Unintentional Information Effects and the WTP-WTA disparity: Theory and Experimental Evidence

Abstract

Dozens of economics experiments have found a disparity between a consumer's willingness-to-pay and willingness-to-accept, prompting many researchers to assume the WTP-WTA disparity as a starting point for further inquiry into the substance of human economic behavior and modifications to existing welfare theory. However, formal mathematical model in the literature has examined the relationship between WTP-WTA disparities and experimental subject misconceptions arising from unintentional information effects. We extend the work of Pollak (1969) by introducing uncertainty into the formation of quantity-restricted welfare measures. The extension provides a formal model demonstrating that subject misconceptions alone can lead to a WTP-WTA disparity and contaminate experimental results. We also report on the results of new experiments that provide evidence in support of these findings and reconcile the broad set of apparently contradictory findings of other researchers.

Introduction and Background on the WTP-WTA Disparity

There have been dozens of economic experiments examining the difference between a consumer's willingness-to-pay (WTP) and her willingness-to-accept (WTA) and many have found a disparity between WTP and WTA (for a review, see Horowitz and McConnell, 2002). The widespread evidence that a consumer's willingness to accept may be larger than her willingness to pay has had tremendous practical consequences for

applied economists and for the broader theory of the rational paradigm in modeling economic behavior. Using static models of consumer choice, Pollak (1969), Randall and Stoll (1980), and Hanneman (1991) have demonstrated that quantity-restricted measures of compensating and equivalent welfare measures should be approximately equal, in the absence of income effects. Thus, the prediction of parity between WTP and WTA stands as pivotal testable implication of the neo-classical program.

Researchers interested in non-market valuation have used willingness-to-accept methods to value many items, ranging from genetically engineered foods (Lusk et al. 2004) to the value of a statistical life (Gurial et al. 2005). Other researchers have used this evidence to claim people value items they own more than items they don't own, i.e., there is an endowment effect (e.g., see Knetsch et al. 2001). Recent research has even used this framework to find an endowment effect in Chimpanzees (Brosnan et al. 2008).

While many studies have found a disparity, there are alternative hypotheses by researchers over the cause of these effects. One explanation is that the WTP-WTA disparity is driven by the closeness of substitutes. Hannemann (1991) uses a theoretical model to examine the effect of substitutes on the WTP-WTA disparity and finds that when a product has few close substitutes, there will be a greater WTP-WTA disparity. Several studies have used experimental methods to examine the effects of substitutes, and experimental results have generally confirmed Hannemann's (1991) theory (see review of studies in Horowitz and McConnell, 2002)

More recent evidence has highlighted the role of experience at reducing the WTP-WTA disparity. List (2003 , 2004a) found little difference between WTP and WTA for sportscards from consumers with a significant amount of experience buying and selling

sportscards. Inexperienced consumers, however, had a WTA that was several times higher than WTP. Experience has also shown to reduce the endowment effect in non-WTA-WTP studies (List 2004b). Recent theoretical evidence has supported experimental results finding experience should reduce the WTP-WTA disparity (Tsur, 2008).

Another area that has received significant attention is the role of experimental procedures on the WTP-WTA disparity. In the introduction of their review on WTP-WTA disparities, Horowitz and McConnell (2002) find no evidence that procedures matter in the set of WTP-WTA studies they examined. They go as far as to state: "high WTA/WTP ratios are not the results of experimental design features that would be considered suspect" (page 427). This is important, as if the disparity between WTP and WTA is only caused by misconceptions, many conclusions reached by other researchers could be false.

Despite the experimental evidence, there has been no theory or formal mathematical model about the relationship between the WTP-WTA disparity and misconceptions arising from unintentional information effects. Because of the absence of theoretical models on the subject along with a lack of experiments explicitly designed to test the claim by Horowitz and McConnell (2002), Plott and Zeiler (2005) ran several experiments in an effort to test the robustness of the WTP-WTA disparity. They identified a set of protocols for valuation experiments, which when implemented eliminated statistically significant differences between reported measures of WTP and WTA when selling coffee mugs. Their approach did not attempt to eliminate all sources of information, but instead implemented a set of standardized protocols.

Isoni, Loomes, and Sugden (forthcoming) found that the WTP-WTA gap still existed with lotteries but not with coffee mugs when implemting the same set of standardized procedures, and Plott and Zeiler (forthcoming) confirmed this result. Accepting the results of the Plott and Zeiler (2005) however, would not allow us to conclude whether the reduction of the WTP-WTA disparity they reported was achieved by preventing misconceptions, or by providing information that countered or dampened the information signals that provided the misconceptions.

In this paper, we extend the static theory of quantity-restricted welfare measurement by explicitly introducing uncertainty by appealing the work of Arrow and Debreu. The introduction of uncertainty shows the relationship between the WTP-WTA disparity and subject misconceptions about future states of the world. Our model does not rely on transactions costs, irreversibility, indivisibility, income effects, substitution effects or any other artifact previously mentioned in the literature as a possible explanation for the WTP-WTA disparity. Yet, our model demonstrates that informative signals imbedded in experimental protocols can create "misconceptions" when they are correlated with subject's future probability assessments, which in turn can cause a WTP-WTA disparity. The product of our theoretical investigation is two-fold. The theory provides (1) a refinement of the testable implications of the rational choice model, and (2) well-posed advice to experimental designers on techniques for avoiding experimental protocols that will unintentionally bias the responses of subject's probability assessments. We report the results of new experiments designed to control for potential subject misconceptions and discuss how our design avoids unintentional signals that are likely

imbedded in many of the protocols in the literature that report an observed WTP-WTA disparity.

The Model

An agent with von Neumann-Morgenstern type preferences derives utility from two goods, *X* and *Y*, which are consumed at two dates, $t = 0, 1$. The agent faces future uncertainty in the form of two states of nature at date $t = 1$, $s = 1, 2$. Both *X* and *Y* are freely traded in well functioning markets as contingent commodities at date $t = 0$. The states of nature relevant for exposing our results concern the value of good *X*. To develop these ideas clearly, we place the agent in the equilibrium of a representative agent endowment economy. So, we can refer to the relative price of good *X* and scarcity without equivocating. State, $s = 1$, is *X*-scarce relative to date $t = 0$ and state, $s = 2$, is *X*-abundant. Though, the presence of absence of scarcity, *per se*, is not vital to our analysis. Rather, in the context of WTP and WTA experiments, it provides a clear interpretation of the difference between the implications of our experimental design and those in the extant literature. The results that follow depend only on the possibility of correlation between features of an experimental protocol and future uncertainty, which will be made explicit shortly in our first theorem.

