of heterogeneity on the distribution of bid and offer values (Figure 6). For gains, the distribution functions of bid values for the better off subjects ($8) shifts leftwards, with notable movement in the lower part of the distribution, suggesting that some subjects demonstrate other-regarding behavior by reducing their bids or offers (votes) downward. In these treatments, subjects submit bids or offers that are an average of $0.22 below (WTP-Gains, Table 1) and $0.41 below (WTA-Gains, Table 2) their induced value, respectively. While this altruism effect is significant in the WTA setting, the p value in the WTP treatment is 0.061 and thus not significant at the 5% level used in this analysis. For statistical comparisons associated

with hypotheses 4 – 7, this marginal rejection is the only failure to reject the null hypotheses using a 5% significance level.

Hypothesis 5: Heterogeneous Treatments - Worse-Off Subjects, Gains. In contrast, as predicted by our theoretical framework, the bids and offers for worse-off subjects submit increase in heterogeneous settings, with notable shifts in the upper parts of the distributions (see Figure 7). On average, subjects report bids of $0.64 above their induced value in WTP-Gains (Table 1). In WTA-Gains, the worse-off subjects report offers that are statistically higher then their induced value by an average of $0.47 (Table 2). In both settings, the differences are significant, thereby rejecting the null hypothesis.

Figure 6: Heterogeneous Treatments, Bid and Offer Distributions
Better Off Subjects – Gains

a. WTP-Gains - $8



b. WTA-Gains - $8

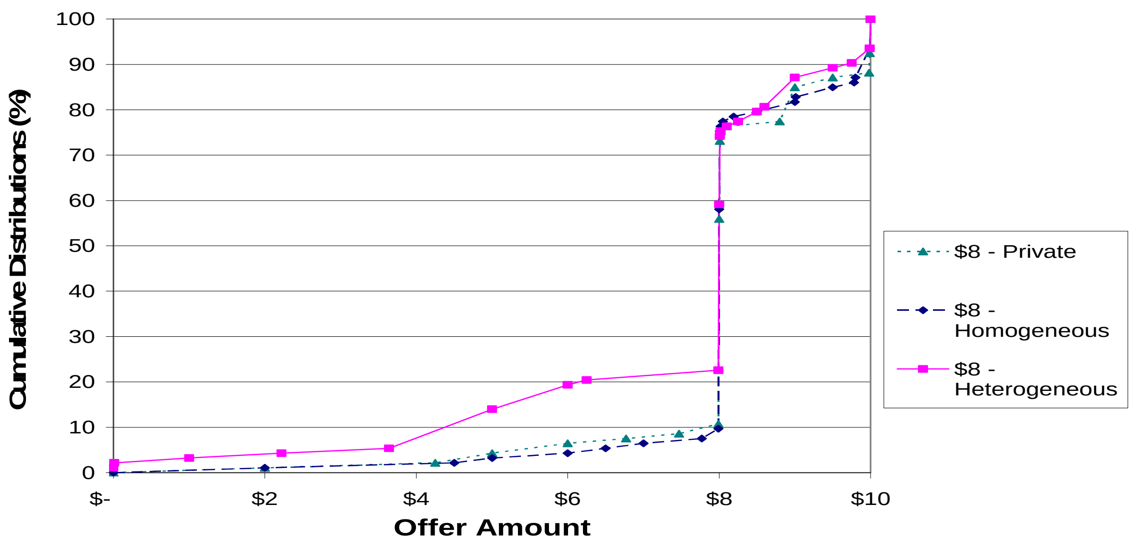
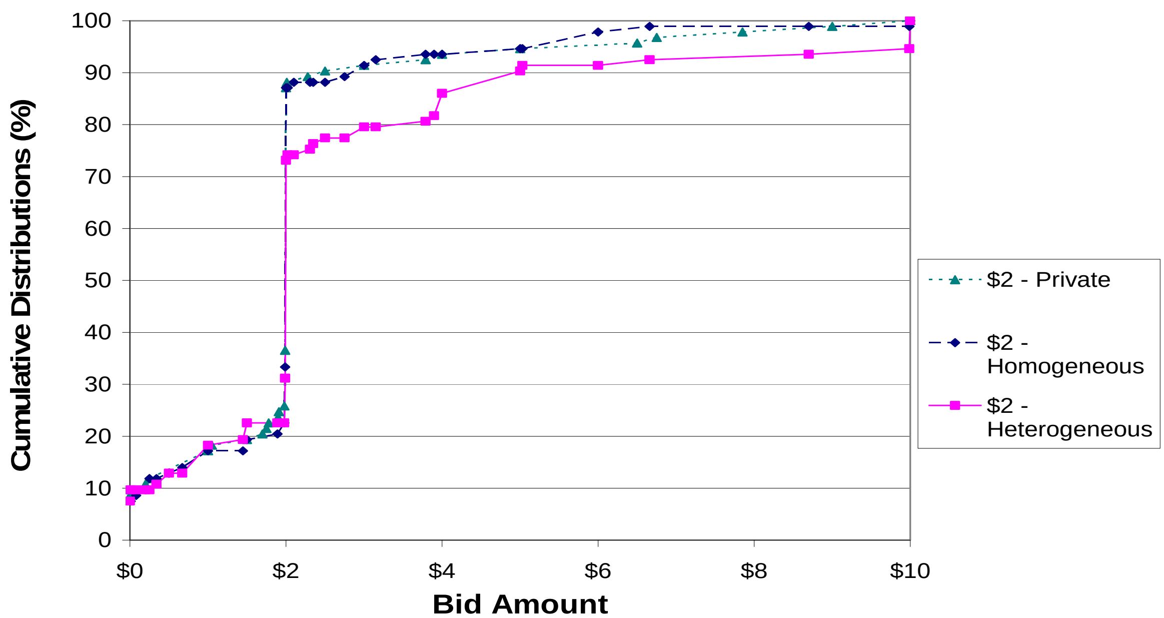
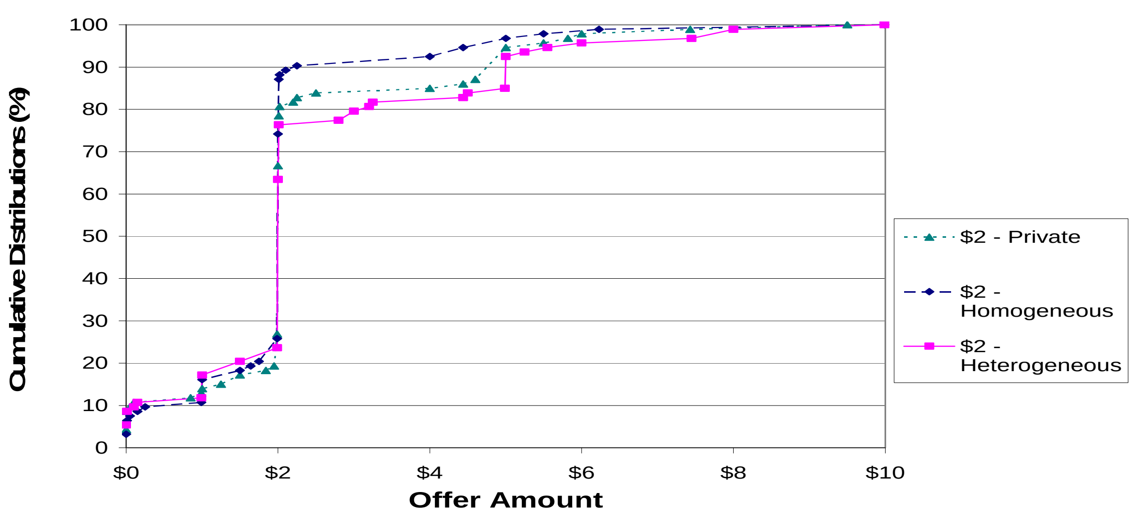


Figure 7: Heterogeneous Treatments, Bid and Offer Distributions
Worse-Off Subjects – Gains

a. WTP Gains - $2



b. WTA Gains - $2

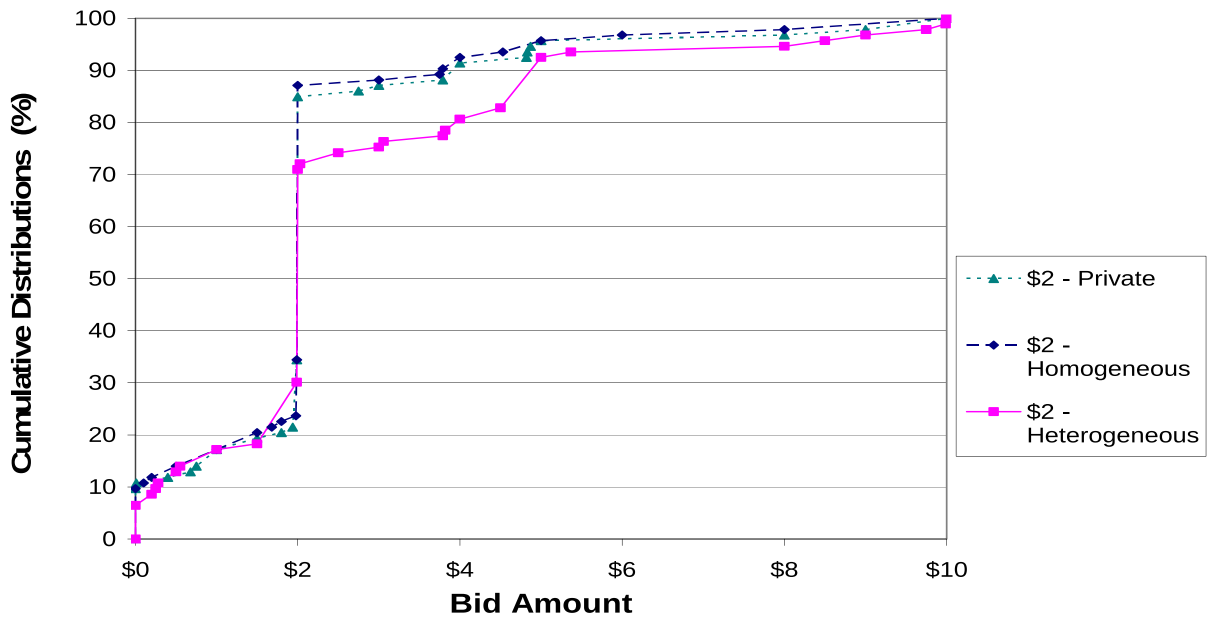


Hypothesis 6: Heterogeneous Treatments – Better-Off Subjects, Losses. In heterogeneous treatments with induced losses, behavior from the better-off subjects shows evidence of significant other-regarding behavior. As discussed earlier, with induced losses the better-off subjects in the public heterogeneous treatments inur induced losses of -$2.00. Therefore, our economic-theoretic construct predicts that best-off subjects will over-report their WTP and WTA. Indeed, best-off subjects submitted average bids of $2.67 and $2.54 (Table 3 and Table 4, respectively), which are significantly above their induced value, thereby rejecting the null hypothesis. These average results are reflected in the distributional shifts presented in Figure 8.

Hypothesis 7: Heterogeneous Treatments - Worse-Off Subjects, Losses. With losses, the worst-off subjects had induced losses of -$8.00 and our theoretical framework predicts that they would under-report their WTP and WTA. Consistent with this prediction, the worse-off subjects submitted bids that were $0.32 below the induced value in WTP-Losses and $0.71 below in WTA-Losses (Table 3 and Table 4, respectively). These distributional shifts are significant, and, as depicted in Figure 9, mirror those of the previously discussed with the greatest shifts occurring a the lower end of the distribution.

Figure 8: Heterogeneous Treatments, Bid and Offer Distributions
Better-Off Subjects – Losses

a. WTP Losses - $2



b. WTA-Losses - $2

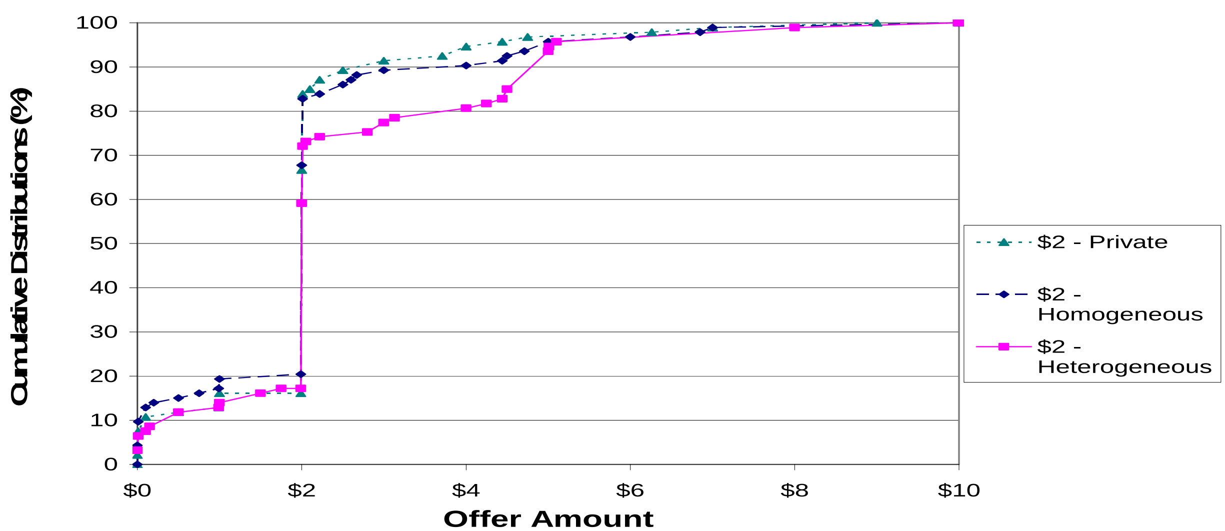
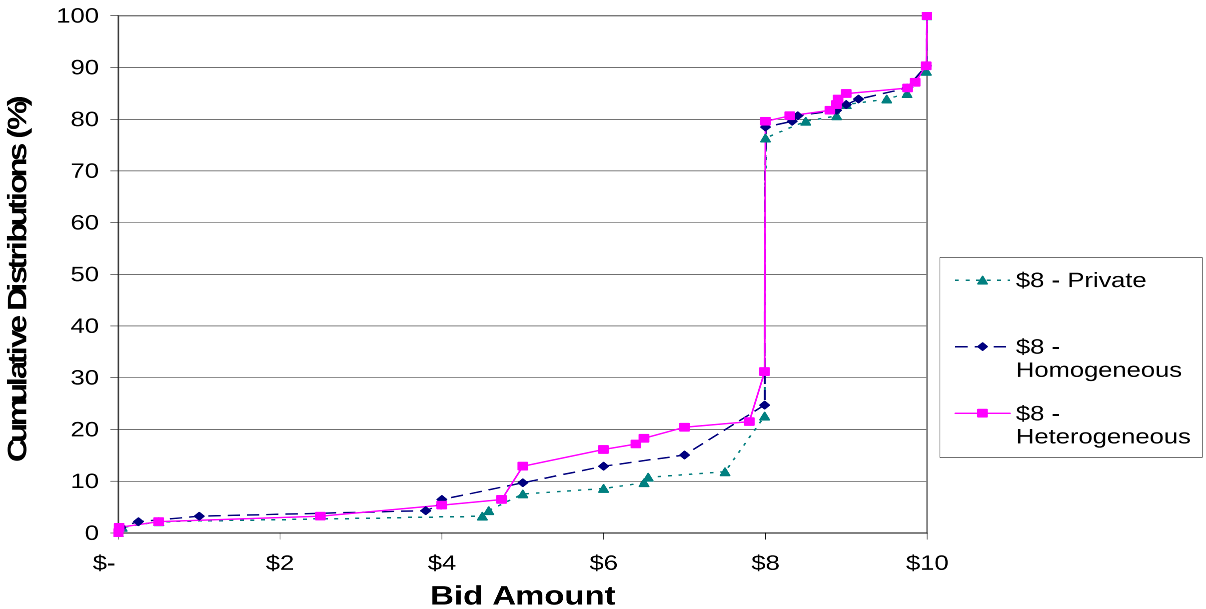
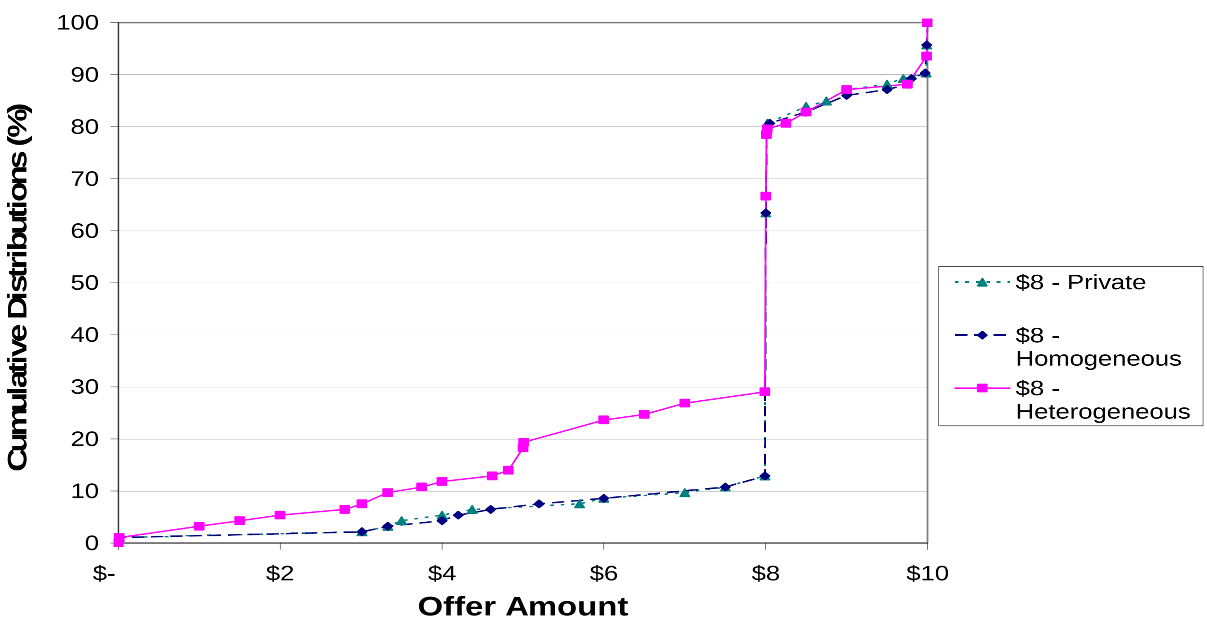


Figure 8: Heterogeneous Treatments, Bid and Offer Distributions
Worse-Off Subjects – Losses

a. WTP-Losses - $8



b. WTA-Losses - $8



5. Conclusion

The empirical evidence presented in this report demonstrates that the public good version of the incentive compatible BDM mechanism, when combined with voting in a coercive tax setting, is demand revealing. This is demonstrated by the failure to reject the null hypotheses that bids and offers equal induced values in private and homogeneous settings. Because there is no overbidding in the homogeneous settings, warm glow or impure altruism does not appear to carry over to this mechanism. In addition, the use of monetary incentives excludes the possibility of paternalistic altruism and both WTP and WTA approximate induced values (and hence approximate each other). However, concerns about equity arise with this mechanism. That is, subjects with high induced gains tend to understate their WTP and WTA in a public good coercive tax setting with a heterogeneous distribution of induced values. This pattern is mirrored in the induced loss treatments where the subjects with low induced losses demonstrate other-regarding concern by overstating their WTP.

The results of these experiments indicate that other-regarding behavior in a coercive tax setting is quite different that found in voluntary contribution settings. The experiments presented in the paper represent a potential starting point from which these issues can be examined further, and assumptions in our theoretical framework can be relaxed. Promising extensions of this include changes in voting group size when values are heterogeneously distributed and changes in the distributions of the heterogeneous treatments. Given our positive results using induced values, we maintain that this mechanism is ready to be applied to "real commodities" commodities, and the demand revealing characteristics of this mechanism offers a means for exploring various behavioral anomalies in the context of public settings.

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Appendix A. Experimental Instructions

WTP Instructions – Version Gains First
Instructions – (Part A)

This is an experiment in the economics of decision making. In the course of the experiment, you will have opportunities to earn money. Any money earned during this experiment is yours to keep. It is therefore important that you read these instructions carefully. Please do not communicate with other participants during the experiment.

In today's experiment, you will be asked to indicate the highest amount of money you would pay and still vote for different programs. In this experiment, a *program* is simply a distribution of money. As you will see, the amount that you indicate as the highest amount that you would pay for the program will become a vote in favor or against the program, and will determine whether or not the program is funded. It is therefore important that you consider all of the information given to you about the different programs and that you make judicious decisions. The procedures that will be followed are the same for all programs. However, each program and vote is independent from the other. Therefore, the decisions you make and the result of a vote for one program will not affect the results for other programs.

For each program, the experiment proceeds as follows:
First, you will receive an initial balance of $10.00.

You will then be informed of your "personal payoff amount" for this program. Your personal payoff amount is the amount of money that you will receive if the program is **funded**. Your personal payoff amount will vary during the course of the experiment. The possible amounts are $2.00, $5.00, and $8.00.

You will then be asked to write down the highest amount that you would pay and still vote for this program; we will call this your "bid". For each program, you can bid any amount between $0.00 and your initial balance of $10.00. Once you have decided your bid, you will write it on a Voting Sheet and enter it into the computer spreadsheet. We will then collect the Voting Sheets and determine the cost of the program.

The cost of the program will be determined by reading off three numbers from a random number table. The starting number will be determined by dropping a pen onto the random number table. (If more than one mark occurs from the drop, then the one closest to the upper-left corner will be used.) The numbers will be read from left to right on the table. The first number will represent the dollar amount. The second number will represent the dime amount. The third number will represent the penny amount. Together, the three numbers will form a cost between $0.00 and $9.99. Note: since these numbers have been generated by a random number table each cost between $0.00 and $9.99 is equally likely. Once the cost has been determined, you will be asked to enter it into the spreadsheet on your computer.

Whether or not the program is funded depends on the amount of your bid and the cost of the program. There are two possible outcomes:
<u>The program is NOT FUNDED:</u> The program is not funded if your bid is *less than* the cost determined from the random number table. In this case, you will not receive your personal payoff amount and you will not have to pay the cost. Therefore, your earnings for this portion of the experiment would simply be your initial balance of $10.00.

<u>The program is FUNDED:</u> The program is funded if your bid is *equal to or greater than* the cost determined from the random number table. In this case, you will receive your personal payoff amount in addition to your initial balance. However, you will also have to pay the determined cost. Therefore, your cash earnings for this portion of the experiment would be your initial balance ($10.00), plus your personal payoff amount, minus the cost.

Note how your bid is like a vote for or against funding the program. With your bid, you are writing down the highest amount you would pay and still vote for the program. Therefore, your bid is like a vote in favor of the program if you are prepared to pay an amount equal to or greater than the randomly determined cost. On the other hand, your bid is like a vote against the program if your bid turns out to be less than the cost. Since you are the only voter, your bid will determine whether the program is funded or not.

While your bid helps determine whether the program is funded or not, your earnings for a particular program are based on your initial balance, your personal payoff amount and the determined cost. For example, if a program was *not funded* and your personal payoff was $5.00 and the determined cost was $9.00, your earnings would be $10.00. However, if the program was *funded* with the same personal payoff and cost, your earnings would be $6.00 ($10 + $5 - $9). Consider another example where your personal payoff was $5.00 and the determined cost was $2.00. In this example if the program was *not funded* your earnings would again be $10.00, while if the program was *funded*, your earnings would be $13.00 ($10 + $5 - $2).

<u>Calculation of Your Earnings</u>
Once you enter the cost of the program determined from the random number table, the computer will automatically determine whether the program was funded and calculate your earnings. The computer will add your experimental earnings for all of the programs, and convert this amount to US dollars by applying an exchange rate of one US dollar for fifteen experimental dollars. For example, if you earn $230.45 experimental dollars, your monetary payoff from this part of the experiment would be $15.50. At the end of the experiment, we will audit all of the spreadsheets to ensure accuracy.

It is important that you clearly understand these instructions.
Please raise your hand if you have any questions.
Please do not talk with other participants in the experiment.

Instructions – (Part B)

You will now be asked to indicate how much you would pay for each of 9 separate programs. The procedures are similar to the ones used in Part A of the experiment, except for three important differences.

1) For each of the programs, you may be the only voter (as in the first part of the experiment), but you may also be part of a group of 3 voters. For programs where the group size is 3, the payoff amounts that the other voters in your group would receive if the program is funded are indicated on your Voting Sheet.

2) Only one of the 9 programs will actually be implemented and result in cash earnings. Therefore, all votes will be made prior to determination of any costs. At the conclusion of the experiment, we will randomly determine which of the programs will generate cash earnings by drawing from a bag containing 9 chips lettered A through I which correspond to each of the programs.

3) For the program that generates cash earnings, the exchange rate will be one US dollar for one experimental dollar. For example, if you earn $12.25 experimental dollars in the second part of the experiment, your monetary payoff would be $12.25.

<u>For each program, the experiment proceeds as follows:</u>

You and every other member of your group will receive an initial balance of $10.00.

For the nine programs, your personal payoff amount may be $2.00, $5.00, or $8.00. Other participants will also receive one of these three payoff amounts and you will be informed of these amounts.

For each program, you will be asked to write your bid on the Voting Sheet provided and enter the same amount into the second spreadsheet on the computer. You can bid any amount between $0.00 and your initial balance of $10.00.

Once everyone has written down his/her bid and entered the bid into the computer, we will collect the Voting Sheets and distribute the new Voting Sheets for the next program.

While we will not implement the program until the end of the experiment, please note that the cost for the program will be determined in the same manner as in Part A using a new random number table. However, the cost will now be a cost that each person in your group will have to pay if the program is funded.

Whether or not the programs are funded depends on the bids by members of your group and the cost of the particular program. There are two possible outcomes:

<u>The program is NOT FUNDED:</u> The program is not funded if a majority of bids from your group are *less than* the cost determined from the random number table. In this case, neither you nor any other member of your group will receive a personal payoff amount and no one will pay the cost. Therefore, your cash earnings for this part of the experiment would simply be your initial balance of $10.00.

<u>The program is FUNDED:</u> The program is funded if a majority of bids from your group are *equal to or greater than* the cost determined from the random number table. In this case, you will receive your personal payoff amount in addition to your initial balance. However, you will also have to pay the randomly determined cost. Every other member of your group will also receive their personal payoff amount and they will also have to pay the determined cost. Therefore, your cash earnings would be your initial balance ($10.00), plus your personal payoff amount, minus the cost.

The programs, in which you are a group of one, are identical to the programs you experienced in the first part of the experiment. Therefore, the program is not funded if your bid is *less* than the cost determined from the random number table, and program is funded if your bid is *equal to or greater than* the determined cost.

Note once again how your bid is like a vote for or against funding the program. With your bid, you are writing down the highest amount you would pay and still vote for the program. Therefore, your bid is like a vote in favor of the program if you are prepared to pay an amount equal to or greater than the randomly determined cost. On the other hand, your bid is like a vote against the program if it turns out to be less than the cost. When a majority of bids are equal to or greater than the determined cost, this translates into a majority vote in favor of the program. Similarly, a majority of bids below the cost translates into a majority vote against the program at that cost.

It is important that you clearly understand these instructions.

Please raise your hand if you have any questions.

Please do not talk with other participants in the experiment.

Instructions – (Part C)
Similar to Part A, you will again be asked to indicate the highest amount of money you would pay and still vote for different programs. However, in this part you will be informed of your "personal *loss* amount" instead of your "personal *payoff* amount". Your personal loss amount is the amount of money that you will lose if the program is **not funded**. Your personal loss amount will vary during the course of the experiment. The possible loss amounts are minus $2.00, minus $5.00, and minus $8.00.

There two possible outcomes are as follows:

The program is NOT FUNDED: The program is not funded if your bid is *less than* the cost determined from the random number table. In this case, you will not pay the cost, but you will pay your personal loss amount. Therefore, your earnings would be initial balance of $10.00 minus the loss amount.

The program is FUNDED: The program is funded if your bid is *equal to or greater than* the cost determined from the random number table. In this case, you pay the determined cost, but do not pay your personal loss amount. Therefore, your earnings would be initial balance of $10.00 minus the determined cost.

For example, if a program was *not funded* and your personal loss amount was minus $5.00 and the determined cost was $9.00, your earnings would be $5.00 ($10 - $5). However, if the program was *funded* with the same personal loss amount and determined cost, your earnings would be $1.00 ($10 - $9). Consider another example where your personal loss amount was minus $5.00 and the determined cost was $2.00. In this example if the program was *not funded* your earnings would be $5.00 ($10 - $5), while if the program was *funded*, your earnings would be $8.00 ($10 - $2).

Calculation of Your Earnings
Similar to part A, the computer will automatically determine your earnings and the exchange rate will again be one US dollar for fifteen experimental dollars.

Instructions – (Part D)
Similar to Part B, you again will now be asked to indicate how much you would pay for each of 9 separate programs. Again the exchange rate is one experimental dollar equaling one US dollar and all other procedures are identical to those of Part B, except that now you will have personal loss amounts of minus $2.00, minus $5.00, and minus $8.00. For these programs, other participants will also receive one of these three loss amounts.

After all of the Voting Sheets have been collected, a program from Part D will be selected for implementation by drawing from a bag containing 9 chips lettered J through R, which correspond to each of the programs. At that time we will also implement and produce cash earnings for Part B.

For Part D, there are two possible outcomes:

The program is NOT FUNDED: The program is not funded if a majority of bids from your group are *less than* the cost determined from the random number table. In this case, neither you nor any other member of your group will pay the cost, but everyone pays his/her personal loss amount. Therefore, your earnings would be your initial balance of $10.00 minus the loss amount.

The program is FUNDED: The program is funded if a majority of bids from your group are *equal to or greater than* the cost determined from the random number table. In this case, you and every other member of your group will pay the determined cost, but no one pays his/her personal loss amount. Therefore, your earnings would be your initial balance of $10.00 minus the determined cost.

Calculation of Final Earnings
To calculate your earnings from Part D and Part B, you will be asked to enter into the spreadsheet the cost for the implemented programs and whether these programs were funded. Your computer will then calculate your earnings for Part D and Part B, add them to your earnings from Part A and Part C, and award you an additional $5 show up fee. We will audit the spreadsheets to ensure accuracy.

WTA Instructions – Version Gains First
Instructions – (Part A)

This is an experiment in the economics of decision making. In the course of the experiment, you will have opportunities to earn money. Any money earned during this experiment is yours to keep. It is therefore important that you read these instructions carefully. Please do not communicate with other participants during the experiment.

In today's experiment, you will be asked to indicate the lowest amount of money you would accept as compensation and still vote against different programs. In this experiment, a *program* is simply a distribution of monetary gain. As you will see, the amount that you indicate as the lowest amount of compensation that you would accept for the program will become a vote in favor or against the program, and will determine whether or not the program is implemented. It is therefore important that you consider all of the information given to you about the different programs and that you make judicious decisions. The procedures that will be followed are the same for all programs. However, each program and vote is independent from the other. Therefore, the decisions you make and the result of a vote for one program will not affect the results for other programs.

For each program, the experiment proceeds as follows:
First, you will receive an initial balance of $5.00.

You will then be informed of your "personal payment amount" for this program. Your personal payoff amount is the amount of money that you will receive if the program is **implemented**. Your personal payoff amount will vary during the course of the experiment. The possible payoff amounts are $2.00, $5.00, and $8.00.

