

# **The Endowment Effect and the Willingness to Accept-Willingness to Pay Gap: Subject Misconceptions or Reference Dependence?**

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## Abstract

Plott and Zeiler (2005) suggest that the WTA-WTP gap arises from subject misconceptions rather than asymmetric valuation of gains and losses relative to the reference state because the gap disappeared when they adopted all known procedural controls for misconceptions. We modified subtle details of their procedures to make the reference state of possession more salient to sellers, vice versa for buyers, keeping all necessary controls for misconceptions. These changes resulted in a highly statistically significant gap. Thus, the gap's on-off results for different procedures are more likely to be due to procedurally induced saliency of the reference states than misconceptions.

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

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## 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, Knetsch, and Thaler, 1990; and reviewed in, Samuelson and Zeckhauser, 1988; Rabin, 1998; Horowitz and McConnell, 2002; DellaVigna, 2009). A key, but largely implicit, pre-requisite for the finding of 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.<sup>2</sup>

Some researchers have questioned whether these valuation disparities are really due to asymmetry in preferences over gains and losses. Most notably, Plott and Zeiler (2005) – PZ henceforth – ask: if they adopt the union of all previous experimental procedures that *supposedly* 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 paid training rounds, as long as the other controls were present. They interpret this finding as evidence that the gap is due to some subject misconceptions rather than reference dependence.

Evidently, because PZ never specify what misconceptions are, what procedures are needed to eliminate them, and how these procedures should be implemented, PZ's conclusion that subject misconceptions are what account for the WTA-WTP gap cannot have been based on direct evidence that misconceptions are

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<sup>2</sup> As Kahneman, Knetsch, and Thaler (1990) explain, “...if a good is evaluated as a loss when it is given up and as a gain when it is acquired, loss aversion will, on average, induce a higher dollar value for owners than for potential buyers, reducing the set of mutually acceptable trades” (p.1328).

present when the gap is found, and vice versa.<sup>3</sup> Instead, their conclusion hinges critically on their assumption or interpretation of what their procedures do, or equally importantly, do not do, which is open to alternative interpretation.

Specifically, we investigate whether subtle details of PZ's procedures such as buyers having physical possession of the good at the time of decision and having to give it up if they fail to buy, not saliently framing buy/sell as gain/loss, and asking subjects to answer a series of yes/no questions to plausible values for the good being valued during training, may have weakened the reference states of buyers and sellers, thereby making gains and losses less salient, and it is the lack of saliency of the reference states rather than the elimination of subject misconceptions that leads to the disappearance of the WTA-WTP gap. If our interpretation that these manipulations eliminate the gap by weakening the reference states is correct, then we should observe a gap if we modify their procedures to make the respective reference states of sellers and buyers (i.e., having versus not having the good) more salient while keeping all procedures that they deem necessary to control for misconceptions. In contrast, if PZ's interpretation is correct and the gap is really due to subject misconceptions, then reference states are irrelevant and we should continue to observe no gap despite procedural modifications to make the reference states more salient.

## **Experimental Design and Results**

Four experimental sessions were carried out at the National University of Singapore with 120 students recruited from an advertisement posted on the university's Virtual Learning Environment, a website that publishes all university course materials.<sup>4</sup> Table 1 summarizes the major characteristics of each session and the results.<sup>5</sup> 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

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<sup>3</sup> PZ acknowledge that “...we have neither a general theory of what might constitute misconceptions nor a set of operational definitions characterizing them. Constructing a full set of procedures to control for them could be very difficult, as they might depend on such subtle features as the speed with which experimental instructions are delivered, the distance of subjects from the chalkboard if it is used, the size of writing on the board, how loud the instructions are read, and the nature of pauses or emphasis... In fact, we have no direct evidence that our procedures actually eliminate all misconceptions...” (pp.542-543).

<sup>4</sup> Every university student could view and respond to the advertisement.

<sup>5</sup> Results from an earlier, and unrelated, series of experiments carried out in Canada and in Singapore showed very similar preferences and choices, suggesting little reason for any lack of generality in the present findings (Knetsch et al., 2001).

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.”<sup>6</sup>

All payments and transactions were in Singapore dollars.<sup>7</sup> All four sessions were completed in about half an hour and every subject was eventually paid S\$5 for taking part in the 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 S\$3.75 and not otherwise available.<sup>8</sup> 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, instead of using a random number generator like PZ (which subjects might be unfamiliar with), we simply 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 only 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,

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<sup>6</sup> To avoid any confusion about the reimbursement, when the participants were selected and notified, they were explicitly told that since the experiment was expected to last for 30 minutes, the participation fee was expected to amount to S\$5.

