**The Willingness to Accept-Willingness to Pay Gap: Do Possession and Framing Still Matter Despite Important Controls for Subject Misconceptions?**

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Abstract

This paper empirically investigates whether treatments that are expected to affect the strength of reference states – such as possession and framing – still affect the endowment effect and the WTA-WTP gap when important controls for subject misconceptions are present. We find a highly statistically significant gap when these treatments make gains and losses salient, and result in strong reference states. In contrast, we find weak and ambiguous evidence that depends on the sample size when these treatments lead to weak reference states. Thus, possession and framing still have a non-trivial effect on the endowment effect despite important controls for subject misconceptions.

Keywords: Endowment Effect, WTA-WTP Gap, Prospect Theory, Reference State, Subject Misconceptions

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## 1. Introduction

Endowment effect or the increased value of a good to an individual when the good becomes part of the individual's endowment is often thought to be a manifestation of loss aversion or reference dependence – changes in the domain of losses short of the reference state are valued more than commensurate changes in the domain of gains beyond the reference state (Kahneman and Tversky, 1979; Thaler, 1980). Consequently, people commonly ask for more compensation to give up a good than they are willing to bid to acquire it, leading to an observed gap between willingness to accept (WTA) and willingness to pay (WTP) (for example, documented by Kahneman et al., 1990; and reviewed in, Samuelson and Zeckhauser, 1988; Rabin, 1998; Horowitz and McConnell, 2002; DellaVigna, 2009). A key, but largely implicit, pre-requisite for finding a WTA-WTP gap is that sellers must perceive "having the good" as their reference state and evaluate "giving it up" as a loss, whereas buyers must perceive "not having the good" as their reference state so that they evaluate "acquiring the good" as a gain.

Some researchers have questioned whether these valuation disparities are really due to asymmetry in preferences over gains and losses. For example, Plott and Zeiler (2005) ask: if they adopt the union of all previous experimental procedures that aim to control for subject misconceptions – including ensuring subject anonymity, using an incentive-compatible elicitation mechanism, carefully explaining to subjects why truthful revelation of personal values is the optimal response under the mechanism, and providing subjects with practice and training on the elicitation mechanism – will they still observe a WTA-WTP gap? It turned out that they observed no gap in the WTA and WTP elicited for mugs even without any paid practice lottery rounds, as long as the other controls were present. Consequently, they interpret this finding as evidence that the gap is due to some subject misconceptions rather than reference dependence or loss aversion.

We empirically investigate whether the subtle procedural details of Plott and Zeiler (2005) – such as buyers and sellers both having physical possession of the good at the time of decision, not always framing buy/sell in gain/loss terms, and asking subjects to answer a series of yes/no questions to plausible values for the good being valued during training – may have caused the WTA-WTP gap to disappear. Because these procedural details make gains and losses less salient, it is unclear that they only control for subject misconceptions without also weakening the reference states of buyers and sellers. To put it differently, these procedures may have the effect of downplaying the endowment status and leading the subjects to treat the endowment status as largely irrelevant in their decision. Hence the subjects behave as though the endowment status is irrelevant, resulting in little or no endowment effect.

Specifically, we investigate whether procedural details such as physical possession and framing can affect the finding of endowment effect despite important controls for subject misconceptions. We ran three treatments: a benchmark treatment with weak reference and two manipulation treatments with strong reference. All treatments maintained the other controls for subject misconceptions, including ensuring subject anonymity, using an incentive-compatible elicitation mechanism, and explaining to subjects why truthful revelation is the optimal response.

Following Plott and Zeiler (2005), the benchmark treatment gives both buyers and sellers physical possession of the good at the time of decision, does not always frame buy/sell in gain/loss terms, and asks subjects to answer a series of yes/no questions to plausible values for the good being valued during training. In contrast, our first manipulation treatment makes three changes to strengthen the reference states: we remove the good from buyers' possession at the time of decision, always frame buy/sell in gain/loss terms, and use hypothetical values that are clearly implausible for the good being valued. Our second manipulation treatment implements only the first two changes to check whether possession and framing alone could account for the results.

With weak reference states in the benchmark treatment, we find that the WTA-WTP gap is statistically insignificant in small samples and only shows sign of statistical significance in large sample. In contrast, with strong reference states in our manipulation treatments, the gap tends to be highly statistically significant in both small and large samples. The evidence suggests that procedural details such as possession and framing do have an effect on the finding of endowment effect and the WTA-WTP gap even when important controls for subject misconceptions are present. Because these procedural details are expected to affect the strength of reference states from which buyers and sellers evaluate gains and losses, our results suggest that experimental procedures could have affected the finding of endowment effect and the WTA-WTP gap through the strength of reference states rather than the extent of subject misconceptions. More generally, to the extent that the endowment effect is interpreted as a manifestation of reference dependence or loss aversion, procedurally-induced weak reference states cannot be ruled out as the cause for the disappearance of endowment effect in situations where complex experimental procedures significantly reduce the salience of endowment or the strength of reference states.

## 2. Related Literature

Our procedural manipulations were motivated by theory and evidence in the existing literature. One literature investigates the determinants of the reference state with the following findings. First, actual and physical possessions matter and people seem unaware of changes in their tastes that come with possessions. Loewenstein and Adler (1995) show that sellers who were asked to imagine having the mugs but not given actual and physical possessions underestimated their valuations when given possessions. Furthermore, Bushong et al. (2010) show that compared to text or image displays, the physical presence of a desirable item that appears accessible at the time of decision can result in a sizable increase in subject's willingness to pay for reasons unrelated to transaction costs or decreased uncertainty with the item. Moreover, Strahilevitz and Loewenstein (1998) and Wolf et al. (2008) show that the duration of ownership or possession positively affects the valuation of objects in laboratory experiments. Nevertheless, in these experiments, the experimenters typically do not maintain the other controls for subject misconceptions. Thus, it is natural to ask whether physical possession still matters when important controls for misconceptions are present.

Second, while ownership is generally an important factor in determining people's reference, ownership alone is neither necessary nor sufficient because other determinants of reference state such as expectations could result in the adoption of a different reference state. For example, Köszegi and Rabin (2006) propose a model in which the reference states are people's expectations of outcomes and suggest that experimental procedures could sometimes decouple subjects' expectations from their initial ownership status. Some researchers have found some empirical support for expectation-based reference-dependent preferences: Abeler et al. (2011) find that subjects' earnings expectations determine their effort provision in real-effort experiment, whereas Ericson and Fuster (2011) find that subjects who have a lower exogenous probability of being able to trade their item (and who therefore expect to keep it) are less likely to choose to trade when trade opportunity arises in an exchange experiment.