You will then be asked to write down the lowest amount that you would accept as compensation and still vote against this program; we will call this your "offer". For each program, you can offer any amount between $0.00 and $10.00. Once you have decided your offer, you will write it on a Voting Sheet and enter it into the computer spreadsheet. We will then collect the Voting Sheets and determine the compensation for the program.

The **compensation** for the program will be determined by reading off three numbers from a random number table. The starting number will be determined by dropping a pen onto the random number table. (If more than one mark occurs from the drop, then the one closest to the upper-left corner will be used.) The numbers will be read from left to right on the table. The first number will represent the dollar amount. The second number will represent the dime amount. The third number will represent the penny amount. Together, the three numbers will form a compensation amount between $0.00 and $9.99. Note: since these numbers have been generated by a random number table each compensation amount between $0.00 and $9.99 is equally likely. Once the compensation has been determined, you will be asked to enter it into the spreadsheet on your computer.

Whether or not the program is implemented depends on the amount of your offer and the compensation for the program. There are two possible outcomes:

<u>The program is NOT IMPLEMENTED:</u> The program is not implemented if your offer is *equal to or less than* the compensation determined from the random number table. In this case, you will not receive your personal payoff amount, but you do receive the compensation. Therefore, your earnings for this portion of the experiment would be your initial balance of $5.00 plus the compensation.

<u>The program is IMPLEMENTED:</u> The program is implemented if your offer is *greater than* the compensation determined from the random number table. In this case, you will receive your personal payoff amount in addition to your initial balance. Therefore, your cash earnings for this portion of the experiment would be your initial balance ($5.00) plus your personal payoff amount.

Note how your offer is like a vote for or against implementing the program. With your offer, you are writing down the lowest amount of compensation you would accept to vote against the program. Therefore, your offer is like a vote against the program if your offer turns out to be equal to or less than the randomly determined compensation. On the other hand, your offer is like a vote in favor of the program if your offer turns out to be more than the compensation. Since you are the only voter, your offer will determine whether the program is implemented or not.

While your offer helps determine whether the program is implemented or not, your earnings for a particular program are based on your initial balance, your personal payoff amount and the determined compensation. For example, if a program was *not implemented* and your personal payoff was $5.00 and the determined compensation was $9.00, your earnings would be $14.00 ($5 + $9). However, if the program was *implemented* with the same personal payoff and compensation, your earnings would be $10.00 ($5 + $5). Consider another example where your personal payoff was $5.00 and the determined compensation was $2.00. In this example if the program was *not implemented* your earnings would again be $7.00 ($5 + $2), while if the program was *implemented*, your earnings would be $10.00 ($5 + $5).

<u>Calculation of Your Earnings</u>
Once you enter the compensation of the program determined from the random number table, the computer will automatically determine whether the program was implemented and calculate your earnings. The computer will add your experimental earnings for all of the programs, and convert this amount to US dollars by applying an exchange rate of one US dollar for fifteen experimental dollars. For example, if you earn $230.45 experimental dollars, your monetary payoff from this part of the experiment would be $15.50. At the end of the experiment, we will audit all of the spreadsheets to ensure accuracy.

It is important that you clearly understand these instructions.

Please raise your hand if you have any questions.

Please do not talk with other participants in the experiment.

<u>Instructions – (Part B)</u>
You will now be asked to indicate the lowest amount of compensation you would accept for each of 9 separate programs. The procedures are similar to the ones used in Part A of the experiment, except for three important differences.

1) For each of the programs, you may be the only voter (as in the first part of the experiment), but you may also be part of a group of 3 voters. For programs where the group size is 3, the personal payoff amounts that the other voters in your group would receive if the program is not implemented are indicated on your Voting Sheet.

2) Only one of the 9 programs will actually be selected and result in cash earnings. Therefore, all votes will be made prior to determination of any of the compensations. At the conclusion of the experiment, we will randomly determine which of the programs will generate cash earnings by drawing from a bag containing 9 chips lettered A through I which correspond to each of the programs.

3) For the program that generates cash earnings, the exchange rate will be one US dollar for one experimental dollar. For example, if you earn $12.25 experimental dollars in the second part of the experiment, your monetary payoff would be $12.25.

<u>For each program, the experiment proceeds as follows:</u>
You and every other member of your group will receive an initial balance of $5.00.

For the nine programs, your personal payoff amount may be $2.00, $5.00, or $8.00. Other participants will also receive one of these three payoff amounts and you will be informed of these amounts.

For each program, you will be asked to write your offer on the Voting Sheet provided and enter the same amount into the second spreadsheet (Sheet B) on the computer. You can offer any amount between $0.00 and $10.00.

Once everyone has written down his/her offer and entered the offer into the computer, we will collect the Voting Sheets and distribute the new Voting Sheets for the next program.

While we will not select the program until the end of the experiment, please note that the compensation for the program will be determined in the same manner as in Part A using a new random number table. However, the compensation will now be received by each person in your group if the program is not implemented.

Whether or not the programs are implemented depends on the offers by members of your group and the compensation of the particular program. There are two possible outcomes:

<u>The program is NOT IMPLEMENTED:</u> The program is not implemented if a majority of offers from your group are *equal to or less than* the compensation determined from the random number table. In this case, neither you nor any other member of your group will receive a personal payoff amount, but everyone does receive the compensation. Therefore, your cash earnings for this part of the experiment would be your initial balance of $5.00 plus the compensation.

<u>The program is IMPLEMENTED:</u> The program is implemented if a majority of offers from your group are *greater than* the compensation determined from the random number table. In this case, you and everyone else in your group will receive their personal payoff amount in addition to the initial balance. Therefore, your cash earnings would be your initial balance ($5.00) plus your personal payoff amount.

The programs, in which you are a group of one, are identical to the programs you experienced in the first part of the experiment. Therefore, the program is implemented if your offer is *greater than* the compensation determined from the random number table, and the program is not implemented if your offer is *equal to or less than* the determined compensation.

Note once again how your offer is like a vote for or against implementing the program. With your offer, you are writing down the lowest amount of compensation you would accept and still vote against the program. Therefore, your offer is like a vote against the program if your offer is equal to or less than the randomly determined compensation. On the other hand, your offer is like a vote in favor of the program if the randomly determined compensation turns out to be more than your offer. When a majority of offers are equal to or less than the determined compensation, this translates into a majority vote against the program. Similarly, a majority of offers greater than the compensation translates into a majority vote in favor of the program for that compensation.

It is important that you clearly understand these instructions.

Please raise your hand if you have any questions.

Please do not talk with other participants in the experiment.

<u>Instructions – (Part C)</u>
Similar to Part A, you will again be asked to indicate the lowest amount of compensation you would accept in different programs. However, in this part, your offer is the lowest amount of compensation you would accept to vote *in favor* of the program. In addition, you will be informed of your "personal *loss* amount" instead of your "personal *payment* amount". Your personal loss amount is the amount of money that you will lose if the program is **implemented**. Your personal loss amount will vary during the course of the experiment. The possible loss amounts are minus $2.00, minus $5.00, and minus $8.00.

There two possible outcomes are as follows:

<u>The program is IMPLEMENTED:</u> The program is implemented if your offer is *less than or equal to* the compensation determined from the random number table. In this case, you will receive the determined compensation in addition to your initial balance. However, you will also have to pay your personal loss amount. Therefore, your earnings for this portion of the experiment would be your initial balance ($5.00), plus the compensation, minus your personal loss amount.

<u>The program is NOT IMPLEMENTED:</u> The program is not implemented if your offer is *greater than* the compensation determined from the random number table. In this case, you do not receive the random compensation or pay your personal loss amount. Therefore, your earnings for this portion of the experiment would simply be your initial balance of $5.00.

For example, if a program was *not implemented* and your personal loss amount was minus $5.00 and the determined compensation was $9.00, your earnings would be $5.00 (your initial balance). However, if the program was *implemented* with the same personal loss amount and determined compensation, your earnings would be $9.00 ($5 + $9 - $5). Consider another example where your personal loss amount was minus $5.00 and the determined compensation was $2.00. In this example if the program was *not implemented* your earnings would be again be $5.00, while if the program was *implemented*, your earnings would be $2.00 ($5 + $2 - $5).

<u>Calculation of Your Earnings</u>
Similar to part A, the computer will automatically determine your earnings and the exchange rate will again be one US dollar for fifteen experimental dollars.

<u>Instructions – (Part D)</u>
Similar to Part C, you again will now be asked to indicate the lowest amount of compensation you would accept for each of 9 separate programs. Again the exchange rate is one experimental dollar equaling one US dollar and all other procedures are identical to those of Part B, except that now you will have personal loss amounts of minus $2.00, minus $5.00, and minus $8.00. For these programs, other participants will also receive one of these three loss amounts.

After all of the Voting Sheets have been collected, a program from Part D will be selected by drawing from a bag containing 9 chips lettered J through R, which correspond to each of the programs. At that time we will also select and produce cash earnings for Part B.

For Part D, there are two possible outcomes:

<u>The program is IMPLEMENTED:</u> The program is implemented if a majority of offers from your group are *less than or equal to* the compensation determined from the random number table. In this case, you and every other member of your group will receive the determined compensation in addition to your initial balance. However, everyone will also have to pay their personal loss amount. Therefore, your earnings for this portion of the experiment would be your initial balance ($5.00), plus the compensation, minus your personal loss amount.

<u>The program is NOT IMPLEMENTED:</u> The program is not implemented if the majority of the offers from your group are *greater than* the compensation determined from the random number table. In this case, neither you nor any member of your group will receive the random compensation nor pay your personal loss amount. Therefore, your earnings for this portion of the experiment would simply be your initial balance of $5.00.

<u>Calculation of Final Earnings</u>
To calculate your earnings from Part D and Part B, you will be asked to enter into the spreadsheet the compensation for the implemented programs and whether these programs were implemented. Your computer will then calculate your earnings for Part D and Part B, add them to your earnings from Part A and Part C, and award you an additional $5 show up fee. We will audit the spreadsheets to ensure accuracy.

Appendix B: WTP and WTA Data and Coding:

WTP Demographic Data:

Obs #	Exp Session	Sub# within Session	age	gender	year	major	ecncours
1	1	1	32	0	9	99	3
2	1	2	19	0	3	1	3
3	1	3	19	1	2	8	1
4	1	4	20	0	2	1	2
5	1	5	19	1	3	33	4
6	1	6	20	1	4	32	0
7	1	7	22	1	4	26	2
8	1	8	19	0	3	17	5
9	1	9	22	1	4	10	1
10	1	10	21	0	3	34	2
11	1	11	21	0	9	35	2
12	1	12	23	0	9	1	6
13	1	13	22	0	4	1	3
14	1	14	23	1	4	1	1
15	1	15	20	1	4	28	1
16	2	1	24	0	9	1	3
17	2	2	44	0	9	1	99
18	2	3	24	1	4	1	7
19	2	4	18	1	9	8	2
20	2	5	21	1	4	15	4
21	2	6	18	0	2	1	2
22	2	7	20	1	2	10	2
23	2	8	27	0	9	1	10
24	2	9	19	0	2	36	2
25	2	10	19	0	3	35	1
26	2	11	20	0	3	37	1
27	2	12	28	1	9	38	99
28	2	13	19	0	3	1	6
29	2	14	22	0	9	10	3
30	2	15	21	0	4	17	8
31	2	16	46	1	9	1	2
32	2	17	19	1	1	17	99
33	2	18	21	0	4	17	9
34	3	1	20	1	3	1	4
35	3	2	22	0	9	1	5
36	3	3	19	0	2	35	2
37	3	4	19	1	3	1	2
38	3	5	21	0	4	1	2
39	3	6	22	1	4	4	5
40	3	7	21	1	9	39	0
41	3	9	25	0	9	35	0
42	3	10	22	0	4	10	5
43	3	11	19	1	3	22	0
44	3	12	28	1	9	5	0

45	3	13	25	1	9	14	1
46	3	14	18	1	2	1	2
47	3	15	20	1	3	37	2
48	3	16	23	1	9	40	0
49	3	17	19	0	2	1	2
50	4	1	19	0	3	17	7
51	4	2	19	1	3	10	1
52	4	3	21	0	4	4	5
53	4	5	29	0	9	17	99
54	4	7	18	1	2	26	2
55	4	8	20	0	3	29	0
56	4	9	18	1	2	6	2
57	4	10	20	1	3	23	3
58	4	11	18	0	3	35	1
59	4	12	21	0	3	1	4
60	4	13	24	1	9	1	8
61	4	14	20	1	3	6	1
62	4	15	18	0	9	30	1
63	4	16	18	0	4	30	1
64	4	18	20	1	3	41	1
65	5	1	20	1	3	25	0
66	5	2	19	0	3	25	3
67	5	3	21	0	4	1	1
68	5	4	19	0	2	1	3
69	5	5	20	1	3	2	3
70	5	6	21	0	3	19	2
71	5	7	19	1	2	3	1
72	5	8	20	1	4	24	0
73	5	9	22	1	4	2	0
74	5	10	21	0	4	2	1
75	5	11	19	0	2	1	1
76	5	12	19	1	2	1	1
77	5	13	19	0	2	25	1
78	5	14	20	0	3	1	1
79	5	15	18	0	1	1	1
80	6	1	19	1	2	1	3
81	6	2	19	0	3	25	0
82	6	3	20	1	3	8	2
83	6	4	19	1	2	25	2
84	6	5	22	1	3	21	1
85	6	6	22	0	4	16	1
86	6	7	19	1	2	17	3
87	6	8	19	0	3	1	1
88	6	9	20	0	3	8	3
89	6	10	20	0	3	6	1
90	6	11	20	0	3	1	3
91	6	12	21	1	4	25	0
92	6	13	99	99	9	99	99
93	6	15	19	0	2	1	2

WTP Gains:

Obs #	Private $2	Homo $2	Hetero $2	Private $5	Homo $5	Hetero $5	Private $8	Homo $8	Hetero $8
1	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
2	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
3	0.00	2.00	2.00	0.00	0.00	5.00	8.00	8.00	8.00
4	0.20	0.25	0.50	1.00	5.00	1.00	10.00	10.00	10.00
5	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
6	2.00	0.00	0.00	5.00	5.00	5.00	8.00	8.00	8.00
7	9.00	3.00	10.00	6.00	5.00	8.00	10.00	8.00	5.00
8	1.50	0.67	0.50	1.00	1.25	0.35	9.68	9.50	9.00
9	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
10	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
11	1.91	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
12	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
13	1.00	1.00	1.50	4.50	4.50	4.50	9.99	9.99	9.99
14	1.00	1.00	1.00	6.00	6.00	5.00	8.00	9.00	7.00
15	10.00	10.00	10.00	10.00	10.00	10.00	10.00	10.00	10.00
16	2.00	2.00	1.00	5.00	5.00	5.50	8.00	8.00	8.50
17	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
18	1.00	1.50	5.00	2.00	5.00	2.00	3.00	6.00	2.00
19	5.00	5.00	5.00	6.00	6.00	5.00	5.00	6.00	6.00
20	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
21	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
22	6.50	6.66	5.03	7.32	4.99	6.03	7.63	7.77	7.14
23	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
24	1.99	1.99	1.99	4.99	4.99	4.99	7.99	7.99	7.99
25	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
26	1.00	1.50	1.50	8.00	5.00	5.00	9.99	9.99	9.99
27	3.00	2.00	3.90	6.00	6.00	5.00	10.00	10.00	8.00
28	1.99	1.99	1.99	4.99	4.99	4.99	7.99	7.99	7.99
29	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
30	1.99	1.99	1.99	4.99	4.99	4.99	7.99	7.99	7.99
31	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
32	0.00	0.00	10.00	5.00	6.00	10.00	10.00	10.00	10.00
33	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
34	1.99	1.99	1.99	4.99	4.99	4.99	7.99	7.99	7.99
35	1.99	1.99	1.99	4.99	4.99	4.99	7.99	7.99	7.99
36	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
37	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	5.00
38	2.00	2.00	5.00	5.00	5.00	5.00	8.00	8.00	5.00
39	2.00	2.00	0.00	5.00	5.00	5.00	8.00	8.00	10.00
40	2.00	2.00	4.00	5.00	5.00	5.00	8.00	8.00	8.00
41	2.00	2.00	2.00	5.00	5.00	6.50	8.00	8.00	6.50
42	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
43	2.00	2.00	3.00	5.00	5.00	5.00	8.00	8.00	8.00
44	2.01	2.00	2.00	5.00	5.00	5.00	8.00	10.00	8.00

45	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
46	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
47	4.00	3.00	3.00	5.00	5.00	5.00	8.00	8.00	8.00
48	2.00	2.00	0.00	5.00	5.00	5.00	8.00	8.00	8.00
49	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	5.00
50	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
51	0.25	0.50	0.01	1.75	4.00	3.00	9.50	9.99	9.99
52	2.00	2.00	5.00	5.00	5.00	5.00	8.00	8.00	8.00
53	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
54	7.85	6.00	6.66	9.99	8.99	7.92	9.99	9.99	9.99
55	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
56	2.00	2.00	2.00	5.00	5.00	2.00	8.00	8.00	2.00
57	1.99	1.99	0.01	4.99	4.99	4.99	7.99	7.99	9.99
58	1.75	0.25	2.50	8.75	7.50	7.25	9.95	9.50	9.99
59	0.00	0.00	0.00	5.00	5.00	5.00	10.00	10.00	10.00
60	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
61	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
62	2.50	2.10	1.50	5.25	4.50	4.50	6.50	7.20	7.50
63	1.90	1.98	4.00	4.40	5.60	6.00	8.90	8.70	9.95
64	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
65	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
66	0.00	0.00	10.00	10.00	5.00	5.00	10.00	10.00	0.00
67	0.00	0.00	0.00	1.00	2.00	4.00	2.00	2.00	3.00
68	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
69	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
70	1.78	3.15	1.45	4.19	4.90	6.25	8.58	8.77	7.29
71	2.00	2.00	0.00	5.00	5.00	5.00	8.00	8.00	8.00
72	1.99	1.99	1.99	4.99	4.99	4.99	7.99	7.99	7.99
73	0.01	0.20	9.99	4.00	4.89	7.50	9.99	9.99	9.99
74	1.00	1.00	1.00	5.00	5.00	5.00	8.00	8.00	8.00
75	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
76	1.99	1.99	1.99	4.99	4.99	4.99	7.99	7.99	7.99
77	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
78	6.75	6.00	8.70	8.50	7.00	9.00	8.90	7.50	9.50
79	1.99	1.99	2.35	4.99	4.99	4.99	7.99	7.99	7.99
80	3.79	3.79	3.79	8.22	4.20	8.22	8.88	8.88	8.88
81	1.70	1.89	4.00	4.80	4.90	6.00	9.00	10.00	7.80
82	2.00	2.00	1.00	3.00	2.00	6.00	5.00	8.00	8.00
83	2.00	2.00	2.00	5.00	5.00	8.00	8.00	8.00	7.00
84	0.00	0.00	10.00	10.00	10.00	10.00	10.00	10.00	10.00
85	1.99	1.99	1.99	4.99	4.99	4.99	7.99	7.99	7.99
86	1.98	1.98	0.34	3.90	6.80	3.90	9.99	10.00	9.95
87	0.00	0.00	1.00	2.00	2.00	3.00	4.00	5.00	5.00
88	1.05	0.08	2.31	3.13	8.62	2.85	4.32	4.52	3.21
89	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
90	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
91	2.28	2.75	2.02	5.62	5.37	5.45	8.53	8.00	8.63
92	0.00	6.00	4.00	7.50	7.50	5.00	9.00	3.00	4.00
93	2.00	2.00	0.00	5.00	5.00	3.00	8.00	8.00	9.99

WTP Losses:

Obs #	Private - $2	Homo - $2	Hetero - $2	Private - $5	Homo - $5	Hetero - $5	Private - $8	Homo - $8	Hetero -$8
1	1.99	1.99	1.99	4.99	4.99	4.99	7.99	7.99	7.99
2	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
3	2.00	2.00	5.00	5.00	5.00	5.00	8.00	8.00	5.00
4	1.00	1.50	0.20	2.00	1.00	9.00	0.50	1.00	0.50
5	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
6	2.00	0.00	0.00	5.00	5.00	5.00	8.00	8.00	8.00
7	9.00	3.00	4.50	7.00	6.50	10.00	10.00	6.00	8.00
8	1.99	1.80	1.99	8.50	2.50	1.99	4.50	5.00	7.00
9	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
10	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
11	1.94	2.00	2.03	5.00	5.00	5.01	8.00	8.00	4.74
12	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
13	1.50	1.50	2.50	4.50	5.00	4.50	6.50	7.00	6.00
14	1.00	2.00	3.00	5.00	5.00	5.00	8.00	7.00	7.00
15	10.00	10.00	9.75	7.99	8.00	10.00	8.00	4.00	5.00
16	2.00	2.00	1.00	5.00	5.00	5.80	8.00	8.00	8.00
17	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
18	1.50	2.00	1.50	6.00	5.00	8.00	9.99	8.00	8.00
19	5.00	5.00	5.00	6.00	6.00	6.00	5.00	6.00	6.00
20	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
21	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
22	2.00	4.53	0.50	6.63	5.98	4.93	8.00	8.33	8.30
23	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
24	1.99	1.99	1.99	4.99	4.99	4.99	7.99	7.99	7.99
25	2.00	2.00	5.00	5.00	5.00	5.00	8.00	8.00	5.00
26	10.00	10.00	8.50	7.50	3.50	5.00	0.05	0.03	0.01
27	1.80	2.00	4.00	4.80	5.10	6.00	8.50	8.00	10.00
28	1.99	1.99	1.99	4.99	4.99	4.99	7.99	7.99	7.99
29	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
30	1.99	1.99	1.99	4.99	4.99	4.99	7.99	7.99	7.99
31	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
32	0.00	0.00	9.00	1.00	0.00	9.00	10.00	10.00	10.00
33	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
34	1.99	1.99	1.99	4.99	4.99	4.99	7.99	7.99	7.99
35	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
36	2.00	2.00	2.00	5.00	5.00	5.00	10.00	10.00	10.00
37	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
38	2.00	2.00	5.00	5.00	5.00	5.00	8.00	8.00	5.00
39	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
40	2.00	2.00	4.50	5.00	5.00	6.00	8.00	8.00	8.00
41	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
42	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
43	2.00	2.00	2.00	5.00	5.00	4.50	8.00	8.00	6.00
44	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
45	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
46	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00

47	4.00	4.00	4.00	5.00	5.00	5.00	9.00	10.00	8.00
48	2.00	2.00	2.00	4.99	4.99	5.00	7.99	8.00	8.00
49	2.00	2.00	5.00	5.00	5.00	5.00	8.00	8.00	5.00
50	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
51	0.75	0.50	0.25	6.50	4.00	7.00	9.50	9.75	10.00
52	2.00	2.00	5.00	5.00	5.00	5.00	8.00	8.00	8.00
53	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
54	4.83	5.00	3.82	6.29	8.69	6.15	7.50	3.99	9.76
55	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
56	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	5.00
57	1.99	1.99	1.99	4.99	4.99	4.99	7.99	7.99	7.99
58	2.75	0.50	0.50	7.50	5.75	8.00	9.75	9.75	9.85
59	0.00	0.00	0.00	4.00	5.00	5.00	10.00	10.00	10.00
60	2.00	2.00	5.00	5.00	5.00	5.00	8.00	8.00	8.00
61	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
62	3.00	3.75	2.50	4.50	6.90	6.75	8.50	9.15	8.90
63	1.99	1.50	1.99	4.80	6.00	3.50	8.50	8.40	8.80
64	1.00	1.00	1.00	3.00	3.00	3.00	9.00	9.00	9.00
65	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
66	0.00	0.00	0.00	5.00	5.00	5.00	10.00	10.00	10.00
67	0.00	0.00	0.00	3.75	3.50	3.00	5.00	5.00	6.50
68	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
69	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
70	0.40	0.20	0.55	3.37	4.00	6.95	6.55	3.80	6.40
71	0.00	0.00	0.00	5.00	5.00	5.00	10.00	10.00	10.00
72	1.99	1.99	1.99	4.99	4.99	4.99	7.99	7.99	7.99
73	0.01	0.10	0.20	9.99	3.00	3.89	9.99	9.99	9.99
74	8.00	8.00	8.00	5.00	5.00	5.00	8.00	8.00	8.00
75	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
76	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
77	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
78	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
79	1.99	1.99	1.99	4.99	4.99	4.99	7.99	7.99	7.99
80	3.79	3.79	3.79	8.22	8.22	8.22	8.88	8.88	8.88
81	0.00	0.00	0.00	5.00	5.00	5.00	10.00	10.00	10.00
82	4.00	1.00	2.00	2.00	2.00	3.00	6.00	5.00	8.00
83	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
84	0.00	0.00	10.00	10.00	10.00	10.00	10.00	10.00	10.00
85	1.99	1.99	1.99	4.99	4.99	4.99	7.99	7.99	7.99
86	4.82	1.98	3.06	5.10	4.68	5.00	10.00	9.99	7.80
87	0.00	1.00	1.00	3.00	3.00	3.00	5.00	6.00	4.00
88	0.68	1.68	0.28	3.92	3.43	6.66	4.58	0.25	2.50
89	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
90	1.99	1.99	5.00	4.99	4.99	5.50	8.00	8.00	8.00
91	4.87	6.00	5.37	8.50	8.00	7.50	9.99	9.99	9.99
92	4.00	4.00	5.00	6.00	7.50	7.50	10.00	10.00	4.00
93	0.00	0.00	9.99	5.00	5.00	5.00	9.99	9.99	9.99

WTA Demographic Data:

Obs #	Exp Session	Sub# within Session	age	gender	year	major	ecncours
1	1	1	21	0	3	50	4
2	1	2	19	0	2	1	4
3	1	3	19	0	2	1	3
4	1	4	19	0	2	1	3
5	1	5	21	1	4	8	3
6	1	6	20	1	2	1	3
7	1	7	20	1	2	1	3
8	1	8	27	1	9	99	4
9	1	9	22	0	4	1	3
10	1	10	19	1	2	1	3
11	1	11	19	0	2	1	3
12	1	12	19	1	1	5	1
13	1	13	23	0	4	1	3
14	1	14	19	1	2	1	3
15	1	15	19	0	2	1	3
16	1	16	22	1	4	17	8
17	1	17	20	0	3	1	4
18	1	18	22	0	4	10	2
19	2	1	20	0	3	1	4
20	2	2	22	0	4	52	1
21	2	3	23	1	9	1	25
22	2	4	23	0	9	15	1
23	2	5	22	1	4	45	0
24	2	7	26	1	9	1	5
25	2	8	24	0	9	1	14
26	2	9	21	0	4	15	2
27	2	10	20	1	2	1	2
28	2	11	22	0	3	17	8
29	2	12	21	1	4	24	1
30	2	14	20	0	2	41	2
31	2	15	18	0	1	30	2
32	2	16	20	0	3	1	5
33	2	17	19	0	2	15	0
34	2	18	21	0	4	15	2
35	3	1	20	1	3	1	4
36	3	2	20	0	2	17	3
37	3	3	22	0	4	19	3
38	3	4	19	1	2	27	2
39	3	5	22	1	4	51	0
40	3	7	19	1	2	1	3
41	3	8	20	0	2	1	3
42	3	9	20	1	2	5	1
43	3	10	21	0	3	1	6
44	3	11	19	1	2	5	1
45	3	12	99	9	9	99	99
46	3	13	20	1	2	30	2

47	3	14	19	1	2	1	3
48	3	15	20	1	3	1	3
49	4	1	20	0	2	1	3
50	4	2	19	0	2	1	3
51	4	3	19	1	2	4	5
52	4	4	19	0	2	1	2
53	4	5	20	0	2	52	3
54	4	6	19	1	2	1	8
55	4	7	19	0	3	4	5
56	4	8	21	0	4	15	3
57	4	9	21	0	4	1	3
58	4	10	24	1	9	53	20
59	4	11	18	1	1	1	3
60	4	12	25	1	9	54	2
61	4	13	23	0	4	55	0
62	4	14	22	1	3	27	2
63	4	15	19	0	2	4	2
64	4	16	23	0	4	17	10
65	4	17	22	0	4	8	4
66	4	18	18	1	1	1	3
67	5	1	18	0	1	3	2
68	5	2	20	0	2	10	1
69	5	3	20	1	2	4	2
70	5	4	22	0	4	1	2
71	5	5	22	1	4	5	8
72	5	6	19	0	1	6	1
73	5	7	18	1	1	1	2
74	5	8	19	0	2	1	3
75	5	9	19	0	2	7	1
76	5	10	18	1	2	2	5
77	5	11	22	1	4	8	2
78	5	12	21	1	4	1	4
79	5	13	21	1	4	1	3
80	5	14	34	1	5	1	18
81	5	15	21	1	4	9	2
82	6	1	21	1	3	11	0
83	6	2	26	0	5	2	15
84	6	3	37	0	1	2	7
85	6	4	44	1	99	2	8
86	6	5	20	0	3	12	2
87	6	6	23	1	5	2	16
88	6	7	21	1	4	13	0
89	6	8	27	1	5	1	12
90	6	9	20	0	2	14	2
91	6	10	22	0	3	14	0
92	6	11	22	1	2	14	2
93	6	12	20	1	2	15	0

WTA Gains:

Obs #	Private $2	Homo $2	Hetero $2	Private $5	Homo $5	Hetero $5	Private $8	Homo $8	Hetero $8
1	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
2	2.01	2.01	2.01	5.01	5.01	5.01	8.01	8.01	8.01
3	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
4	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
5	0.00	0.00	0.00	5.00	9.00	3.00	9.00	9.50	9.50
6	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	1.00
7	2.01	2.01	2.01	5.01	5.01	5.01	8.01	8.01	8.01
8	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
9	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
10	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
11	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
12	1.95	1.75	1.00	4.75	4.00	3.00	6.77	6.50	5.00
13	2.01	2.01	2.01	5.01	5.01	5.01	8.01	8.01	8.01
14	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
15	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
16	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
17	2.00	2.00	0.00	5.00	5.00	5.00	8.00	8.00	10.00
18	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
19	2.01	2.01	2.01	5.01	5.01	5.01	8.01	8.01	8.01
20	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
21	2.00	2.00	2.00	5.00	5.00	8.00	8.00	8.00	5.00
22	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
23	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
24	0.00	2.00	0.00	7.50	0.00	0.00	10.00	10.00	10.00
25	2.20	2.10	3.20	5.10	5.10	5.10	8.10	8.05	8.02
26	1.00	1.00	1.00	5.00	5.00	5.00	10.00	10.00	10.00
27	1.50	2.00	3.00	5.00	5.00	5.00	8.00	8.00	8.00
28	2.00	2.00	5.00	6.00	5.00	5.00	9.50	8.00	5.00
29	2.25	2.25	5.25	5.25	5.25	5.25	9.00	9.99	6.00
30	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
31	0.99	0.99	0.99	5.00	5.00	5.00	10.00	9.99	9.99
32	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
33	0.01	0.01	0.01	4.84	4.84	4.84	10.00	10.00	10.00
34	1.99	1.99	4.99	4.99	4.99	4.99	7.99	7.99	7.99
35	2.01	2.01	2.01	5.01	5.01	5.01	8.01	8.01	8.01
36	1.99	1.99	1.99	4.99	4.99	4.99	7.99	7.99	7.99
37	4.60	5.50	3.25	6.10	5.25	6.50	8.80	9.00	8.25
38	2.02	2.01	2.01	5.02	5.02	5.25	8.02	8.02	9.00
39	2.00	2.00	8.00	5.00	5.00	8.00	8.00	8.00	8.00
40	1.99	1.99	1.99	4.99	4.99	4.99	8.01	8.01	8.10
41	2.01	2.01	2.01	5.01	5.01	5.01	8.01	8.01	8.01
42	5.00	4.00	5.00	6.25	6.00	6.25	9.00	9.00	8.60
43	1.99	9.99	9.99	5.01	4.99	4.99	2.00	2.00	9.50
44	2.00	2.00	2.00	5.00	4.99	4.99	10.00	10.00	10.00
45	0.01	0.01	0.01	0.01	0.01	0.01	10.00	10.00	0.01
46	2.01	1.00	1.00	5.01	6.00	6.00	8.01	8.01	9.00