<sup>7</sup> The Singapore dollar was worth about USD 0.70 at the time.

<sup>8</sup> For the actual experimental instructions, go to:

<http://courses.nus.edu.sg/course/ecswong/research/Instructions.doc>

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.<sup>9</sup> 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.

All sessions maintained the procedural controls that PZ deemed necessary to prevent 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, but excluded practice rounds involving lottery tasks.

We excluded paid practice rounds because PZ repeatedly emphasized that “the main point, however, is that paid practice rounds seem unnecessary in the presence of other procedures thought to control for subject misconceptions” (p.541).<sup>10</sup> We excluded unpaid practice rounds because PZ mentioned very little about them, presumably because they thought unpaid rounds were unimportant. Furthermore, Isoni, Loomes, and Sugden (2009) – ILS henceforth – showed that despite using unpaid practice rounds and the same rigorous experimental controls that PZ used for the mug round, the WTA-WTP gap remained throughout all fourteen paid lottery rounds and showed no tendency to decline in their replication of PZ.<sup>11</sup> This evidence suggests that practice lottery rounds, paid or unpaid, are very unlikely to have provided training or contributed to the overall elimination of the WTA-WTP gap and subject misconceptions.

More importantly, practice lottery rounds seemed to have introduced, instead of eliminated, some misconceptions. Specifically, out of the three sources of

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<sup>9</sup> Before the subjects left the room, the experimenters informed the cashiers located outside the room what the fixed offer was.

<sup>10</sup> In PZ's experiment 2, the mug round came before paid practice rounds, yet they observed no WTA-WTP gap in the mug round.

<sup>11</sup> ILS also found the same pattern in the original PZ's data set. However, for these paid practice rounds in the original PZ's data set, PZ did not adopt the same rigorous experimental controls that they used for the mug round.

misconceptions that PZ explicitly identified in their footnote (p.540), two seemed mostly attributable to the introduction of practice lottery rounds into their experiments and not generally associated with the endowment effect experiments.<sup>12</sup> First, subjects have misconceptions about “the concept of randomization and the nature of probability” and they believe that a random outcome is predictable, either because they believe that “they can predict the future from past events” or “a particular outcome will occur with probability zero or one.”<sup>13</sup> Second, “valuing lotteries is not a common activity for most subjects” and “they struggle in various ways to quantify their preferences over lotteries.” With the express purpose of controlling for misconceptions, it seems strange to introduce these practice lottery rounds and two out of three misconceptions, then attempt to eliminate them with more training and practice, when the mug round itself – the round that PZ focus on to derive their conclusion – seems exempt from these types of misconceptions to begin with.

Because each of our experimental sessions consisted of only buyers or only sellers, buyers only received instructions for the buying task while 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.

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<sup>12</sup> The source of misconceptions that is unrelated to the use of lotteries is “the elicitation mechanism and the BDM procedure, in particular... Some subjects do not realize that overbidding (underbidding) in the buying (selling) task exposes them to a loss (if the price falls between the true valuation and the bid). Other subjects do not realize that underbidding (overbidding) in the buying (selling) task exposes them to an opportunity cost of a forgone profitable transaction.” We used PZ’s instructions and illustrative numerical examples to eliminate this source of misconceptions.

We acknowledge that the specific sources of misconceptions that PZ highlighted in their footnote were only “some hints about the sources and nature of the misconceptions that the procedures help remove” and they passed these along as “mere speculations and conjectures” (p.540). Nevertheless, without any theory of misconceptions, these speculations and conjectures are the best we can directly address, in addition to following PZ’s experimental procedures as closely as possible.

<sup>13</sup> While randomization is needed to determine the fixed offer, we argue that it can be done and explained more simply without the use of a random device like the random number table as PZ did.

Sessions 1 and 2 replicated all verbal and written instructions of PZ for buyers and sellers respectively.<sup>14</sup> Sessions 3 and 4 implemented our manipulation treatments for buyers and sellers, making three small modifications to the procedures in Sessions 1 and 2 while maintaining all controls that PZ deemed necessary to prevent misconceptions on the part of participants. The three modifications were: first, strengthening buyers' reference state by not giving buyers physical possession of the mugs at the time of decision so that they evaluate a purchase as a gain rather than as an avoidance of a loss; second, strengthening sellers' reference state by framing a sale as a relinquishment or loss of their mugs; and third, removing possible suggestion of value and anchoring effect in the numerical examples given. We discuss each modification in turn.