Within each date-state of our two date, two state model, the agent has a well-behaved preference ordering defined over, $X_{t,s}$, and a state-contingent numeraire commodity, $Y_{t,s}$ for $t = 0,1$ and s = $1,2$. In our notation, we suppress reference to $t = 1$ for convenience. These preferences are captured by the utility function of a state preference model with the form:

$$U(X,Y \mid \pi) = v(X_0, Y_0) + \delta\pi_i \cdot v(X_i, Y_i), \qquad \text{for } i = 1,2 \tag{1}$$

where, π_1 and π_2 represent subjective prior probabilities and δ represents a discount factor that the agent applies to $t = 1$.

The agent maximizes $U(X,Y|\pi)$ subject to $(X,Y) \in B(p)$, where B is the budget and p is a vector of equilibrium prices based on trade in contingent commodities at $t = 0$. The resulting contingent consumption plan for X and Y yield the agent's indirect utility function:

$$V(X^*, Y^* \mid \pi) = v(X_0^*, Y_0^*) + \delta\pi_i \cdot v(X_i^*, Y_i^*), \qquad \text{for } i = 1,2 \tag{2}$$

Below, we derive the theoretical analogs of the agent's willingness to pay for a change in X at date $t = 0$, which we will refer to as C, and willingness to accept compensation for a change in X at date $t = 0$, which we will refer to as E. These are, respectively, the *Hicksian* compensating and equivalent surplus, conditioned upon the agent's information set. Following the work of Freeman (1993) and Pollak (1969), our development uses the participant's restricted expenditure function. Thus, E is defined by the following expenditure minimization problem:

$$\begin{aligned} &\min M = p_Y \cdot Y + p_X \cdot X \\ &s.t. \\ U(X,Y|\pi) &= v(X_0, Y_0) + \delta\pi_i \cdot v(X_i, Y_i), \qquad \text{for } i = 1,2 \\ &= \hat{u} \end{aligned} \tag{3}$$

The solution to this problem $M(X^*, Y^* | \hat{u}, \pi)$ represents the amount of income, measured in terms of the date $t = 0$ numeraire, needed to give the agent the utility, $\hat{u}$. In the absence of transactions costs, as we have assumed, the difference in expenditure needed to produce equivalent utility for two different levels of X can be defined using the agent's

restricted expenditure function, $\hat{M}(X^*, Y^* | \hat{u}, \pi)$, by adding the constraint $X_0 = \hat{X}$ to (3). [1]

The associated quantity restricted welfare measures *E* and *C* are defined as:

$$E(\pi) = \hat{M}(X^*, Y^* | \hat{\hat{u}}, \pi) - \hat{\hat{M}}(X^*, Y^* | \hat{\hat{u}}, \pi) \qquad (4a)$$

Similarly, an expression for *C* can be developed.

$$C(\pi) = \hat{M}(X^*, Y^* | \hat{u}, \pi) - \hat{\hat{M}}(X^*, Y^* | \hat{u}, \pi) \qquad (4b)$$

Equations (4a) and (4b) give the difference in expenditures required to produce equivalent utility for the two different levels of the good *X*. [2]

Welfare Measurement with Incomplete Information

To our model of uncertainty, we introduce an information source. The information source sends "messages/signals," which cause the agent to revise her probability estimates of future states of the world, or *beliefs,* according to *Bayes'* Theorem.

Formally, the information source is a *Markov* matrix, *L(θ)*. The columns of this matrix represent conditional probability distributions over the states of nature at *t =1,* or *messages. L(θ)* describes the probability of receiving a particular message conditional on a particular state being the "true" state. The agent's state space is defined with respect to the scarcity of good *X*, without loss of generality we consider a two-dimensional state

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[1] The quantity restrict expenditure function $\hat{M}(X^*, Y^* | \hat{u}, \pi)$ is defined by the *Hicksian* demand functions for *X* and *Y*, which depend on the level of utility $\hat{u}$ and quantity restricted amount of *X* at *t = 0*, $\hat{X}$, as well as the equilibrium prices of the traded commodities, *p*. Thus,
$\hat{M}(X^*, Y^* | \hat{u}, \pi) \equiv M\left(X^*(p, \hat{u}, \hat{X}, \pi), Y^*(p, \hat{u}, \hat{X}, \pi)\right)$.

[2] The notation for the two different level of expenditure, $\hat{M}$ & $\hat{\hat{M}}$, refers the income required to achieve a given level of utility, $\hat{\hat{U}}$, with two different amounts of the quantity restricted good, $\hat{X}$ & $\hat{\hat{X}}$, respectively.

space. Thus, there are two possibilities regarding the messages: either they signal scarcity relative to the agent's prior beliefs, or they do not. Our model exposes the extent to which spurious signals regarding the scarcity of the good in question, *X*, generate *misconceptions*, affecting *C & E*.

Specifically, we assign to the label $m = 1$ the *X*-scarce signal and to $m = 2$ the *X*-abundance signal. We use $\theta \in [0,1]$ as a measure of the informative potential of the matrix of signals $L(\theta)$, where $\theta = 0$ corresponds to a matrix of messages that are completely uninformative and $\theta = 1$ indicates that the matrix $L(\theta)$ contains messages that perfectly inform the agent. Increases in θ indicate a matrix of signals that is "more informative" in the sense of Blackwell (1953), or equivalently signals that are more *differentiated*. Denote the agent's *potential posterior* matrix of beliefs conditional on the set of signals in $L(\theta)$, $\Pi(\theta)$, where θ refers to the Blackwell ranking of the informative potential of $L(\theta)$.

First, consider an $L(\theta)$ matrix that is non-informative in the Blackwell (1953) sense. When the space of signals is uninformative, $L(0)$, then the participant's potential posterior matrix $\Pi(\theta)$ merely returns the participant's prior beliefs.[3] For the reader's convenience, we repeat the relationship between π, $\Pi(\theta)$, and $L(\theta)$ for a 2-dimensional state space:

[3] An observationally equivalent interpretation to a non-informative matrix of signals occurs in our experiment when all participants receive the *same* signal. This equivalence prevents us from identifying the *ex ante* value for *C & E*, even though we can measure the difference between them.

$$\pi = \begin{pmatrix} \pi_1 \\ \pi_2 \end{pmatrix}; L(0) = \begin{pmatrix} l_{1,1} & (1 - l_{1,1}) \\ l_{1,1} & (1 - l_{1,1}) \end{pmatrix} \Rightarrow \Pi(0) = \begin{pmatrix} \pi_1 & \pi_1 \\ \pi_2 & \pi_2 \end{pmatrix}$$

$$\text{where, } \Pi_{s,m} = \frac{\pi_s l_{s,m}}{\sum_s \pi_s l_{s,m}}$$

It is easy to show in the non-informative case that $E(\pi,0)=E(\pi)=C(\pi)=C(\pi,0)$, our first result.[4] This result demonstrates that when information is excluded from the analysis, the predictions of our model are the same as those of the standard static analysis.