47	1.99	1.99	2.80	4.99	4.99	4.99	8.01	8.01	8.01
48	0.05	0.05	0.10	5.00	3.00	9.00	9.99	9.99	9.99
49	2.02	2.02	2.00	5.02	5.02	5.00	8.02	8.00	8.00
50	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
51	2.00	2.00	2.00	5.00	5.00	5.00	6.00	6.00	6.00
52	0.01	0.01	0.01	5.00	5.00	5.00	9.99	9.99	9.99
53	5.00	5.00	5.00	5.00	5.00	5.00	5.00	5.00	5.00
54	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
55	2.01	2.01	2.01	5.01	5.01	5.01	8.01	8.01	8.01
56	0.00	0.00	5.00	4.50	4.50	4.50	10.00	10.00	6.00
57	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
58	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
59	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
60	1.25	1.00	4.50	6.50	6.00	2.50	9.50	9.80	9.00
61	4.44	4.44	4.44	4.44	4.44	4.44	6.00	7.00	5.00
62	1.84	1.64	7.45	9.52	6.50	5.55	9.98	9.01	6.25
63	2.01	2.01	2.01	5.01	5.01	5.01	8.01	8.01	8.01
64	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	6.00
65	5.00	5.00	5.00	6.00	6.50	7.00	9.00	9.78	9.00
66	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
67	1.99	1.99	1.99	5.01	5.01	5.01	8.01	8.01	8.01
68	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
69	2.50	2.01	2.01	5.01	5.01	5.01	8.01	8.01	8.01
70	2.01	2.01	2.01	5.01	5.01	5.01	8.01	8.01	8.01
71	1.50	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
72	9.50	0.15	0.15	9.99	9.50	9.50	9.99	9.99	9.75
73	6.00	0.25	1.50	3.00	3.00	7.00	9.00	9.50	9.00
74	2.00	2.00	0.00	5.00	5.00	5.00	8.00	8.00	10.00
75	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
76	0.10	1.50	3.00	5.00	5.00	5.00	8.00	8.00	5.00
77	1.99	1.00	1.00	5.00	5.00	5.00	8.00	8.50	8.00
78	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
79	2.01	2.01	2.01	5.01	5.01	5.01	8.01	8.01	8.01
80	5.82	4.44	5.55	3.33	5.55	5.25	4.25	7.77	2.22
81	5.00	1.00	1.50	5.00	8.00	3.50	9.99	9.99	5.00
82	7.43	6.23	8.00	4.70	7.20	6.50	9.00	8.00	8.50
83	5.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
84	5.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	0.00
85	2.00	2.00	5.00	5.00	5.00	5.00	8.00	8.00	8.00
86	2.00	2.00	5.00	5.00	5.00	5.00	8.00	8.00	5.00
87	5.50	4.00	6.00	9.00	6.00	6.00	5.00	4.50	9.00
88	5.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
89	2.00	2.00	1.50	5.00	5.00	4.50	8.00	8.00	8.50
90	0.00	0.00	0.00	5.00	5.00	5.00	8.01	8.01	8.01
91	4.00	2.00	2.00	5.00	4.00	5.00	9.00	8.00	6.00
92	0.01	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
93	0.85	1.50	1.00	4.07	3.41	4.20	7.47	8.19	3.64

WTA Losses:

Obs #	Private -$2	Homo - $2	Hetero -$2	Private -$5	Homo - $5	Hetero - $5	Private - $8	Homo - $8	Hetero -$8
1	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
2	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
3	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	5.00
4	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	6.50
5	9.00	0.00	0.00	3.00	2.00	0.00	9.00	9.00	1.50
6	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
7	2.01	2.01	2.01	5.01	5.01	5.01	8.01	8.01	8.01
8	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
9	2.00	0.20	0.50	5.00	4.75	5.05	9.70	9.75	9.75
10	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
11	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
12	3.00	4.00	1.50	3.50	7.00	4.00	8.00	6.00	8.00
13	4.00	4.50	5.00	5.50	5.50	6.00	8.01	8.01	8.50
14	2.00	1.00	1.50	5.50	5.00	3.50	8.50	8.50	7.00
15	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
16	2.00	2.00	8.00	5.00	5.00	8.00	8.00	8.00	8.01
17	2.00	2.00	0.00	5.00	5.00	5.00	8.00	8.00	10.00
18	2.00	2.00	2.00	5.00	5.00	0.01	8.00	8.00	9.99
19	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
20	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
21	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
22	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
23	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
24	1.00	0.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
25	2.00	2.00	5.00	5.00	5.00	5.00	8.00	8.00	8.00
26	1.00	1.00	1.00	4.83	4.83	4.83	10.00	10.00	10.00
27	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
28	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	5.00
29	0.00	0.00	4.00	4.00	4.00	4.25	9.50	9.50	6.00
30	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
31	0.99	0.99	0.99	5.00	5.00	5.00	9.99	9.99	9.99
32	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
33	0.01	0.01	0.01	0.01	0.01	0.01	10.00	10.00	10.00
34	2.01	2.01	5.01	5.01	5.01	5.01	8.01	8.01	8.01
35	2.01	2.01	2.01	5.01	5.01	5.01	8.01	8.01	8.01
36	2.01	2.01	2.01	5.01	5.01	5.01	8.01	8.01	8.01
37	2.00	2.50	1.75	4.75	5.00	5.25	8.50	8.00	9.00
38	2.01	2.01	2.05	5.01	5.01	5.50	8.01	8.01	8.25
39	2.00	2.00	2.00	5.00	5.00	2.00	8.00	8.00	2.00
40	2.01	2.01	2.01	5.00	5.00	5.00	8.01	8.01	8.01
41	2.01	2.01	2.01	5.01	5.01	5.01	8.01	8.01	8.01
42	4.00	5.00	5.00	6.00	6.00	6.00	8.75	8.50	8.50
43	2.01	9.99	9.99	0.01	0.01	9.50	0.01	0.01	3.33
44	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
45	0.01	0.01	5.00	5.00	5.00	5.00	10.00	10.00	0.01
46	2.00	2.00	2.00	5.00	5.50	5.00	7.00	9.80	1.00

47	2.01	2.01	2.01	5.01	5.01	5.01	7.99	7.99	7.99
48	0.10	0.10	0.10	5.00	5.00	5.00	9.99	9.99	9.99
49	2.00	2.00	2.00	5.02	5.02	4.22	8.02	8.02	8.02
50	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
51	2.50	2.50	4.00	5.01	5.01	4.00	7.50	7.50	4.00
52	0.01	0.01	0.01	4.99	4.99	4.99	9.99	9.99	9.99
53	2.01	2.01	8.00	5.01	5.01	8.00	8.01	8.01	8.00
54	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
55	2.01	2.01	2.01	5.01	5.01	5.01	8.01	8.01	8.01
56	0.00	0.00	4.50	5.00	5.00	4.50	10.00	10.00	10.00
57	2.01	2.01	2.01	5.01	5.01	5.01	8.01	8.01	8.01
58	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
59	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
60	4.75	6.00	4.25	4.00	4.75	4.50	3.50	7.50	3.75
61	4.44	4.44	4.44	4.44	4.00	4.44	6.00	4.00	6.00
62	6.26	6.85	2.80	8.14	8.50	4.45	9.98	9.97	4.62
63	2.01	2.01	2.01	5.01	5.01	5.01	8.01	8.01	8.01
64	2.00	2.00	5.00	5.00	5.00	5.00	8.00	8.00	6.00
65	1.00	0.75	0.50	5.00	4.50	3.00	8.50	9.00	9.00
66	3.00	3.00	5.00	5.00	5.00	5.00	8.00	8.00	8.00
67	2.01	1.99	2.01	4.99	4.99	4.99	7.99	7.99	7.99
68	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
69	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
70	2.01	2.01	2.01	5.01	5.01	5.01	8.01	8.01	8.01
71	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
72	0.50	0.50	0.50	0.50	9.99	0.50	9.99	9.99	9.99
73	0.10	0.10	8.00	5.00	5.00	5.00	9.00	9.00	1.00
74	2.00	2.00	0.00	5.00	5.00	5.00	8.00	8.00	10.00
75	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
76	0.10	0.10	3.00	5.00	5.00	5.00	8.00	8.00	5.00
77	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
78	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
79	2.01	2.01	2.01	5.01	5.01	5.01	8.01	8.01	5.01
80	2.22	2.22	2.22	1.11	1.11	1.11	3.33	3.33	3.33
81	0.01	0.01	0.15	5.00	6.00	4.00	9.99	9.99	10.00
82	3.71	5.00	4.50	4.71	4.24	3.21	4.00	5.20	6.00
83	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
84	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	9.00
85	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
86	2.00	2.00	5.00	5.00	5.00	5.00	8.00	8.00	5.00
87	2.50	2.60	3.00	5.10	5.20	8.00	8.01	8.05	8.00
88	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
89	2.00	2.00	5.00	5.00	4.90	5.00	8.00	8.00	9.00
90	7.00	7.00	2.00	5.00	6.00	5.00	8.00	8.00	7.00
91	2.10	4.71	5.10	3.20	3.02	6.02	5.70	4.60	2.80
92	0.01	0.01	0.01	0.01	0.01	0.01	3.01	3.01	3.01
93	2.22	2.67	3.13	1.48	1.19	3.23	4.37	4.20	4.82

Coding Summary

Participant #: Number in combined data set

W/in Experiment #: In each experiment session of 15, the participant number which corresponds with the source data.

Experiment #:

Age:	Minimum of 18 years old			99 = Missing
Gender	0 = Male	1 = Female		9 = Missing
Year:	1 = Freshman	2 = Soph.	3 = Junior	4 = Senior
	5 = Graduate	9 = Missing		

Major:

1. AEM
2. Biology/Biological Sciences
3. Communications
4. ILR
5. Natural Resources
6. ..
7. Molecular and Cell Biology
8. PAM
9. Africana Studies
10. ABEN/BEE
11. Neurobiology & Behavior
12. Physics
13. Biometry
14. City & Regional Planning
15. ORIE
16. Computer Science
17. Economics
18. American Studies
19. History
20. General Studies
21. Education
22. Sociology
23. CEE
24. Animal Science
25. Rural Sociology
26. Hotel
27. Government
28. TXA
29. HBHS
30. Undecided
31. Electrical Eng.
32. Biology & Society
33. Econ and Government
34. ..
35. Mechanical Eng.
36. Mathematics
37. HD and DEA
38. Hospitality
39. English
40. Theoretical & Applied Mathematics
41. Nutritional Sciences
42. Horticulture
43. Engineering and Econ
44. Public Affairs
45. Meteorology
46. SES and Oceanography
47. Chemistry
48. Psychology
49. Science and Technology Studies
50. Civil Engineering
51. Biological Engineering
52. French Area Studies
53. Finance
54. Food Science
55. Comparative Literature
56. Engineering
57. Material Science
58. Plant Science
59. CRP
99. Missing

# of Econ. Courses: 99 Missing

<u>**Appendix C: Sorted Bids and Offers by Experiment (Optimal Responses in Bold)**</u>

Sorted Bid Values - WTP Gains

Private $2	Homog. $2	Hetero. $2	Private $5	Homog. $5	Hetero. $5	Private $8	Homog. $8	Hetero. $8
0.00	0.00	0.00	0.00	0.00	0.35	2.00	2.00	0.00
0.00	0.00	0.00	1.00	1.25	1.00	3.00	3.00	2.00
0.00	0.00	0.00	1.00	2.00	2.00	4.00	4.52	2.00
0.00	0.00	0.00	1.00	2.00	2.00	4.32	5.00	3.00
0.00	0.00	0.00	1.75	2.00	2.85	5.00	6.00	3.21
0.00	0.00	0.00	2.00	4.00	3.00	5.00	6.00	4.00
0.00	0.00	0.00	2.00	4.20	3.00	6.50	7.20	5.00
0.00	0.08	0.01	3.00	4.50	3.00	7.63	7.50	5.00
0.01	0.20	0.01	3.13	4.50	3.90	**7.99**	7.77	5.00
0.20	0.25	0.34	3.90	4.89	4.00	**7.99**	**7.99**	5.00
0.25	0.25	0.50	4.00	4.90	4.50	**7.99**	**7.99**	5.00
1.00	0.50	0.50	4.19	4.90	4.50	**7.99**	**7.99**	6.00
1.00	0.67	1.00	4.40	**4.99**	4.99	**7.99**	**7.99**	6.50
1.00	1.00	1.00	4.50	**4.99**	4.99	**7.99**	**7.99**	7.00
1.00	1.00	1.00	4.80	**4.99**	4.99	**7.99**	**7.99**	7.00
1.00	1.00	1.00	**4.99**	**4.99**	4.99	**7.99**	**7.99**	7.14
1.05	1.50	1.00	**4.99**	**4.99**	4.99	**7.99**	**7.99**	7.29
1.50	1.50	1.45	**4.99**	**4.99**	4.99	**7.99**	**7.99**	7.50
1.70	1.89	1.50	**4.99**	**4.99**	4.99	**8.00**	**7.99**	7.80
1.75	1.98	1.50	**4.99**	**4.99**	4.99	**8.00**	**8.00**	7.99
1.78	1.98	1.50	**4.99**	**4.99**	4.99	**8.00**	**8.00**	7.99
1.90	**1.99**	1.99	**4.99**	**4.99**	4.99	**8.00**	**8.00**	7.99
1.91	**1.99**	1.99	**4.99**	**4.99**	5.00	**8.00**	**8.00**	7.99
1.98	**1.99**	1.99	**4.99**	**5.00**	5.00	**8.00**	**8.00**	7.99
1.99	**1.99**	1.99	**4.99**	**5.00**	5.00	**8.00**	**8.00**	7.99
1.99	**1.99**	1.99	**5.00**	**5.00**	5.00	**8.00**	**8.00**	7.99
1.99	**1.99**	1.99	**5.00**	**5.00**	5.00	**8.00**	**8.00**	7.99
1.99	**1.99**	1.99	**5.00**	**5.00**	5.00	**8.00**	**8.00**	7.99
1.99	**1.99**	1.99	**5.00**	**5.00**	5.00	**8.00**	**8.00**	8.00
1.99	**1.99**	2.00	**5.00**	**5.00**	5.00	**8.00**	**8.00**	8.00
1.99	**1.99**	2.00	**5.00**	**5.00**	5.00	**8.00**	**8.00**	8.00
1.99	**2.00**	2.00	**5.00**	**5.00**	5.00	**8.00**	**8.00**	8.00
1.99	**2.00**	2.00	**5.00**	**5.00**	5.00	**8.00**	**8.00**	8.00
1.99	**2.00**	2.00	**5.00**	**5.00**	5.00	**8.00**	**8.00**	8.00
2.00	**2.00**	2.00	**5.00**	**5.00**	5.00	**8.00**	**8.00**	8.00
2.00	**2.00**	2.00	**5.00**	**5.00**	5.00	**8.00**	**8.00**	8.00
2.00	**2.00**	2.00	**5.00**	**5.00**	5.00	**8.00**	**8.00**	8.00
2.00	**2.00**	2.00	**5.00**	**5.00**	5.00	**8.00**	**8.00**	8.00
2.00	**2.00**	2.00	**5.00**	**5.00**	5.00	**8.00**	**8.00**	8.00
2.00	**2.00**	2.00	**5.00**	**5.00**	5.00	**8.00**	**8.00**	8.00
2.00	**2.00**	2.00	**5.00**	**5.00**	5.00	**8.00**	**8.00**	8.00
2.00	**2.00**	2.00	**5.00**	**5.00**	5.00	**8.00**	**8.00**	8.00
2.00	**2.00**	2.00	**5.00**	**5.00**	5.00	**8.00**	**8.00**	8.00
2.00	**2.00**	2.00	**5.00**	**5.00**	5.00	**8.00**	**8.00**	8.00
2.00	**2.00**	2.00	**5.00**	**5.00**	5.00	**8.00**	**8.00**	8.00

2.00	**2.00**	2.00	**5.00**	**5.00**	5.00	**8.00**	**8.00**	8.00
2.00	**2.00**	2.00	**5.00**	**5.00**	5.00	**8.00**	**8.00**	8.00
2.00	**2.00**	2.00	**5.00**	**5.00**	5.00	**8.00**	**8.00**	8.00
2.00	**2.00**	2.00	**5.00**	**5.00**	5.00	**8.00**	**8.00**	8.00
2.00	**2.00**	2.00	**5.00**	**5.00**	5.00	**8.00**	**8.00**	8.00
2.00	**2.00**	2.00	**5.00**	**5.00**	5.00	**8.00**	**8.00**	8.00
2.00	**2.00**	2.00	**5.00**	**5.00**	5.00	**8.00**	**8.00**	8.00
2.00	**2.00**	2.00	**5.00**	**5.00**	5.00	**8.00**	**8.00**	8.00
2.00	**2.00**	2.00	**5.00**	**5.00**	5.00	**8.00**	**8.00**	8.00
2.00	**2.00**	2.00	**5.00**	**5.00**	5.00	**8.00**	**8.00**	8.00
2.00	**2.00**	2.00	**5.00**	**5.00**	5.00	**8.00**	**8.00**	8.00
2.00	**2.00**	2.00	**5.00**	**5.00**	5.00	**8.00**	**8.00**	8.00
2.00	**2.00**	2.00	**5.00**	**5.00**	5.00	**8.00**	**8.00**	8.00
2.00	**2.00**	2.00	**5.00**	**5.00**	5.00	**8.00**	**8.00**	8.00
2.00	**2.00**	2.00	**5.00**	**5.00**	5.00	**8.00**	**8.00**	8.00
2.00	**2.00**	2.00	**5.00**	**5.00**	5.00	**8.00**	**8.00**	8.00
2.00	**2.00**	2.00	**5.00**	**5.00**	5.00	**8.00**	**8.00**	8.00
2.00	**2.00**	2.00	**5.00**	**5.00**	5.00	**8.00**	**8.00**	8.00
2.00	**2.00**	2.00	**5.00**	**5.00**	5.00	**8.00**	**8.00**	8.00
2.00	**2.00**	2.00	**5.00**	**5.00**	5.00	**8.00**	**8.00**	8.00
2.00	**2.00**	2.00	**5.00**	**5.00**	5.00	**8.00**	**8.00**	8.00
2.00	**2.00**	2.00	**5.00**	**5.00**	5.00	**8.00**	**8.00**	8.00
2.00	**2.00**	2.00	**5.00**	**5.00**	5.00	**8.00**	**8.00**	8.00
2.00	**2.00**	2.02	**5.00**	**5.00**	5.00	**8.00**	**8.00**	8.00
2.00	**2.00**	2.31	**5.00**	**5.00**	5.00	**8.00**	**8.00**	8.00
2.00	**2.00**	2.35	**5.00**	**5.00**	5.00	8.53	**8.00**	8.00
2.00	**2.00**	2.50	**5.00**	**5.00**	5.00	8.58	**8.00**	8.00
2.00	**2.00**	3.00	**5.00**	**5.00**	5.00	8.88	8.70	8.50
2.00	**2.00**	3.00	**5.00**	**5.00**	5.00	8.90	8.77	8.63
2.00	**2.00**	3.79	**5.00**	**5.00**	5.00	8.90	8.88	8.88
2.00	**2.00**	3.90	**5.00**	**5.00**	5.45	9.00	9.00	9.00
2.00	**2.00**	4.00	**5.00**	**5.00**	5.50	9.00	9.50	9.50
2.00	**2.00**	4.00	5.25	**5.00**	6.00	9.50	9.50	9.95
2.00	**2.00**	4.00	5.62	**5.00**	6.00	9.68	9.99	9.95
2.00	**2.00**	4.00	6.00	5.37	6.00	9.95	9.99	9.99
2.00	**2.00**	5.00	6.00	5.60	6.03	9.99	9.99	9.99
2.01	2.10	5.00	6.00	6.00	6.25	9.99	9.99	9.99
2.28	2.75	5.00	6.00	6.00	6.50	9.99	9.99	9.99
2.50	3.00	5.00	7.32	6.00	7.25	9.99	10.00	9.99
3.00	3.00	5.03	7.50	6.00	7.50	9.99	10.00	9.99
3.79	3.15	6.66	8.00	6.80	7.92	10.00	10.00	9.99
4.00	3.79	8.70	8.22	7.00	8.00	10.00	10.00	9.99
5.00	5.00	9.99	8.50	7.50	8.00	10.00	10.00	10.00
6.50	6.00	10.00	8.75	7.50	8.22	10.00	10.00	10.00
6.75	6.00	10.00	9.99	8.62	9.00	10.00	10.00	10.00
7.85	6.00	10.00	10.00	8.99	10.00	10.00	10.00	10.00
9.00	6.66	10.00	10.00	10.00	10.00	10.00	10.00	10.00
10.00	10.00	10.00	10.00	10.00	10.00	10.00	10.00	10.00

Sorted Bid Values - WTP Losses

private2	homog2	heterog2	private5	homog5	heterog5	private8	homog8	heterog8
0.00	0.00	0.00	1.00	0.00	1.99	0.05	0.03	0.01
0.00	0.00	0.00	2.00	1.00	3.00	0.50	0.25	0.50
0.00	0.00	0.00	2.00	2.00	3.00	4.50	1.00	2.50
0.00	0.00	0.00	3.00	2.50	3.00	4.58	3.80	4.00
0.00	0.00	0.00	3.00	3.00	3.00	5.00	3.99	4.00
0.00	0.00	0.00	3.37	3.00	3.50	5.00	4.00	4.74
0.00	0.00	0.20	3.75	3.00	3.89	5.00	5.00	5.00
0.00	0.00	0.20	3.92	3.43	4.50	6.00	5.00	5.00
0.00	0.00	0.25	4.00	3.50	4.50	6.50	5.00	5.00
0.01	0.10	0.28	4.50	3.50	4.93	6.55	6.00	5.00
0.40	0.20	0.50	4.50	4.00	4.99	7.50	6.00	5.00
0.68	0.50	0.50	4.80	4.00	4.99	**7.99**	6.00	5.00
0.75	0.50	0.55	4.80	4.68	4.99	**7.99**	7.00	6.00
1.00	1.00	1.00	**4.99**	**4.99**	4.99	**7.99**	7.00	6.00
1.00	1.00	1.00	**4.99**	**4.99**	4.99	**7.99**	**7.99**	6.00
1.00	1.00	1.00	**4.99**	**4.99**	4.99	**7.99**	**7.99**	6.40
1.50	1.50	1.50	**4.99**	**4.99**	4.99	**7.99**	**7.99**	6.50
1.50	1.50	1.99	**4.99**	**4.99**	4.99	**7.99**	**7.99**	7.00
1.80	1.50	1.99	**4.99**	**4.99**	4.99	**7.99**	**7.99**	7.00
1.94	1.68	1.99	**4.99**	**4.99**	5.00	**7.99**	**7.99**	7.80
1.99	1.80	1.99	**4.99**	**4.99**	5.00	**7.99**	**7.99**	7.99
1.99	1.98	1.99	**4.99**	**4.99**	5.00	**8.00**	**7.99**	7.99
1.99	**1.99**	1.99	**4.99**	**4.99**	5.00	**8.00**	**7.99**	7.99
1.99	**1.99**	1.99	**4.99**	**4.99**	5.00	**8.00**	**8.00**	7.99
1.99	**1.99**	1.99	**5.00**	**5.00**	5.00	**8.00**	**8.00**	7.99
1.99	**1.99**	1.99	**5.00**	**5.00**	5.00	**8.00**	**8.00**	7.99
1.99	**1.99**	1.99	**5.00**	**5.00**	5.00	**8.00**	**8.00**	7.99
1.99	**1.99**	1.99	**5.00**	**5.00**	5.00	**8.00**	**8.00**	7.99
1.99	**1.99**	2.00	**5.00**	**5.00**	5.00	**8.00**	**8.00**	7.99
1.99	**1.99**	2.00	**5.00**	**5.00**	5.00	**8.00**	**8.00**	8.00
1.99	**1.99**	2.00	**5.00**	**5.00**	5.00	**8.00**	**8.00**	8.00
1.99	**1.99**	2.00	**5.00**	**5.00**	5.00	**8.00**	**8.00**	8.00
2.00	**2.00**	2.00	**5.00**	**5.00**	5.00	**8.00**	**8.00**	8.00
2.00	**2.00**	2.00	**5.00**	**5.00**	5.00	**8.00**	**8.00**	8.00
2.00	**2.00**	2.00	**5.00**	**5.00**	5.00	**8.00**	**8.00**	8.00
2.00	**2.00**	2.00	**5.00**	**5.00**	5.00	**8.00**	**8.00**	8.00
2.00	**2.00**	2.00	**5.00**	**5.00**	5.00	**8.00**	**8.00**	8.00
2.00	**2.00**	2.00	**5.00**	**5.00**	5.00	**8.00**	**8.00**	8.00
2.00	**2.00**	2.00	**5.00**	**5.00**	5.00	**8.00**	**8.00**	8.00
2.00	**2.00**	2.00	**5.00**	**5.00**	5.00	**8.00**	**8.00**	8.00
2.00	**2.00**	2.00	**5.00**	**5.00**	5.00	**8.00**	**8.00**	8.00
2.00	**2.00**	2.00	**5.00**	**5.00**	5.00	**8.00**	**8.00**	8.00
2.00	**2.00**	2.00	**5.00**	**5.00**	5.00	**8.00**	**8.00**	8.00
2.00	**2.00**	2.00	**5.00**	**5.00**	5.00	**8.00**	**8.00**	8.00
2.00	**2.00**	2.00	**5.00**	**5.00**	5.00	**8.00**	**8.00**	8.00
2.00	**2.00**	2.00	**5.00**	**5.00**	5.00	**8.00**	**8.00**	8.00
2.00	**2.00**	2.00	**5.00**	**5.00**	5.00	**8.00**	**8.00**	8.00
2.00	**2.00**	2.00	**5.00**	**5.00**	5.00	**8.00**	**8.00**	8.00

2.00	**2.00**	2.00	**5.00**	**5.00**	5.00	**8.00**	**8.00**	8.00
2.00	**2.00**	2.00	**5.00**	**5.00**	5.00	**8.00**	**8.00**	8.00
2.00	**2.00**	2.00	**5.00**	**5.00**	5.00	**8.00**	**8.00**	8.00
2.00	**2.00**	2.00	**5.00**	**5.00**	5.00	**8.00**	**8.00**	8.00
2.00	**2.00**	2.00	**5.00**	**5.00**	5.00	**8.00**	**8.00**	8.00
2.00	**2.00**	2.00	**5.00**	**5.00**	5.00	**8.00**	**8.00**	8.00
2.00	**2.00**	2.00	**5.00**	**5.00**	5.00	**8.00**	**8.00**	8.00
2.00	**2.00**	2.00	**5.00**	**5.00**	5.00	**8.00**	**8.00**	8.00
2.00	**2.00**	2.00	**5.00**	**5.00**	5.00	**8.00**	**8.00**	8.00
2.00	**2.00**	2.00	**5.00**	**5.00**	5.00	**8.00**	**8.00**	8.00
2.00	**2.00**	2.00	**5.00**	**5.00**	5.00	**8.00**	**8.00**	8.00
2.00	**2.00**	2.00	**5.00**	**5.00**	5.00	**8.00**	**8.00**	8.00
2.00	**2.00**	2.00	**5.00**	**5.00**	5.00	**8.00**	**8.00**	8.00
2.00	**2.00**	2.00	**5.00**	**5.00**	5.00	**8.00**	**8.00**	8.00
2.00	**2.00**	2.00	**5.00**	**5.00**	5.00	**8.00**	**8.00**	8.00
2.00	**2.00**	2.00	**5.00**	**5.00**	5.00	**8.00**	**8.00**	8.00
2.00	**2.00**	2.00	**5.00**	**5.00**	5.00	**8.00**	**8.00**	8.00
2.00	**2.00**	2.00	**5.00**	**5.00**	5.00	**8.00**	**8.00**	8.00
2.00	**2.00**	2.03	**5.00**	**5.00**	5.00	**8.00**	**8.00**	8.00
2.00	**2.00**	2.50	**5.00**	**5.00**	5.00	**8.00**	**8.00**	8.00
2.00	**2.00**	2.50	**5.00**	**5.00**	5.00	**8.00**	**8.00**	8.00
2.00	**2.00**	3.00	**5.00**	**5.00**	5.00	**8.00**	**8.00**	8.00
2.00	**2.00**	3.06	**5.00**	**5.00**	5.00	**8.00**	**8.00**	8.00
2.00	**2.00**	3.79	**5.00**	**5.00**	5.00	8.50	**8.00**	8.00
2.00	**2.00**	3.82	**5.00**	**5.00**	5.01	8.50	**8.00**	8.00
2.00	**2.00**	4.00	**5.00**	**5.00**	5.50	8.50	8.33	8.00
2.00	**2.00**	4.00	**5.00**	**5.00**	5.80	8.88	8.40	8.30
2.00	**2.00**	4.50	**5.00**	**5.00**	6.00	9.00	8.88	8.80
2.00	**2.00**	4.50	**5.00**	**5.00**	6.00	9.00	9.00	8.88
2.00	**2.00**	5.00	5.10	**5.00**	6.00	9.50	9.15	8.90
2.00	**2.00**	5.00	6.00	**5.00**	6.15	9.75	9.75	9.00
2.75	**2.00**	5.00	6.00	**5.00**	6.66	9.99	9.75	9.76
3.00	**2.00**	5.00	6.00	5.10	6.75	9.99	9.99	9.85
3.79	3.00	5.00	6.29	5.75	6.95	9.99	9.99	9.99
4.00	3.75	5.00	6.50	5.98	7.00	9.99	9.99	9.99
4.00	3.79	5.00	6.63	6.00	7.50	10.00	9.99	9.99
4.00	4.00	5.00	7.00	6.00	7.50	10.00	10.00	10.00
4.82	4.00	5.00	7.50	6.50	8.00	10.00	10.00	10.00
4.83	4.53	5.37	7.50	6.90	8.00	10.00	10.00	10.00
4.87	5.00	8.00	7.99	7.50	8.22	10.00	10.00	10.00
5.00	5.00	8.50	8.22	8.00	9.00	10.00	10.00	10.00
8.00	6.00	9.00	8.50	8.00	9.00	10.00	10.00	10.00
9.00	8.00	9.75	8.50	8.22	10.00	10.00	10.00	10.00
10.00	10.00	9.99	9.99	8.69	10.00	10.00	10.00	10.00
10.00	10.00	10.00	10.00	10.00	10.00	10.00	10.00	10.00