Empirically, Knetsch and Wong (2009) find that the effect of ownership on the reference state can be undermined by other manipulations that weaken the perception of this otherwise natural reference state. They show that "in a treatment that provides essentially all of the procedural controls that are deemed necessary to control for incentives recognized in standard theory but does not a priori create a large dichotomy between the perceived reference state and the initial entitlement, the participants exhibit a strong reluctance to trade away initial entitlements despite a lack of ownership of this entitlement", whereas "in a treatment that includes procedures that may control for other incentives but can also be expected to create a large dichotomy between the perceived reference states and the initial endowments, the participants show no such reluctance to trade despite having ownership of the original entitlement" (p.408). Nevertheless, Knetsch and Wong's (2009) results give no indication of the monetary strength of endowment effect. This paper extends their work to measure the monetary strength of endowment effect while controlling for misconceptions over complex elicitation procedures and other classical incentives such as transaction costs and cascade effects.[1]

Closely related is the literature on framing. A central finding in behavioral economics is that framing matters, where "extensionally equivalent descriptions lead to different choices by altering the relative salience of different aspects of the problem" (Kahneman, 2003, p.1458). Rabin (1998) argues that "an important and predictable influence of framing on choice relates to loss aversion… Because losses resonate with people more than gains, a frame that highlights the losses associated with a choice makes that choice less attractive" (p.36). This literature shows that the reference state is determined by how choices are framed and presented, providing the motivation for framing in gain/loss terms in our manipulation treatments.

Another literature investigates anchoring effect (Tversky and Kahneman, 1974) in valuation tasks. Johnson and Schkade (1989) show that valuation of lotteries may be affected by the anchoring effect. For example, asking subjects whether their certainty equivalent for a lottery is above or below an anchor value influences subsequently stated certainty equivalents. Ariely et al. (2003) apply the anchoring

[1] Using exchange experiments, Heffetz and List (2013) had found some empirical support for an assignment-based endowment effect but little support for an expectation-based endowment effect. While they control for concerns raised by Plott and Zeiler (2007), assignment is a weaker condition than endowment. Some of their experimental procedures may also have the effect of weakening the reference state and their results also do not give any indication of the monetary strength of endowment effect.

manipulation to the valuation of products and hedonic experiences. They show that an incentive-compatible Becker-DeGroot-Marschak's (1964) mechanism and full information conditions fail to remove anchoring effect in the WTA elicited. Moreover, Jacowitz and Kahneman (1995) document an anchoring-like effect in responses to the Yes/No question itself: the mere consideration of a proposition tends to increase the plausibility of that proposition itself, especially if subjects reason that a value mentioned in the question is unlikely to be absurd. Green et al. (1998) find similar effects in the WTP for public goods from Yes/No referendum responses and open-ended responses. Thus, asking subjects to answer a series of yes/no questions to plausible values for the good during training may cause the values elicited to depend on the values used in the yes/no questions.

## 3. Experimental Design and Results

### 3.1 General Setup for All Treatments

Twelve experimental sessions were carried out at the National University of Singapore with 345 students recruited from an advertisement posted on the university's Virtual Learning Environment, a website that publishes all university course materials. Every university student could view and respond to the advertisement. Sessions 1-4 were run in 2009 whereas sessions 5-12 were run in 2011.[2] Table 1 summarizes the major characteristics of each session and the results. The advertisement announced that:

> "We are looking for participants for an experiment in individual decision-making. The participants will be asked to perform some valuation tasks, which will take about 30 minutes to complete. The participants are not personally identified, but are instead given ID; the responses made by participants will be anonymous" and "The subjects will be reimbursed $10 per hour of participation in the research."

All payments and transactions were in Singapore dollars. One Singapore dollar was worth about USD0.70 at the time. All sessions were completed in about half an hour and every subject was eventually paid $5 for taking part in the

[2] It is worth emphasizing that all sessions used the same recruiting system and recruiting information (study duration, compensation), the same general subject pool, and the same experimental setup (including the same mugs, the same actual participation fee, the same experimenter, and very similar time of day for the experimental sessions in the same room).

experiment. Subjects were asked to state their offers: maximum WTP for buyers and minimum WTA for sellers, for a coffee mug with the university insignia imprinted on it that was specially ordered at a cost of $3.75 and not otherwise available. Subjects were not informed about the cost or the special order. All transactions were real and conducted with an incentive-compatible Becker-DeGroot-Marschak's (1964) random price auction. If a buyer's offer was more than or the same as a randomly determined fixed offer then the buyer bought the mug and paid the fixed offer. If a seller's offer was less than or the same as a randomly determined fixed offer then the seller sold the mug and received the fixed offer. Sellers who did not sell at the fixed offer took home the mugs they were endowed with. To generate a fixed offer, we told the subjects that:

> "Your offer will be compared to a fixed offer. The fixed offer was determined in the following way:
>
> We prepared 21 pieces of identical papers, with 0, 0.5, 1, 1.5, 2, 2.5, …, 8.5, 9, 9.5, 10 written on them. We folded the papers and dropped them into a bag. Just before the session, we randomly drew a number from the bag and sealed the number into the envelope you see here. We redraw the number for every session. This randomly drawn number in the envelope will be the fixed offer.
>
> As you can see, the fixed offer will be completely unrelated to your offer and to the offers of all other persons in the room."

The fixed offer was revealed after all subjects had stated their offers on their record sheets. Based on their offers and the fixed offer, they had to circle the transaction outcome: BUY or NO BUY for buyers and SELL or NO SELL for sellers, on both their record sheets and a separate sheet on their written instructions. They would then drop their record sheets (containing their offers, the fixed offer, and their transaction outcomes) into a box located in front of the room. Finally, they would take the written instructions containing their transaction outcomes (but not their offers) to the cashiers located outside the room, who would execute the transactions accordingly, in addition to paying them their participation fees. To ensure anonymity, subjects were not personally identified but were randomly assigned an identification number. Thus, the subjects' offers would be known only to themselves. We explained these procedures to the subjects before they stated their offers.

Because each of our experimental sessions consisted of only buyers or only sellers, buyers only received instructions for the buying task whereas sellers only received instructions for the selling task. In addition to simplifying the experimental instructions and illustrations, this between-subject design is desirable because switching between the roles of buyer and seller in a within-subject design may induce a mental frame and reference state of trading, instead of reference states of endowment versus no endowment. Brown (2005) shows that switching between the roles of buyer and seller triggered a buy-low sell-high good-deal seeking mentality that was unrelated to personal valuation and this misconception was prevalent despite the adoption of an incentive-compatible BDM mechanism. Having both buyers and sellers in the same session for WTA-WTP experiments may also lead to other confounding factors. For example, the different treatment of buyers and sellers in the same session may highlight the issue of fairness and lead buyers to feel equally entitled to the endowment as sellers.