How, then, does an informative signal matrix represent a source of "misconceptions" and affect the agent's value for X? We define the *signal contingent willingness to accept*, $E(\Pi_m)$, and the *signal contingent willingness to pay*, $C(\Pi_m)$, for $m = 1,2$ as:

$$\begin{aligned} E(\Pi_m) &= \hat{M}(X^*, Y^* | \hat{\hat{u}}, \Pi_m) - \hat{\hat{M}}(X^*, Y^* | \hat{\hat{u}}, \Pi_m) \\ C(\Pi_m) &= \hat{M}(X^*, Y^* | \hat{u}, \Pi_m) - \hat{\hat{M}}(X^*, Y^* | \hat{u}, \Pi_m) \end{aligned} \qquad \text{(5a,5b)}$$

The notation Π_m, $m = 1,2$, in (5a) and (5b) refers to the vector of posterior beliefs associated with each of the signals in the potential posterior matrix, $\Pi(\theta)$. Our analysis allows us to give conditions under which the signal contingent measures of *C & E* are equivalent, which we codify in Theorem 1:

Theorem 1: If $\Pi_m = \Pi_n$, then $E(\Pi_m) - C(\Pi_n) \cong 0$ for small changes in X.

Proof: Using 5a, 5b, and that the levels of utility used to define C & E imply that the agent spends all of her income, we have:

[4] This follows immediately from Theorem 1, which will be introduced and proved shortly.

$\hat{M}(X^*, Y^* | \hat{u}, \Pi_m(\theta)) = \hat{\hat{M}}(X^*, Y^* | \hat{\hat{u}}, \Pi_m(\theta)) = \overline{M}$ *, which implies*

$E(\Pi_m) - C(\Pi_m) = \hat{M}(X^*, Y^* | \hat{\hat{u}}, \Pi_m) + \hat{\hat{M}}(X^*, Y^* | \hat{u}, \Pi_m) - 2\overline{M}$. *Since, M(•) is monotonic in u and* $\hat{\hat{u}} > \hat{u}$ *the following approximation applies:*

$$E(\Pi_m) - C(\Pi_m) =$$
$$\overline{M} + \frac{\partial \hat{M}(X^*, Y^* | \hat{u}, \Pi_m)}{\partial u} \bullet \Delta u + \overline{M} - \frac{\partial \hat{\hat{M}}(X^*, Y^* | \hat{\hat{u}}, \Pi_m)}{\partial u} \bullet \Delta u - 2\overline{M},$$

which recognizes that the change in utility has the same absolute magnitude in both expressions.

$E(\Pi_m) - C(\Pi_m) = \left[\frac{\partial \hat{M}(X^*, Y^* | \hat{u}, \Pi_m)}{\partial u} - \frac{\partial \hat{\hat{M}}(X^*, Y^* | \hat{\hat{u}}, \Pi_m)}{\partial u} \right] \bullet \Delta u$. *Continuity of the expenditure function guarantees that for sufficiently small changes in the restricted quantity of X, this is approximately zero.* ■

Corollary 1: If $\Pi_m \neq \Pi_n$, *then* $E(\Pi_m) - C(\Pi_n) \neq 0$ *for small changes in X.*

Comment: In non-trivial cases, the conclusion of Theorem 1 cannot be sustained when the posterior beliefs are differentiated by disparate signals.

Theorem 1 is a simple restatement of static *Hicksian* welfare theory: Willingness to pay and willingness to accept should be equal to their theoretical counterparts *C* & *E*, and to each other (approximately). However, *Theorem 1* exposes a critical assumption upon which the conclusions of the *Hicksian* interpretation rests. Namely, *C & E* are equal only when their computations refer to the same information set (i.e., the same vector of beliefs regarding future states of the world). Thus, we are left to conclude that

observed differences between *C & E* can be caused by differences in the signals received by experimental subjects during the elicitation process.

Theorem 1 does not predict the direction or magnitude of the difference between *C* and *E* that might be expected in an experiment. With mild restrictions such predictions can be made as *Theorem 2* illustrates.

Theorem 2: Assuming a constant marginal utility of income, differentiated signals regarding the scarcity of X, $\Pi_m \neq \Pi_n$, result in $E(\Pi_m) > C(\Pi_n)$, *when E & C measure the welfare associate with a small change in the quantity restricted good.*

Proof: Without loss of generality, let signal "m" be associated with the scarce state, s = 1. To save on notation, we will equivocate between the posterior vector of beliefs, Π_m, *and its first component,* π_m, *which corresponds to the probability of s = 1. So, we write* $\Pi_m > \Pi_n$, *which means that the scarce state is more likely when signal "m" is received.* From (5a,5b), we have

$$
\begin{aligned}
E(\Pi_m) - C(\Pi_n) = \hat{M}(X^*, Y^* | \hat{u}, \Pi_m) &+ \frac{\partial \hat{M}(X^*, Y^* | \hat{u}, \Pi_m)}{\partial u} \cdot \Delta u \\
- \hat{\hat{M}}(X^*, Y^* | \hat{u}, \Pi_m) &- \frac{\partial \hat{\hat{M}}(X^*, Y^* | \hat{u}, \Pi_m)}{\partial u} \cdot \Delta u \\
- \hat{M}(X^*, Y^* | \hat{u}, \Pi_n) &+ \hat{\hat{M}}(X^*, Y^* | \hat{u}, \Pi_n)
\end{aligned}
$$

The derivatives are the inverse of the marginal utility of income from the primal-dual relationship between the agent's utility maximization and expenditure minimization programs. The sum of the remaining terms is positive because signal "m" implies scarcity and thus the expenditure required to achieve a given level of utility is greater.■

In the context of a "small stakes" experiment, *Theorem 2* implies that a protocol signaling future scarcity of the experimental good will increase *E* and *C*, *ceteris paribus.* The model also suggests predictions consonant with observations from previous empirical work regarding the affects of experience and learning on the WTP-WTA gap, which we codify in *Theorem 3* (proof provided in appendix A).