Sorted Offer Values - WTA Gains

private2	homog2	heterog2	private5	homog5	heterog5	private8	homog8	heterog8
0.00	0.00	0.00	0.01	0.00	0.00	2.00	2.00	0.00
0.00	0.00	0.00	3.00	0.01	0.01	4.25	4.50	0.01
0.00	0.00	0.00	3.33	3.00	2.50	5.00	5.00	1.00
0.00	0.01	0.00	4.07	3.00	3.00	5.00	6.00	2.22
0.01	0.01	0.00	4.44	3.41	3.00	6.00	6.50	3.64
0.01	0.01	0.01	4.50	4.00	3.50	6.00	7.00	5.00
0.01	0.05	0.01	4.70	4.00	4.20	6.77	7.77	5.00
0.01	0.15	0.01	4.75	4.44	4.44	7.47	7.99	5.00
0.05	0.25	0.10	4.84	4.50	4.50	7.99	7.99	5.00
0.10	0.99	0.15	4.99	4.84	4.50	7.99	**8.00**	5.00
0.85	1.00	0.99	4.99	4.99	4.84	**8.00**	**8.00**	5.00
0.99	1.00	1.00	4.99	4.99	4.99	**8.00**	**8.00**	5.00
1.00	1.00	1.00	4.99	4.99	4.99	**8.00**	**8.00**	5.00
1.25	1.00	1.00	**5.00**	4.99	4.99	**8.00**	**8.00**	6.00
1.50	1.00	1.00	**5.00**	4.99	4.99	**8.00**	**8.00**	6.00
1.50	1.50	1.00	**5.00**	4.99	4.99	**8.00**	**8.00**	6.00
1.84	1.50	1.50	**5.00**	**5.00**	4.99	**8.00**	**8.00**	6.00
1.95	1.64	1.50	**5.00**	**5.00**	5.00	**8.00**	**8.00**	6.00
1.99	1.75	1.50	**5.00**	**5.00**	5.00	**8.00**	**8.00**	6.25
1.99	1.99	1.99	**5.00**	**5.00**	5.00	**8.00**	**8.00**	7.99
1.99	1.99	1.99	**5.00**	**5.00**	5.00	**8.00**	**8.00**	7.99
1.99	1.99	1.99	**5.00**	**5.00**	5.00	**8.00**	**8.00**	8.00
1.99	1.99	2.00	**5.00**	**5.00**	5.00	**8.00**	**8.00**	8.00
1.99	1.99	2.00	**5.00**	**5.00**	5.00	**8.00**	**8.00**	8.00
1.99	**2.00**	2.00	**5.00**	**5.00**	5.00	**8.00**	**8.00**	8.00
2.00	**2.00**	2.00	**5.00**	**5.00**	5.00	**8.00**	**8.00**	8.00
2.00	**2.00**	2.00	**5.00**	**5.00**	5.00	**8.00**	**8.00**	8.00
2.00	**2.00**	2.00	**5.00**	**5.00**	5.00	**8.00**	**8.00**	8.00
2.00	**2.00**	2.00	**5.00**	**5.00**	5.00	**8.00**	**8.00**	8.00
2.00	**2.00**	2.00	**5.00**	**5.00**	5.00	**8.00**	**8.00**	8.00
2.00	**2.00**	2.00	**5.00**	**5.00**	5.00	**8.00**	**8.00**	8.00
2.00	**2.00**	2.00	**5.00**	**5.00**	5.00	**8.00**	**8.00**	8.00
2.00	**2.00**	2.00	**5.00**	**5.00**	5.00	**8.00**	**8.00**	8.00
2.00	**2.00**	2.00	**5.00**	**5.00**	5.00	**8.00**	**8.00**	8.00
2.00	**2.00**	2.00	**5.00**	**5.00**	5.00	**8.00**	**8.00**	8.00
2.00	**2.00**	2.00	**5.00**	**5.00**	5.00	**8.00**	**8.00**	8.00
2.00	**2.00**	2.00	**5.00**	**5.00**	5.00	**8.00**	**8.00**	8.00
2.00	**2.00**	2.00	**5.00**	**5.00**	5.00	**8.00**	**8.00**	8.00
2.00	**2.00**	2.00	**5.00**	**5.00**	5.00	**8.00**	**8.00**	8.00
2.00	**2.00**	2.00	**5.00**	**5.00**	5.00	**8.00**	**8.00**	8.00
2.00	**2.00**	2.00	**5.00**	**5.00**	5.00	**8.00**	**8.00**	8.00
2.00	**2.00**	2.00	**5.00**	**5.00**	5.00	**8.00**	**8.00**	8.00
2.00	**2.00**	2.00	**5.00**	**5.00**	5.00	**8.00**	**8.00**	8.00
2.00	**2.00**	2.00	**5.00**	**5.00**	5.00	**8.00**	**8.00**	8.00
2.00	**2.00**	2.00	**5.00**	**5.00**	5.00	**8.00**	**8.00**	8.00
2.00	**2.00**	2.00	**5.00**	**5.00**	5.00	**8.00**	**8.00**	8.00
2.00	**2.00**	2.00	**5.00**	**5.00**	5.00	**8.00**	**8.00**	8.00

2.00	**2.00**	2.00	**5.00**	**5.00**	5.00	**8.00**	**8.00**	8.00
2.00	**2.00**	2.00	**5.00**	**5.00**	5.00	**8.00**	**8.00**	8.00
2.00	**2.00**	2.00	**5.00**	**5.00**	5.00	**8.00**	**8.00**	8.00
2.00	**2.00**	2.00	**5.00**	**5.00**	5.00	**8.00**	**8.00**	8.00
2.00	**2.00**	2.00	**5.00**	**5.00**	5.00	**8.00**	**8.00**	8.00
2.00	**2.00**	2.00	**5.00**	**5.00**	5.00	**8.01**	**8.00**	8.00
2.00	**2.00**	2.00	**5.00**	**5.00**	5.00	**8.01**	**8.00**	8.00
2.00	**2.00**	2.00	**5.00**	**5.00**	5.00	**8.01**	**8.01**	8.00
2.00	**2.00**	2.00	**5.00**	**5.00**	5.00	**8.01**	**8.01**	8.01
2.00	**2.00**	2.00	**5.00**	**5.00**	5.00	**8.01**	**8.01**	8.01
2.00	**2.00**	2.00	**5.00**	**5.00**	5.00	**8.01**	**8.01**	8.01
2.00	**2.00**	2.00	**5.00**	**5.00**	5.00	**8.01**	**8.01**	8.01
2.00	**2.00**	2.01	**5.00**	**5.00**	5.00	**8.01**	**8.01**	8.01
2.00	**2.00**	2.01	**5.00**	**5.00**	5.00	**8.01**	**8.01**	8.01
2.00	**2.00**	2.01	**5.00**	**5.00**	5.00	**8.01**	**8.01**	8.01
2.01	**2.00**	2.01	**5.00**	**5.00**	5.00	**8.01**	**8.01**	8.01
2.01	**2.00**	2.01	**5.00**	**5.00**	5.00	**8.01**	**8.01**	8.01
2.01	**2.00**	2.01	**5.00**	**5.00**	5.00	**8.01**	**8.01**	8.01
2.01	**2.00**	2.01	**5.00**	**5.01**	5.01	**8.01**	**8.01**	8.01
2.01	**2.00**	2.01	**5.01**	**5.01**	5.01	**8.01**	**8.01**	8.01
2.01	**2.00**	2.01	**5.01**	**5.01**	5.01	**8.01**	**8.01**	8.01
2.01	**2.00**	2.01	**5.01**	**5.01**	5.01	8.02	**8.01**	8.01
2.01	**2.01**	2.01	**5.01**	**5.01**	5.01	8.02	**8.01**	8.02
2.01	**2.01**	2.01	**5.01**	**5.01**	5.01	8.10	8.02	8.10
2.01	**2.01**	2.80	**5.01**	**5.01**	5.01	8.80	8.05	8.25
2.01	**2.01**	3.00	**5.01**	**5.01**	5.01	9.00	8.19	8.50
2.02	**2.01**	3.00	**5.01**	**5.01**	5.01	9.00	8.50	8.50
2.02	**2.01**	3.20	**5.01**	**5.01**	5.01	9.00	9.00	8.60
2.20	**2.01**	3.25	**5.01**	**5.01**	5.01	9.00	9.00	9.00
2.25	**2.01**	4.44	**5.01**	**5.01**	5.01	9.00	9.01	9.00
2.50	**2.01**	4.50	**5.01**	5.02	5.10	9.00	9.50	9.00
4.00	**2.01**	4.99	**5.01**	5.02	5.25	9.00	9.50	9.00
4.44	**2.01**	5.00	**5.01**	5.10	5.25	9.50	9.78	9.00
4.60	**2.01**	5.00	5.02	5.25	5.25	9.50	9.80	9.00
5.00	2.02	5.00	5.02	5.25	5.55	9.98	9.99	9.50
5.00	2.10	5.00	5.10	5.55	6.00	9.99	9.99	9.50
5.00	2.25	5.00	5.25	6.00	6.00	9.99	9.99	9.75
5.00	4.00	5.00	6.00	6.00	6.25	9.99	9.99	9.99
5.00	4.00	5.00	6.00	6.00	6.50	9.99	9.99	9.99
5.00	4.44	5.25	6.10	6.00	6.50	10.00	9.99	9.99
5.00	4.44	5.55	6.25	6.50	7.00	10.00	10.00	10.00
5.50	5.00	6.00	6.50	6.50	7.00	10.00	10.00	10.00
5.82	5.00	7.45	7.50	7.20	8.00	10.00	10.00	10.00
6.00	5.50	8.00	9.00	8.00	8.00	10.00	10.00	10.00
7.43	6.23	8.00	9.52	9.00	9.00	10.00	10.00	10.00
9.50	9.99	9.99	9.99	9.50	9.50	10.00	10.00	10.00

Sorted Offer Values - WTA Losses

private2	homog2	heterog2	private5	homog5	heterog5	private8	homog8	heterog8
0.00	0.00	0.00	0.01	0.01	0.00	0.01	0.01	0.01
0.00	0.00	0.00	0.01	0.01	0.01	3.01	3.01	1.00
0.01	0.00	0.00	0.01	0.01	0.01	3.33	3.33	1.00
0.01	0.00	0.01	0.50	1.11	0.01	3.50	4.00	1.50
0.01	0.01	0.01	1.11	1.19	0.50	4.00	4.20	2.00
0.01	0.01	0.01	1.48	2.00	1.11	4.37	4.60	2.80
0.01	0.01	0.10	3.00	3.02	2.00	5.70	5.20	3.01
0.10	0.01	0.15	3.20	4.00	3.00	6.00	6.00	3.33
0.10	0.01	0.50	3.50	4.00	3.21	7.00	7.50	3.33
0.10	0.10	0.50	4.00	4.24	3.23	7.50	7.50	3.75
0.50	0.10	0.50	4.00	4.50	3.50	7.99	7.99	4.00
0.99	0.10	0.99	4.44	4.75	4.00	7.99	7.99	4.62
1.00	0.20	1.00	4.71	4.75	4.00	**8.00**	**8.00**	4.82
1.00	0.50	1.50	4.75	4.83	4.00	**8.00**	**8.00**	5.00
1.00	0.75	1.50	4.83	4.90	4.22	**8.00**	**8.00**	5.00
2.00	0.99	1.75	4.99	4.99	4.25	**8.00**	**8.00**	5.00
2.00	1.00	2.00	4.99	4.99	4.44	**8.00**	**8.00**	5.00
2.00	1.00	2.00	**5.00**	**5.00**	4.45	**8.00**	**8.00**	5.01
2.00	1.99	2.00	**5.00**	**5.00**	4.50	**8.00**	**8.00**	6.00
2.00	**2.00**	2.00	**5.00**	**5.00**	4.50	**8.00**	**8.00**	6.00
2.00	**2.00**	2.00	**5.00**	**5.00**	4.83	**8.00**	**8.00**	6.00
2.00	**2.00**	2.00	**5.00**	**5.00**	4.99	**8.00**	**8.00**	6.00
2.00	**2.00**	2.00	**5.00**	**5.00**	4.99	**8.00**	**8.00**	6.50
2.00	**2.00**	2.00	**5.00**	**5.00**	5.00	**8.00**	**8.00**	7.00
2.00	**2.00**	2.00	**5.00**	**5.00**	5.00	**8.00**	**8.00**	7.00
2.00	**2.00**	2.00	**5.00**	**5.00**	5.00	**8.00**	**8.00**	7.99
2.00	**2.00**	2.00	**5.00**	**5.00**	5.00	**8.00**	**8.00**	7.99
2.00	**2.00**	2.00	**5.00**	**5.00**	5.00	**8.00**	**8.00**	8.00
2.00	**2.00**	2.00	**5.00**	**5.00**	5.00	**8.00**	**8.00**	8.00
2.00	**2.00**	2.00	**5.00**	**5.00**	5.00	**8.00**	**8.00**	8.00
2.00	**2.00**	2.00	**5.00**	**5.00**	5.00	**8.00**	**8.00**	8.00
2.00	**2.00**	2.00	**5.00**	**5.00**	5.00	**8.00**	**8.00**	8.00
2.00	**2.00**	2.00	**5.00**	**5.00**	5.00	**8.00**	**8.00**	8.00
2.00	**2.00**	2.00	**5.00**	**5.00**	5.00	**8.00**	**8.00**	8.00
2.00	**2.00**	2.00	**5.00**	**5.00**	5.00	**8.00**	**8.00**	8.00
2.00	**2.00**	2.00	**5.00**	**5.00**	5.00	**8.00**	**8.00**	8.00
2.00	**2.00**	2.00	**5.00**	**5.00**	5.00	**8.00**	**8.00**	8.00
2.00	**2.00**	2.00	**5.00**	**5.00**	5.00	**8.00**	**8.00**	8.00
2.00	**2.00**	2.00	**5.00**	**5.00**	5.00	**8.00**	**8.00**	8.00
2.00	**2.00**	2.00	**5.00**	**5.00**	5.00	**8.00**	**8.00**	8.00
2.00	**2.00**	2.00	**5.00**	**5.00**	5.00	**8.00**	**8.00**	8.00
2.00	**2.00**	2.00	**5.00**	**5.00**	5.00	**8.00**	**8.00**	8.00
2.00	**2.00**	2.00	**5.00**	**5.00**	5.00	**8.00**	**8.00**	8.00
2.00	**2.00**	2.00	**5.00**	**5.00**	5.00	**8.00**	**8.00**	8.00
2.00	**2.00**	2.00	**5.00**	**5.00**	5.00	**8.00**	**8.00**	8.00
2.00	**2.00**	2.00	**5.00**	**5.00**	5.00	**8.00**	**8.00**	8.00
2.00	**2.00**	2.00	**5.00**	**5.00**	5.00	**8.00**	**8.00**	8.00
2.00	**2.00**	2.00	**5.00**	**5.00**	5.00	**8.00**	**8.00**	8.00

2.00	**2.00**	2.00	**5.00**	**5.00**	5.00	**8.00**	**8.00**	8.00
2.00	**2.00**	2.00	**5.00**	**5.00**	5.00	**8.00**	**8.00**	8.00
2.00	**2.00**	2.00	**5.00**	**5.00**	5.00	**8.00**	**8.00**	8.00
2.00	**2.00**	2.00	**5.00**	**5.00**	5.00	**8.00**	**8.00**	8.00
2.00	**2.00**	2.00	**5.00**	**5.00**	5.00	**8.00**	**8.00**	8.00
2.00	**2.00**	2.00	**5.00**	**5.00**	5.00	**8.00**	**8.00**	8.00
2.00	**2.00**	2.00	**5.00**	**5.00**	5.00	**8.00**	**8.00**	8.00
2.00	**2.00**	2.01	**5.00**	**5.00**	5.00	**8.00**	**8.00**	8.00
2.00	**2.00**	2.01	**5.00**	**5.00**	5.00	**8.00**	**8.00**	8.00
2.00	**2.00**	2.01	**5.00**	**5.00**	5.00	**8.00**	**8.00**	8.00
2.00	**2.00**	2.01	**5.00**	**5.00**	5.00	**8.00**	**8.00**	8.00
2.00	**2.00**	2.01	**5.00**	**5.00**	5.00	**8.01**	**8.01**	8.00
2.00	**2.00**	2.01	**5.00**	**5.00**	5.00	**8.01**	**8.01**	8.00
2.00	**2.00**	2.01	**5.00**	**5.00**	5.00	**8.01**	**8.01**	8.00
2.01	**2.00**	2.01	**5.00**	**5.00**	5.00	**8.01**	**8.01**	8.01
2.01	**2.01**	2.01	**5.00**	**5.00**	5.00	**8.01**	**8.01**	8.01
2.01	**2.01**	2.01	**5.00**	**5.00**	5.00	**8.01**	**8.01**	8.01
2.01	**2.01**	2.01	**5.00**	**5.00**	5.00	**8.01**	**8.01**	8.01
2.01	**2.01**	2.01	**5.00**	**5.00**	5.00	**8.01**	**8.01**	8.01
2.01	**2.01**	2.05	**5.00**	**5.00**	5.00	**8.01**	**8.01**	8.01
2.01	**2.01**	2.22	**5.00**	**5.00**	5.00	**8.01**	**8.01**	8.01
2.01	**2.01**	2.80	**5.00**	**5.01**	5.00	**8.01**	**8.01**	8.01
2.01	**2.01**	3.00	**5.00**	**5.01**	5.00	**8.01**	**8.01**	8.01
2.01	**2.01**	3.00	**5.00**	**5.01**	5.00	**8.01**	**8.01**	8.01
2.01	**2.01**	3.13	**5.00**	**5.01**	5.01	**8.01**	**8.01**	8.01
2.01	**2.01**	4.00	**5.01**	**5.01**	5.01	**8.01**	8.02	8.02
2.01	**2.01**	4.00	**5.01**	**5.01**	5.01	8.02	8.05	8.25
2.01	**2.01**	4.25	**5.01**	**5.01**	5.01	8.50	8.50	8.50
2.01	**2.01**	4.44	**5.01**	**5.01**	5.01	8.50	8.50	8.50
2.01	2.22	4.50	**5.01**	**5.01**	5.01	8.50	9.00	9.00
2.10	2.50	4.50	**5.01**	**5.01**	5.01	8.75	9.00	9.00
2.22	2.50	5.00	**5.01**	**5.01**	5.01	9.00	9.00	9.00
2.22	2.60	5.00	**5.01**	**5.01**	5.01	9.00	9.50	9.00
2.50	2.67	5.00	**5.01**	**5.01**	5.01	9.50	9.75	9.75
2.50	3.00	5.00	**5.01**	**5.01**	5.01	9.70	9.80	9.99
3.00	4.00	5.00	**5.01**	5.02	5.05	9.98	9.97	9.99
3.00	4.44	5.00	**5.01**	5.20	5.25	9.99	9.99	9.99
3.71	4.50	5.00	**5.01**	5.50	5.50	9.99	9.99	9.99
4.00	4.71	5.00	**5.01**	5.50	6.00	9.99	9.99	9.99
4.00	5.00	5.01	5.02	6.00	6.00	9.99	9.99	10.00
4.44	5.00	5.10	5.10	6.00	6.02	9.99	9.99	10.00
4.75	6.00	8.00	5.50	6.00	8.00	10.00	10.00	10.00
6.26	6.85	8.00	5.50	7.00	8.00	10.00	10.00	10.00
7.00	7.00	8.00	6.00	8.50	8.00	10.00	10.00	10.00
9.00	9.99	9.99	8.14	9.99	9.50	10.00	10.00	10.00

Appendix C: Econometric Output (LIMDEP) by Experiment (See Eq. 10 in text)

WTP Gains - Pooled Model (n=93)

```
+-----------------------------------------------------------------------+
| Linearly restricted regression                                        |
| Ordinary    least squares regression    Weighting variable = none     |
| Dep. var. = ACT_IND  Mean=   .1362962963    , S.D.=   1.671220838     |
| Model size: Observations =     837, Parameters = 101, Deg.Fr.=    736 |
| Residuals:  Sum of squares= 1356.973161    , Std.Dev.=        1.35783 |
| Fit:        R-squared=  .418838, Adjusted R-squared =          .33988 |
|             (Note:  Not using OLS.  R-squared is not bounded in [0,1] |
| Model test: F[100,    736] =    5.30,    Prob value =          .00000 |
| Diagnostic: Log-L =  -1389.8657, Restricted(b=0) Log-L =   -1616.9963 |
|             LogAmemiyaPrCrt.=     .726, Akaike Info. Crt.=      3.562 |
| Note, when restrictions are imposed, R-squared can be less than zero. |
| F[ 2,   734] for the restrictions =       .0000, Prob =  1.0000       |
| Autocorrel: Durbin-Watson Statistic =   1.73390,   Rho =       .13305 |
+-----------------------------------------------------------------------+
+----------------+--------------+----------------+--------+---------+----------+
|Variable        | Coefficient  | Standard Error |b/St.Er.|P[|Z|>z] | Mean of X|
+----------------+--------------+----------------+--------+---------+----------+
 Individual (93) Fixed Effect Dummy Variables Suppressed
 (WTPGPRIV2 - $2)  -.3296296296E-01      .13274831    -.248   .8039     .11111111
 (WTPGHOMO2 - $2)  -.8091995221E-01      .13274831    -.610   .5421     .11111111
 (WTPGHET2 - $2)       .5032735962       .13274831    3.791   .0001     .11111111
 (WTPGPRIV5 - $5)  -.4253285544E-01      .13274831    -.320   .7487     .11111111
 (WTPGHOMO5 - $5)  -.2436081243E-01      .13274831    -.184   .8544     .11111111
 (WTPGHET5 - $5)    .5316606930E-01      .13274831     .401   .6888     .11111111
 (WTPGPRIV8 - $8)  -.2640382318E-01      .13274831    -.199   .8423     .11111111
 (WTPGHOMO8 - $8)   .5531660693E-02      .13274831     .042   .9668     .11111111
 (WTPGHET8 - $8)      -.3547909200       .13274831   -2.673   .0075     .11111111
 Constant            .1362962963    .46933614E-01    2.904   .0037
 (Note: E+nn or E-nn means multiply by 10 to + or -nn power.)
```

Derived coefficients for Table 1.

```
+--------------------------+--------------+----------------+--------+---------+
|Variable                  | Coefficient  | Standard Error |b/St.Er.|P[|Z|>z] |
+--------------------------+--------------+----------------+--------+---------+
 Const. + (WTPGPRIV2 - $2)     .1033333333       .14080084     .734   .4630
 Const. + (WTPGHOMO2 - $2)  .5537634409E-01      .14080084     .393   .6941
 Const. + (WTPGHET2 - $2)      .6395698925       .14080084    4.542   .0000
 Const. + (WTPGPRIV5 - $5)  .9376344086E-01      .14080084     .666   .5055
 Const. + (WTPGHOMO5 - $5)     .1119354839       .14080084     .795   .4266
 Const. + (WTPGHET5 - $5)      .1894623656       .14080084    1.346   .1784
 Const. + (WTPGPRIV8 - $8)     .1098924731       .14080084     .780   .4351
 Const. + (WTPGHOMO8 - $8)     .1418279570       .14080084    1.007   .3138
 Const. + (WTPGHET8 - $8)     -.2184946237       .14080084   -1.552   .1207
 (Note: E+nn or E-nn means multiply by 10 to + or -nn power.)
```

WTP Losses - Pooled Model (n=93)

```
+-----------------------------------------------------------------------+
| Linearly restricted regression                                        |
| Ordinary    least squares regression    Weighting variable = none     |
| Dep. var. = ACT_IND  Mean=   .1186021505    , S.D.=   1.719323704     |
| Model size: Observations =     837, Parameters = 101, Deg.Fr.=    736 |
| Residuals:  Sum of squares= 1706.961663    , Std.Dev.=        1.52291 |
| Fit:        R-squared=  .309280, Adjusted R-squared =          .21543 |
|             (Note:  Not using OLS.  R-squared is not bounded in [0,1] |
| Model test: F[100,    736] =    3.30,    Prob value =          .00000 |
| Diagnostic: Log-L =  -1485.8940, Restricted(b=0) Log-L =   -1640.7475 |
|             LogAmemiyaPrCrt.=     .955, Akaike Info. Crt.=      3.792 |
| Note, when restrictions are imposed, R-squared can be less than zero. |
| F[ 2,   734] for the restrictions =       .0000, Prob =  1.0000       |
| Autocorrel: Durbin-Watson Statistic =   1.36256,   Rho =       .31872 |
+-----------------------------------------------------------------------+
+----------------+--------------+----------------+--------+---------+----------+
```

```
|Variable        | Coefficient  | Standard Error |b/St.Er.|P[|Z|>z] | Mean of X|
+----------------+--------------+----------------+--------+---------+----------+
 Individual (93) Fixed Effect Dummy Variables Suppressed
 (WTPGPRIV2 - $2)     .1127956989       .14888647     .758   .4487     .11111111
 (WTPGHOMO2 - $2)  .2365591398E-01      .14888647     .159   .8738     .11111111
 (WTPGHET2 - $2)      .5500000000       .14888647    3.694   .0002     .11111111
 (WTPGPRIV5 - $5)  .6688172043E-01      .14888647     .449   .6533     .11111111
 (WTPGHOMO5 - $5)    -.1170967742       .14888647    -.786   .4316     .11111111
 (WTPGHET5 - $5)      .2605376344       .14888647    1.750   .0801     .11111111
 (WTPGPRIV8 - $8)    -.1329032258       .14888647    -.893   .3720     .11111111
 (WTPGHOMO8 - $8)    -.3207526882       .14888647   -2.154   .0312     .11111111
 (WTPGHET5 - $5)     -.4431182796       .14888647   -2.976   .0029     .11111111
 Constant             .1186021505   .52639317E-01    2.253   .0243
 (Note: E+nn or E-nn means multiply by 10 to + or -nn power.)
```

Derived coefficients for Table 2.

```
+--------------------------+--------------+----------------+--------+---------+
|Variable                  | Coefficient  | Standard Error |b/St.Er.|P[|Z|>z] |
+--------------------------+--------------+----------------+--------+---------+
 Const. + (WTPGPRIV2 - $2)     .2313978495       .15791795    1.465   .1428
 Const. + (WTPGHOMO2 - $2)     .1422580645       .15791795     .901   .3677
 Const. + (WTPGHET2 - $2)      .6686021505       .15791795    4.234   .0000
 Const. + (WTPGPRIV2 - $2)     .1854838710       .15791795    1.175   .2402
 Const. + (WTPGHOMO2 - $2) .1505376344E-02       .15791795     .010   .9924
 Const. + (WTPGHET2 - $2)      .3791397849       .15791795    2.401   .0164
 Const. + (WTPGPRIV2 - $2)-.1430107527E-01       .15791795    -.091   .9278
 Const. + (WTPGHOMO2 - $2)    -.2021505376       .15791795   -1.280   .2005
 Const. + (WTPGHET2 - $2)     -.3245161290       .15791795   -2.055   .0399
 (Note: E+nn or E-nn means multiply by 10 to + or -nn power.)
```

WTA Gains - Pooled Model (n=93)

```
+-----------------------------------------------------------------------+
| Linearly restricted regression                                        |
| Ordinary    least squares regression    Weighting variable = none     |
| Dep. var. = ACT_IND  Mean=   .6905615293E-01, S.D.=   1.403042641     |
| Model size: Observations =     837, Parameters = 101, Deg.Fr.=    736 |
| Residuals:  Sum of squares= 1301.841669    , Std.Dev.=        1.32996 |
| Fit:        R-squared=  .208939, Adjusted R-squared =          .10146 |
|             (Note:  Not using OLS.  R-squared is not bounded in [0,1] |
| Model test: F[100,    736] =    1.94,    Prob value =          .00000 |
| Diagnostic: Log-L =  -1372.5077, Restricted(b=0) Log-L =   -1470.5956 |
|             LogAmemiyaPrCrt.=     .684, Akaike Info. Crt.=      3.521 |
| Note, when restrictions are imposed, R-squared can be less than zero. |
| F[ 2,   734] for the restrictions =       .0000, Prob =  1.0000       |
| Autocorrel: Durbin-Watson Statistic =   1.50787,   Rho =       .24607 |
+-----------------------------------------------------------------------+
+----------------+--------------+----------------+--------+---------+----------+
|Variable        | Coefficient  | Standard Error |b/St.Er.|P[|Z|>z] | Mean of X|
+----------------+--------------+----------------+--------+---------+----------+
 Individual (93) Fixed Effect Dummy Variables Suppressed
(WTAGPRIV2 - $2)    -.1105615293       .13002368    -.850   .3951     .11111111
(WTAGHOMO2 - $2) -.5615292712E-02      .13002368    -.043   .9656     .11111111
(WTAGHET2 - $2)      .3963201912       .13002368    3.048   .0023     .11111111
(WTAGPRIV5 - $5)  .4911589008E-01      .13002368     .378   .7056     .11111111
(WTAGHOMO5 - $5) -.3518518519E-01      .13002368    -.271   .7867     .11111111
(WTAGHET5 - $5)  -.7765830346E-02      .13002368    -.060   .9524     .11111111
(WTAGPRIV8 - $8)  .7621266428E-01      .13002368     .586   .5578     .11111111
(WTAGHOMO8 - $8)     .1158900836       .13002368     .891   .3728     .11111111
(WTAGHET8 - $8)     -.4784109916       .13002368   -3.679   .0002     .11111111
 Constant         .6905615293E-01  .45970312E-01    1.502   .1330
 (Note: E+nn or E-nn means multiply by 10 to + or -nn power.)
```

Derived coefficients for Table 3.

Variable	Coefficient	Standard Error	b/St.Er.	P[|Z|>z]
Const. + (WTAGPRIV2 - $2)	-.4150537634E-01	.13791093	-.301	.7634
Const. + (WTAGHOMO2 - $2)	.6344086022E-01	.13791093	.460	.6455
Const. + (WTAGHET2 - $2)	.4653763441	.13791093	3.374	.0007
Const. + (WTAGPRIV5 - $5)	.1181720430	.13791093	.857	.3915
Const. + (WTAGHOMO5 - $5)	.3387096774E-01	.13791093	.246	.8060
Const. + (WTAGHET5 - $5)	.6129032258E-01	.13791093	.444	.6567
Const. + (WTAGPRIV8 - $8)	.1452688172	.13791093	1.053	.2922
Const. + (WTAGHOMO8 - $8)	.1849462366	.13791093	1.341	.1799
Const. + (WTAGHET8 - $8)	-.4093548387	.13791093	-2.968	.0030

(Note: E+nn or E-nn means multiply by 10 to + or -nn power.)