All sessions maintained the other controls for subject misconceptions, which included ensuring subject anonymity, using an incentive-compatible elicitation mechanism, explaining to subjects how they would work out their maximum WTP and minimum WTA and why truthful revelation was the optimal response using illustrative numerical examples.

## 3.2 Benchmark Treatment vs. Manipulation Treatment 1

### 3.2.1 Experimental Design

The benchmark treatment and our first manipulation treatment differ in three aspects: first, whether buyers have physical possession of the mugs at the time of decision; second, whether a sale is framed as a relinquishment of sellers' possessions for money; and third, whether the numerical examples given during training use hypothetical values that are plausible for the good being valued. As a general rule, the benchmark treatment adopted Plott and Zeiler's (2005) treatment, whereas our manipulation treatments changed these procedural details to strengthen the reference states of buyers and sellers. We will discuss each in details.

First, in the benchmark treatment, sellers were told that they owned the mug whereas buyers were told that they could inspect the mug but they did not own it, but both buyers and sellers had possession of the mugs at the time of decision. Having possession regardless of ownership weakens the reference states because it creates conflicting reference states and evaluations for buyers. On the one hand, because buyers did not own the mug, they should evaluate the mug as a gain when they acquired it. On the other hand, because buyers had possession of the mug at the time of decision, if they failed to acquire the mug, this would actually entail a loss because they had to literally give it up; so loss aversion would induce higher valuation from buyers if they wanted to avoid parting with the mugs. In some experimental settings, possession could be a more salient status quo than ownership.

To strengthen the reference states and to avoid any ambiguity in the framing of transactions, in our manipulation treatments, only sellers had possession of the mugs at the time of decision; buyers passed around the mugs for inspection and the mugs were removed from them before they were asked to value the mugs. We paid special attention to make sure that buyers were given ample time to inspect the mugs until they were fully satisfied with the inspection. In addition, to clarify what an instantaneously created ownership in the laboratory meant to sellers and to make sure that they understood that ownership was real, sellers in our first manipulation treatment were told that "the mug you are inspecting is yours to take home with you at the end of the experiment, if you decide to keep it later."

Second, the benchmark treatment referred to "the item" or "the mug" for both buyers and sellers, and it sometimes did not frame a sale as a relinquishment of seller's possession for money. In contrast, to strengthen the reference state of possession for sellers and to induce a gain/loss mental frame, our manipulation treatments referred to "the item" or "the mug" for buyers, but replaced the neutral determiner "the" with a possessive determiner such as "my" or "your" for sellers. We also consistently framed a sale as a transaction where the sellers have to give up their items or their mugs for money. For example, we told buyers "to determine the maximum you would be willing to pay for the mug". But instead of telling sellers "to determine the minimum you would be willing to accept for the mug", we told sellers "to determine the minimum you would be willing to accept to give up your mug."

Third, to illustrate how subjects would work out the minimum WTA and maximum WTP, the benchmark treatment asked subjects to answer a series of exploratory Yes/No questions on WTA and WTP in response to specific dollar figures that were between 0 and 10 (i.e., plausible values for a coffee mug), finally arriving at a minimum WTA of $6.50 and a maximum WTP of $5.25 in the examples. In contrast, to remove possible anchoring effect or suggestion of value, our first manipulation treatment replaced these dollar figures with numerical values that were clearly implausible for a coffee mug (by using constant multiples, typically 100, of the original values that were between 0 and 10) and we emphasized that these examples were purely hypothetical.[3]

#### 3.2.2. Experimental Results

Sessions 1 & 5 and 2 & 6 implemented the benchmark treatment for buyers and sellers, respectively. Similarly, sessions 3 & 7 and 4 & 8 implemented our first manipulation treatment for buyers and sellers, respectively. Table 2A and 2B report the summary statistics for individual sessions and pooled samples, respectively. In what follows, our analyses focus on median values rather than mean values because medians are less sensitive to outliers.

With weak reference states in the benchmark treatment, the evidence appears to be mixed and ambiguous in small samples. Specifically, for the benchmark treatment, using the 2009 samples, we find a median WTP of $2.5 and a median WTA of $3, leading to a median WTA-WTP ratio of 1.2. Using the 2011 samples, however, we find a median WTP of $1.8 and a median WTA of $3.5, leading to a median WTA-WTP ratio of 1.94. In contrast, with strong reference states in our manipulation treatment 1, the evidence appears to be more consistent despite the small sample size. Specifically, for our manipulation treatment 1, using the 2009 samples, we find a median WTP of $2 and a median WTA of $4, leading to a median WTA-WTP ratio of 2. Similarly, using the 2011 samples, we find a median WTP of $2.5 and a median WTA of $4.25, leading to a median WTA-WTP ratio of 1.7.

The evidence becomes clearer with a larger sample size. Using pooled samples from 2009 and 2011, in the benchmark treatment, we find a median WTP of $2.5 and a median WTA of $3.25, leading to a median WTA-WTP ratio of 1.3. In contrast, in our manipulation treatment 1, we find a median WTP of $2 and a median WTA of $4.25, leading to a median WTA-WTP ratio of 2.13.

Table 3 reports the results of statistical tests to determine whether the data support the hypothesis that WTP is different from WTA. We perform the Wilcoxon-

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[3] For instance, the benchmark treatment gave the following numerical example to illustrate how one would work out the minimum one is willing to accept [with our modifications in square brackets]:
Would I accept $10 [$1000] to give up Item B [my item]? Yes.
Would I accept $8 [$800] for B [to give up my item]? Yes.
Would I accept $7 [$700] for B [to give up my item]? Yes.
Would I accept $6 [$600] for B [to give up my item]? No, not $6 [$600]. So I need to increase.
Would I accept $6.50 [$650]? I don't care whether I end up with $6.50 [$650] or Item B [keeping my item]. Then that is the minimum I'd be willing to accept for Item B [to give up my item].

Mann-Whitney test, which tests for whether the WTP and WTA samples were drawn from identical distributions, and median test, which tests for whether the WTP and WTA samples were drawn from distributions with identical medians.