First, in PZ, "sellers were told that they owned the mug" whereas "buyers were told that they could inspect the mug but they did not own it" (p.539), but both buyers and sellers had *physical* possession of the mugs at the time of decision. Having physical possession regardless of ownership is likely to weaken the reference state for buyers, resulting in ambiguity in the framing of gains and losses. Even assuming that the reference state is determined only by the status quo, it is unclear whether the status quo implies a reference state of having or not having the mugs for buyers because two aspects of the status quo – ownership status and possession status – create conflicting reference states and evaluations. 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 physical possession of the mug, they should evaluate the mug as a loss when they failed to acquire it because they had to physically give it up; so loss aversion would induce a higher dollar value for buyers if they wanted to avoid parting with the mugs.<sup>15</sup> Having physical possession may be a more salient status quo when ownership is given instantaneously and arbitrarily by the

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<sup>14</sup> We adopted a few very minor modifications suggested in ILS to make the instructions more consistent and complete. These minor changes could not have accounted for any change in the experimental results, as they did not change the experimental results in ILS's replication of PZ in the mug experiment.

<sup>15</sup> The words "possession" and "endowment" are ambiguous enough to imply either ownership or no ownership. For example, one of the definitions of "possession" is "actual holding or occupancy with or without rightful ownership" (The American Heritage Dictionary of the English Language, 2000, Fourth Edition, Houghton Mifflin Company).

experimenters; buyers may feel no less entitled to owning the mugs than sellers under these conditions.<sup>16</sup>

As a result, to strengthen the reference states of buyers and sellers and to avoid any ambiguity in the framing of the transactions<sup>17</sup>, only sellers had possession of the mugs at the time of decision in our manipulation treatments; buyers passed around a few 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. Like PZ, sellers were told that they could inspect the mug and they now owned the mug in their possession, while buyers were told that they could inspect the mug but they did not own it. 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 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.”<sup>18</sup>

Second, PZ referred to “the item” or “the mug” for both buyers and sellers. Moreover, they sometimes did not frame a sale as sellers “giving up” their possessions for money. To induce a gain/loss mental frame and to strengthen the reference state of possession for sellers, in our manipulation treatments, we 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 always framed a sale as sellers “giving up” their items or their mugs. 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.”

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<sup>16</sup> In other words, if sellers feel a much greater sense of entitlement to the good than buyers, then possession or non-possession may be less salient and may make little difference to the establishment of the reference states.

<sup>17</sup> Framing in gains and losses matters if and only if there is reference dependence.

<sup>18</sup> Some sellers in a pilot experiment revealed that despite being told that they owned the mugs, they did not feel that they really owned the mugs, i.e., they did not feel that they were entitled to take the mugs home with them if they decided not to sell. As a result, some sellers in a pilot experiment left without taking the mugs with them, even though they had been told that they owned the mugs and they did not sell the mugs at the fixed offer. In this pilot experiment, to convey ownership, we followed PZ and said, “You can inspect the mug. You now own the mug.”

It is worth emphasizing that telling sellers that they could take the mugs home with them at the end of the experiment if they decided to keep the mugs later could not be construed as a signal that the mugs were very valuable, that the mugs were gifts, or that the “correct” choice was to keep the mugs because the instructions did no more than explaining to sellers the consequence of a choice they got to make later.

Third, to illustrate how subjects would work out the minimum WTA and maximum WTP, PZ's instructions 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. However, the anchoring effect literature shows that answering a Yes/No question to an arbitrary anchor tends to bias the value elicited in a subsequent open-ended question towards the anchor value (Tversky and Kahneman, 1974). Furthermore, subjects might take dollar figures in the examples as the experimenter's suggestion of "correct" values, again leading to bias in the values elicited. Thus, to remove possible anchoring effect and suggestion of value, our manipulation treatments 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.<sup>19</sup>

Table 2 reports the individual subject data and their summary statistics. In our replication of PZ in Sessions 1 and 2, we find a median WTP of \$2.5 (mean of \$2.57) and a median WTA of \$3 (mean of \$4.1), leading to a median WTA-WTP ratio of 1.2 (mean of 1.60). If we exclude two potential outliers exceeding \$10 in Session 2, then our replication of PZ yields a median WTA of \$3 as before (but a much lower mean of \$3.39), leading to a median WTA-WTP gap of 1.2 as before (but a much lower mean of 1.32). In contrast, in our manipulation treatments in Sessions 3 and 4, we find a median WTP of \$2 (mean of \$2.56) and a median WTA of \$4 (mean of \$3.8), leading to a median WTA-WTP ratio of 2 (mean of 1.48). None of these offers exceeded \$10.