WTA Losses - Pooled Model (n=93)

```
+----------------------------------------------------------------------+
| Linearly restricted regression                                       |
| Ordinary    least squares regression    Weighting variable = none    |
| Dep. var. = ACT_IND  Mean=  -.1055077658    , S.D.=   1.617985009    |
| Model size: Observations =     837, Parameters = 101, Deg.Fr.=    736 |
| Residuals:  Sum of squares= 1666.230768    , Std.Dev.=        1.50463 |
| Fit:        R-squared=  .238658, Adjusted R-squared =          .13521 |
|           (Note:  Not using OLS.  R-squared is not bounded in [0,1] |
| Model test: F[100,    736] =    2.31,    Prob value =          .00000 |
| Diagnostic: Log-L =  -1475.7868, Restricted(b=0) Log-L =   -1589.9002 |
|           LogAmemiyaPrCrt.=     .931, Akaike Info. Crt.=      3.768 |
| Note, when restrictions are imposed, R-squared can be less than zero. |
| F[ 2,   734] for the restrictions =       .0000, Prob =  1.0000       |
| Autocorrel: Durbin-Watson Statistic =   1.70224,   Rho =       .14888 |
+----------------------------------------------------------------------+
```

Variable	Coefficient	Standard Error	b/St.Er.	P[|Z|>z]	Mean of X
Individual (93) Fixed Effect Dummy Variables Suppressed					
(WTALPRIV2 - $2)	.1667980884	.14709941	1.134	.2568	.11111111
(WTALHOMO2 - $2)	.2119593787	.14709941	1.441	.1496	.11111111
(WTALHET2 - $2)	.6499163680	.14709941	4.418	.0000	.11111111
(WTALPRIV5 - $5)	-.2178255675	.14709941	-1.481	.1387	.11111111
(WTALHOMO5 - $5)	-.7567502987E-01	.14709941	-.514	.6069	.11111111
(WTALHET5 - $5)	-.1936320191	.14709941	-1.316	.1881	.11111111
(WTALPRIV8 - $8)	.1894862605E-01	.14709941	.129	.8975	.11111111
(WTALHOMO8 - $8)	.4647550777E-01	.14709941	.316	.7520	.11111111
(WTALHET5 - $5)	-.6069653524	.14709941	-4.126	.0000	.11111111
Constant	-.1055077658	.52007495E-01	-2.029	.0425	

(Note: E+nn or E-nn means multiply by 10 to + or -nn power.)

Derived coefficients for Table 3.

Variable	Coefficient	Standard Error	b/St.Er.	P[|Z|>z]
Const. + (WTALPRIV2 - $2)	.6129032258E-01	.15602249	.393	.6944
Const. + (WTALHOMO2 - $2)	.1064516129	.15602249	.682	.4951
Const. + (WTAGHET2 - $2)	.5444086022	.15602249	3.489	.0005
Const. + (WTALPRIV5 - $5)	-.3233333333	.15602249	-2.072	.0382
Const. + (WTALHOMO5 - $5)	-.1811827957	.15602249	-1.161	.2455
Const. + (WTALHET5 - $5)	-.2991397849	.15602249	-1.917	.0552
Const. + (WTALPRIV8 - $8)	-.8655913978E-01	.15602249	-.555	.5790
Const. + (WTALHOMO8 - $8)	-.5903225806E-01	.15602249	-.378	.7052
Const. + (WTAGHET8 - $8)	-.7124731183	.15602249	-4.566	.0000

(Note: E+nn or E-nn means multiply by 10 to + or -nn power.)

THE VALUE OF PRIVATE RISK VERSUS THE VALUE OF PUBLIC RISK:

AN EXPERIMENTAL ANALYSIS

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Abstract: In 1996 Johannesson *et al.* published a paper in the *Journal of Risk and Uncertainty* entitled "The Value of Private Safety Versus the Value of Public Safety." Using hypothetical questions about voting for improvements in public safety or purchases of private safety, they reject the notion of paternalistic preferences for safety and argue, based on preliminary evidence, that consumers are "pure altruists." These pure altruists consider the cost of a program that might be imposed on other voters in their decision on whether to vote yes or no for public safety programs. They conclude that further empirical research is warranted. This paper presents a set of laboratory economics experiments designed to test the Johannesson *et al.* conjecture under controlled conditions where participants face an actual risk of financial loss rather than a hypothetical risk of loss of life. The results presented here support those of Johannesson *et al*. in that we do not find evidence in support of paternalistic altruism for risk but do find evidence for pure altruism. This work develops an experimental methodology for identifying the type of altruistic risk preferences held by participants that can contribute to the debate over correct methods for evaluating the benefits of risk reduction programs.

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THE VALUE OF PRIVATE RISK VERSUS THE VALUE OF PUBLIC RISK:

AN EXPERIMENTAL ANALYSIS

1. Introduction

In 1996 Johannesson *et al.* published a paper in the *Journal of Risk and Uncertainty* entitled "The Value of Private Safety Versus the Value of Public Safety." Using hypothetical survey questions about voting for improvements in public safety or purchases of private safety, they do not find support for the notion of paternalistic preferences for safety. Paternalistic preferences imply that voters will bias their voting in favor of public safety programs compared to their own selfish interests. Rather they argue, based on preliminary evidence, that consumers are "pure altruists." These pure altruists consider the costs of a program that might be imposed on other voters in their decision on whether to vote yes or no for public safety programs. Thus, they find that voters take into account the distribution of net benefits to others in their votes, but further note that further empirical research is warranted. This paper presents a set of laboratory economics experiments designed to test the Johannesson *et al.* conjecture under controlled conditions where participants face an actual risk of financial loss rather than a hypothetical risk of loss of life.

It is important to distinguish the types of altruism that might be present because benefit-cost analysis should not generally (with some exceptions) incorporate pure altruism in benefit measures (Bergstrom, 1982, 2004; Johansson, 1992,; Milgrom, 1993; Flores, 2002,[1] but should generally incorporate paternalistic altruism from a welfare theoretic perspective (Jones Lee,

[1] Bergstrom (1982) provides a neoclassical economic rationale for not accounting for altruism in Kaldor-Hicks welfare tests, leading Johannesson (1994) to argue that for the appropriate aggregate valuation of public goods, individual, and therefore aggregate WTP should be conditional on everybody else paying so as to remain at their initial level of utility. However, public projects are rarely, if ever, financed under such conditions: most typically the funding for specific public projects imposes coercive costs that result in utility gains and losses. Moreover, projects evaluated tend to be discrete, and the initial allocation of public goods is inefficient. Under these conditions Flores (2002) argues that the extrapolation of Bergstrom's results for marginal changes at the optimum do not carry over to the "more modest problem [of benefit-cost analysis], determining whether a specific project can lead to a Pareto improvement". Under these more realistic conditions, Flores further argues that pure altruism should be accounted for in applied welfare analyses. Bergstrom disagrees and his analysis is presented in Bergstrom, 2004.

1991, 1992, Quiggin, 1998). Note that paternalistic altruism is present when one individual cares only about the level of consumption of a particular commodity by another person, but not about the other person's utility or overall well-being. Quiggin, 1998, sums up the situation for public goods as follows: "If individuals display paternalistic altruism towards each other, and are concerned only with consumption of the public good, household WTP will be equal to the sum of household members' individual WTP. If individuals have well-defined personal utility functions but display non-paternalistic altruism towards each other, it is possible to define a measure of private WTP which will be less than the [sum of] individual WTP." (p 58)

Most field research on voter behavior with respect to risk has been directed toward situations in which altruism is expected to *increase* the proportion of affirmative votes above what would be anticipated for self-interested preferences, consistent with paternalistic altruism as well as pure altruism. This reflects the prevalent view frequently articulated in the value of statistical life literature that accounting for concern about the well-being of others would increase the aggregated benefits in public benefit-cost analyses (e.g., Jones-Lee, 1991, 1992). Empirical testing of this hypothesis with respect to risk improvements has been inconclusive, however, as some studies of ballot initiatives find voting patterns consistent with self-interest, narrowly defined (e.g., Deacon and Shapiro, 1975) while other research provides evidence of "public regardingness" in voting behavior (e.g. Holmes, 1990, Shabman and Stephenson, 1994). To some extent, these mixed empirical findings might be anticipated by inherent incompatibilities of using aggregate voting data as a measure of underlying, but not directly observable, preferences. Thus, by necessity, aggregate modeling approaches rely on indirect proxies for costs (e.g. income, Deacon and Shapiro), expected risk reduction (e.g. location, Shabman and Stephenson), and/or altruistic concerns (e.g. political affiliation, Holmes).

Yet, as conjectured by Johannesson *et al*. (1996), the coercive nature of voting and taxation also raises another possibility that some people who are pure altruists will vote “no” on a project that would provide them private net benefits for risk reduction, narrowly defined, because they desire not to impose costs on others for whom costs exceed the benefits.

> Let us assume that [an individual] is willing to pay $t for a ceteris paribus increase in his own safety. His total WTP for a uniform public risk reduction of the same magnitude will fall short of $t if he believes that others are willing to pay less than $t but will still be forced to pay that amount ($t) for the project. This is because other individuals, for whom he cares will experience a lower utility if the program is implemented. In turn, this decrease in the utility of others reduces the pure altruist’s WTP for the public safety project. (p. 264)

In other words, purely altruistic behavior may in some instances *lower* the proportion of affirmative votes relative to a self-interested model.

This paper examines two hypotheses: 1) That voters are paternalistic with respect to losses in the sense that they tend to vote “yes” for projects that provide loss reductions to others, even if the individual voter incurs a financial loss; and 2) The Johannesson conjecture that there will be a tendency to vote “no” on projects that impose excessive costs on others and to vote “yes” for projects that benefits others while imposing financial losses on the individual. To do this we employ the Random-Price Voting Mechanism (RPVM) introduced in Messer (2003) and Messer *et al.* (2005). This mechanism extends the private good Becker-DeGroot-Marshack (BDM) mechanism (1964) to a public good setting wherein subjects indicate the highest uniform tax they would pay for insurance before voting “no”. The tax is then drawn randomly. If a majority of participants indicate that they would vote “yes” at this random tax level or higher then the insurance policy is purchased, the risk is alleviated, and taxes are collected from all members in the group. If a majority of affirmative votes is not attained, then the policy is not implemented and no tax is collected. Because of the voting context, this mechanism is theoretically incentive compatible. Using induced value experiments, Messer *et al.* (2005) further demonstrate that the mechanism is demand revealing in public settings, and that the continuous willingness to pay (WTP) values correspond to those obtained from analogous dichotomous choice referenda.

In the experiments described here, subjects provide the highest uniform price or tax that they would pay for an insurance policy that protects against a probabilistic loss in both a private and public good setting. In addition, expected losses are varied across players in such a way as to allow tests of pure or paternalistic altruism. For completeness, we utilize two experimental designs. The first varies the probability of a loss, while the second varies the loss. It should be noted that considerable support has been found for pure altruism in the recent experimental literature (Charness and Rabin, 2002, Engelmann and Strobel, 2004, Messer *et al*, 2005). However, because these studies use induced values under certainty, they cannot test for paternalistic altruism that requires use of a commodity. In this study, either private or publicly provided insurance against risk of loss are the commodities of interest.

The paper is organized as follows: Section 2 presents the conceptual foundations and demonstrates how voting behavior can be used to test for the presence of paternalistic versus non-paternalistic altruism; Section 3 presents the experimental design; Section 4 provides results and statistical tests; and Section 5 provides a summary and discussion of the results.

2. Conceptual Framework

Messer *et al.* (2005) build upon the "social welfare" preferences model of Charness and Rabin (2002) to derive symmetric Bayesian Nash equilibria expectations appropriate for the RPVM framework. The conceptual foundations of this paper are similar but induced values are replaced by public or private insurance against loss, consistent with the framework introduced in Johannesson *et al.* (1996).

Preferences: Since these experiments are conducted in the laboratory with small stakes, and since there is no generally accepted theoretical model of risk aversion in the small, it is initially assumed that subjects are not risk averse. An approximate method for dealing with risk aversion is introduced later. As Rabin (2000) has pointed out, the expected utility model cannot explain the presence of risk aversion in the small, since observable levels of risk aversion in the small and large are theoretically inconsistent. It should be noted that the purpose of this study is to examine the nature of altruistic preferences for risk, not to explore risk aversion in the small. However, the potential for risk aversion in the small must be accounted for to avoid a potential confound. Thus, to begin, individual selfish utility is specified as expected payoff or income minus private expected loss,

1) $$U^i = y^i - \rho_{i,s} L_{i,s} \qquad i = 1, 2, 3, \ldots\ldots, n,$$

where y^i denotes the income of participant *i*, and $\rho_{i,s}$ denotes the probability of dollar loss, $L_{i,s}$, in treatment *s* for participant *i* in a group of n individuals.

If, in addition, a participant cares about the private utility of other participants, then purely altruistic utility, consistent both with Johannesson *et al*. (1996) and with the efficiency motive of Charness and Rabin (2002), can be specified as

2) $$A^i = U^i + \alpha \sum_{j \neq i} U^j ,$$

where α denotes the relative weight placed on the sum of others' utilities each denoted *j*. This is a particular form of pure altruism as defined by Johannesson, *et al.*, 1996.

Alternatively, in the case of paternalistic preferences for others' exposure to losses, paternalistic utility can be written as

3) $$P^i = U^i - \beta \sum_{j \neq i} \rho_{j,s} L_{j,s} ,$$

where participant *i* loses utility at rate β in the sum of others' expected losses. Thus, paternalistic preferences imply that individuals care not about others' utility but only about the losses they face. For example, paternalists might condemn smoking as bad behavior and show interest in helping others quit, but they may not be interested in other aspects of smokers' well-being.

Thus, there are three models of behavior that can be analyzed in the public good voting experiments on risk presented here. First, individuals may be self-interested or selfish and not consider the impact of their vote on others, consistent with (1). Second, individuals may show purely altruistic preferences as shown in (2), and consider the utilities of others. Or, as shown in (3), individuals may have paternalistic preferences and be interested in the risks that others face, but not their overall well being or utility.

Selfish Preferences: In the experiments to follow, participants are given the opportunity to decide whether or not to purchase private insurance for price *c* to eliminate a known risk of losing money. The risk neutral individual will purchase insurance if the individual utility of accepting the risk is less that or equal to the individual utility of paying *c* to eliminate the risk,

4) $$y^i - \rho_{i,s} L_{i,s} \leq y^i - c \ .$$

The maximum price that participant *i* will pay to eliminate risk when equality holds in (4) is equal to expected value,

5) $$c_{i,s}^{\max} = \rho_{i,s} L_{i,s} .$$

Under the assumption of risk neutrality, both selfish and altruistic individuals will behave this way. However, if an individual is truly selfish and does not care about the utilities or risks facing others, then the individual will vote for a public program to eliminate risk to his or her voting group if the tax cost to each, t, is such that

6) $y^i - \rho_{i,s} L_{i,s} \leq y^i - t$.

In this case, the maximum tax that individual *i* would pay to eliminate risk for the group is the same as the private value and remains equal to expected value,

7) $t_{i,s}^{\max} = \rho_{i,s} L_{i,s}$, so $t_{i,s}^{\max} - c_{i,s}^{\max} = 0$.

Purely Altruistic Preferences: In the case of pure altruism, individual *i* will consider the utilities of other individuals $j \neq i$ in a vote as to whether or not to fund a public risk reduction which eliminates risk to all but imposes a tax cost t on each voter. Thus, individual *i* will vote yes if

8) $$y^i - \rho_{i,s} L_{i,s} + \alpha \sum_{j \neq i} \left(y^j - \rho_{j,s} L_{j,s} \right) \leq y^i - t + \alpha \sum_{j \neq i} \left(y^j - t \right)$$

and the maximum tax that *i* will pay is

9) $$t_{i,s}^{\max} = \rho_{i,s} L_{i,s} + \frac{\alpha}{(n-1)\alpha + 1} Z_{i,s}, \text{ where}$$

10) $$Z_{i,s} = (n-1)\left(\sum_{j \neq i} \rho_{j,s} L_{j,s} / (n-1) - \rho_{i,s} L_{i,s}\right)$$

and there are *n* voters in a voting group. Note that the difference between the maximum uniform tax for reducing risks to all and own private value that *i* will pay to reduce risk to herself is determined by $Z_{i,s}$, which compares own risk to the average of others risks:

11) $$t_{i,s}^{\max} - \rho_{i,s} L_{i,s} \begin{cases} > 0 & for\ Z_{i,s} > 0 \\ = 0 & for\ Z_{i,s} = 0 \\ < 0 & for\ Z_{i,s} < 0 \end{cases}.$$

Thus, from (10) and (11), if her risk is greater than the average of others' risk (as measured by expected value), she will pay less than her private value and if her risk is less than the average of other's risk she will pay more than her private value.

Paternalistic Preferences: An individual with paternalistic preferences will vote in favor of a public risk elimination program if

12) $$y^i - \rho_{i,s}L_{i,s} - \beta X_{i,s} \;\leq\; y^i - t\,,$$

where,

13) $$X_{i,s} = \sum_{j \neq i} \rho_{j,s} L_{j,s}\,.$$

and will be willing to vote for a maximum tax of

14) $$t_{i,s}^{\max} = \rho_{i,s}L_{i,s} + \beta X_{i,s}.$$

This implies that a paternalist in public risk will always pay more for a successful public risk prevention program than for an individually identical private risk reduction since

15) $$t_{i,s}^{\max} - \rho_{i,s}L_{i,s} \begin{cases} = 0 \;\; for \;\; X_{i,s} = 0 \\ > 0 \;\; for \;\; X_{i,s} > 0 \end{cases}$$

Treatments and hypotheses drawn from the theory presented above are presented in Table 1.

Risk Aversion: Given the lack of accepted models of risk aversion in the small, it should be noted that a number of studies have documented systematic deviations from the prediction of expected utility which implies that risks should be valued at expected value in the laboratory (See for example, Holt and Laury, 2002, 2004; Harrison, 2004, Holt and Laury,). To attempt to capture these deviations, define a risk premium $r_{i,s}$ that biases private values such that

16) $$c_{i,s}^{\max} = \rho_{i,s}L_{i,s} + r_{i,s}\,.$$

Alternatively, $r_{i,s}$ can be interpreted as a systematic error with a positive mean. In either case, we suppose that this risk aversion or positive systematic error may be a function of the experimental parameters of income, probability of loss, and loss, so that

17) $$r_{i,s} = r(y^i, \rho_{i,s}, L_{i,s}) \geq 0\,.$$

If (16) is used to replace $\rho_{i,s}L_{i,s}$ everywhere in the analysis above except in the definition of X, and in the special case where it is assumed that on average across subjects that $\sum_{j\neq i} r_{j,s}/(n-1) = r_{i,s}$, so that the average of others' risk premium is the same as own risk premium over relevant treatments, the predictions of Table 1 remain unaltered in that the predicted difference between public and private values (that include risk aversion or systematic error under this special assumption), remain unaltered. However, this requires the extreme assumption that $r_{i,s}$ does not vary systematically with ρ or L so that the average of others risk premiums is always equal to own risk premium. Since the random price voting mechanism used in this study obtains both private (n=1) and public values (n=3) the difference between these measures is the value of interest. We can estimate a mixed model with fixed effects to predict this difference as a function of $Z_{i,s}$ and $X_{i,s}$ as competing hypotheses. However, we must use predicted values for $c_{i,s}^{\max}$ in calculating $Z_{i,s}$ which can be obtained by using predicted values of $c_{i,s}^{\max}$ obtained from an estimated second order Taylor series expansion of (16) after $r_{i,s}$ has been substituted in from (17). The estimated version of (16), which also requires a mixed model with fixed coefficients, because we collect multiple observations from each subject, is of interest in-and-of itself for theorists because the second order expansion contains expected value as well as other values that can explain any systematic differences from expected value.

3. Experimental Design:

All experiments were conducted in the Laboratory for Experimental Economics and Decision Research at Cornell University. 282 subjects (93 in a baseline treatment with certainty, and 189 in the risk treatments) volunteered for the experiments and were recruited from a variety of undergraduate economics courses. Subjects received written instructions (see Appendix) and were permitted to ask questions at the beginning of each part of the experiment. The instructions used language parallel to that found in surveys for referendum voting settings (Carson et al., 2000). The instructions directed each subject to *vote* whether to *fund* an *insurance program* by submitting a *bid* that represented the "highest amount that you would pay and still vote for the insurance program." We subsequently refer to this as the maximum WTP value.

Each subject was seated at an individual computer with a privacy shield and was assigned to groups of varying size of either one or three. For the groups of three, the administrators announced the groups and asked each group member to raise their hand so that they could be identified by other members of their group. This ensured that subjects were aware of who was in their group for all treatments. However, individuals were not informed about which subjects received which payoffs – they only new the distribution of payoffs. No communication was allowed and subjects in the same group size of three were not seated next to each other. Subjects decided how much to bid ranging from zero to the entire initial balance of $25. Using Excel spreadsheets programmed with Visual Basic for Applications, subjects submitted their WTP for insurance to the experiment administrator.

The RPVM operates in much the same way as the traditional private good BDM with a couple of key differences (see Messer *et al.* 2005 for a complete discussion). In the RPVM a majority of the bids determines whether the program is funded. Consequently, in the RPVM, treatments with group size of one are identical to the private good BDM as each subject's bid constitutes a majority. The cost of the program was determined by using a random numbers table with values from 0 to 2500 where the number represented the cost in pennies. For example, if the random number was a 1529, then the determined cost would have been $15.29.

Consequently, the cost was uniformly distributed between $0 and $25 with discrete intervals of $0.01.

Following the procedures developed by Doyle (1997) to help participants understand risk, the probabilistic loss was determined by having volunteer subjects draw ten chips, with replacement, from a bag containing a known number of red and white chips. Each red chip drawn meant that the subject lost a predetermined amount of money; the amount of probabilistic loss for each red chip drawn depended upon the experiment design as described below. After the random cost and loss were determined, subjects retrieved this information and their spreadsheets calculated the profit.

All sessions consisted of two parts. The first part consisted of ten low-incentive private RPVM rounds where the subjects received feedback as the cost and loss were determined at the end of each round, thus giving subjects an opportunity to become familiar with the computer interface and to experience the incentive compatible characteristics. The second part consisted of high-incentive private and public RPVM treatments where the one treatment that resulted in cash payment was determined at the end of the experiment. Thus, subjects did not receive any feedback in the RPVM treatments until the very end of the experiment, thereby ensuring independence of bids and preventing potential deterioration of other regarding behavior as traditionally observed in public good settings with multiple rounds (Davis and Holt 1993). The exchange rate for the second part of the experiment was set at forty times greater than the exchange rate for the first part of the experiment and subjects received an average payoff of $22 for the one and one-half hour experiment.

The experiment is designed to test the hypotheses developed in the second section above by structuring groups of voters who face identical risks (homogeneous treatments) in expected value of loss of $2 for one group of three voters, $5 for another group of voters and $8 for the third group of three voters (Table 2). In the case of different risks (heterogeneous treatments) the groups of three voters were made up of one individual with each expected value of loss of $2, $5, and $8 so there was a low, medium and high-risk voter in each group. To further explore voting behavior three different designs were used. The "Probability Variation" treatments varied the probability (0.2, 0.5, 0.8) with a fixed loss of $10. The "Loss Variation" treatments varied the losses ($5, $12.50, $20) with a fixed probability of 0.4. The former approach is analogous to eliciting WTP for changes in the probability of an outcome. The latter approach varies the conditional loss. Finally, the "Baseline/Calibration" treatments had an initial balance of $10 and the losses of $2, $5, and $8 occurred with certainty.

4. Results

In the private good treatments, a risk neutral individual's optimal strategy is to submit a bid equal to the expected value of their induced loss.[2] In the baseline/calibration experiments which had the losses occur with certainty, subjects' average bids were statistically indistinguishable from the induced loss ($2.10, $5.09, and $8.11, respectively; paired t-test) (Table 3). Such results are consistent with previous BDM research (Irwin *et al*., 1998), that in private induced value WTP experiments the BDM mechanism is demand revealing. However, in the designs where the loss is probabilistic and where the potential exists for experiencing a higher than expected loss, a subject exhibiting some form of risk aversion might submit bids higher than the expected loss. As seen in Table 3, subjects consistently submitted bids that were higher than expected loss in both the Loss Variation ($2.12, $5.45 and $8.53) and Probability Variation ($2.31, $5.57 and $8.78) designs. However, the mean bids were only statistically

[2] Due to the discrete costs, another optimal strategy for a risk neutral person is to submit a bid which is one penny less than the expected value of the induced value.

different from the expected loss for the higher expected loss treatments of $5.00 and $8.00, thereby suggesting that subjects showed behavior that could be interpreted as risk aversion. However, the relatively small magnitude of the possible loss rules out the possibility of measurable risk aversion from expected utility (Rabin 2000; Rabin and Thaler 2001;). Thus, these results support risk aversion in the small. However, another possible explanation for this over-bidding is bidding error, which would likely raise bids since the range of possible bids ($0 to $25) was asymmetric around the expected losses. Note that the certainty baseline also shows overbidding in that bids lie above induced value. There may also be an income effect, as the endowment provided to subjects in the present paper was $2, and the endowment in the baseline study was $10.

The data are also consistent with the possibility of systematic error. As seen in Table 3, "trimming" the data by dropping all observations from individuals that were two standard deviations away from the mean in the private case, shows values that are consistent with expected value predictions. Similar to the pattern observed in the private treatments, subjects in the probabilistic public homogenous loss treatments submitted bids higher than then the expected loss (Table 3). Consistent with prior research on the RPVM (Messer *et al.* 2005), subjects' bids in the public homogeneous loss treatments were not statistically different than the subjects' bids in the private treatment consistent with the theoretical predictions of Table 1 for pure altruism, but not for paternalistic altruism. As with the private bids, the average bids in the homogeneous treatments also are statistically indistinguishable from the expected loss once the data is trimmed.

Inspection of Table 3 shows the effects of shifting from private goods to public homogeneous and heterogeneous voting settings are evident in the comparisons of the mean bids for each of the expected loss amounts. In general, as can be see in Figures 1 and 2, the WTP in the heterogeneous voting situation appears to shift upward for subjects with the lowest expected losses ($2). Such a shift would lend support for the predictions of pure altruism, that some of those who have the least to gain from the insurance policy are willing to incur net financial losses for the benefit of others in a coercive voting situation. For subjects with the highest induced expected losses ($8), and hence those that stand the most to gain from the insurance policy, in the heterogeneous voting situation, the WTP distribution shifts downward relative to the other treatments. Such a trend is also consistent with the Johannesson *et al.* conjecture of pure altruism. For the middle group ($5) in the heterogeneous voting situation, there is no consistent directional shift between treatments, again consistent with the predictions of Table 1 for the case of pure altruism.

To compare the alternative hypotheses regarding the pure altruism measure Z, and the paternalistic altruism measure, X, as well as controlling for the potential impact of risk aversion or systematic error, a two-stage random effects mixed model was used. As described above, in the first stage, bids in the private treatments were estimated using a Taylor series expansion of (16) such that

18) $$c_{i,s}^{\max} = \beta_0 + \beta_1 y_i + \beta_2 \rho_{i,s} + \beta_3 L_{i,s} + \beta_4 \rho_{i,s} L_{i,s} + \beta_5 \rho_{i,s}^2 + \beta_6 L_{i,s}^2 + \varepsilon_{i,s} + \mu_i .$$

The results show that the constant and expected loss, $\rho*L$, are the only statistically significant variables, suggesting that the observed overbidding may not due to systematic behavior except an upward shift in bids (Table 4). Furthermore, tests of the coefficient $\rho*L$ fail to reject that it is different than 1. If the insignificant terms are dropped from (18), so private bids are predicted by a constant and expected value, the constant (equal to 0.17) becomes insignificant but positive and the coefficient on expected value (equal to 1.04) is significant at the 1% level.[3] The significant negative constant in Table 4 apparently offsets some of the positive net effect of the remaining insignificant variables. Thus it appears that a systematic upward bias exists in private values, which is not explainable by the probability of loss, or the loss itself. As noted in Section 2, under this special situation, the predictions of Table 1 should hold.

Using predicted $c_{i,s}^{\max}$ from (18), a second set of random effects mixed model regressions was conducted, as shown in Table 5, to test the hypotheses outlined in Table 1. To test the potential relative importance of purely altruistic preferences versus paternalistic preferences, the last three columns in Table 5 pool all of the risk treatment data and includes both *Z* and *X* to explain the difference between public and private bids. Pooling is supported by a likelihood ratio test statistic of 1.33, which lies far below the critical chi-square test statistic of 5.99. As predicted, in the public treatments with homogeneously distributed losses, we are unable to reject the hypothesis that the coefficient on *Z* equals zero (Table 5). However, in the heterogeneous treatments with heterogeneously distributed losses, the coefficient on *Z* is positive and significant at the 0.01 level as predicted. When all treatments are pooled in the last column, *Z* remains highly significant and has the correct sign. In contrast, the coefficient on *X* should be positive in both the homogeneous and heterogeneous treatments to be consistent with paternalistic altruism. The coefficient on *X* is non-negative but not significantly different from zero in the all treatments. Thus, we do not find support for the hypotheses of paternalistic altruism but do find support for pure altruism.

Dropping *Z* from the model yields the middle three columns of Table 5. In none of these models is *X* significant. Dropping *X* from the model yields the first three columns of Table 5.

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[3] This reduction can be justified given a likelihood ratio test which fails to reject that the two models are different at the 5% level.

Again, Z is not significant in the homogeneous treatment and significant in both the heterogeneous treatment and the pooled treatment. These observations of individual coefficient significance are supported statistically by likelihood ratio tests. The estimated coefficient on Z of about 0.07 implies a value of α of 0.08, which supports the theoretical predictions of Table 1, where the worst off subjects ($8 expected loss) will lower their WTP, the best-off subjects ($2 expected loss) will raise their WTP, and the middle subject ($5 expected loss) will not change their bid relative to their private WTP.

5. Conclusion

There has been a longstanding interest in welfare economics about the degree and type of altruism that individuals exhibit in public decision settings and how these effects should be accounted for in benefit cost analysis. Such concerns have also been prominent in the value of safety and rational voting behavior literatures. Despite these interests, there remains a lack of consensus over whether or not individuals account for the relative gains and losses of others in coercive voting situations involving risk.

In a departure from previous efforts in this field, this research used experimental economic techniques to investigate directly whether the distribution of expected payoffs affects individual decision-making in referenda. Consistent with the Johannesson *et al.* conjecture, we find that individuals with the most to gain from a risk reducing policy do tend to shade their voting decision downward. That is they express a maximum WTP in a heterogeneous public setting that is significantly lower than their WTP for an equal reduction in their private risk. Similarly, but in an alternative direction, our results suggest that those who derive the least benefit from a public insurance policy tend to indicate a higher WTP in a public voting setting than in a private decision-making setting. In a coercive tax setting this suggests that individuals are willing to incur costs upon themselves in the form of higher taxes so as to provide benefits or transferring income to others. Finally, little support can be found for paternalistic altruism with respect to mitigation of risk in this study. Thus, the results here support the type of social preferences for efficiency found in recent studies by Charness and Rabin (1998) and Englemann and Strobel (2004). These results obtained using induced values under certainty seem to extend to risky situations as well. However, these studies could not realistically examine the possibility of paternalistic altruism because of the use of induced values.

We recognize that the risks investigated here involve much higher probabilities and much smaller losses than health risk and safety decision that come before policy makers. Nonetheless, these results provide fodder for the continuing debate concerning altruism in welfare economics. Bergstrom (2004) has recently summarized the relevant theoretical literature specifically for risk as follows:

> Bergstrom (1982) claimed that in general, when altruism takes a "purely altruistic" form, the appropriate way to measure benefits is to sum private valuations, excluding altruistic valuations. Bergstrom's argument is based on the observation that with pure altruism, the marginal first-order optimality conditions are the same as those that apply if account is taken only of the private valuations. Jones-Lee (1991, 1992), who referred to Bergstrom's result as "rather arresting," offered a more thorough discussion. Jones-Lee showed that the same conclusion extends to an interesting case of paternalistic preferences, and showed that when concern for others is "safety-focussed" rather than purely altruistic, the appropriate benefit measures are intermediate between the private and social value. (p. 2)

To date, little additional evidence beyond that presented in Johannesson et al. has been discovered to determine whether risks engender paternalistic or pure altruism. This paper attempts to develop a methodology that can be utilized to distinguish between paternalistic and pure altruism in the controlled environment of the experimental economics laboratory and so contribute to this policy debate. This methodology can potentially be applied to other commodities that involve or affect risk that might engender paternalistic preferences such as healthful as opposed to unhealthy snack foods, alcoholic versus non-alcoholic beverages, smoking aids such as gum containing nicotine versus cigarettes, condoms, etc. The goal would be to find convincing evidence of paternalistic preferences.