Theorem 3: Holding the marginal utility of income constant, agents with experience (i.e., informed priors) have a smaller WTA/WTP gap, ceteris paribus.

Note that our model did not include transactions costs, irreversibility, indivisibility, income effects, substitution effects or any other artifact previously mentioned in the literature as a possible explanation for the WTP-WTA disparity. Thus, any of the effects previously mentioned in the literature could be introduced into our model with effect of exacerbating any differences we identify.

An Example

Below, the Lagrangian formulation of a (quantity-restricted) expenditure minimization, as in Pollak (1969), for two goods y , the numeraire, and x, which is quantity-restricted at date $t = 0$ is given (dots are used to designate state contingent commodity prices at date, $t = 1$):

$$Min\ \mathcal{L} = p_x\breve{x}_0 + p_y y_0 + \dot{p}_x x_1 + \dot{p}_y y_1 + \ddot{p}_x x_2 + \ddot{p}_y y_2 + \lambda\big(\overline{u} - Ln(\breve{x}_0 \cdot y_0) - \pi Ln(x_1 \cdot y_1) - (1-\pi)Ln(x_2 \cdot y_2)\big).$$

The associated first-order conditions are:

$$\mathcal{L}_{y_0} = p_y - \lambda y_0^{-1} = 0,$$
$$\mathcal{L}_{x_1} = \dot{p}_x - \lambda\pi x_1^{-1} = 0,$$
$$\mathcal{L}_{y_1} = \dot{p}_y - \lambda\pi y_1^{-1} = 0,$$
$$\mathcal{L}_{x_2} = \ddot{p}_x - \lambda\,(1-\pi)x_2^{-1} = 0,$$
$$\mathcal{L}_{y_2} = \ddot{p}_y - \lambda\,(1-\pi)y_2^{-1} = 0,$$
$$\mathcal{L}_{\lambda} = \overline{u} - Ln(\breve{x}_0 \cdot y_0) - \pi Ln(x_1 \cdot y_1) - (1-\pi)Ln(x_2 \cdot y_2) = 0.$$

Thus:

$$\overline{u} = Ln(\breve{x}_0 \cdot y_0) + \pi Ln(x_1 \cdot y_1) + (1-\pi)Ln(x_2 \cdot y_2)$$

$$\overline{u} = Ln(\breve{x}_0 \cdot y_0) + Ln\left(\left(\frac{\pi p_y y_0}{\dot{p}_x}\right)\cdot\left(\frac{\pi p_y y_0}{\dot{p}_y}\right)\right)^{\pi} + Ln\left(\left(\frac{(1-\pi)p_y y_0}{\ddot{p}_x}\right)\cdot\left(\frac{(1-\pi)p_y y_0}{\ddot{p}_y}\right)\right)^{(1-\pi)}$$

$$y_0^* = \left(\frac{e^{\overline{u}}}{\breve{x}_0}\right)^{\frac{1}{3}} \cdot \left[\frac{\left(\dot{p}_y \dot{p}_x\right)^{\pi} \cdot \left(\ddot{p}_y\, \ddot{p}_x\right)^{1-\pi}}{p_y^2 \cdot \pi^{(2\pi)} \cdot\ (1-\pi)^{[2(1-\pi)]}}\right]^{\frac{1}{3}}$$

The Expenditure Function:

$$M(x, y; \breve{x}, \pi) = p_x\breve{x}_0 + p_y y_0^* + \dot{p}_x x_1^* + \dot{p}_y y_1^* + \ddot{p}_x x_2^* + \ddot{p}_y y_2^*$$

$$M(x,y;\breve{x},\pi) = p_x\breve{x}_0 + p_y\left\{\left(\frac{e^{\overline{u}}}{\breve{x}_0}\right)^{\frac{1}{3}}\cdot\left[\frac{\left(\dot{p}_y\dot{p}_x\right)^{\pi}\cdot\left(\ddot{p}_y\,\ddot{p}_x\right)^{1-\pi}}{p_y^2\cdot\pi^{(2\pi)}\cdot\ (1-\pi)^{[2(1-\pi)]}}\right]^{\frac{1}{3}}\right\} + \dot{p}_x\left\{\left(\frac{\pi}{\dot{p}_x}\right)\cdot\left[\frac{p_y\cdot\left(\dot{p}_y\dot{p}_x\right)^{\pi}\cdot\left(\ddot{p}_y\,\ddot{p}_x\right)^{1-\pi}}{\pi^{(2\pi)}\cdot\ (1-\pi)^{[2(1-\pi)]}}\right]^{\frac{1}{3}}\cdot\left(\frac{e^{\overline{u}}}{\breve{x}_0}\right)^{\frac{1}{3}}\right\}$$

$$+\dot{p}_y\left\{\left(\frac{\pi}{\dot{p}_y}\right)\cdot\left[\frac{p_y\cdot\left(\dot{p}_y\dot{p}_x\right)^{\pi}\cdot\left(\ddot{p}_y\,\ddot{p}_x\right)^{1-\pi}}{\pi^{(2\pi)}\cdot\ (1-\pi)^{[2(1-\pi)]}}\right]^{\frac{1}{3}}\cdot\left(\frac{e^{\overline{u}}}{\breve{x}_0}\right)^{\frac{1}{3}}\right\}$$

$$+\ddot{p}_x\left\{\left(\frac{(1-\pi)}{\ddot{p}_x}\right)\cdot\left[\frac{p_y\cdot\left(\dot{p}_y\dot{p}_x\right)^{\pi}\cdot\left(\ddot{p}_y\,\ddot{p}_x\right)^{1-\pi}}{\pi^{(2\pi)}\cdot\ (1-\pi)^{[2(1-\pi)]}}\right]^{\frac{1}{3}}\cdot\left(\frac{e^{\overline{u}}}{\breve{x}_0}\right)^{\frac{1}{3}}\right\}$$

$$+\ddot{p}_y\left\{\left(\frac{(1-\pi)}{\ddot{p}_y}\right)\cdot\left[\frac{p_y\cdot\left(\dot{p}_y\dot{p}_x\right)^{\pi}\cdot\left(\ddot{p}_y\,\ddot{p}_x\right)^{1-\pi}}{\pi^{(2\pi)}\cdot\ (1-\pi)^{[2(1-\pi)]}}\right]^{\frac{1}{3}}\cdot\left(\frac{e^{\overline{u}}}{\breve{x}_0}\right)^{\frac{1}{3}}\right\}$$