The results show that with weak reference states in the benchmark treatment, we cannot reject the null hypotheses of identical distributions or identical medians for WTP and WTA at the conventional significance levels in small samples. Specifically, using WTP and WTA from 2009, the Wilcoxon-Mann-Whitney rank sum test results in a $z$ value of -1.484 ($p$=0.1378) and the median test results in a Pearson $X^2$ statistic of 1.0513 ($p$=0.305). Similarly, using the values from 2011, the Wilcoxon-Mann-Whitney rank sum test results in a $z$ value of -1.411 ($p$=0.1583) and the median test results in a Pearson $X^2$ statistic of 0.4191 ($p$=0.517). In other words, with weak reference states, using a sample size of about 30 buyers and 30 sellers, there was no evidence that the WTP and WTA were drawn from different distributions or from distributions with different medians.

In contrast, when the procedures were modified to strengthen the reference states in our first manipulation treatment, there was strong evidence that WTP was different from WTA even in small samples: we can always reject the null hypotheses of identical distributions or identical medians in our first manipulation treatment at close to the one percent level. Specifically, using WTP and WTA from 2009, the Wilcoxon-Mann-Whitney rank sum test results in a $z$ value of -2.495 ($p$=0.0126) and the median test results in a Pearson $X^2$ statistic of 9.7738 ($p$=0.002). Similarly, using the values from 2011, the Wilcoxon-Mann-Whitney rank sum test results in a $z$ value of -2.832 ($p$=0.0046) and the median test results in a Pearson $X^2$ statistic of 7.3665 ($p$=0.007).

Not surprisingly, with strong reference states in our manipulation treatment, there was even stronger evidence that WTP was different from WTA when we pooled the data; the Wilcoxon-Mann-Whitney rank sum test now results in a $z$ value of -3.654 ($p$=0.0003) and the median test results in a Pearson $X^2$ statistic of 12.7482 ($p$=0.000). We can reject the null hypotheses of identical distributions and identical medians in our manipulation treatment at the one percent level. However, with pooled data and a larger sample size, it turns out that we can also reject the null hypothesis of identical distributions for WTP and WTA in the benchmark treatment at the five percent level, although we still cannot reject the null hypothesis of identical medians at the conventional levels; the Wilcoxon-Mann-Whitney rank sum test now results in a $z$ value of -2.102 ($p$=0.0356), whereas the median test results in a Pearson $X^2$ statistic of 1.3898 ($p$=0.238).[4]

It may seem surprising that by pooling the data, the benchmark treatment now shows some evidence that WTP was different from WTA, even though we find no statistically significant difference when we consider the samples separately. Nevertheless, this result is predicted by our hypothesis of weak reference states in the benchmark treatment. Weak reference states would cause statistical tests to have low power, i.e., low probability of rejecting the null hypothesis of no endowment effect when there is in fact an endowment effect. Increasing sample size is expected to increase the power of tests.

### 3.3 Benchmark Treatment vs. Manipulation Treatment 2

#### 3.3.1 Experimental Design

Our first two procedural modifications strengthen the reference states of buyers and sellers through possession and framing, while the last removes possible anchoring or suggestion of value by changing the values in the numerical examples. Because the last change seems quite different from the first two, we now investigate to what extent the first two changes, i.e., possession and framing alone, could account for the finding of the gap. Specifically, in our manipulation treatment 2, we implemented only the possession and framing manipulations to strengthen the reference states, but used the same numerical examples as the benchmark treatment. Furthermore, like the benchmark treatment, sellers in our manipulation treatment 2 were simply told that: "You can inspect the mug. You now own the mug."

#### 3.3.2 Experimental Results

We ran another four experimental sessions (sessions 9-12) at the National University of Singapore on two consecutive days in 2011. We recruited another 105

[4] If we exclude the two offers for WTA that exceed \$10 in session 2, we get similar conclusions. If we exclude the two offers for WTA that exceed \$10 in session 2 and consider the 2009 and 2011 sessions separately, in small samples the WTA-WTP gap is still statistically insignificant in the benchmark treatment at the ten percent level. On the other hand, if we exclude these two offers in the benchmark treatment from pooled data, the Wilcoxon-Mann-Whitney rank sum test results in a $z$ value of -1.831 ($p$=0.0671), whereas the median test results in a Pearson $X^2$ statistic of 1.4157 ($p$= 0.234). Thus, sample size still matters for statistical significance in the benchmark treatment, such that the $z$ value from rank sum test is now statistically significant at the ten percent level.

students using similar procedures as before. Sessions 9 & 11 and 10 & 12 implemented our second manipulation treatment for buyers and sellers, respectively.

Table 2B shows that in our second manipulation treatment, we find a median WTP of $2 and a median WTA of $4.5, leading to a median WTA-WTP ratio of 2.25. Table 3 shows that in our second manipulation treatment, there was again strong evidence that WTP was different from WTA at the conventional significance levels in both small and large samples; the Wilcoxon-Mann-Whitney rank sum test results in a $z$ value of -4.12 ($p$=0.0000) and the median test results in a Pearson $X^2$ statistic of 16.4586 ($p$=0.000) when the data are pooled. Similarly, comparing between individual sessions conducted on the same day, we can generally reject identical distributions or identical medians at the conventional levels. Thus, the evidence suggests that possession and framing could account for the finding of the WTA-WTP gap when important controls for subject misconceptions are present.

## 4. Further Discussion and Conclusions

In summary, when we adopt Plott and Zeiler's (2005) treatments for possession and framing that are expected to produce weak reference states for both buyers and sellers, we always find statistically insignificant valuation disparities at the conventional levels in small samples of around thirty buyers and thirty sellers. With larger samples and improved power of tests, the evidence remains ambiguous, but there is some evidence that the valuation disparities may be small and somewhat statistically significant. In contrast, when we modify the treatments for possession and framing to strengthen the reference states, we generally find moderate but highly statistically significant valuation disparities in both small and large samples.

All of our treatments maintain important controls for subject misconceptions, including ensuring subject anonymity, using an incentive-compatible elicitation mechanism, explaining to subjects how they will work out their maximum WTP and minimum WTA and why truthful revelation is the optimal response using illustrative numerical examples. They are also exempt from classical incentives that Plott and Zeiler (2007) argue can induce asymmetric choices in an exchange experiment where subjects are endowed with one good but given the opportunity to trade for an alternative good, such as different transaction costs for traders and non-traders, possible signals of relative value of the endowed good versus the alternative good, cascade effects due to public revelation of choices, as well as non-classical behavioral motives such as subjects' expectations of experimenter's altruism in the choice of endowment; in all of our treatments, both buyers and sellers incurred basically the same transaction costs, valued the same good privately and anonymously after being given ample time to inspect the good being valued.