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Figure 1a. Loss Variation

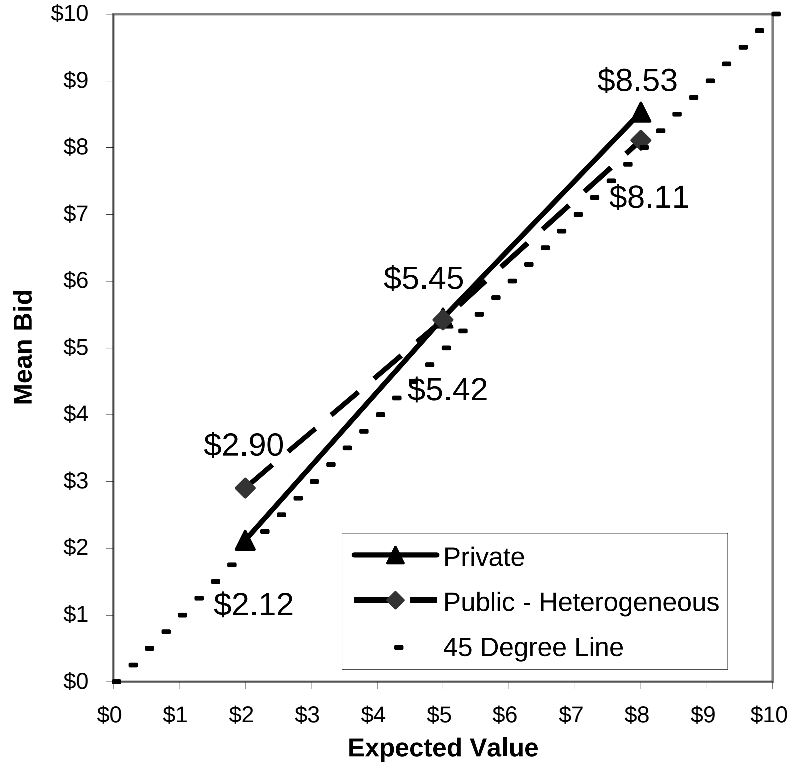

Figure 1b. Loss Variation (Trimmed Data)

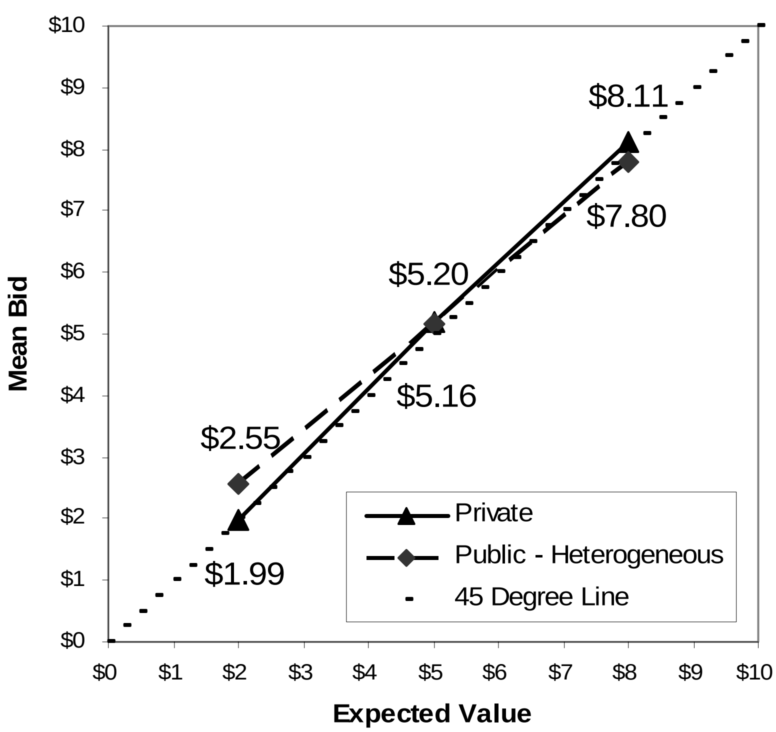

Figure 2a. Probability Variation

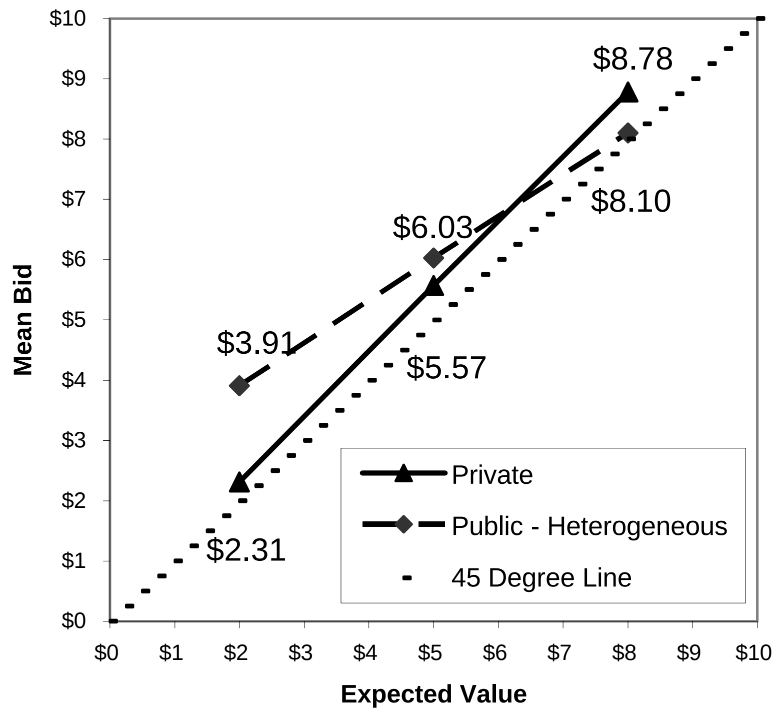

Figure 2b. Probability Variation (Trimmed)

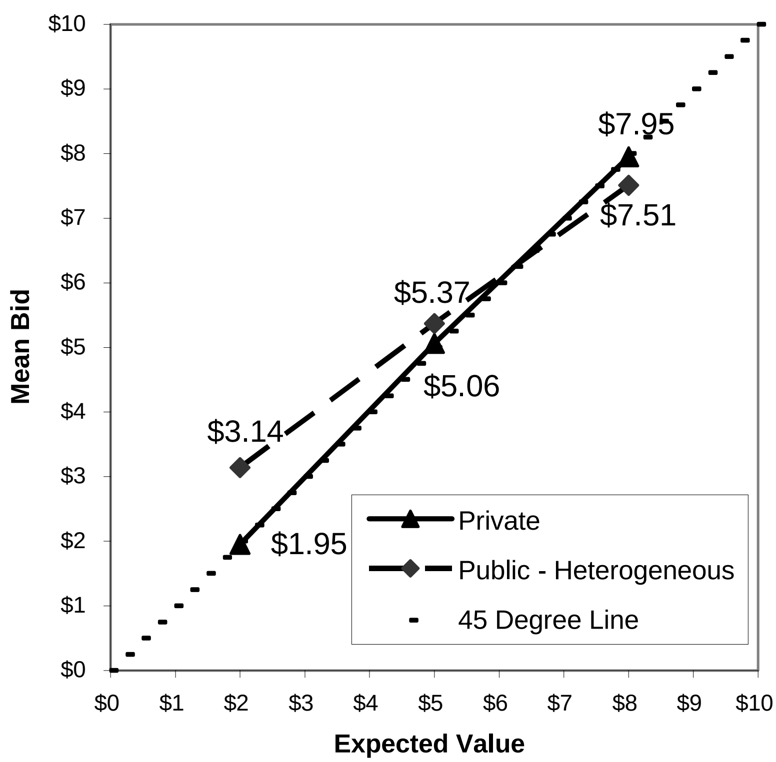

Table 1: Predictions of Public - Private Values

Treatment \ Preferences	Homogeneous Risk						Heterogeneous Risk					
	Loss Variation			*Probability Variation*			*Loss Variation*			*Probability Variation*		
	$2	$5	$8	$2	$5	$8	$2	$5	$8	$2	$5	$8
Selfish	0	0	0	0	0	0	0	0	0	0	0	0
Purely Altruistic Preferences	0	0	0	0	0	0	+	0	-	+	0	-
Paternalistic Preferences	+	+	+	+	+	+	+	+	+	+	+	+

Table 2. Experimental Designs

	Probability of Loss	Loss Amount	Expected Losses (out of 10 draws)	Expected Loss
Loss Variation	0.4	$5.00	4	$2
	0.4	$12.50	4	$5
	0.4	$20.00	4	$8
Probability Variation	0.2	$10	2	$2
	0.5	$10	5	$5
	0.8	$10	8	$8
Baseline/Calibration	1.0	$2	-NA-	$2
	1.0	$5	-NA-	$5
	1.0	$8	-NA-	$8

Table 3. Mean Bids for Each Treatment

	Group Size = 1	Group Size = 3	
	Private	**Homogeneous**	**Heterogeneous**
Loss Variation (n=102)			
Expected Loss: $2	$2.12	$2.10	$2.90
Expected Loss: $5	$5.45	$5.52	$5.42
Expected Loss: $8	$8.53	$8.50	$8.11
Outliers removed (n=93)			
Expected Loss: $2	$1.99	$1.85	$2.55
Expected Loss: $5	$5.20	$5.23	$5.16
Expected Loss: $8	$8.11	$8.14	$7.80
Probability Variation (n=87)			
Expected Loss: $2	$2.31	$2.62	$3.91
Expected Loss: $5	$5.57	$5.75	$6.03
Expected Loss: $8	$8.78	$9.09	$8.10
Outliers removed (n=73)			
Expected Loss: $2	$1.95	$2.12	$3.14
Expected Loss: $5	$5.06	$5.12	$5.37
Expected Loss: $8	$7.95	$8.27	$7.51
Baseline/Calibration (n = 93)			
Expected Loss: $2	$2.10	--	--
Expected Loss: $5	$5.09	--	--
Expected Loss: $8	$8.11	--	--

Table 4. Random Effects Model for Taylor Series Expansion for Private Bid

Variable		Coefficient	Coefficient
Constant		-1.2970 (0.9990)	0.1739 (0.1441)
Baseline		-1.0605 (0.5494)	
ρ		2.5334 (2.4565)	
L		0.1718 (0.1139)	
$\rho*L$		0.8160** (0.1029)	1.0350** (0.0218)
ρ^2		0.1083 (2.0655)	
L^2		-0.0029 (0.0031)	
Sigma_u		1.2953 (0.0825)	1.3023 (0.0830)
Sigma_e		1.5457 (0.0460)	1.5547 (0.0463)
N		846	846
Log Likelihood		-1728.69	-1733.51
LR Chi^2	=	936.0	926.4
Prob > Chi^2	=	0.0000	0.0000

Notes: Standard Errors in Parentheses
* < 0.05 significance, ** < 0.01 significance

Table 5. Random Effects Model Results for Public – Private Bids in Public Treatments.

	Homo.	Hetero.	Pooled	Homo	Hetero.	Pooled	Homo.	Hetero.	Pooled
Constant	0.1256	0.2689**	0.1973*	0.1256	-0.1891	0.1110	0.1257	-0.1174	0.1152
	(0.0907)	(0.0967)	(0.0806)	(0.1744)	(0.8797)	(0.1935)	(0.1741)	(0.8172)	(0.1896)
Z	-0.0408	0.0733**	0.0660**				-0.0408	0.0733**	0.0660**
	(0.0263)	(0.0113)	(0.0095)				(0.0263)	(0.0113)	(0.0095)
X				0.0000	0.0382	0.0078	0.0000	0.0322	0.0075
				(0.0149)	(0.0703)	(0.0161)	(0.0149)	(0.0676)	(0.0156)
Sigma_u	0.7436	0.1948	0.7629	0.7392	0.0000	0.7091	0.7436	0.1972	0.7630
	(0.1183)	(0.5694)	(0.0853)	(0.0149)	(0.3545)	(0.0887)	(0.1183)	(0.5624)	(0.0853)
Sigma_e	1.7332	2.2775	1.9668	1.7387	2.3681	2.0228	1.7332	2.2769	1.9665
	(0.0631)	(0.0829)	(0.0453)	(0.0632)	(0.0703)	(0.0465)	(0.0631)	(0.0829)	(0.0453)
N	567	567	1134	567	567	1134	567	567	1134
Log Likelihood	-1157.9	-1273.3	-2436.9	-1159.1	-1293.3	-2460.2	-1157.9	-1273.2	-2436.8
LR Chi^2	2.41	40.42	46.76	0.00	0.29	0.24	2.41	40.64	46.99
Prob > Chi^2	0.120	0.000	0.000	1.000	0.587	0.625	0.299	0.000	0.000

Note: Standard Errors in Parentheses
* < 0.05 significance, ** < 0.01 significance

Appendix A: Sample Instructions

Probability Variation
Instructions (Part A)
This is an experiment in the economics of decision making. In the course of the experiment, you will have opportunities to earn money. Any money earned during this experiment is yours to keep. Please read these instructions carefully and do not communicate with other participants during the experiment.

You will receive a **starting balance** of $25. At the front of the room are three bags full of one hundred (100) poker chips. The red bag has 20 red chips and 80 white chips. The green bag has 40 red chips and 60 white chips. The blue bag has 80 red chips and 20 white chips. In each round, you will be informed of which bag will be used and ten chips are going to be drawn randomly, and replaced, from this bag by subjects in the experiment. For every white chip drawn, you will have to pay nothing. For every red chip drawn, you will have you pay your **personal loss amount** of $1.00. For example, if 4 of the 10 chips drawn are red, then you would have to pay $4 ($1.00 x 4) from your starting balance of $25.

Rather than taking the chance of losing of your personal loss amount for each red chip drawn, you have the option of purchasing an **insurance program** that will remain in effect for the duration of the round of ten chip draws. With an insurance program, you will not owe the personal loss amount in the event that red chips are drawn. But, you will have to pay the experimenter, out of your starting balance, for the insurance program before any chips are drawn that round.

The amount that you indicate as the highest amount that you would pay for the insurance program will become a **vote** in favor or against the program, and will determine whether or not the insurance program is **funded**. Since you are the only voter, your vote will determine whether the insurance program is funded or not.

You will decide the highest amount that you would pay and still vote for this insurance program; we will call this your **bid**. You submit your bid, by typing it into spreadsheet, pressing the “Enter” key, and then clicking on the “Submit” button. For each insurance program, you can bid any amount between $0 and your initial balance of $25. Once the administrators receive everyone’s bid, the cost of the insurance program will be determined.

Your bid will be compared to the **cost** of the insurance program, as described below. The cost of the insurance program will be determined by reading off a random number table which has numbers from 0 to 2500. The cost will be determined by dropping a pen onto the random number table. (If more than one mark occurs from the drop, then the one closest to the upper-left corner will be used.) In subsequent programs, the cost will be determined starting from the initial cost and reading from left to right. The number will represent the cost in pennies. For example, the number 790 would represent the cost of $7.90. Note: since these numbers have been generated by a random number table each cost between $0 and $25 is equally likely.

Whether or not the insurance program is funded depends on the amount of your bid and the cost of the program. Note that you will be informed which bag will be used in the round and then you submit your bid *prior* to knowing the cost of the insurance program. It is therefore important that you consider all of the information given to you about the different insurance programs and that you make judicious decisions.

Remember, you have the option of purchasing the insurance program at the beginning of each round and the insurance program will remain in effect or not be in effect for the duration of the round of ten chip draws. There are two possible outcomes for each round:

The insurance program is NOT FUNDED: The insurance program is not funded if your bid is *less than* the determined cost. For each red chip drawn (on average 20%, 40% or 80% of the time depending upon which bag is used) of the 10, you will pay your personal loss amount from your starting balance.

The insurance program is FUNDED: The insurance program is funded if your bid is *equal to or greater than* the determined cost. In this case, you are protected from the loss regardless of the color of the chips pulled from the bag. Your cash earnings for the round would be your initial balance ($25) minus the cost of the insurance program.

Note how your bid is like a vote for or against funding the insurance program. With your bid, you are deciding the highest amount you would pay and still vote for the program. Therefore, your bid is like a vote in favor of the insurance program if your bid turns out to be equal to or greater than the cost. On the other hand, your bid is like a vote against the program if your bid turns out to be less than the cost.

While your bid helps determine whether the insurance program is funded or not, your earnings for a particular program are based on four items: your initial balance, your personal loss amount, the number of red chips drawn, and the determined cost of the insurance program. Consider the case where the insurance program is *not funded*, 2 red chips are drawn (therefore, 8 white chips are drawn) and the determined cost is $5. In this case, your earnings would be $23 [$25 – (2 x $1)]. However, if the insurance program was *funded*, your earnings would be same regardless of the number of red chips pulled from the bag. In this case your balance would be $20 ($25 - $5). Consider another case where the insurance program is *not funded*, 6 red chips are drawn (therefore, 4 white chips are drawn) and the determined cost is $5. In this case, your earnings would be $19 [$25 – (6 x $1)]. However, if the insurance program was *funded*, your balance would be $20 ($25 - $5). Note that regardless of whether the program is funded or not we will still conduct the drawing of the ten chips.

Calculation of Your Earnings

Recall that you initially entered your bid into the computer spreadsheet. Once the cost of the insurance program and the number of red chips are determined, the administrator will advise you to click the "Update" button. The computer will automatically indicate whether or not the program was funded, and your earnings will be calculated. Your earnings will be listed in experimental dollars and the exchange ratio for this part of the experiment will be 60 experimental dollars for 1 US dollar. For example, if you earn 30 experimental dollars your payoff would be $0.50 US. At the end of the experiment, we will audit all of the spreadsheets to ensure accuracy.

Instructions (Part B)

For the second part of this experiment, you will now be asked to decide how much you would pay for separate insurance programs. The procedures are similar to those used in the first part of the experiment, except for four important differences.

1) For each of the insurance programs, you may be the only voter (as in the first part of the experiment). However, you may also be part of a group of three people whose **votes** will decide whether or not to purchase the insurance program. For programs where the group size is three, which bag will be used for the other voters in your group will be shown to you prior to determining your bid.

2) Whether the insurance program is funded or not will be determined by whether the **majority of bids** are greater than or less than the determined cost. If the majority of bids is greater than or equal to the cost, everyone will have to purchase the insurance program at the determined cost. If the majority of bids is lower than the randomly drawn cost, no one will purchase the insurance program.

3) Only one of the nine insurance programs will actually be selected and result in cash earnings. Therefore, all bids will be made prior to determination of the cost of the program or the drawing of the chips. After the bids are submitted, we will randomly determine which of the insurance programs will generate cash earnings by drawing from a bag containing nine chips lettered A through I. Each letter corresponds to one of the nine programs.

4) The exchange rate has changed. You will now earn two US dollars for every three experimental dollars earned. Thus, if you earn 15 experimental dollars, your payoff would be $10 US.

For each insurance program, the experiment proceeds as follows:

You will be given a **starting balance** of $25 and told how much money you and the other members of your group could lose. Once again, the **personal loss amount** per each red chip drawn is $1.

Whether or not you lose your personal loss amount depends on the ten chips drawn. At the front of the room are three bags full of one hundred (100) poker chips. The red bag has 20 red chips and 80 white chips. The green bag has 40 red chips and 60 white chips. The blue bag has 80 red chips and 20 white chips. One or more bags will be selected and ten chips are going to be drawn randomly, and replaced, from this bag by subjects in the experiment. For every white chip drawn, you will have to pay nothing. For every red chip drawn you will have you pay your **personal loss amount** of $1 unless your group has purchased an insurance program.

For each program, you will be informed which bag will be used for you and for the other members of your group and you will be asked to decide upon your bid and enter that amount into the second spreadsheet on the computer. Consider all of the information for the insurance program before submitting your bid. For each insurance program, you can bid any amount between $0 and your initial balance of $25.

Once everyone has submitted his/her bid for one program, the administrators will show you the information for the next program. After the bids for all the insurance programs have been entered, we will determine which of the programs will be selected and produce cash earnings.

Next, we will determine the cost of the selected insurance program and whether it will be funded by your group. The cost of the insurance program will be determined in exactly the same manner as before, except that a new random number table with values from 0 to 2,500 will be used. This cost will now be the cost that each person in your group will have to pay if the insurance program is funded. Recall that since these numbers have been generated by a random number table each cost between $0 and $25 is equally likely.

Once again, there are two possible outcomes in the round:

<u>The insurance program is NOT FUNDED:</u> The insurance program is not funded if a majority of bids from your group are *less than* the determined cost. For each red chip drawn (on average 20%, 40% or 80% of the time depending upon which bag is selected) of the 10, you and everyone else in your group will pay their personal loss amount from the starting balance.

<u>The insurance program is FUNDED:</u> The insurance program is funded if a majority of bids from your group are *equal to or greater than* the determined cost. In this case, you and everyone else in your group are protected from the loss regardless of the color of the chips pulled from the bag. For you and everyone else in your group, your cash earnings for the round would be your initial balance ($25) minus the cost of the insurance program. Note that each person in your group will pay the same cost even if the personal loss amounts in your group differed.

The programs, in which you are a group of one, are identical to the insurance programs you experienced in the first part of the experiment. Therefore, the insurance program is not funded if your bid is *less than* the cost determined from the random number table, and program is funded if your bid is *equal to or greater than* the determined cost.

Note once again how your bid is like a vote for or against funding the insurance program. With your bid, you are deciding the highest amount you would pay and still vote for the insurance program. Therefore, your bid is like a vote in favor of the program if your bid turns out to be equal to or greater than the cost. On the other hand, your bid is like a vote against the program if it turns out to be less than the cost. When a majority of bids are equal to or greater than the determined cost, this translates into a majority vote in favor of the insurance program. Similarly, a majority of bids below the cost translates into a majority vote against the program at that cost.

<u>Calculation of Final Earnings</u>

To calculate your earnings from Part B, the administrator will inform you which "Update" button to click. The computer will then calculate your earnings for Part B and add them to your earnings from Part A. We will audit the spreadsheets to ensure accuracy.

Instructions (Part A)
This is an experiment in the economics of decision making. In the course of the experiment, you will have opportunities to earn money. Any money earned during this experiment is yours to keep. Please read these instructions carefully and do not communicate with other participants during the experiment.

You will receive a **starting balance** of $25. At the front of the room is a bag full of one hundred (100) poker chips: 40 red ones and 60 white ones. In each round, ten chips are going to be drawn randomly, and replaced, from the bag by subjects in the experiment For every white chip drawn, you will have to pay nothing. For every red chip drawn you will have you pay your **personal loss amount**. Your personal loss amount will vary during the course of the experiment. The possible personal loss amounts are -$0.50, -$1.25, and -$2.00. For example, if your personal loss amount is -$1.25, and 4 of the 10 chips drawn are red, then you would have to pay $5 ($1.25 x 4) from your starting balance of $25.

Rather than taking the chance of losing of your personal loss amount for each red chip drawn, you have the option of purchasing an **insurance program** that will remain in effect for the duration of the round of ten chip draws. With an insurance program, you will not owe the personal loss amount in the event that red chips are drawn. But, you will have to pay the experimenter, out of your starting balance, for the insurance program before any chips are drawn that round.

The amount that you indicate as the highest amount that you would pay for the insurance program will become a **vote** in favor or against the program, and will determine whether or not the insurance program is **funded**. Since you are the only voter, your vote will determine whether the insurance program is funded or not.

You will decide the highest amount that you would pay and still vote for this insurance program; we will call this your **bid**. You submit your bid, by typing it into spreadsheet, pressing the "Enter" key, and then clicking on the "Submit" button. For each insurance program, you can bid any amount between $0 and your initial balance of $25. Once the administrators receive everyone's bid, the cost of the insurance program will be determined.

The **cost** of the insurance program will be determined by reading off a random number table which has numbers from 0 to 2500. The cost will be determined by dropping a pen onto the random number table. (If more than one mark occurs from the drop, then the one closest to the upper-left corner will be used.) In subsequent programs, the cost will be determined starting from the initial cost and reading from left to right. The number will represent the cost in pennies. For example, the number 790 would represent the cost of $7.90. Note: since these numbers have been generated by a random number table each cost between $0 and $25 is equally likely.

Whether or not the insurance program is funded depends on the amount of your bid and the cost of the program. Note that you submit your bid *prior* to knowing the price of the insurance program. It is therefore important that you consider all of the information given to you about the different insurance programs and that you make judicious decisions.

Remember, you have the option of purchasing the insurance program at the beginning of each round and the insurance program will remain in effect or not be in effect for the duration of the round of ten chip draws. There are two possible outcomes for each round:

<u>The insurance program is NOT FUNDED:</u> The insurance program is not funded if your bid is *less than* the determined cost. For each red chip drawn (on average 40% of the time) of the 10, you will pay your personal loss amount from your starting balance.

<u>The insurance program is FUNDED:</u> The insurance program is funded if your bid is *equal to or greater than* the determined cost. In this case, you are protected from the loss regardless of the color of the chips pulled from the bag. Your cash earnings for the round would be your initial balance ($25) minus the cost of the insurance program.

Note how your bid is like a vote for or against funding the insurance program. With your bid, you are deciding the highest amount you would pay and still vote for the program. Therefore, your bid is like a vote in favor of the insurance program if your bid turns out to be equal to or greater than the cost. On the other hand, your bid is like a vote against the program if your bid turns out to be less than the cost.

While your bid helps determine whether the insurance program is funded or not, your earnings for a particular program are based on four items: your initial balance, your personal loss amount, the number of red chips drawn, and the determined cost of the insurance program. Consider the case where the insurance program is *not funded*, your personal loss amount is -$1.25, 2 red chips are drawn (therefore, 8 white chips are drawn) and the determined cost is $5. In this case, your earnings would be $22.50 [$25 – (2 x $1.25)]. However, if the insurance program was *funded*, your earnings would be same regardless of the number of red chips pulled from the bag. In this case your balance would be $20 ($25 - $5). Consider another case where the insurance program is *not funded*, your personal loss amount is $1.25, 6 red chips are drawn (therefore, 4 white chips are drawn) and the determined cost is $5. In this case, your earnings would be $17.50 [$25 – (6 x $1.25)]. However, if the insurance program was *funded*, your balance would be $20 ($25 - $5).

Note that regardless of whether the program is funded or not we will still conduct the drawing of the ten chips.

<u>Calculation of Your Earnings</u>

Recall that you initially entered your bid into the computer spreadsheet. Once the cost of the insurance program and the number of red chips are determined, the administrator will advise you to click the "Update" button. The computer will automatically indicate whether or not the program was funded, and your earnings will be calculated. Your earnings will be listed in experimental dollars and the exchange ratio for this part of the experiment will be 60 experimental dollars for 1 US dollar. For example, if you earn 30 experimental dollars your payoff would be $0.50 US. At the end of the experiment, we will audit all of the spreadsheets to ensure accuracy.

It is important that you clearly understand these instructions.

Please raise your hand if you have any questions.
Please do not talk with other participants in the experiment.

<u>Loss Variation Instructions:</u> (Note the instructions refer to 15 subjects. Data for these large group settings were collected and are provided for completeness in Appendix B, but are not used in this paper).

<u>Instructions (Part B)</u>
For the second part of this experiment, you will now be asked to decide how much you would pay for separate insurance programs. The procedures are similar to those used in the first part of the experiment, except for four important differences.

1) For each of the insurance programs, you may be the only voter (as in the first part of the experiment). However, you may also be part of a group of 3 or 15 people whose **votes** will decide whether or not to purchase the insurance program. For programs where the group size is 3 or 15, the personal loss amounts of the other voters in your group will be shown to you prior to determining your bid.

2) Whether the insurance program is funded or not will be determined by whether the **majority of bids** are greater than or less than the determined cost. If the majority of bids is greater than or equal to the cost, everyone will have to purchase the insurance program at the determined cost. If the majority of bids is lower than the randomly drawn cost, no one will purchase the insurance program.

3) Only one of the 15 insurance programs will actually be selected and result in cash earnings. Therefore, all bids will be made prior to determination of the cost of the program or the drawing of the chips. After the bids are submitted, we will randomly determine which of the insurance programs will generate cash earnings by drawing from a bag containing 15 chips lettered A through O. Each letter corresponds to one of the 15 programs.

4) The exchange rate has changed. You will now earn two US dollars for every three experimental dollars earned. Thus, if you earn 15 experimental dollars, your payoff would be $10 US.

<u>For each insurance program, the experiment proceeds as follows:</u>
You will be given a **starting balance** of $25 and told how much money you and the other members of your group could lose. Once again, the **personal loss amounts** per each red chip drawn are -$0.50, -$1.25, and -$2, and will vary from program to program.

Whether or not you lose your personal loss amount depends on the ten chips drawn. At the front of the room, there is a bag full of one hundred (100) poker chips: 40 red ones and 60 white ones. Ten chips are going to be drawn randomly, and replaced, from the bag in the experiment. For every white chip drawn, you will have to pay nothing. For every red chip drawn you will have you pay your **personal loss amount** unless your group has purchased an insurance program.

For each program, you will be asked to decide upon your bid and enter that amount into the second spreadsheet on the computer. Consider all of the information for the insurance program before submitting your bid. For each insurance program, you can bid any amount between $0 and your initial balance of $25.

Once everyone has submitted his/her bid for one program, the administrators will show you the information for the next program. After the bids for all the insurance programs have been entered, we will determine which of the programs will be selected and produce cash earnings.

Next, we will determine the cost of the selected insurance program and whether it will be funded by your group. The cost of the insurance program will be determined in exactly the same manner as before, except that a new random number table with values from 0 to 2,500 will be used. This cost will now be the cost that each person in your group will have to pay if the insurance program is funded. Recall that since these numbers have been generated by a random number table each cost between $0 and $25 is equally likely.

Once again, there are two possible outcomes in the round:

<u>The insurance program is NOT FUNDED:</u> The insurance program is not funded if a majority of bids from your group are *less than* the determined cost. For each red chip drawn (on average 40% of the time) of the 10, you and everyone else in your group will pay their personal loss amount from the starting balance.

<u>The insurance program is FUNDED:</u> The insurance program is funded if a majority of bids from your group are *equal to or greater than* the determined cost. In this case, you and everyone else in your group are protected from the loss regardless of the color of the chips pulled from the bag. For you and everyone else in your group, your cash earnings for the **round** would be your initial balance ($25) minus the cost of the insurance program. Note that each person in your group will pay the same cost even if the personal loss amounts in your group differed.

The programs, in which you are a group of one, are identical to the insurance programs you experienced in the first part of the experiment. Therefore, the insurance program is not funded if your bid is *less than* the cost determined from the random number table, and program is funded if your bid is *equal to or greater than* the determined cost.

Note once again how your bid is like a vote for or against funding the insurance program. With your bid, you are deciding the highest amount you would pay and still vote for the insurance program. Therefore, your bid is like a vote in favor of the program if your bid turns out to be equal to or greater than the cost. On the other hand, your bid is like a vote against the program if it turns out to be less than the cost. When a majority of bids are equal to or greater than the determined cost, this translates into a majority vote in favor of the insurance program. Similarly, a majority of bids below the cost translates into a majority vote against the program at that cost.