Which reduces to:

$$M(x,y;\overline{u},\breve{x},\pi) = p_x\breve{x}_0 + 3\left(\frac{e^{\overline{u}}}{\breve{x}_0}\right)^{\frac{1}{3}}\cdot\left\{\left[\frac{p_y\cdot\left(\dot{p}_y\dot{p}_x\right)^{\pi}\cdot\left(\ddot{p}_y\,\ddot{p}_x\right)^{1-\pi}}{\pi^{(2\pi)}\cdot\ (1-\pi)^{[2(1-\pi)]}}\right]^{\frac{1}{3}}\right\}$$

Using the definitions of $E(\cdot)$ and $C(\cdot)$ for two different levels of the quantity-restricted good, $\breve{x}_0 < \hat{x}_0$:

$$E = 3\cdot\left\{\left[\frac{p_y\cdot\left(\dot{p}_y\dot{p}_x\right)^{\pi}\cdot\left(\ddot{p}_y\,\ddot{p}_x\right)^{1-\pi}}{\pi^{(2\pi)}\cdot\ (1-\pi)^{[2(1-\pi)]}}\right]^{\frac{1}{3}}\right\}\cdot\left(\frac{e^{\hat{u}/3}-e^{\breve{u}/3}}{\hat{x}_0^{1/3}}\right)$$

$$C = 3\cdot\left\{\left[\frac{p_y\cdot\left(\dot{p}_y\dot{p}_x\right)^{\pi}\cdot\left(\ddot{p}_y\,\ddot{p}_x\right)^{1-\pi}}{\pi^{(2\pi)}\cdot\ (1-\pi)^{[2(1-\pi)]}}\right]^{\frac{1}{3}}\right\}\cdot\left(\frac{e^{\hat{u}/3}-e^{\breve{u}/3}}{\breve{x}_0^{1/3}}\right)$$

Notice, that if subjective state probability estimates are the same for both welfare measures, then $\frac{E}{C}\approx 1$.

$$\frac{E}{C} = \left(\frac{\breve{x}_0}{\hat{x}_0}\right)^{\frac{1}{3}}$$

A Theory of Misconceptions in Valuation Experiments

Assuming incentive compatible methods of elicitation are used when conducting experiments, our analysis suggests two factors may affect *announcements* of *C & E*: signals regarding the scarcity of the good being valued and an agent's preferences. As long as the signal is held constant (Theorem 1), our model does not suggest a divergence between *C & E* based solely on agent preferences. This insight suggests protocols used in previous studies finding a significant WTP-WTA disparity sent signals to experimental subjects that were *systematically differentiated* as a result of the experimental design. What is troubling about this is that any feature of a protocol exposing experiment participants to differences in instructions, auction methods, grouping, or other aspects of the experiments design before collecting their announcement cannot be excluded as a source of differentiated signals *a priori.*

Despite a long list of possible causes for the disparity results reported in the literature, there is one design feature common to experiments finding a difference between WTA and WTP that seems a likely source of spurious signals and to which we turn our attention for the remainder of the paper. Prior experiments conducted begin by dividing participants into separate groups based on whether they are asked to contemplate an increase or decrease in the good being valued. Specifically, when participants are assigned to a WTP group and asked how much they would pay for an improvement, this can give the impression of a willing seller, or that there is a *likely abundance* of the good. Receiving a message regarding the abundance of the good being valued, according to our model, should generated a *downward bias* in the participant's announcement relative to her *ex ante* value. Likewise, when participants are assigned to a WTA group and asked how much they would be willing to accept for a decrement, this can give the impression of a willing buyer, or that there is a *likely scarceness* of the good. Receiving this sort of message should generate an *upward bias* in the participant's announcement relative to her *ex ante* value.[5] According to our model, this sort of signal differentiation can generate a divergence between *C & E* of the sort typically observed in the literature.

To provide evidence for our theory, we have designed a new experiment that elicits WTP and WTA and eliminates treatment differences which might generate informative signals. Instead of using rigorous training rounds in an attempt to eliminate WTP-WTA disparities (e.g. Plott and Zeiler 2005), our experiment provided participants with implicit signals on the relative scarcity of the goods. To illustrate the phenomenon

[5] Plott and Zeiler (2005) raise this issue on pages 537-538 when they discuss how simply being asked to "buy" or "sell" could create a WTP-WTA disparity.

identified in our model, the experimental treatments described below were designed to change the degree of signal differentiation between the WTA and WTP participants.

Experimental Design

We designed and conducted a laboratory experiment to further examine how implicit signals of scarcity can cause a WTP-WTA disparity. One hundred and twenty-seven undergraduate economics students from a central Pennsylvania University took part in this study in late April and early May of 2007. Participants were each paid $10 for their participation.

The 2nd price auction works as follows: (1) each participant places a *bid* on the food products; (2) the bids are ranked from highest to lowest; and (3) the top-bidding participant wins the auction and pays the price equal to the 2nd highest bid. We used the 2nd price auction because it is demand-revealing and because of its widespread use in experiments that have elicited a WTP-WTA disparity (e.g., see Shogren et al. 1994).

The products and treatments

Participants considered two products in this experiment: a standard size Hershey's chocolate bar (1.55 oz.) and a standard size Dove chocolate bar (3.53oz). Chocolate bars were used largely because it was easy to find a more-commonly consumed product (the Hershey's bar) and a less-commonly consumed product (the Dove bar). Further, candy bars have been used in other WTP-WTA experiments (e.g., see Shogren et al. 1994). During the experiment, we described the Hershey's bar as the conventional bar, and the Dove bar as the premium bar. Participants were endowed with one type of bar or the other, and placed bids in order to exchange the bar they received for the alternative bar.

If a participant received the conventional bar, her bid represented her willingness to pay to exchange the conventional bar for the premium bar. If a participant received the premium bar, her bid represented her willingness to accept to exchange the premium bar for a conventional bar.

We had three treatments in this experiment. A summary of the number of participants in each treatment is presented in table 1. The treatments differed based on the signals of product scarcity to participants.