The results suggest that although different experimental procedures could very well have affected the finding of endowment effect and the WTA-WTP gap by inducing or eliminating various subject misconceptions or classical incentives, they might also have worked by strengthening or weakening the reference states from which gains and losses were evaluated without necessarily changing the extent of control for misconceptions. The strength of the reference states also seems to be a more parsimonious explanation than subject misconceptions or classical incentives.

The strength of the reference states may depend subtly on the economic environment and its complexity. Procedurally-induced weak reference states may be an especially important issue in laboratory experiments – when the experimental procedures get more complex, it is possible that the experimenters may have established ownership but not any sense of endowment or entitlement to the newly endowed good. In these experiments, the determinants of reference state such as ownership, status quo, expectations, and possession may sometimes not coincide with the establishment of the same reference state, resulting in weak reference states. To put it differently, ownership needs to be internalized before it will become the reference state that affects decision and behavior. The "internalization" of the endowment or thinking about the good as part of the individual's endowment may require more than cognitively understanding ownership status or the probability of ownership.

Explicitly or implicitly, some researchers have equated endowment with ownership. We think that ownership is generally a very important determinant of the reference states. But if we were to interpret the "ownership effect" as a test of Prospect theory, reference dependence or loss aversion, then we must also acknowledge that the validity of the test depends critically on the success of using ownership to establish the reference state. In other words, when we interpret the finding of "ownership effect" as a test of reference dependence, we are really testing a joint hypothesis: first, ownership is a sufficient determinant of the reference state regardless of the other experimental setup or there is only one determinant of the reference state and it is ownership, and second, valuation depends on the reference state (i.e., there is reference dependence). Hence, the finding of no "ownership effect" is necessarily a rejection of the joint hypothesis: it could be due to a rejection of the first hypothesis or the second hypothesis, or both. But in practice, it is almost always interpreted as a rejection of the second hypothesis alone and the researchers conclude that there is no reference dependence.

Once we accept that the reference state has more than one determinants, it becomes difficult to specify a set of sufficient conditions for the reference state to be potent *regardless* of the other procedural details. For example, we are not proposing physical possession as a necessary or sufficient condition for endowment effect; what suffices in a simple and intuitive environment may be inadequate in a complicated environment when other forces are also at work; as more complicated procedural controls are included to rule out potential confounding factors, they may also have the unintended side effect of weakening the reference states induced by the other treatments. This difficulty highlights the need for more future research to uncover the relative importance of individual determinants of reference state and their interactions with the economic environment.

For example, Isoni et al. (2011) ask what makes experiments that use goods like mugs to elicit valuations different from experiments that use lotteries, "such that when the same experimental procedures are applied to both, WTP-WTA disparities can persist in lottery valuations but not in mug valuations" (p.19)? If experimental procedures could affect valuation disparities by inducing strong or weak reference states, as we have argued, it is natural to ask whether there are any reasons why the same experimental procedures may have induced stronger reference states in lottery valuations than in mug valuations. We offer a conjecture: for lottery tasks involving uncertain money outcomes, when there is feedback on the realizations of lottery outcomes for lottery forgone, such as in Plott and Zeiler's (2005) and Isoni et al.'s (2011) experiments, anticipatory regrets (Loomes and Sugden, 1982) may be stronger for sellers who have sold the lotteries they are endowed with than for buyers who have not bought the lotteries. Anticipated regret aversion triggers stronger loss aversion, resulting in more significant WTA-WTP disparities in lottery valuations than in mug valuations.[5]

[5] Empirical evidence suggests that providing feedback on ex-post outcomes for gambles forgone makes the reference states of "what would have been had the subjects made a different choice" more salient, inducing anticipated regrets (Zeelenberg et al., 1996) and a higher WTA for sellers of lottery

It is worth noting that Plott and Zeiler (2011) conjecture that any procedural changes from their treatment – such as the location of the mugs or framing in gains and losses – may have led to very subtle enhancement effects and demand effects rather than loss aversion. Nevertheless, the exact mechanism through which this enhancement is supposed to take place is unclear. Furthermore, while demand effects can certainly affect experimental findings, neither this conjecture nor the opposite conjecture can be rejected using the data from our experiments or Plott and Zeiler's (2005) experiments. The opposite conjecture is that for every enhancement effect or demand effect hypothesized, there is an equally plausible demand effect working in the opposite direction. Specifically, from the elaborate measures the experimenters took to ensure that there are few differences between having ownership and no ownership, it is natural for subjects to infer that they are supposed to behave as if ownership is irrelevant. In fact, it seems plausible that demand effects are more likely when subjects observe unexpected treatments or apparent deviations from the more intuitive treatments – such as buyers having possession of the mugs they do not own and framing buy/sell as similar valuation task – because these features register more saliently in the mind of subjects, not the other way around.

It is also worth emphasizing that the primary purpose of this paper is to investigate whether small variations in procedural details could affect the finding of valuation disparities, i.e., whether there are valuation disparities or not. Our primary object of interest is the *difference* between WTA and WTP. We design our experiments specifically for this purpose. Because of this, we adopt Plott and Zeiler's (2005) treatment for possession and framing as the benchmark. To investigate how possession and framing affect WTP and WTA individually are beyond the scope of this paper because it requires a different experimental design. To test the individual effect, we will need sharper differences between the procedures across different treatments. For example, there were very few procedural differences between our manipulation treatments and the benchmark treatment for sellers except some instances of framing. We will also need an endowment that allows more room for change in valuation across different treatments. For example, for buyers, there was

(Humphrey et al., 2005). There is also some evidence that actual ownership is not necessary to evoke anticipated regret as long as there is some sense of entitlement to the lottery even when one is not playing. Specifically, Zeelenberg and Pieters (2004) show that in the Netherlands, people anticipate more regret over not playing the Postcode Lottery, where one's postcode is the ticket number and so non-participants may feel a sense of endowment to the postcode lottery and they may still find out that they would have won had they played, than in a traditional State Lottery.

little room for downward change given that the median WTP in the benchmark treatment was already quite low at $2.5 and WTP is bounded below by zero.

The bottom line is that knowing which procedures positively affect the finding of endowment effect in the laboratory setting is important for future research and discussion. The finding of no endowment effect does not seem robust to small variations in procedural details. Specifically, maintaining important controls for subject misconceptions, there is some evidence that changes in physical possession and framing affect the finding of the WTA-WTP gap. If no valuation disparities are observed only under very singular experimental setup with little tolerance for small variations, then valuation disparities are expected to prevail in most cases.