Calculation of Final Earnings

To calculate your earnings from Part B, the administrator will inform you which "Update" button to click. The computer will then calculate your earnings for Part B and add them to your earnings from Part A. We will audit the spreadsheets to ensure accuracy.

Appendix B: Data

Probability Loss Data:

sub#	exp#	within#	Private -$2	Homo -$2	Hetero -$2	Private -$5	Homo -$5	Hetero - $5	Private - $8	Homo -$8	Hetero -$8
1	1	1	0.00	0.00	0.00	3.50	4.50	4.80	7.80	7.80	7.50
2	1	2	3.00	3.00	8.00	6.00	6.00	8.00	12.00	12.00	9.00
3	1	3	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
4	1	4	5.00	5.00	5.00	10.00	10.00	10.00	15.00	17.00	18.00
5	1	5	2.00	2.00	2.00	5.00	5.00	4.00	8.00	8.00	8.00
6	1	6	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
7	1	7	2.00	2.00	4.00	5.00	5.00	5.00	8.00	7.00	6.00
8	1	8	1.75	1.75	4.00	4.00	4.25	4.25	7.50	7.75	4.25
9	1	9	10.00	4.00	9.00	7.00	7.50	8.00	9.00	9.00	9.00
10	1	10	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
11	1	11	3.00	2.00	1.00	5.00	4.00	6.00	8.00	8.50	8.50
12	1	12	2.00	2.00	5.00	5.00	5.00	5.00	8.00	8.00	5.00
13	1	13	2.00	2.00	0.00	5.00	5.00	5.00	8.00	8.00	8.50
14	1	14	2.00	2.00	4.00	5.00	4.00	4.00	6.00	7.00	4.00
15	1	15	3.00	3.00	3.00	5.00	5.00	5.00	7.00	7.00	7.00
16	1	16	2.50	3.00	3.00	5.25	5.75	5.50	7.50	8.25	8.25
17	1	17	2.00	2.00	8.00	13.00	14.50	12.00	18.00	19.00	17.00
18	1	18	0.15	0.15	5.00	5.00	5.00	5.00	10.00	10.00	10.00
19	2	1	3.00	3.00	8.00	6.00	6.00	8.00	10.00	10.00	9.00
20	2	2	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
21	2	3	4.00	4.00	18.00	14.00	18.00	14.00	18.00	20.00	17.50
22	2	4	1.50	1.45	2.00	4.00	4.15	4.78	7.00	6.75	5.00
23	2	5	1.00	0.00	5.00	3.00	5.00	5.00	10.00	10.00	5.00
24	2	6	5.00	5.00	5.00	8.00	8.00	8.00	10.00	10.00	10.00
25	3	1	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
26	3	2	2.00	2.50	3.00	8.00	5.00	6.00	10.00	11.00	11.00
27	3	3	1.00	1.00	1.00	4.00	4.00	4.00	7.00	7.00	7.00
28	3	5	1.00	2.00	4.00	4.00	4.00	4.00	7.00	7.00	4.00
29	3	6	0.00	2.00	3.00	5.00	3.00	4.00	8.00	8.00	6.00
30	3	7	3.00	3.50	3.50	5.00	6.00	6.00	8.00	8.00	8.00
31	3	8	2.86	2.25	3.00	5.69	5.19	5.25	1.00	8.95	8.07
32	3	9	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
33	3	10	0.00	0.00	10.00	5.50	5.50	7.00	9.00	8.50	6.00
34	3	11	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
35	3	12	3.29	23.00	12.25	6.99	8.83	14.79	18.53	19.99	5.83
36	3	13	2.00	0.00	0.00	5.00	5.00	5.00	8.00	8.00	8.00
37	3	14	3.00	3.00	5.00	8.00	6.00	5.00	9.00	8.00	8.00
38	3	15	2.05	2.00	1.87	5.00	6.00	5.04	8.50	7.50	9.00
39	3	16	4.00	8.00	10.00	7.00	10.00	12.00	15.50	15.50	14.00
40	3	17	2.00	2.00	5.00	5.00	5.00	7.00	10.00	7.00	10.00
41	3	18	1.75	1.50	3.25	4.50	4.00	5.00	8.00	8.50	6.50
42	3	19	3.00	3.00	3.00	5.65	5.65	8.00	8.00	8.00	8.00
43	3	20	3.00	2.00	3.00	5.00	5.00	5.00	8.00	8.00	8.00
44	3	21	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00

45	4	1	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
46	4	3	8.00	8.00	14.00	12.00	12.00	10.00	15.00	15.00	13.00
47	4	4	1.00	1.00	1.00	3.00	3.00	3.00	6.00	6.00	6.00
48	4	5	7.17	6.07	8.45	12.35	11.14	12.75	20.21	17.35	14.92
49	4	6	2.00	2.00	3.00	5.00	5.00	3.00	5.00	7.00	5.00
50	4	7	1.00	1.00	2.00	3.00	4.00	3.00	4.00	5.00	2.00
51	4	8	1.50	1.50	4.50	4.50	4.50	4.50	7.50	7.50	4.50
52	4	9	2.00	3.00	3.00	4.50	5.00	4.00	6.50	6.00	6.00
53	4	10	3.00	3.00	3.00	6.00	6.00	6.00	9.00	9.00	8.00
54	4	11	3.00	7.00	15.00	6.50	15.00	7.50	9.00	23.00	10.00
55	4	12	2.00	3.50	2.00	6.00	5.00	5.00	8.00	0.00	8.00
56	4	13	2.00	2.02	4.42	4.33	4.35	4.40	7.69	7.70	4.41
57	4	14	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
58	4	15	1.00	1.00	1.00	4.00	4.00	4.00	7.00	7.00	7.00
59	4	16	4.00	4.00	5.00	6.36	6.75	6.50	8.75	9.00	8.75
60	4	17	1.00	1.00	1.00	5.00	5.00	4.00	7.00	7.00	5.00
61	4	18	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
62	5	1	1.99	1.99	1.99	4.99	4.99	4.99	7.99	7.99	10.00
63	5	2	1.75	1.75	1.75	4.75	4.75	4.75	7.75	7.75	7.75
64	5	3	0.00	0.00	0.00	4.00	4.00	5.00	7.00	7.00	8.00
65	5	4	2.00	2.00	13.00	7.00	10.00	12.00	15.00	12.00	11.00
66	5	5	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
67	5	6	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
68	5	7	2.00	2.00	0.00	5.00	5.00	5.00	8.00	8.00	5.00
69	5	8	1.00	0.50	2.00	2.00	2.00	3.00	5.00	5.00	4.00
70	6	1	2.50	2.50	2.05	4.00	4.00	4.00	7.00	7.00	6.00
71	6	2	2.00	2.00	0.00	5.00	5.00	5.00	7.00	7.00	8.00
72	6	3	6.00	4.00	4.00	10.00	9.00	11.00	15.00	17.00	13.00
73	6	4	2.00	2.00	3.00	5.00	5.00	5.00	8.00	9.00	9.00
74	6	5	1.50	1.50	1.50	4.00	4.00	4.00	7.50	7.50	7.50
75	6	6	1.00	1.00	2.00	6.00	4.00	6.00	7.00	8.00	7.00
76	6	7	1.00	2.00	3.00	5.00	4.00	6.00	7.00	8.00	5.00
77	6	8	3.00	3.00	3.00	5.00	5.00	5.00	7.00	8.00	8.00
78	6	9	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
79	6	10	2.00	2.00	4.75	4.25	4.25	4.75	7.25	7.50	5.00
80	6	11	2.00	3.75	12.89	6.17	7.89	16.85	11.00	13.86	12.15
81	6	12	2.00	2.00	2.00	4.90	5.00	4.90	8.00	8.00	7.00
82	7	1	2.00	1.50	2.00	7.00	5.50	6.00	14.00	14.00	14.00
83	7	2	2.00	2.00	2.00	4.00	4.00	4.00	8.50	8.00	8.00
84	7	3	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
85	7	4	2.00	2.00	2.00	4.00	4.00	4.00	7.00	7.00	7.00
86	7	5	4.00	6.00	5.00	9.00	9.00	11.00	9.00	10.00	11.00
87	7	6	2.00	5.00	7.00	8.00	9.00	10.00	12.00	15.00	13.00

Loss Variation Data - Private and Public N=3:

Obs #	Exp Session	Sub# within Session	Private $-2	Homo $-2	Hetero $-2	Private $-5	Homo $-5	Hetero $-5	Private $-8	Homo $-8	Hetero $-8
1	1	1	2.00	1.50	1.00	5.00	4.00	2.50	8.00	6.00	5.00
2	1	2	1.50	1.50	1.50	3.75	3.75	3.75	8.00	8.00	8.00
3	1	3	2.00	2.00	1.00	6.25	6.25	6.25	8.00	8.00	10.00
4	1	4	2.00	2.00	2.00	5.00	5.00	5.00	7.00	7.00	7.00
5	1	5	2.50	2.50	2.50	6.25	6.25	6.25	10.00	10.00	10.00
6	1	6	0.00	0.00	0.00	5.00	5.00	0.00	8.00	8.00	0.00
7	1	7	3.00	3.00	3.00	5.00	5.00	8.00	10.00	10.00	12.00
8	1	8	2.00	2.00	2.00	5.00	5.00	4.00	8.00	8.00	8.00
9	1	9	2.00	0.00	0.00	5.00	8.00	10.00	6.00	9.00	20.00
10	1	10	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
11	1	11	3.00	3.00	2.00	6.00	6.00	6.00	8.00	8.00	7.00
12	1	12	3.00	3.00	3.00	5.00	8.00	5.00	8.00	8.00	6.00
13	1	13	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
14	1	14	3.24	2.25	3.03	8.73	8.96	7.45	10.04	12.58	9.94
15	1	15	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
16	2	1	5.00	5.50	5.00	10.00	6.50	5.95	15.00	20.00	4.00
17	2	2	1.50	1.50	1.50	4.00	4.00	4.00	7.00	7.00	7.00
18	2	3	2.50	2.00	2.00	4.00	3.50	3.75	6.00	5.50	6.00
19	2	4	1.75	1.75	1.75	4.50	4.50	4.50	7.25	7.25	7.25
20	2	5	1.50	1.50	1.50	5.05	5.00	5.05	6.98	6.98	6.98
21	2	6	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
22	2	7	2.50	2.50	2.00	6.75	7.00	6.60	11.20	11.50	12.00
23	2	8	1.90	1.90	1.90	4.75	4.75	4.75	7.50	7.50	7.50
24	2	9	2.00	3.00	3.00	8.00	8.00	9.00	14.00	14.00	15.00
25	2	10	0.30	0.25	0.30	5.00	2.00	3.00	5.00	5.00	5.00
26	2	11	3.00	2.00	3.00	7.00	7.00	7.00	10.00	10.00	11.00
27	2	12	1.00	1.00	1.00	8.00	8.00	8.00	10.00	10.00	10.00
28	2	13	2.50	2.50	2.50	6.00	7.00	8.00	8.00	10.00	10.00
29	2	14	2.50	2.25	2.50	6.25	6.25	4.25	15.00	17.00	18.00
30	2	15	1.75	1.75	1.75	6.25	5.50	5.50	7.50	7.50	7.50
31	3	1	2.72	2.50	2.73	7.50	7.26	7.50	16.00	16.22	15.00
32	3	2	0.10	2.85	8.25	17.74	13.92	23.25	20.00	1.50	12.50
33	3	3	2.50	2.50	2.50	6.25	6.25	6.25	10.00	10.00	10.00
34	3	5	2.00	2.00	2.00	6.00	6.00	6.00	8.00	10.00	8.00
35	3	6	2.40	2.10	2.00	5.10	5.00	5.10	7.65	7.60	7.60
36	3	7	2.00	2.00	2.50	7.00	7.75	6.50	9.50	10.00	10.00
37	3	8	1.50	1.25	1.50	5.75	6.25	6.25	8.50	8.50	8.25
38	3	9	3.00	2.00	3.00	4.75	5.00	5.00	12.00	11.00	10.75
39	3	10	2.00	2.00	2.00	6.00	6.00	6.00	11.00	10.00	10.00
40	3	11	2.76	2.58	2.27	7.27	6.50	9.64	11.56	9.81	11.27
41	3	12	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
42	3	13	2.50	2.00	2.50	5.00	4.50	5.00	6.00	7.00	8.00
43	3	14	2.87	2.96	2.89	7.98	7.86	7.75	11.06	10.50	10.67
44	3	15	1.50	0.50	3.00	10.00	10.00	7.00	15.00	21.00	17.00
45	4	1	2.50	2.50	2.50	6.25	10.00	7.50	10.00	10.00	10.00

46	4	2	5.00	3.50	3.00	2.00	2.25	2.00	1.00	1.00	3.00
47	4	3	2.00	1.50	2.00	5.00	5.00	5.00	8.00	9.00	8.00
48	4	4	1.00	1.00	1.00	4.00	5.00	5.00	6.00	6.00	8.00
49	4	5	1.50	1.50	1.50	4.50	4.50	4.50	6.00	6.00	8.00
50	4	6	1.00	1.00	1.00	3.75	3.75	3.75	6.00	6.00	6.00
51	4	7	1.50	1.50	1.50	7.50	8.50	7.50	13.00	15.00	13.00
52	4	8	2.00	2.00	2.00	6.00	5.00	5.00	7.00	8.00	12.00
53	4	9	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
54	4	10	1.50	1.00	0.01	3.76	4.00	4.00	6.00	7.00	7.25
55	4	11	2.50	3.00	3.00	5.00	3.00	5.00	6.00	8.00	7.00
56	4	12	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
57	4	13	1.21	1.67	1.50	4.00	4.13	4.00	7.00	7.00	7.50
58	4	14	2.00	2.00	2.00	3.00	3.00	3.00	5.00	5.00	5.00
59	4	15	3.00	0.75	0.25	5.75	1.25	0.75	7.75	2.25	1.25
60	5	1	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
61	5	2	3.00	2.50	3.00	5.00	5.00	5.00	9.50	9.50	9.00
62	5	3	5.00	5.00	5.00	6.00	12.00	10.00	10.00	15.00	12.00
63	5	4	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
64	5	5	2.00	0.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
65	5	6	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
66	5	7	2.00	1.50	2.00	5.00	5.00	5.00	6.00	7.00	6.00
67	5	8	2.00	2.00	1.75	6.00	6.00	6.00	8.00	8.00	8.00
68	5	9	1.90	1.90	2.00	5.00	5.00	4.75	7.50	7.80	7.50
69	5	10	1.00	1.00	1.00	8.00	10.00	12.50	14.00	15.00	15.00
70	5	11	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
71	5	13	2.00	2.00	2.00	5.00	5.00	5.00	8.00	2.00	8.00
72	5	14	2.00	1.50	5.00	5.00	3.25	6.00	9.00	8.00	4.00
73	5	15	2.50	2.00	2.50	6.00	5.00	7.00	9.00	9.00	9.00
74	6	1	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
75	6	2	2.00	2.00	2.50	5.60	5.60	5.60	8.00	8.00	8.60
76	6	3	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
77	6	4	1.00	1.00	1.00	5.00	5.00	5.00	6.00	8.00	8.00
78	6	5	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
79	6	7	2.50	3.00	2.50	4.50	10.00	5.00	9.00	12.00	9.00
80	6	8	10.00	20.00	10.00	5.00	10.00	5.00	10.00	2.00	10.00
81	6	9	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
82	6	10	1.00	0.50	1.00	2.00	1.25	2.00	2.00	2.00	2.00
83	6	11	2.00	2.00	2.00	4.00	2.00	2.00	10.00	8.00	4.00
84	6	12	2.00	2.00	2.00	3.75	3.75	3.75	7.75	7.75	7.75
85	6	13	1.50	1.50	1.00	5.00	4.00	4.00	7.50	7.00	7.00
86	6	14	3.00	3.50	3.00	6.25	6.25	5.00	10.00	10.00	10.00
87	6	15	1.50	2.00	2.00	4.00	4.00	4.00	6.00	6.00	6.00
88	7	1	1.75	1.90	1.70	4.44	4.75	4.30	7.75	7.50	7.00
89	7	2	1.75	1.75	2.00	3.75	6.75	5.00	7.00	7.00	8.00
90	7	3	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
91	7	4	2.00	2.00	2.00	5.00	5.00	5.00	10.00	10.00	10.00
92	7	5	3.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
93	7	6	1.75	1.75	1.75	3.50	3.75	3.50	7.50	7.50	7.50
94	7	7	0.00	0.00	0.00	5.00	5.00	3.50	8.00	10.00	8.00
95	7	8	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00

96	7	9	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
97	7	10	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
98	7	11	1.95	2.00	2.00	4.95	3.00	3.50	8.00	8.00	8.00
99	7	12	0.00	0.00	0.00	5.00	5.00	5.00	8.00	8.00	8.00
100	7	13	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
101	7	14	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00
102	7	15	2.00	2.00	2.00	5.00	5.00	5.00	8.00	8.00	8.00

Loss Variation Data - n=15:

Obs #	Exp Session	Sub# within Session	Homo15 $-5	Hetero15 $-5	Homo15 $-8	Hetero15 $-8
1	1	1	2.50	2.50	10.00	10.00
2	1	2	3.75	3.75	8.00	8.00
3	1	3	7.00	8.00	5.00	8.00
4	1	4	5.00	5.00	7.00	7.00
5	1	5	6.25	6.25	10.00	10.00
6	1	6	5.00	0.00	8.00	10.00
7	1	7	8.00	8.00	12.00	8.00
8	1	8	5.00	5.00	8.00	8.00
9	1	9	7.00	10.00	8.00	8.00
10	1	10	5.00	5.00	8.00	8.00
11	1	11	4.00	5.00	8.00	5.00
12	1	12	4.00	5.00	4.00	5.00
13	1	13	5.00	5.00	5.00	5.00
14	1	14	8.66	8.63	8.38	10.01
15	1	15	5.00	5.00	8.00	8.00
16	2	1	7.00	9.00	4.00	4.00
17	2	2	4.00	4.00	7.00	7.00
18	2	3	4.50	4.00	4.00	4.50
19	2	4	4.50	4.50	4.50	7.25
20	2	5	5.05	5.05	6.98	6.98
21	2	6	5.00	5.00	8.00	8.00
22	2	7	7.60	6.90	10.50	10.50
23	2	8	4.75	4.75	7.00	7.50
24	2	9	9.00	7.50	11.00	11.00
25	2	10	3.00	2.75	6.50	7.00
26	2	11	7.00	5.00	12.00	13.00
27	2	12	8.00	8.00	10.00	10.00
28	2	13	7.50	8.00	10.00	12.00
29	2	14	6.25	6.25	18.00	19.00
30	2	15	6.00	4.50	8.00	7.85
31	3	1	7.00	7.23	15.00	14.50
32	3	2	13.62	10.00	14.75	1.50
33	3	3	6.00	8.00	6.00	8.00
34	3	5	6.00	6.00	10.00	12.00
35	3	6	5.00	5.00	8.00	7.60

36	3	7	7.00	6.50	9.50	7.00
37	3	8	5.78	6.00	8.11	8.50
38	3	9	6.50	6.00	11.00	9.00
39	3	10	6.00	6.00	12.00	10.00
40	3	11	5.92	7.27	8.81	11.81
41	3	12	5.00	5.00	8.00	8.00
42	3	13	5.00	5.00	8.00	8.00
43	3	14	7.87	7.75	10.96	11.23
44	3	15	5.00	18.00	12.00	20.00
45	4	1	8.00	7.00	10.00	10.00
46	4	2	1.50	2.25	1.00	1.50
47	4	3	5.00	5.00	8.00	8.00
48	4	4	5.00	5.00	5.00	5.00
49	4	5	4.50	4.50	6.00	8.00
50	4	6	3.75	3.75	6.00	6.00
51	4	7	4.50	7.00	5.50	10.00
52	4	8	5.00	6.00	10.00	8.00
53	4	9	5.00	5.00	8.00	8.00
54	4	10	4.00	4.00	7.00	7.25
55	4	11	5.00	5.00	8.00	6.00
56	4	12	5.00	5.00	8.00	8.00
57	4	13	4.00	4.00	4.00	7.00
58	4	14	4.00	4.00	5.00	5.00
59	4	15	1.00	0.50	1.50	3.00
60	5	1	5.00	5.00	8.00	8.00
61	5	2	7.00	7.00	9.50	9.50
62	5	3	10.00	12.00	14.00	12.00
63	5	4	5.00	5.00	8.00	8.00
64	5	5	5.00	5.00	12.00	20.00
65	5	6	5.00	5.00	8.00	8.00
66	5	7	5.00	4.50	6.00	6.75
67	5	8	5.00	6.50	8.00	8.25
68	5	9	4.80	4.90	7.50	7.80
69	5	10	12.50	12.50	15.00	8.00
70	5	11	5.00	5.00	8.00	8.00
71	5	13	5.00	5.00	8.00	8.00
72	5	14	5.00	6.00	6.00	7.00
73	5	15	5.00	7.00	8.00	9.00
74	6	1	5.00	5.00	8.00	8.00
75	6	2	4.00	3.00	8.00	10.00
76	6	3	5.00	5.00	8.00	8.00
77	6	4	6.00	5.00	6.00	8.00
78	6	5	5.00	5.00	8.00	8.00
79	6	7	5.00	10.00	8.00	10.00
80	6	8	10.00	7.00	10.00	5.00
81	6	9	5.00	5.00	8.00	8.00
82	6	10	1.25	2.00	2.00	2.00
83	6	11	2.00	1.00	12.00	12.00
84	6	12	3.75	3.75	8.00	8.00
85	6	13	3.00	4.00	7.60	7.00

86	6	14	6.25	6.25	6.25	6.25
87	6	15	3.00	3.00	6.00	2.00
88	7	1	4.60	5.00	7.95	8.12
89	7	2	6.50	5.00	6.50	7.00
90	7	3	5.00	5.00	8.00	8.00
91	7	4	5.00	5.00	10.00	10.00
92	7	5	3.00	5.00	8.00	8.00
93	7	6	3.00	3.50	7.50	7.50
94	7	7	5.00	5.00	8.00	7.00
95	7	8	5.00	5.00	5.00	5.00
96	7	9	5.00	5.00	8.00	8.00
97	7	10	5.00	5.00	8.00	8.00
98	7	11	7.00	3.75	6.00	8.00
99	7	12	5.00	5.00	8.00	8.00
100	7	13	8.00	5.00	8.00	8.00
101	7	14	5.00	5.00	8.00	8.00
102	7	15	5.00	5.00	8.00	8.00

CERTAINTY AND HYPOTHETICAL BIAS IN CONTINGENT VALUATION ELICITATION METHODS

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Abstract:

This research tests the reliability of three stated preference instruments for obtaining hypothetical WTP and compares the values obtained from these instruments to actual WTP behavior in a similar experimental setting that used the Random Price Voting Mechanism. Results suggest that the Yes/No payment card format provides the closest prediction of actual behavior, while the multiple bounded discrete choice format yields statistically similar estimates.

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Certainty and Hypothetical Bias in Contingent Valuation Elicitation Methods

A fundamental issue in the increased use of non-market valuation methods to provide input into public good decision making is the validity of these methods for measuring the benefits of public goods and changes in public goods. While the examination of validity involves many dimensions, undoubtedly a critical component of any validity assessment of a method is criterion validity. Criterion validity involves comparing the values measured by the method in question with those obtained by practitioners of the discipline (Bishop, 2004). While most economists would agree that values obtained from a well-functioning market would provide a satisfactory criterion for assessing validity of a method, such an approach becomes problematic for non-market goods. In lieu of such a market, simulated market experiments have been adopted as reasonable criterion for assessing validity.

Since the seminal field validity test conducted by Bishop and Heberlein (1979), a large body of criterion validity research for contingent valuation method has arisen in the environmental economics and broader economics literature. Contingent valuation is the leading stated preference method for eliciting non-market values, typically involving the elicitation of willingness to pay for improvement in environmental quality or other public good. Evidence from meta-analysis research (e.g. List and Gallet, 2001) suggests that respondents to contingent valuation survey do consistently overstate their preferences, and that calibration of these methods is needed. Importantly, from the perspective of this research, the magnitude of hypothetical bias increases with public goods in comparison to biases found in private goods (e.g. candy bars) and quasi-public goods (e.g. hunting permits).

While a number of factors likely contribute to the increased divergence between hypothetical and real willingness to pay values in simulated market settings for public goods, we conjecture that the greater disparity observed in public good settings can be attributed, in part, to poor demand revelation characteristics of the mechanisms used to collect actual contributions. In previous NSF-funded research (NSF/EPA #R824688 and NSF #SBR9727375) we have shown that the standard voluntary contribution mechanisms widely used in field criterion validity tests of the contingent valuation method, do indeed understate demand in induced value settings analogous to those used in non-market valuation criterion validity tests (Rondeau, Poe and Schulze, 2005). Hence, reliance on such mechanisms may lead the to biased assessments of criterion validity.

Despite their poor performance at eliciting actual payments that are comparable to public good values, the continued reliance on voluntary contribution mechanisms in contingent valuation criterion validity and calibration research is understandable. While incentive compatible mechanisms for public goods, which approach demand revelation, have been developed over the years (e.g. Groves Ledyard, 1977; Smith, 1980, Falkinger et al. 2000) these mechanism often involve complex incentive and provision structures. Moreover, these mechanisms typically involve multiple rounds to approximate group demand revelation and, hence, they are not amenable to one shot field conditions of contingent valuation. In contrast, the Random Price Voting Mechanism (RPVM) developed in Messer (2004) and Messer et al. (2005) offers a promising demand-revealing single shot mechanism that is amenable to simulated market settings.

In this research summary, we build upon this previous research to demonstrate how it could be applied to criterion validity testing of contingent valuation. In the process we also explore convergent validity across alternative contingent valuation elicitation methods.

Experimental Design

Split sample hypothetical data is elicited for both private and public insurance policies using two forms of the Multiple Bounded Discrete Choice (MBDC) method described by Welsh and Poe (1998), and the standard Payment Card (PC) method. In the standard PC format (Rowe et al. 1996), subjects circle a value corresponding with their decision from a table of values (Figure 1). A second sample received an alternative Dichotomous Choice Payment Card (DC-PC), depicted in Figure 2, corresponding to that suggested in Boyle (2003) and employed by Bateman et al. (2005) amongst others. For the DC-PC respondents indicate a vote of "Yes" or "No" to paying specified dollar amounts. As such, this elicitation method has similarities to the standard PC, but differs fundamentally in that respondents are asked to register a decision for each dollar value (rather than simply circling one value from a table). A third sample faced a MBDC version requiring respondents to indicate a level of certainty of WTP for each of a series of values. Certainty response options in the MBDC format are "Definitely Yes" (DY), "Probably Yes" (PY), "Unsure" (UN), "Probably No" (PN), and "Definitely No" (DN) (Figure 3). Past research (Vossler et al. 2004) indicates that the multiple bounded "Probably Yes" (MB-PY) model best predicts actual behavior in contribution settings. To the best of our knowledge, such comparisons have not been provided for DC-C and PC formats.

The structure of the hypothetical risk experiment is taken from Messer, Poe and Schulze (2006) which uses the RPVM. The private market follows the standard Becker–DeGroot-

Marshack (BDM) private goods mechanism, which has been shown to be theoretically incentive compatible and empirically demand revealing. In the private insurance setting, an individual purchases the insurance, and avoids risk of loss, at a price equal to the randomly drawn cost, if that cost is less than the offered bid. If the randomly drawn cost exceeds the bid amount then the insurance policy is not purchased and the individual faces the financial risk. The public good setting has a similar decision rule for groups of three subjects facing the same financial risk, however in this setting the decision to purchase depends on a majority rule criterion. That is, if the majority of offered bids exceeds the randomly drawn cost, then the insurance policy is purchased and each individual pays the randomly drawn cost. Hence the mechanism becomes a coercive, majority rules tax.

Experimental sessions for the hypothetical risk study were conducted at Cornell University's Laboratory for Experimental Economics and Decision Research (LEEDR). Subjects were recruited from undergraduate introductory level economics classes in the Department of Applied Economics and Management (AEM). Sessions consisted of up to 24 subjects participating at one time. Participants were paid at a flat fee of approximately $25 per hour. Most sessions lasted between 35 and 45 minutes, so the payment was typically $15 per participant[1] Participants were seated at individual computers equipped with privacy shields. Subject bids were collected using Excel spreadsheets programmed with Visual Basic for Application. These spreadsheets transmitted the subject responses to an Access database for storage.

A split sample approach was employed, with 95 subjects completing the PC version, 104 subjects completing the MBDC version, and 109 participants completing the DC-PC version.

[1] As an additional incentive for participation, some students were offered extra credit in their AEM course for participation. This incentive applied to participation in any of several experiments occurring during the same period, and was offered by professors of particular courses.

Upon entering the lab, subjects were randomly assigned to a computer workstation, and given time to complete consent forms and read the initial experiment instructions. The experiment proceeds as follows.[2]

Subjects are asked to imagine they have the opportunity to purchase an insurance program to protect them against a loss. They are shown three sets of stacked poker chips, each set with a different composition of red and white colored chips. Poker chips are transferred into three different colored bags by subject volunteers. The red bag contains 20 red chips and 80 white chips, the green bag contains 50 red chips and 50 white chips, and the blue bag contains 80 red chips and 20 white chips.

Subjects are instructed that for each round, they will be assigned to one of the three bags, and volunteer subjects will make ten draws (with replacement) from this bag. Each red chip drawn corresponds with a private loss of $1.00. If the insurance program is purchased, no draws will be made from the assigned bag, and thus any loss will be avoided. Subjects are asked to indicate the "highest amount they would pay and still vote for the insurance program."[3]

For each round, subjects are given an account with a starting balance of $25. If an insurance program is purchased, the cost is deducted from this account. If an insurance program is not purchased, any losses resulting from red chip draws are deducted from the account. To familiarize subjects with the probabilistic component of their decision task, two demonstration rounds are conducted. Volunteers make ten draws with replacement from the green (50 red, 50 white) bag, and the loss for the practice round is announced.

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[2] See Appendix for complete instructions for the DC-PC version of the Hypothetical Risk experiment.
[3] This is the verbiage used in the PC and DC-PC versions. For the MBDC version, subjects are instructed to "indicate if you would pay the designated cost and still vote for the program."

Subjects are next informed that for each round they will be assigned to a group of either one or three members, and will be told which bag each group member must draw from.[4] Group members are announced and identified to each other through a show of hands. In the case where the group size is three, each member faces the same cost for the insurance program, regardless of which bag has been assigned to them. Purchase of the insurance program is based on a majority vote rule, and is thus coercive. Finally, subjects are reminded that while their decisions are hypothetical and do not affect their earnings, they should make the decisions as if the losses and opportunity to purchase the program were real.

It is important to note that the experimental design used here departs from that used by Messer, et al. in two primary ways. First, decision-making in this experiment is hypothetical in nature. Subjects never actually face the process where losses are incurred, nor do they actually purchase insurance programs. Rather they indicate the highest amount they would pay "and still vote Yes for" the program. Second, though the RPVM process is described, the actual cost of the insurance program is never explicitly determined. In the Messer, et al. study, one of the program rounds is randomly chosen to be played through for the determination of subject earnings. Cost of the insurance program is determined randomly by employing the RPVM. But because of the hypothetical nature of this experiment, a final cost need not be specified. In response to questions regarding whether the cost of the program would be revealed, subjects were told that at this time, policy makers are uncertain about how much the program will actually cost. We only know that it will be somewhere between $0 and $25.