- Treatment A (control groups) – In treatment A we had two groups, and these were the control groups participating in a conventional WTP-WTA experiment. In one group, the participants bid to exchange a conventional chocolate bar for a premium chocolate bar. These participants received a packet that only outlined instructions for this willingness-to-pay session. In the other group, participants in treatment A bid to receive compensation to exchange a premium chocolate bar for a conventional chocolate bar. These participants received a packet that only outlined instructions for the willingness-to-accept session. Participants in the two groups of treatment A bid separately from each other and likely did not know there was a session endowing participants with the bar they were bidding to receive.
- Treatment B – in this treatment, some participants are bidding in a WTP session, and some in a WTA session, but all participants are in the same room and the groups are run simultaneously. The set of participants bidding their WTP for a premium bar have their bids counted only against the other WTP bids, while the participants bidding their WTA for a conventional bar have their bids counted

only against other WTA bids. The packets participants receive are identical to those participants receive in the control group. However, both those who are in the WTP session and WTA session of treatment B hear both sets of instructions, and participants are explicitly told that group placement is assigned randomly. The determination of top bidder and winning prices for each session are done publicly, so the WTP bidders can see the bids of the WTA bidders, and vice versa

- Treatment C – In treatment C, all participants now place two bids. One bid is for their WTP should they be endowed with the conventional candy bar, while the other is for their WTA should they be endowed with the premium candy bar. In this treatment, participants do not know which bar they are endowed with prior to the bidding. Participants are informed that when the bidding in all rounds concludes, a coin flip determines whether they are in the WTP session or the WTA session (i.e., their endowment).

The Rounds of Bidding

Following commonly-used procedures in WTP-WTA experiments, we conducted five rounds of bidding (e.g., see Shogren et al. 1994). Participants were informed prior to bidding that there would be five rounds, and that only one of the five rounds would count as binding (to avoid bid reduction due to the possibility of winning more than one candy bar).

Steps in the Experiment

The experimental auction had seven steps:

Step one. Participants were told that the session would take approximately an hour and that they would be paid $10 for their participation. The monitor provided participants a short instruction packet. The packet differed based on the treatment.

Step two. Participants are endowed with a candy bar. Those in the WTP treatments are endowed with a conventional bar, while those in a WTA treatment are endowed with a premium chocolate bar. Those in treatment C are told they will be endowed with a bar, but are not provided the bar prior to the experiment. All participants were given the opportunity to inspect both the conventional and premium chocolate bars at this point.

Step three. Participants learn about the 2nd price auction with both written and oral instructions. Participants were explicitly informed that it was in their best interest to

bid truthfully and were given a chance to ask questions. Participants were also given instructions based on the specific treatment they were in.[6]

Step four. Participants placed their round-one bids for the candy bar. After all the bids in round 1 were collected, bids were posted. This follows standard protocols in WTP-WTA experiments (e.g. Shogren et al. 1994). In the WTP group, the highest and second highest bids were posted. In the WTA group, the lowest and second lowest bids for the WTA treatment, or both were posted. For treatments B and C, both were posted.

Step five. Step 5 was repeated for rounds 2-5.

Step six. The binding round was determined by a random draw, and winners and prices were determined. For participants in treatment C who did not know whether they would be in a WTP group or WTA group, a coin was flipped to determine whether they were endowed the conventional bar and were in the WTP group or the premium bar and were in the WTA group.

Step seven. Participants completed a short survey containing demographic and background information. Participants were then dismissed one-by-one, at which point any individual with a winning bid exchanged the bar he/she was endowed with for the other bar and paid or received compensation.

Most experiments that test the disparity between WTP and WTA simply provide experimental participants with a packet describing their particular valuation task. That is, if a participant is in a WTP session, the participant only sees the instructions on how to

[6] Recall Plott and Zeiler (2005) found that when four features were implemented: anonymity, use of an incentive compatible mechanism, paid practice rounds, and extensive training sessions, WTP-WTA gaps for coffee mugs were eliminated. While our experiments preserve anonymity and use an incentive compatible mechanism, we do not use paid practice rounds nor do we have extensive training sessions. These features were purposefully excluded so we could isolate the impact of signals of scarcity on the WTP-WTA disparity.

proceed in the willingness-to-pay session. Likewise, if a participant is in a WTA session, the participant only sees the instructions on how to proceed in the willingness-to-accept experiment. Our experiment is designed to test whether this design feature contributes to the WTP-WTA disparity. We conjecture that Treatment B provides a less informative signal of scarcity to participants than the control, Treatment A, because a participant's placement into a WTP or WTA session is random and transparent in Treatment B. In treatment C, we eliminated signal differences between the two sessions by exposing all participants to the same set of protocols prior to the coin-flip at the end of the experiment, which determines the participant's assignment to the WTP group or the WTA group.

Results

Both WTP and WTA bids are presented in table 2, and the differences in WTP and WTA are shown in table 3 and figure 1. Several facts are worth noting. First, in rounds 1 and 2, there is a disparity between WTP and WTA for all treatments. However, it is smaller for treatment C than for the other treatments. This provides evidence that, even for initial bids, the signal sent to participants is less differentiated in treatment C than in the other treatments. The initial rounds is worth examining in isolation because of an increase body of evidence suggesting posting prices affects bids (Corrigan and Rousu 2010, 2006; Nunes and Boatwright 2004).

By round 3, the WTP-WTA disparity has been eliminated for treatment C but there is still a statistically significant disparity between WTP and WTA in the other treatments. In rounds 4 and 5, there is no longer a statistically significant WTP-WTA disparity in either the control group or in treatment C. However, we still see a statistically significant disparity in treatment B. This seems a bit counter-intuitive, since

we hypothesized that the signals given to participants about the value of the goods in treatment B were less differentiated than in the control groups, and thus the WTP-WTA disparity would likely be smaller. A closer examination of the both the willingness-to-pay and willingness-to-accept bids will provide more insight regarding this finding.

Table 4 shows the differences in willingness-to-pay bids and willingness-to-accept bids across treatments. Part A examines the WTP bids to exchange the conventional bar for the premium bar. We find no statistically significant difference in bids across treatments, except in round 5. The difference in WTP bids between the control group and treatment B in round 5 is statistically significant at the 10% level (P=.07 using 2-sided t-test). This occurs because, as we saw in table 3 and figure 1, the WTP bids increase more in the control group. The fact that bids increased significantly in the control group seems consistent with the body of work that shows that bids in WTP auctions tend to increase when (high) prices are posted (e.g. see Corrigan and Rousu, Nunes and Boatwright). What is interesting is that when participants were also exposed to the WTA bids; their WTP bids did not increase at a similar rate.