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**Table 1 – Design Features of Experiment Treatments**

| Treatment | Benchmark: Weak Reference | Manipulation 1: Strong Reference | Manipulation 2: Strong Reference |
|---|---|---|---|
| Median WTA-WTP ratio | 1.3 | 2.13 | 2.25 |
| Buying Task | N=31 (Session 1)<br>N=30 (Session 5) | N=30 (Session 3)<br>N=31 (Session 7) | N=28 (Session 9)<br>N=24 (Session 11) |
| Selling Task | N=29 (Session 2)<br>N=31 (Session 6) | N=30 (Session 4)<br>N=28 (Session 8) | N=23 (Session 10)<br>N=30 (Session 12) |
| Possession at the time of decision? | Yes for sellers<br>Yes for buyers | Yes for sellers<br>No for buyers | Yes for sellers<br>No for buyers |
| Framing in possessive and gain/loss terms? | No | Yes | Yes |
| Examples use plausible values for a coffee mug? | Yes | No | Yes |

**Table 2A –Summary Statistics – Individual Sessions**

| Session | Treatment | Schedule | WTA or WTP? | N | Mean | Median | Std. dev. |
|---|---|---|---|---|---|---|---|
| 1 | Benchmark | 17/2/2009, 2-3pm | WTP | 31 | 2.57 | 2.50 | 1.42 |
| 2 | Benchmark | 18/2/2009, 2-3pm | WTA | 29 | 4.10 | 3.00 | 3.60 |
| 3 | Manipulation 1 | 17/2/2009, 3-4pm | WTP | 30 | 2.56 | 2.00 | 2.51 |
| 4 | Manipulation 1 | 18/2/2009, 1-2pm | WTA | 30 | 3.80 | 4.00 | 2.36 |
| 5 | Benchmark | 20/1/2011, 2-3pm | WTP | 30 | 2.75 | 1.80 | 2.35 |
| 6 | Benchmark | 20/1/2011, 3-4pm | WTA | 31 | 3.61 | 3.50 | 2.54 |
| 7 | Manipulation 1 | 21/1/2011, 2-3pm | WTP | 31 | 2.50 | 2.50 | 2.00 |
| 8 | Manipulation 1 | 21/1/2011, 3-4pm | WTA | 28 | 4.54 | 4.25 | 2.83 |
| 9 | Manipulation 2 | 27/1/2011, 2-3pm | WTP | 28 | 2.94 | 2.50 | 1.96 |
| 10 | Manipulation 2 | 27/1/2011, 3-4pm | WTA | 23 | 4.28 | 4.50 | 2.39 |
| 11 | Manipulation 2 | 28/1/2011, 2-3pm | WTP | 24 | 1.69 | 1.75 | 1.39 |
| 12 | Manipulation 2 | 28/1/2011, 3-4pm | WTA | 30 | 4.05 | 3.75 | 2.41 |

**Table 2B –Summary Statistics – Pooled Data**

| Sessions | Treatment | WTA or WTP? | N | Mean | Median | Std. dev. |
|---|---|---|---|---|---|---|
| 1 & 5 | Benchmark (Weak Reference) | WTP | 61 | 2.66 | 2.50 | 1.92 |
| 2 & 6 | Benchmark (Weak Reference) | WTA | 60 | 3.85 | 3.25 | 3.08 |
| 3 & 7 | Manipulation 1 (Strong Reference) | WTP | 61 | 2.53 | 2.00 | 2.25 |
| 4 & 8 | Manipulation 1 (Strong Reference) | WTA | 58 | 4.16 | 4.25 | 2.60 |
| 9 & 11 | Manipulation 2 (Strong Reference) | WTP | 52 | 2.36 | 2.00 | 1.82 |
| 10 & 12 | Manipulation 2 (Strong Reference) | WTA | 53 | 4.15 | 4.50 | 2.38 |

**Table 3 – Statistical Test Results: WTP vs. WTA**

| | Wilcoxon-Mann-Whitney rank sum test (Null hypothesis: identical distributions) | | Median test (Null hypothesis: populations have identical medians) | |
|---|---|---|---|---|
| | $z$ | $p$-value | Pearson $X^2$ | $p$-value |
| <u>Benchmark: Weak Reference</u> | | | | |
| Sessions 1 vs. 2 | -1.484 | 0.1378 | 1.0513 | 0.305 |
| Sessions 5 vs. 6 | -1.411 | 0.1583 | 0.4191 | 0.517 |
| Sessions 1 & 5 vs. 2 & 6 | -2.102 | 0.0356 | 1.3898 | 0.238 |
| <u>Manipulation 1: Strong Reference</u> | | | | |
| Sessions 3 vs. 4 | -2.495 | 0.0126 | 9.7738 | 0.002 |
| Sessions 7 vs. 8 | -2.832 | 0.0046 | 7.3665 | 0.007 |
| Sessions 3 & 7 vs. 4 & 8 | -3.654 | 0.0003 | 12.7482 | 0.000 |
| <u>Manipulation 2: Strong Reference</u> | | | | |
| Sessions 9 vs. 10 | -2.059 | 0.0395 | 3.2073 | 0.073 |
| Sessions 11 vs. 12 | -4.025 | 0.0001 | 19.2000 | 0.000 |
| Sessions 9 & 11 vs. 10 & 12 | -4.120 | 0.0000 | 16.4586 | 0.000 |

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## Appendix: Experimental Instructions

The instructions for the benchmark treatment are shown in black, whereas the instructions for our manipulation treatment are shown in square brackets and in red, e.g., [red], when they deviate from the instructions for the benchmark treatment. Underlined text preceded by Note to Experimenter is meant for the experimenters only and not in the written instructions received by the subjects.

### General Instructions for All Experiments

Note to Experimenter: Prior to the beginning of the experiment, the experimenter placed an example of record sheet on the overhead projector. The subject's offer, fixed offer price, and result calculation were left blank. As the participants come in, tell them that they will get their participation fees at the end.

WRITTEN INSTRUCTIONS – Give to Subjects (Also read aloud)

Please follow my instructions carefully. Do not get ahead of my verbal instructions.

This is an experiment in individual decision-making. Our purpose is to study technical issues involved in decision-making. A research grant has provided funds for this research.

The instructions are simple and your final outcome will depend on the decisions you make.

You have received a record sheet with an individual identification number on it. This is your private information. Do not share it with anyone. We ask that you do not communicate with other people during the experiment. Please refrain from verbally reacting to events that occur during the experiment. This is very important.

If you have any questions, please raise your hand and wait for the experimenter to attend to you.