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[4] This aspect of the experiment was originally designed by Messer, et al. to examine altruism in a public goods provision situation. While this is not pertinent to the current analysis, it was retained in order to facilitate potential future analysis between hypothetical and real versions of the experiment.

Nine separate rounds of insurance program are offered, each with a different variation of group size and bag assignment. Table 1 lists the specifications of each of the nine rounds. For the MBDC, PC, and DC-PC versions of the experiment, information for each round is distributed and collected using a program written for use with the LEEDR computer network. Subjects do not have access to information about subsequent rounds until they have submitted their decision for the current round, nor can they change a previously submitted vote. Subjects are instructed not to communicate with each other during the experiment.

This report focuses only on the comparison of the private $5 BDM and the homogenous ($5,$5,$5) RPVM settings. This allows us to confine our attention to our proposed method of criterion validity testing, and not be diverted by the issues of paternalistic and pure altruism explored in Messer et al. (2005) and Messer, Poe and Schulze (2006).

Instrument Design

Following Rowe, et al. (1996), cell values for the PC table are set by using the log function described by Weber's law:

1) $B_n = B_1 * (1 + k)^{n-1}$

where B_n is the bid in cell n, B_1 is the bid in the first cell, and k is a percentage of increase in the scale. Use of an exponential function allows the distance between cell values to increase at increasing rate, providing the advantage of keeping the PC table down to a manageable size. In addition, a logarithmic scale takes into account evidence of log-normal distribution of measurement errors in past studies (such as Rowe, et al. 1996). As the quantity being estimated grows larger, the precision of estimates tends to decline.

Having selected the range as between \$0 and the initial account balance of \$25, it is possible to determine *k* and calculate rough values for the cells. Specifically,

2) $k = [(B_N / B_1)^{1/(N-1)}] - 1$

where B_N is set equal to the upper limit of the range, and *N* is set equal to the number of desired cells in the table. For the scale calculations, values greater than 1 are used for both B_1s, and an *N* larger than the desired number of cells is used. This is because the incremental changes for the first few B_ns are very small, and are meaningless in terms of the valuation task at hand. Therefore, the first several cells are collapsed into a smaller, more efficient set of increases.

Finally, the remaining values are smoothed in order to provide a manageable choice set. Cell values for the PC table were chosen such that they straddle the induced values of the insurance program (\$2, \$5, and \$8 for the red, green, and blue bags, respectively), but never exactly hit these values. The resulting PC table contains 30 cells. In the DC-PC format, subjects are instructed to answer "Yes" or "No" to each of the values listed in the standard PC format.

Design of the MBDC version follows directly from the format introduced by Welsh and Poe (1998). For each of a set of increasing values, respondents are asked to indicate their certainty that true quantity is greater than that value. Certainty is indicated by selecting among the answers "Definitely Yes," "Probably Yes," "Unsure," "Probably No," and "Definitely No." Two versions of the MBDC format were developed for the experiment, each containing 18 specified values within the range of \$0 to \$25. Between the two MBDC versions, each of the values contained in the PC table are represented. Hence the elicitation formats have the same range and matching points to facilitate comparison. As indicated previously, Figures 1, 2 and 3 illustrate the three different elicitation formats described above.

To control for potential order effects, for each of the elicitation methods, the nine decision rounds are presented in two different rotations, each to approximately half of the treatment-specific respondents. In each version, the program presented in the first round is the homogeneous public good case with an induced value of $5 for all group members. The following eight rounds are reversed in the second order variation. Table 2 exhibits the difference in the rotations.

Results

For reasons indicted above, our analysis here is directed toward the insurance programs presented in Rounds A (5,5,5) and I (5). Data are examined using nonparametric methods. In the nonparametric setting, subject responses are plotted in a reverse cumulative distribution function (1-CDF). Figures 7-9 display these distributions. Tests of equality between distributions is undertaken using the Smirnov Test (Conover1980). The Smirnov Test statistic is exactly the same as the Kolmorgorav-Smirnov D-statistic.:

$$D = \max[F(x) - G(x)]$$

Where F(x) and G(x) depict the cumulative distribution function for the two distributions being compared and the critical test statistic for large sample approximations is given in Conover (p. 473). Mean estimates calculated following the Kriström (1992) method described by Vaughn and Rodriguez (2001) are reported in Tables 3 and 4.

Convergent Validity:

Prior research indicates that the MBDC Probably Yes (MB-PY) estimates tend to coincide with estimates obtained through the standard PC format for the case of non-market environmental goods (Vossler 2003). This suggests that the two elicitation methods provide convergent validity in terms of valuing the provision of non-market goods. In contrast, for the case of the risk experiments conducted for this study, we find that the distributions of the PC and MB-PY differ for both the private and public goods insurance provision programs. Significance tests are performed using the Smirnov[5] method and yield a D-statistic of 0.21, indicating that these two distributions are significantly different at the five percent level in the public goods case with induced values at $5, $5, $5. Similarly in the private goods case, with an induced value of $5, the K-S D-statistic is 0.23, indicating significant difference at the one percent level. (See Figures 6 and 7 for the "Best Candidates" nonparametric distributions.)

Nonparametric means calculated using the Kriström method are $4.95 and $5.75 for PC and MB-PY in the public goods case, and $5.77 and $5.95 in the private goods case (See Figures 4 and 5). Upon first inspection it appears that the difference in mean WTP estimates using the two different elicitation methods are greater in the public goods case than for the private good. However we are cautious in drawing such an interpretation. It may be the case that subjects exhibit learning as the rounds progress, and thus bid closer to the induced value in later rounds. As such we take the public goods case presented in the first round as the better indicator of elicitation effects.

If no cognitive methodological elicitation effects influence subject behavior, one would expect the standard PC and the DC-PC to display convergent validity, and exhibit closely aligned distributions and mean estimates. However Smirnov significance tests here again show that for the public goods case these two distributions are significantly different at the one percent level, with a D-statistic of 0.23. For the private good, the D-statistic is 0.19, indicating significant difference at the ten percent level. Mean estimates here also show variation, with the DC-PC

[5] See Tables 5 and 6 for a matrix comparison of K-S significance tests.

format yielding a mean of $6.40, as compared with $5.77 in the PC format. For the private good, the DC-PC mean value is $6.34, as compared with $4.95.

Comparison of the DC-PC and MB-DY is much closer to displaying convergent validity in terms of the underlying distributions. The D-statistic for the private goods case is 0.06, and for the public goods case is 0.14. In both cases we cannot reject the null hypothesis that the distributions are the same. The corresponding private mean value comparison is $5.95 vs. $6.34 for the MB-PY and DC-PC respectively while the public mean value comparison is $5.74 vs. $6.40. Comparing these two elicitation methods we find that the estimates across public and private settings are closer than in other pair comparisons.

Criterion Validity:

The unique opportunity of this analysis is the availability of the actual preference data from an identical experiment for a comparison with the hypothetical values. Thus we are able to examine the criterion validity of the three elicitation methods. First we compare the PC distribution and mean estimates with the Actual data. Smirnov D-statistics for PC vs. Actual are 0.27 (significant at the one percent level) for the $5, $5, $5 case, and 0.23 (significant at the five percent level) for the $5 case. The actual mean is $6.20 (as compared with $4.95) for the public insurance program, and is $5.82 (as compared with $5.77) for the private insurance program.

Comparing the MB-PY with actual, we find a Smirnov D-statistic of 0.13 for in the public setting and 0.09 in the private setting. Neither case indicates any significant difference in the distribution. Mean comparisons here are $5.95 to $5.82 (MB-PY v. Actual) for the private good, and $5.75 to $6.20 for the public good. It thus seems reasonable to consider the MBDC elicitation method with a Probably Yes modeling approach as a viable proxy for actual behavior.

Finally we compare the DC-PC elicitation estimates with the actual data. For the private goods case, K-S significance tests yield a D-statistic of 0.07, and in the public goods setting the D-statistic is 0.04. In neither case can the null hypothesis of equality be rejected. In terms of means comparison, we have for the private good an actual mean value of $5.82, while the DC-PC provides an estimated mean of $6.34. For the public good, the actual mean is $6.20 and the DC-PC mean is $6.40.

Conclusion

Nonparametric analysis shows that the DC-PC and the MB-PY formats provide statistically similar estimates of WTP and are not statistically different from each other, while the WTP distribution form the DC format is significantly different from the other elicitation methods and the actual distribution of WTP. The implication of this research is that a simple DC-PC format may be preferred to the MBDC format in terms of predicting actual WTP, as well as being less cognitively taxing on the respondent.

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Instructions (Part A)   (DC-PC)

This is a hypothetical experiment in the economics of decision making. Please read the following instructions carefully and do not communicate with other participants during the experiment. If you have a question, please raise your hand and a lab administrator will respond.

In today's experiment you will be asked to consider a number of situations and to make a decision in each one of them. In each situation you will be facing the chance of a loss and will be asked to indicate if you would purchase an insurance program that protects you against the possibility of this loss. The same procedures will be followed for each situation. However, each situation is independent from the others. The decision you make in one situation will not affect others. Each situation proceeds as follows.

Imagine that you receive a **starting balance** of $25. At the front of the room are three bags full of one hundred (100) poker chips. The red bag has 20 red chips and 80 white chips. The green bag has 50 red chips and 50 white chips. The blue bag has 80 red chips and 20 white chips. In each situation you will be informed of which bag will be used and ten chips will be drawn randomly, and replaced, from this bag by subjects participating in the experiment. For every white chip drawn, you will have to pay nothing. For every **red** chip drawn, you will have to pay your **personal loss amount** of $1.00. For example, if 4 of the 10 chips drawn are red, then you would have to pay $4 ($1.00 x 4) from your starting balance of $25.

Rather than taking the chance of losing your personal loss amount for each red chip drawn, you have the option of purchasing an **insurance program** that will remain in effect for the duration of the round of ten chip draws. With an insurance program, you will not owe the personal loss amount in the event that red chips are drawn. But, you will have to pay the experimenter, out of your starting balance, for the insurance program before any chips are drawn that round.

We will ask you to consider a number of designated costs for the insurance program and to indicate if you would purchase the insurance program at each of these costs: we will call this your **vote**. For each cost, you submit your vote by clicking on the button on the spreadsheet that best corresponds with your voting decision. After you have finished voting on all of the designated prices, click on the SUBMIT button and wait for the administrator to instruct you to continue to the next round. For each insurance program, the costs you will be asked to consider are between $0 and your initial balance of $25.

Experiments like this have been conducted for real money at Cornell University. Although this experiment is hypothetical, please think about the questions carefully as if you really were facing this chance of a financial loss and offered an opportunity to purchase an insurance program.

It is important that you clearly understand these instructions.
Please raise your hand if you have any questions.
Please do not talk with other participants in the experiment.

<u>Instructions (Part B)</u> **<u>(DC-PC)</u>**

Now that you understand the basic features of this insurance program and the voting process, you will be asked to indicate if would vote for each of nine separate insurance programs at various designated costs. The procedures are similar to those discussed previously, except for three important differences.

1) For each of the insurance programs, you may be the only voter (as described in Part A of the instructions). However, you may also be part of a group of three people whose **votes** would decide whether or not to purchase the insurance program. For programs where the group size is three, information about which bags will be used for you and the other voters in your group will be shown to you prior to determining your vote. Although each person may be drawing their chips from a different bag, each person will have the same personal loss amount of $1 for every red chip that is drawn from their assigned bag. You do not incur any personal loss amount for red chips drawn from bags assigned to other members of your group.

2) For groups of three, you are in a majority rule situation in which the number of votes for the program would be evaluated at each designated cost. Using the procedure described in part A we will ask you to indicate if you would vote for the insurance program at various designated costs. If the insurance program is purchased, the same cost would be paid by <u>you and all other members of the group.</u> This is true even though each member of your group may be drawing from a different bag.

3) Only one of the nine insurance programs would actually be implemented. This is done by randomly drawing from a bag containing nine chips, lettered A through I. Each letter corresponds to one of the nine programs.

<u>For each insurance program, the experiment proceeds as follows:</u>

Imagine that you and each member of your group are given a **starting balance** of $25 and that you will be informed which bags will be used for you and for the other members of your group. Given this information, you will be asked to indicate if you would vote for the program and to enter the corresponding information into the spreadsheet on the computer. Once again, the **personal loss amount** per each red chip drawn is $1. However, different people in your group may be drawing from different bags.

Consider all of the information for the insurance program before submitting your vote. For each insurance program, the costs you will be asked to consider will be between $0 and your initial balance of $25. Once everyone has submitted his/her votes for one insurance program, the administrators will instruct you to move to the next program. After the votes for all the insurance programs have been entered, we will determine which of the programs will be selected.

Again, experiments like this have been conducted for real money at Cornell University. Although this experiment is hypothetical, please think about the questions carefully, as if you really were facing this chance of a financial loss and offered an opportunity to purchase an insurance program.

It is important that you clearly understand these instructions.
Please raise your hand if you have any questions.
Please do not talk with other participants in the experiment.

Table 1: Decision Specifications by Round

Round:	Group Size:	Your Bag: (out of 100 chips)	Bags of the other two members in your group
A	3	Green (50 red chips)	Green (50 red chips) & Green (50 red chips)
B	3	Blue (80 red chips)	Green (50 red chips) & Red (20 red chips)
C	3	Red (20 red chips)	Green (50 red chips) & Blue (80 red chips)
D	1	Blue (80 red chips)	na
E	3	Green (50 red chips)	Red (20 red chips) & Blue (80 red chips)
F	1	Red (20 red chips)	na
G	3	Blue (80 red chips)	Blue (80 red chips) & Blue (80 red chips)
H	3	Red (20 red chips)	Red (20 red chips) & Red (20 red chips)
I	1	Green (50 red chips)	na

Figure 1: The Standard Payment Card (PC) Elicitation Instrument

Given the information above, what is the highest amount that you would pay and still vote for the insurance program?

(Use the mouse to click on the cell containing your answer.)

$0.00	$0.25	$0.50	$0.75	$1.00	$1.25
$1.55	$1.85	$2.15	$2.55	$2.95	$3.35
$3.85	$4.35	$4.85	$5.35	$5.95	$6.55
$7.15	$7.85	$8.55	$9.35	$10.20	$11.15
$12.30	$13.80	$15.65	$18.15	$21.40	$25.00

Submit

Figure 2: The Multiple Bounded Discrete Choice (MBDC) Elicitation Instrument

Given the information above, would you vote for the insurance program at each of the costs listed below?

Please select one of the five choices for ***each*** of the dollar amounts below

(Use the mouse to click on the cell containing your answer. A circle will appear.)

$	0.00	→	Definitely No	Probably No	Unsure	Probably Yes	Definitely Yes
$	0.50	→	Definitely No	Probably No	Unsure	Probably Yes	Definitely Yes
$	1.00	→	Definitely No	Probably No	Unsure	Probably Yes	Definitely Yes
$	1.55	→	Definitely No	Probably No	Unsure	Probably Yes	Definitely Yes
$	2.15	→	Definitely No	Probably No	Unsure	Probably Yes	Definitely Yes
$	2.95	→	Definitely No	Probably No	Unsure	Probably Yes	Definitely Yes
$	3.85	→	Definitely No	Probably No	Unsure	Probably Yes	Definitely Yes
$	4.85	→	Definitely No	Probably No	Unsure	Probably Yes	Definitely Yes
$	5.95	→	Definitely No	Probably No	Unsure	Probably Yes	Definitely Yes
$	7.15	→	Definitely No	Probably No	Unsure	Probably Yes	Definitely Yes
$	8.55	→	Definitely No	Probably No	Unsure	Probably Yes	Definitely Yes
$	10.20	→	Definitely No	Probably No	Unsure	Probably Yes	Definitely Yes
$	11.95	→	Definitely No	Probably No	Unsure	Probably Yes	Definitely Yes
$	13.80	→	Definitely No	Probably No	Unsure	Probably Yes	Definitely Yes
$	15.90	→	Definitely No	Probably No	Unsure	Probably Yes	Definitely Yes
$	18.35	→	Definitely No	Probably No	Unsure	Probably Yes	Definitely Yes
$	21.40	→	Definitely No	Probably No	Unsure	Probably Yes	Definitely Yes
$	25.00	→	Definitely No	Probably No	Unsure	Probably Yes	Definitely Yes

Submit

Figure 3: The Discrete Choice Payment Card (DC-PC) Elicitation Instrument

Given the information above, would you vote for the insurance program at each of the costs listed below?

Please indicate your choice for ***each*** of the dollar amounts below

(Use the mouse to click on the cell containing your answer. A circle will appear.)

$0.00	→	Yes	No
$0.25	→	Yes	No
$0.50	→	Yes	No
$0.75	→	Yes	No
$1.00	→	Yes	No
$1.25	→	Yes	No
$1.55	→	Yes	No
$1.85	→	Yes	No
$2.15	→	Yes	No
$2.55	→	Yes	No
$2.95	→	Yes	No
$3.35	→	Yes	No
$3.85	→	Yes	No
$4.35	→	Yes	No
$4.85	→	Yes	No
$5.35	→	Yes	No
$5.95	→	Yes	No
$6.55	→	Yes	No
$7.15	→	Yes	No
$7.85	→	Yes	No
$8.55	→	Yes	No
$9.35	→	Yes	No
$10.20	→	Yes	No
$11.15	→	Yes	No
$12.30	→	Yes	No
$13.80	→	Yes	No
$15.65	→	Yes	No
$18.15	→	Yes	No
$21.40	→	Yes	No
$25.00	→	Yes	No

Submit

Table 2: Risk Order Variations for the Hypothetical Risk Experiment

Order of Presentation	1	2	3	4	5	6	7	8	9
Risk Order Variation 1	*Rd. A*	*Rd. B*	*Rd. C*	*Rd. D*	*Rd. E*	*Rd. F*	*Rd. G*	*Rd. H*	*Rd. I*
Induced Values*	5,5,5	8,5,2	2,5,8	8	5,2,8	2	8,8,8	2,2,2	5
Risk Order Variation 2	*Rd. A*	*Rd. I*	*Rd. H*	*Rd. G*	*Rd. F*	*Rd. E*	*Rd. D*	*Rd. C*	*Rd. B*
Induced Values	5,5,5	5	2,2,2	8,8,8	2	5,2,8	8	2,5,8	8,5,2

* *Subject's own induced value is displayed first, followed by the induced values of group members.*

Figure 4: Nonparametric Means for the Private Goods Setting

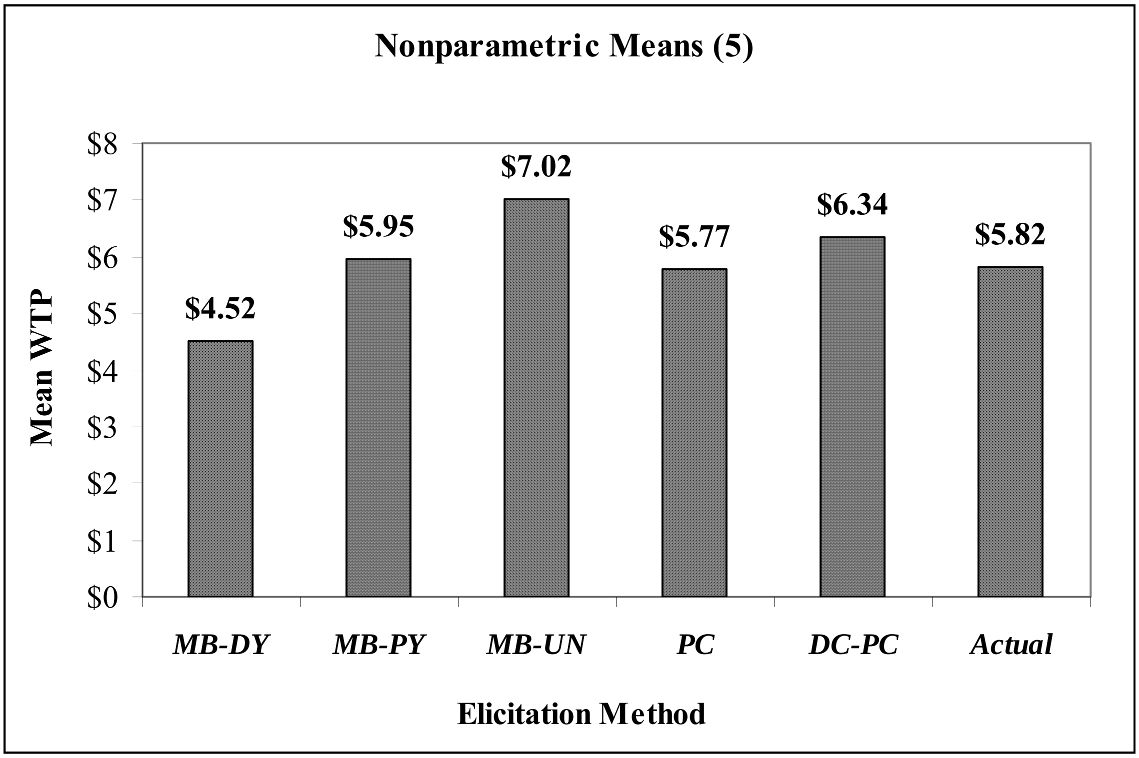

Figure 5: Nonparametric Means for the Public Goods Setting

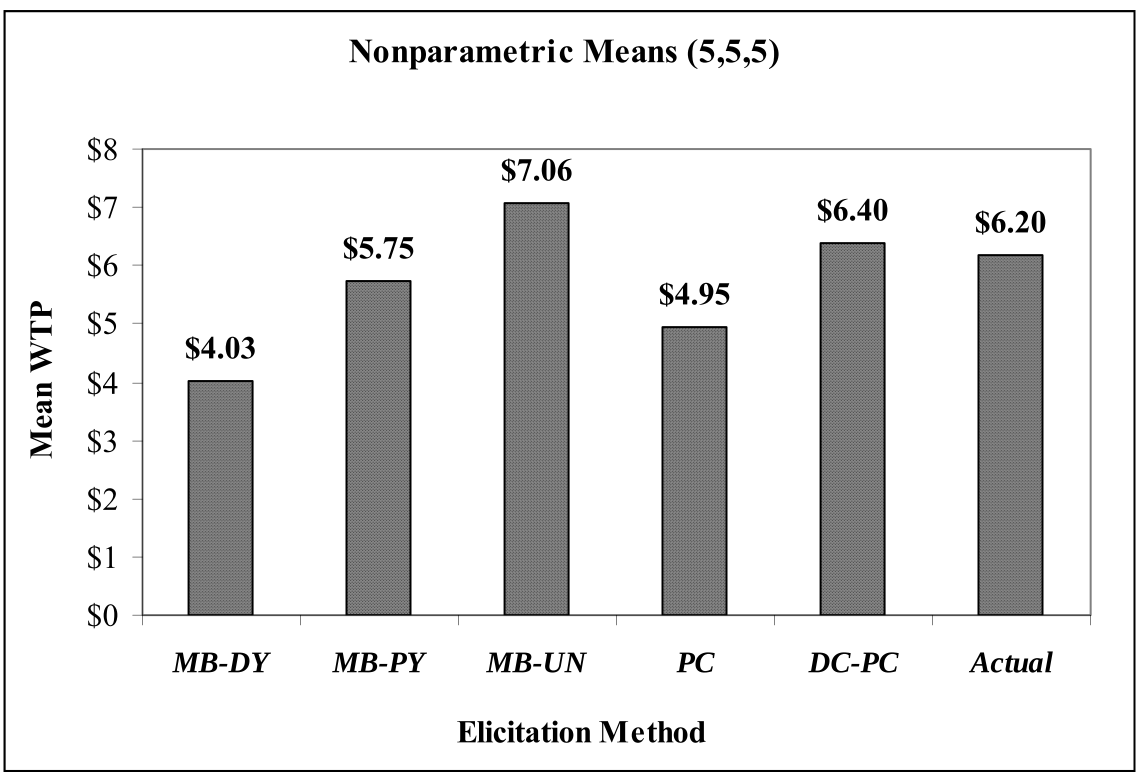

Figure 6: Nonparametric Reverse CDFs (Best Candidates) for the Private Goods Setting

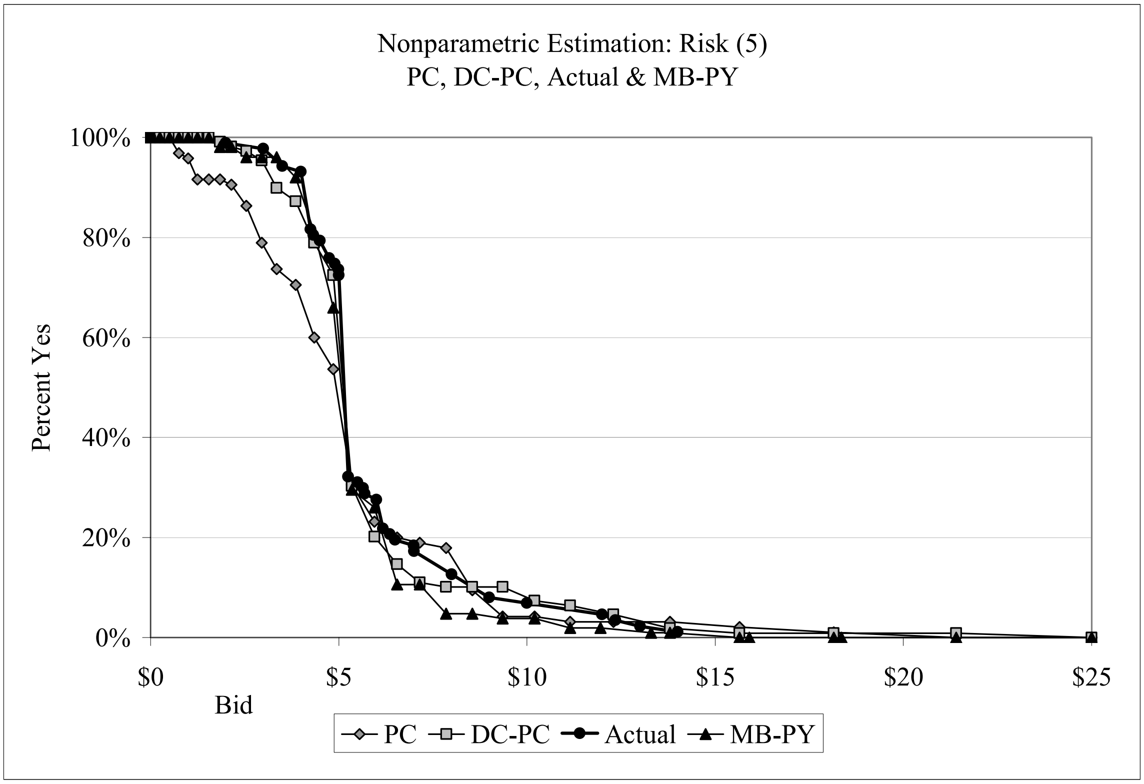

Figure 7: Nonparametric Reverse CDFs (Best Candidates) for the Public Goods Setting

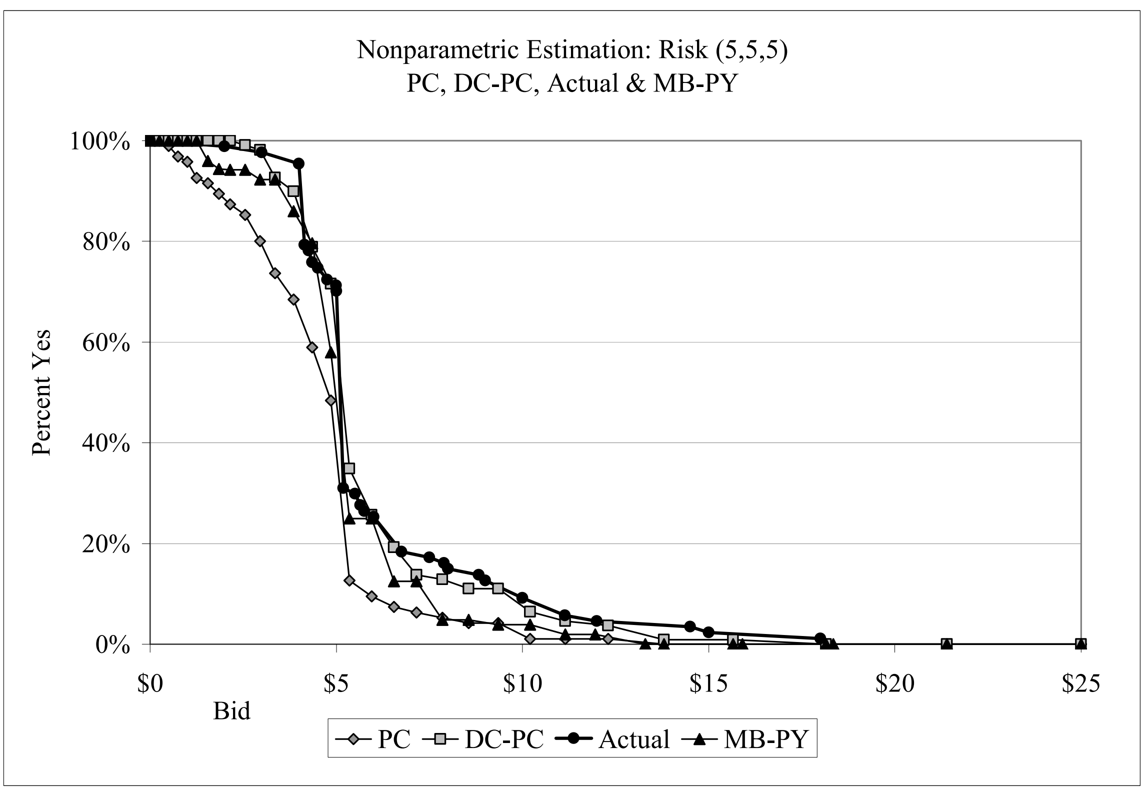

Table 3: Nonparametric Means for the Private Insurance Program (5)

	MBDC					
	DY	PY	UN	PC	DC-PC	Actual
n	104	104	104	95	109	87
Mean	**4.52**	**5.95**	**7.02**	**5.77**	**6.34**	**5.82**
(se)	*(0.081)*	*(0.091)*	*(0.109)*	*(0.122)*	*(0.110)*	*(0.378)*
[95% C.I.]	[4.36, 4.68]	[5.77, 6.12]	[6.80, 7.23]	[5.53, 6.01]	[6.12, 6.55]	[5.08, 6.56]

Table 4: Nonparametric Means for the Public Insurance Program (5,5,5)

	MBDC					
	DY	PY	UN	PC	DC-PC	Actual
n	104	104	104	95	109	87
Mean	**4.03**	**5.75**	**7.06**	**4.95**	**6.40**	**6.20**
(se)	*(0.086)*	*(0.085)*	*(0.119)*	*(0.080)*	*(0.095)*	*(0.279)*
[95% C.I.]	[3.87, 4.20]	[5.58, 5.91]	[6.83, 7.29]	[4.79, 5.10]	[6.21, 6.58]	[5.65, 6.74]

Table 5: Kolmogorov-Smirnov Significance Tests for the Private Good (5)

	PC	**PC PT**	**ACTUAL**
DY	**	***	***
PY	**	N	N
UN	***	***	***
PC	--	*	**
DC-PT	--	--	N

Table 6: Kolmogorov-Smirnov Significance Tests for the Private Good (5,5,5)

Kolmogorov Tests (555)			
	PC	**PC PT**	**Actual**
DY	***	***	***
PY	**	N	N
UN	***	***	***
PC	--	***	***
DC- PT	--	--	N

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