Part B of table 4 shows how WTA bids differed across treatments. In treatment C when participants did not know whether they would be in the WTP or WTA treatment, WTA bids are lower than in either the control group or the group when WTP and WTA bidders were in same room (treatment B).[7] This provides some support of the idea that participants may have misconceptions based on different instructions given to different groups. Contrast this to the WTP sessions, which were likely more familiar to participants as it is similar to many conventional auctions, where we saw less difference.

[7] While t-tests are reported, we also used the non-parametric Wilcoxon Rank-Sum tests and found similar results.

This also provides support for the idea that experience or practice may help reduce the signals that can lead to participant misconceptions, one of the findings from Plott and Zeiler (2005).

Conclusion

Dozens of papers have found a WTP-WTA disparity in laboratory experiments. Most (or all) of these studies attempted to carefully control for the signals that could cause such disparities. However, in this paper we theorize that unintentional signals are present based on the design of experimental auctions. We present a model showing how unintentional signals can lead to subject misconceptions, which cause WTP-WTA disparities. Our model also shows that these disparities can exist even when eliminating the possibility of other (more common) explanations for the WTP-WTA disparity.

We conducted new experiments where we controlled for the levels of implicit signals given to participants. A control group that followed many standard procedures found a larger WTP-WTA disparity than groups that followed a protocol that more carefully controlled for the implicit signals. The experiments show that the presence of informative signals imbedded in the protocols causes a significant WTP-WTA disparity.

Future work on this subject is warranted. Our experiments only examined the incentive compatible 2nd price auction. Replications using the BDM mechanism or random nth-price auction could be valuable. Also, our experiment used an auction setting, but the WTP-WTA disparity has also been found in choice experiments (e.g., see Grutters et al. 2008). Research that examines whether potentially informative signals lead to a gap in choice experiments would also be useful.

The model and results provide additional evidence supporting Plott and Zeiler (2005, 2010), indicating that many of the disparities that were found could simply have been the result of subjects receiving false signals from the experimental design. The results also point to a larger issue in the design of human subject experiments: researchers should take extra caution to try to ensure procedures will not provide unintentional signals which could bias their results.

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Table 1: Treatment summary and participants in each treatment (N=127)

	WTP participants	WTA participants
Control Groups (Treatment A: Separated bidders	17	23
Treatments B: Bidders only bid in WTP or WTA session, but both WTP and WTA sessions run in same room.	25	21
Treatment C: Bidders placed WTP and WTA bids simultaneously	41*	41*

* These were the same 41 participants

Table 2: WTP and WTA bids for the three treatments (N=127)

Part A: Mean Bids (standard deviation in parenthesis)

	Treatment	Round 1	Round 2	Round 3	Round 4	Round 5
WTP - mean	A (Control Group)	$0.60 (0.59)	$0.61 (0.53)	$0.80 (0.53)	$1.12 (0.81)	$1.32 (0.83)
	B	$0.66 (0.49)	$0.72 (0.47)	$0.82 (0.49)	$0.86 (0.48)	$0.94 (0.53)
	C	$0.78	$0.70	$0.91	$0.99	$0.97
		(0.85)	(0.70)	(1.01)	(0.92)	(1.03)
WTA - mean	A (Control Group)	$2.77 (2.16)	$2.45 (1.90)	$2.03 (1.67)	$1.76 (1.43)	$1.63 (1.65)
	B	$2.35 (1.76)	$2.08 (1.14)	$1.90 (1.10)	$1.85 (1.25)	$1.86 (1.41)
	C	$1.12	$1.07	$1.00	$1.03	$0.97
		(0.95)	(0.90)	(0.93)	(1.28)	(1.20)

Part B: Median Bids

	Treatment	Round 1	Round 2	Round 3	Round 4	Round 5
WTP - median	A (Control Group)	$0.35	$0.5	$0.75	$1	$1.4
	B	$0.52	$0.75	$0.85	$0.80	$0.95
	C	$0.50	$0.50	$0.60	$0.75	$0.50
WTA - median	A (Control Group)	$2	$2	$1.7	$1.5	$1
	B	$1.69	$2.00	$1.75	$1.25	$1.01
	C	$0.85	$0.80	$0.75	$0.75	$0.50

Table 3: Comparison of the WTP-WTA disparity across treatments (N=127)

Part A – mean disparity (t-values in parenthesis)

	Round 1	Round 2	Round 3	Round 4	Round 5
Control Groups (Treatment A): Separate	$2.17*** (4.58)	$1.84*** (4.39)	$1.23*** (3.32)	$0.64 (1.78)	$0.31 (0.78)
Treatment Group B: Both WTA and WTP in same group	$1.69*** (4.25)	$1.36*** (5.12)	$1.08*** (4.15)	$0.99*** (3.43)	$0.92*** (2.81)
Treatment Group C: Bid on both WTA and WTP	$0.34** (2.41)	$0.37** (2.56)	$0.09 (0.39)	$0.04 (0.16)	$0.00 (0.02)

Part B – median disparity

	Round 1	Round 2	Round 3	Round 4	Round 5
Control Groups (Treatment A): Separate	$1.65	$1.50	$0.95	$0.50	-$0.40
Treatment Group B: Both WTA and WTP in same group	$1.17	$1.25	$0.90	$0.45	$0.06
Treatment Group C: Bid on both WTA and WTP	$0.35	$0.30	$0.15	$0.00	$0.00

* Statistically significant at the 10% level using a two-sided t-test
** Statistically significant at the 5% level using a two-sided t-test
*** Statistically significant at the 1% level using a two-sided t-test

Table 4: How do WTP and WTA bids differ by treatment?