Table 3 reports the results of statistical tests to determine whether the data support the hypothesis that WTA is significantly different from WTP. Panel A reports

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<sup>19</sup> For instance, PZ 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].  
 To be sure, it is not obvious that anchoring effect has led to PZ's finding of no gap. Nevertheless, given the well-established literature on anchoring effect, we believe that experiments that aim to control for subject misconceptions should be careful to also rule out anchoring effect and possible suggestion of value.

the results using all values elicited. We perform the Wilcoxon-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.<sup>20</sup> Using WTP and WTA from our replication of PZ in Sessions 1 and 2, we cannot reject the null hypotheses of identical distributions or identical medians at the conventional significance levels; 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 0.5880 ( $p=0.443$ ). In other words, when PZ's procedures were used, there was little evidence that WTA was different from WTP.

In contrast, when PZ's procedures were modified to strengthen the reference states and remove possible suggestion of value and anchoring effect, there was strong evidence that WTA was different from WTP. Using WTP and WTA from our manipulation treatments in Sessions 3 and 4, the Wilcoxon-Mann-Whitney rank sum test results in a  $z$  value of -2.495 ( $p=0.0126$ ); therefore, we can reject the null hypothesis of identical distributions at the five percent level. Similarly, a median test results in a Pearson  $X^2$  statistic of 8.2127 ( $p=0.004$ ); therefore, we can reject the null hypothesis of identical medians at the one percent level.

Based on the individual data in Table 2, it is natural to ask to what extent including potential outliers with WTA exceeding \$10 in Session 2 might have affected our conclusions. Panel B of Table 3 reports the results when these potential outliers are excluded. It is clear that excluding these high offers would only make the WTA-WTP gap even less significant in our replication of PZ and we still would not be able to reject the null hypotheses of identical populations or identical medians for WTA and WTP; specifically, the Wilcoxon-Mann-Whitney rank sum test now results in a  $z$  value of -1.080 ( $p=0.2803$ ), whereas a median test results in a Pearson  $X^2$  statistic of 0.2414 ( $p=0.623$ ). Both are highly statistically insignificant.

## **Discussions and Conclusions**

Keeping all procedural controls that PZ deemed necessary to prevent subject misconceptions, we found statistically insignificant valuation disparities between buyers and sellers when we used PZ's procedures, but highly statistically significant

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<sup>20</sup> Following PZ, we did not perform tests for the equality of means because means are more sensitive to outliers, which may be a concern for WTA in Session 2.

disparities when we modified subtle details of PZ's procedures to strengthen the reference states of buyers and sellers and to remove possible suggestion of value and anchoring. These modifications are not expected to reduce the extent of control for misconceptions. 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 endowment versus the alternative, 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 versus the alternative; 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 carefully inspect the good being valued.<sup>21</sup> Thus, the results suggest that the WTA-WTP gap was turned on and off by different experimental procedures in PZ, not because different procedures induced or eliminated various subject misconceptions or classical incentives, but most likely because they made the reference states, from which gains and losses were evaluated,

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<sup>21</sup> In an exchange experiment where two goods (the endowed good and the alternative) are involved, Plott and Zeiler (2007) propose a few hypotheses on how physical possession of one good but not the other may introduce some classical incentives that promote valuation disparities for reasons unrelated to reference dependence. These arguments seem much less plausible in our WTA-WTP experiments where only one good was involved and buyers were separated from sellers. Nevertheless, we briefly discuss them in turn.

First, could physical possession change the transaction cost of buying versus not buying? In our experimental setup, there is no meaningful difference in the marginal private transaction cost of buying versus not buying whether there is physical possession or not. Furthermore, Plott and Zeiler (2007) have ruled out this explanation themselves.

Second, could physical possession induce subjects to perceive the mug as a gift from the experimenters? However, there is no classical preference theory that links physical possession to the idea of a gift. Moreover, our experimental instructions never used words that were suggestive of a gift in any of our treatments. In contrast, in Plott and Zeiler's (2007) gift treatment, the experimenter explicitly announced, "I am giving you X. It's a gift." (p.1454).

Finally, could giving physical possession to sellers but not buyers be construed as a signal of either the experimenters' intention for sellers to keep the mugs or the mugs' value because of their proximity? This argument is untenable for a few reasons. First, to the extent that a signal arises from a comparison between having physical possession versus no possession, there was no signaling in our experiments because in any session, either all subjects have possession or they all have no possession. Second, the offers recorded were blind to the experimenters and so the experimenters' intention was irrelevant. Furthermore, the classical preference theories assume that people maximize self-interest without other-regarding preferences. Third, there is no classical preference theory that links the location of a good to the perception of value; one could just as easily argue that subjects may treasure goods in their possession less than those that are not in their possession (for example "because the grass is always greener on the other side").

more or less salient. Saliency of the reference states also turns out to be a more parsimonious and consistent explanation.