### Instructions for Seller

Note to Experimenter:

*Possession condition*: give a mug to each participant. Leave the mug with the participant. At the end of the experiment, each participant will bring the mug to the person outside (sequentially) and settle the trade – which means either give the mug back in exchange for the fixed offer or walk away with the mug. In the possession condition, make sure that the mugs are out of their boxes while in possession.

As the mugs were being distributed, say: "You can inspect the mug. You now own the mug." [As the mugs were being distributed, say: "You can inspect the mug. You now own the mug. The mug you are inspecting is yours to take home with you at the end of the experiment, if you decide to keep it later."]

The experimenter will offer to buy the mug that you own. Your task is to make an offer for the mug [your mug] and record it on your record sheet.

As you will see, your best strategy is to determine the minimum you would be willing to accept for the mug [to give up your mug] and offer that amount. It will not be to your advantage to offer more than this amount, and it will not be to your advantage to offer less. Simply determine the minimum you would be willing to accept and make that amount your offer.

Your offer will be compared to a fixed offer. The fixed offer was determined in the following way:

We prepared 21 pieces of identical papers, with 0, 0.5, 1, 1.5, 2, 2.5, …, 8.5, 9, 9.5, 10 written on them. We folded the papers and dropped them into a bag. Just before the session, we randomly drew a number from the bag and sealed the number into the envelope you see here. We redraw the number for every session. This randomly drawn number in the envelope will be the fixed offer.

As you can see, the fixed offer will be completely unrelated to your offer and to the offers of all other persons in the room.

If your offer is less than or the same as the fixed offer then you sell the mug [your mug]. You had the low offer, so you are the seller. But, here's the interesting part. **You do not receive your offer**. Instead, you receive the fixed offer, a price equal to or higher than your offer.

[Note to Experimenter: say "As a hypothetical example…"]

Example: if you offer 1,000 and the fixed offer is 1,020, you have the low offer. You sell the item [your item] and you receive the fixed offer of 1,020.

If your offer is more than the fixed offer then you do not sell the item [your item]. You keep the item [your item].

Example: if you offer 1,000 and the fixed offer is 950, you do not have the low offer. Therefore, you do not sell the item [your item].

Note to experimenter: Illustrate how the record sheets will be used in the two examples above. Ask "any questions?"

As a seller, you should offer the **minimum amount you would be willing to accept** in exchange for [to give up] the mug you own.

Remember, there are no advantages to strategic behavior. Your best strategy is to determine your personal value for the mug [your mug] and record that value as your offer. There is not necessarily a "correct" value. Personal values can differ from individual to individual.

The following example illustrates how you work out what's the minimum you are willing to accept.
"Imagine that I am a seller and I own Item B. How do I know what amount is the minimum I'd be willing to accept to give up Item B [my item]?
Start with $100 [$1000]. Would I be willing to give up Item B [my item] in exchange for $100 [$1000]? If so, then decrease the amount to $95 [$950]. If I'm willing to accept $95 [$950] to give up Item B [my item], then decrease further. I keep decreasing until I come to an amount that makes me indifferent between keeping Item B [my item] and getting the money.

[Note to Experimenter: say "As a hypothetical example…"]

Example:
Would I accept $10 [$1000] to give up Item B [my item]? Yes.
Would I accept $8 [$800] for B [to give up my item]? Yes.
Would I accept $7 [$700] for B [to give up my item]? Yes.
Would I accept $6 [$600] for B [to give up my item]? No, not $6 [$600]. So I need to increase.
Would I accept $6.50 [$650]? I don't care whether I end up with $6.50 [$650] or Item B [keeping my item].

Then that is the minimum I'd be willing to accept for Item B [to give up my item]. I'll record that number on my record sheet.

The key to determining the minimum you'd be willing to accept is remembering that you will not receive the amount you ask for. Instead, if you receive anything, you will always get the fixed offer.

Why is my best strategy to bid the minimum I'd be willing to accept? Let's go back to the Example:

[Note to Experimenter: say "As a hypothetical example…"]

Say I decide that the minimum I'd be willing to accept for Item B [to give up my item] is $6.50 [$650].

What happens if I ask for less than $6.50 [$650]? Say I ask for only $6 [$600].
If the fixed offer is, say, $6.25 [$625], then I have to sell my item. I lose out because I have to give up Item B [my item] which I think is worth $6.50 [$650], but I only get $6.25 [$625] in exchange.

What happens if I ask for more than $6.50 [$650]? Say I ask for $7 [$700].
If the fixed offer is $6.75 [$675], then I do not sell. But, had I bid $6.50 [$650], I would have sold the item [my item] and received $6.75 [$675] for an item that I think is worth only $6.50 [$650]. I lose out.

Note to experimenter: Ask "any questions?"

At the end of the exercise, please detach your Record Sheet, fold it and drop it into the box in front. Then take the remaining portion of the written instructions containing your ID number to the cashier outside the room, who will execute the transactions (SELL or NO SELL at the fixed offer) and pay you your participation fees. Note that the cashier outside will not see your offer. The experimenter will not be able to link any specific subject to a subject ID number and his/her offer. Thus, your offer will be known only to yourself.

Note to Experimenter: say "All transactions (SELL or NO SELL) are real. If you SELL, then you will receive the fixed offer (not your offer) from the cashier outside in exchange for the mug [but you will give up your mug]. If NO SELL, then you will not receive the fixed offer but you will take home the mug [but you will keep your mug]."

Encourage and address questions from subjects. After answering questions, start the experiment.

After the experiment, remind subjects to copy only the SELL or NO SELL decision in step 3 of the Record Sheet to the previous page. Ask the subjects to drop their record sheets into the box in front. Then ask them to leave sequentially. The person outside pays the participation fees, settles the trade and collects the ID number back. The person outside will not see the subjects' offers.

| ID Number: _____ |
|---|

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Keep this page to yourself. Show your ID number to the person outside this room after the experiment. At the end of the experiment, based on your decision in Step 3 on the Record Sheet, Circle **SELL** or **NO SELL**

**Not for Publication**

# Record Sheet

ID Number: _____

Step 1: decide on my offer.

MY OFFER : S$_______________

Step 2: listen for fixed offer announcement

FIXED OFFER : S$_______________

Step 3:

Circle **SELL** or **NO SELL**

**SELL**

If MY OFFER ≤ FIXED OFFER, then circle SELL

Receive FIXED OFFER in exchange for the mug.
[Receive FIXED OFFER. Give up my mug.]

**NO SELL**

If MY OFFER > FIXED OFFER, then circle NO SELL

Receive $0. Take Home the mug.
[Receive $0. Keep my mug.]