Part A: WTP mean bid differences by treatment (t-values in parenthesis)

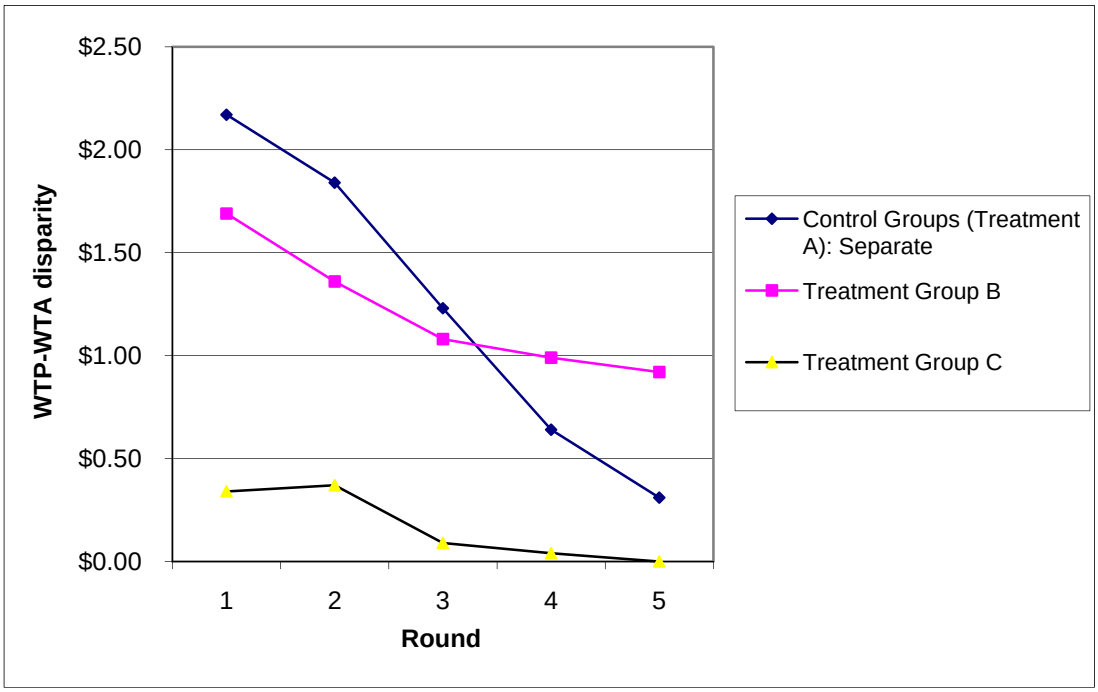
	Round 1	Round 2	Round 3	Round 4	Round 5
Control Group (Treatment A) vs.Treatment B	-$0.06 (-0.35)	-$0.11 (-0.67)	-$0.02 (-0.12)	$0.26 (1.31)	$0.38* (1.81)
Control Group (Treatment A) vs. Treatment C	-$0.18 (-0.79)	-$0.09 (-0.53)	-$0.11 (-0.44)	$0.13 (0.51)	$0.35 (1.26)
Treatment B vs. Treatment C	-$0.12 (-0.64)	$0.02 (0.10)	-$0.09 (-0.44)	-$0.13 (-0.66)	-$0.02 (-0.11)

Part B: WTA mean bid differences by treatment (t-values in parenthesis)

	Round 1	Round 2	Round 3	Round 4	Round 5
Control Group (Treatment A) vs. Treatment B	$0.42 (0.70)	$0.37 (0.77)	$0.13 (0.31)	-$0.09 (-0.24)	-$0.23 (-0.49)
Control Group (Treatment A) vs. Treatment C	$1.65*** (4.23)	$1.38*** (3.93)	$1.03*** (3.18)	$0.74** (2.08)	$0.66* (1.84)
Treatment B vs. Treatment C	$1.23*** (3.58)	$1.01*** (3.82)	$0.90*** (3.37)	$0.82** (2.41)	$0.89** (2.59)

* Statistically significant at the 10% level using a two-sided t-test
** Statistically significant at the 5% level using a two-sided t-test
*** Statistically significant at the 1% level using a two-sided t-test

Figure 1: Mean WTP-WTA disparity in the three treatments (N=127)



Appendix A: "Experience" and Informed Priors

In models of uncertainty where information is accumulated, the receipt of repeated signals reveals the "true" state of the world. "Experience" in such models manifests as an agent with informed (i.e., $\pi_i \neq \pi_j$, $i \neq j$) priors.

Theorem 3: Holding the marginal utility of income constant, agents with experience (i.e., informed priors) have a smaller WTA/WTP gap, ceteris paribus.

Proof: The proof of Theorem 3 has two parts (a) and (b). Part (a) demonstrates that a Bayesian agent with informed priors presented with the same [i.e., equally informative in the sense of Blackwell (1953)] signal will form potential posterior probabilities over states of the world that are closer (in the Euclidean norm) than an agent with less informed priors. Part (b) demonstrates that when an agent's potential posterior probabilities are closer, his information contingent compensating variation, $C(\prod_n)$, and equivalent variation, $E(\prod_m)$, will be closer.

Part (a) : Without loss of generality, let the priors of the agent be represented as $\begin{pmatrix} \pi \\ 1-\pi \end{pmatrix} = \begin{pmatrix} \frac{1+\varepsilon}{2} \\ \frac{1-\varepsilon}{2} \end{pmatrix}$ and her likelihood matrix $L(\theta) = \begin{pmatrix} \frac{1+\theta}{2} & \frac{1-\theta}{2} \\ \frac{1-\theta}{2} & \frac{1+\theta}{2} \end{pmatrix}$, where $(\varepsilon, \theta) \in [0,1]$. These structures generate the following potential posterior matrix :

$\Pi(\theta) = \begin{pmatrix} \frac{1+\varepsilon+\theta+\varepsilon\theta}{2(1+\varepsilon\theta)} & \frac{1+\varepsilon-\theta-\varepsilon\theta}{2(1-\varepsilon\theta)} \\ \frac{1-\varepsilon-\theta+\varepsilon\theta}{2(1+\varepsilon\theta)} & \frac{1-\varepsilon+\theta-\varepsilon\theta}{2(1-\varepsilon\theta)} \end{pmatrix}$. The distance between the potential posteriors can be represented as : $\Delta(\varepsilon) = \frac{\theta(1-\varepsilon^2)}{1-\varepsilon^2\theta^2}$. Over the relevant range $\varepsilon \in [0,1)$, the derivative of this quantity is non - positive : $\Delta'(\varepsilon) = \frac{2\theta\varepsilon(\theta^2-1)}{(1-\varepsilon^2\theta^2)^2} < 0$.

Assuming a constant marginal utility of income (as in Theorem 3) we have:

$$E(\Pi_m) - C(\Pi_n) = \hat{M}(X^*, Y^* | \hat{u}, \Pi_m) - \hat{M}(X^*, Y^* | \hat{u}, \Pi_n) + \hat{\hat{M}}(X^*, Y^* | \hat{u}, \Pi_n) - \hat{\hat{M}}(X^*, Y^* | \hat{u}, \Pi_m)$$

The continuity of $M(\cdot)$ and monotone convergence of $\Delta(\varepsilon)$ guarantee the gap shrinks to zero. ■