Step 4: Detach this record sheet and drop it into the box in front.

## Not for Publication

### Instructions for Buyer

Note to Experimenter: Hand out the mugs. Say: "You can inspect the mug, but you do not own it."

*Possession condition*: give a mug to each participant for inspection. Leave the mug with the participant. At the end of the experiment, each participant will bring the mug to the person outside (sequentially) and settle the trade – which means either give the mug back or pay the fixed offer. In the possession condition, make sure that the mugs are out of their boxes while in possession.

[*No possession condition*: give a few mugs for all participants to pass around and inspect. After everybody has inspected a mug, take the mugs from the participants and leave them in the desk in front.]

The experimenter will offer a mug (the one you inspected) [just like the one you inspected] for sale. [We have enough mugs on hand should everybody decide to buy one.] Your task is to make an offer for the mug and record it on your record sheet.

As you will see, your best strategy is to determine the maximum you would be willing to pay for the mug and offer that amount. It will not be to your advantage to offer more than this amount, and it will not be to your advantage to offer less. Simply determine the maximum you would be willing to pay and make that amount your offer.

Your offer will be compared to a fixed offer. The fixed offer was determined in the following way:

We prepared 21 pieces of identical papers, with 0, 0.5, 1, 1.5, 2, 2.5, …, 8.5, 9, 9.5, 10 written on them. We folded the papers and dropped them into a bag. Just before the session, we randomly drew a number from the bag and sealed the number into the envelope you see here. We redraw the number for every session. This randomly drawn number in the envelope will be the fixed offer.

As you can see, the fixed offer will be completely unrelated to your offer and to the offers of all other persons in the room.

If your offer is more than or the same as the fixed offer then you buy the mug. You had the high offer, so you are the buyer. But, here's the interesting part. **You do not pay the amount you offered**. Instead, you pay the fixed offer, an amount equal to or less than your offer.

[Note to Experimenter: say "As a hypothetical example…"]

Example: if you offer 1,000 and the fixed offer is 950, you have the high offer. You buy the item but pay only 950.

If your offer is less than the fixed offer then you do not buy the item. Instead, you keep your money.

Example: if you offer 1,000 and the fixed offer is 1,020, you do not have the high offer. Therefore, you do not buy the item. You keep your money.

Note to experimenter: Illustrate how the record sheets will be used in the two examples above. Ask "any questions?"

As a buyer, you should offer exactly the **maximum amount you would be willing to pay** in exchange for the mug.

Remember, there are no advantages to strategic behavior. Your best strategy is to determine your personal value for the mug and record that value as your offer. There is not necessarily a "correct" value. Personal values can differ from individual to individual.

The following example illustrates how you work out what's the maximum you are willing to pay.
"Imagine that I am a buyer and Item A is up for sale. How do I know what amount is the maximum I'd be willing to pay for Item A?
Start with 1 cent [$1]. Would I be willing to pay 1 cent [$1] for the item? If so, then increase the amount to 2 cents [$2]. If I'm willing to pay 2 cents [$2], then increase further. I keep increasing until I come to an amount that makes me indifferent between keeping the money and getting Item A.

[Note to Experimenter: say "As a hypothetical example…"]

Example: Would I pay $1 [$100] for A? Yes.
Would I pay $2 [$200] for A? Yes.
Would I pay $5 [$500] for A? Yes.
Would I pay $6 [$600] for A? No, not $6 [$600]. So I need to decrease.
Would I pay $5.50 [$550]? No, not that much.
How about $5.25 [$525]? I don't care whether I end up with $5.25 [$525] or the item.

Then that is the maximum I'd be willing to pay for Item A. I'll record that number on my record sheet.

The key to determining the maximum you'd be willing to pay is remembering that you will not pay the amount you bid. Instead, if you pay anything, you will pay the fixed offer.

Why is my best strategy to bid the maximum I'd be willing to pay? Let's go back to the Example:

[Note to Experimenter: say "As a hypothetical example…"]

Say that I decide that the maximum I'd be willing to pay for Item A is $5.25 [$525].

What happens if I bid less than $5.25 [$525]? Say I bid $5 [$500].
If the fixed offer is, say, $5.10 [$510], then I don't get the item. Had I bid $5.25 [$525], I would have received the item and had to pay only $5.10 [$510] for an item that I think is worth $5.25 [$525]. I lose out.

What happens if I bid higher than $5.25 [$525]? Say I bid $5.50 [$550].
If the fixed offer is $5.45 [$545], then I have to pay $5.45 [$545] for an item that I really think is worth only $5.25 [$525]. I lose out.

Note to experimenter: Ask "any questions?"

At the end of the exercise, please detach your Record Sheet, fold it and drop it into the box in front. Then take the remaining portion of the written instructions containing your ID number to the cashier outside the room, who will execute the transactions (BUY or NO BUY at the fixed offer) and pay you your participation fees. Note that the cashier outside will not see your offer. The experimenter will not be able to link any specific subject to a subject ID number and his/her offer. Thus, your offer will be known only to yourself.

Note to Experimenter: say "All transactions (BUY or NO BUY) are real. If you BUY, then you will pay the fixed offer (not your offer) to the cashier outside in exchange for the mug. If NO BUY, then you will not pay the fixed offer but you will also not get the mug."

Encourage and address questions from subjects. After answering questions, start the experiment. Tell the subjects that if they do not have enough money with them, we will hold the mug for them and they can come back for it later.

After the experiment, remind subjects to copy only the BUY or NO BUY decision in step 3 of the Record Sheet to the previous page. Ask the subjects to drop their record sheets into the box in front. Then ask them to leave sequentially. The person outside pays the participation fees, settles the trade and collects the ID number back. The person outside will not see the subjects' offers. If a subject has not enough money, tell him/her to come back with enough money and we will hold the mug for him/her.

| ID Number: _____ |
| --- |

Keep this page to yourself. Show your ID number to the person outside this room after the experiment. At the end of the experiment, based on your decision in Step 3 on the Record Sheet, Circle **BUY** or **NO BUY**

**Not for Publication**

# Record Sheet

ID Number: _____

Step 1: decide on my offer.

MY OFFER : S$__________________

Step 2: listen for fixed offer announcement

FIXED OFFER : S$__________________

Step 3:

Circle **BUY** or **NO BUY**

**BUY**

If MY OFFER $\geq$ FIXED OFFER, then circle BUY

Pay FIXED OFFER for the mug

**NO BUY**

If MY OFFER $<$ FIXED OFFER, then circle NO BUY

Pay $0. Don’t get the mug.

Step 4: Detach this record sheet and drop it into the box in front.