Our three procedural modifications 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<sup>22</sup> underestimated their valuations of the mugs once they were given possessions.<sup>23</sup> 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 if other determinants are sufficiently salient. Specifically, Köszegi and Rabin (2006) propose a model in which the reference states are people's expectations of outcomes and suggest that one interpretation of PZ's findings is that "they have successfully decoupled subjects' expectations from their initial ownership status" (p.1142).<sup>24</sup>

Consistent with Köszegi and Rabin's interpretation, 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. Specifically, using Plott and Zeiler's (2007) exchange experiments where subjects were endowed with one good but given the opportunity to trade for another good, Knetsch and Wong (2009) 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

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<sup>22</sup> A mug was displayed at the front of the room.

<sup>23</sup> Each subject was actually given a mug. To describe possession, Loewenstein and Adler (1995) used phrases like "obtain the mug", "receive a mug", or "get a mug" but never used words like "own" or "ownership" in their experimental instructions.

<sup>24</sup> The reference state is basically left unspecified in Kahneman and Tversky (1979) and has generally been assumed to be the status quo. But as Köszegi and Rabin (2006) point out, "in virtually all experiments interpreted as supporting this assumption, subjects plausibly expect to keep the status quo, so these studies are also consistent with the reference point being expectations" (p.1142).

perceived reference states and the initial endowments, the participants show no such reluctance to trade despite having ownership of the original entitlement” (p.408). Therefore, they argue that the mechanism through which Plott and Zeiler’s (2007) procedural controls turn the exchange asymmetry off is by weakening the reference states perceived by participants rather than by eliminating misconceptions or classical incentives that can induce asymmetric choices such as transaction costs, signaling of relative value, and cascade effects. While the exchange experiments have the advantage of avoiding rate of substitution measurement and complex elicitation procedures, their results give no indication of the monetary strength of the endowment effect or the WTA-WTP gap. This paper extends Knetsch and Wong (2009) to measure the WTA-WTP gap, while controlling for misconceptions over complex elicitation procedures.

Another literature investigates anchoring effect 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, Loewenstein, and Prelec (2003) apply the anchoring 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, Jacowitz, Kahneman, and McFadden (1998) find similar effects in the WTP for public goods from Yes/No referendum responses and open-ended responses.

Nevertheless, in retrospect, out of the three procedural modifications, anchoring effect and its removal are unlikely to be a key driver of our results. If anchoring effect is important and adjustment from the anchor value is insufficient, then we expect a smaller spread in the values elicited in our replication of PZ (where anchoring is postulated to be present) than in our manipulation treatments (where anchoring is supposedly removed). However, the estimates in Table 2 suggest that this is true for WTP but not for WTA; while the standard deviation for WTP is

smaller in our replication of PZ than in our manipulation treatment (1.42 versus 2.51), the standard deviation for WTA is actually larger in our replication of PZ than in our manipulation treatment (3.60 versus 2.36). Therefore, the contrasting results between our replication of PZ and our manipulation treatments are more likely to be due to the first two modifications that made the reference states of buyers and sellers more salient.

The different experimental conditions and results between PZ's procedures and our manipulation treatments may help us understand some field evidence that shows similar on-off results for loss aversion. For example, Genesove and Mayer (2001) find that "both investors and owner-occupants behave in a loss-averse fashion" in the housing market but "investors exhibit about one-half of the degree of loss aversion as owner-occupants" (p.1235). While experience and transaction cost may well account for the different degree of loss aversion for investors and owner-occupants, our results suggest another parsimonious explanation. Property investors often do not have physical possession of goods traded: they often do not occupy the property they bought specifically for investment. Expecting to trade, investors are also unlikely to feel possessive of their properties and to frame sales as losses.<sup>25</sup> On the other hand, owner-occupants have to give up actual and physical possession to sell, such as owner-occupants vacating their properties, and the act of relinquishment is often salient. So they are likely to frame a sale as a loss of their possessions.

Finally, it is natural to 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" (ILS, p.19)?<sup>26</sup> Specifically, assuming that saliency of the reference states is the reason behind the on-off results in WTA-WTP experiments, as we have argued, are there any reasons why the reference state may be more salient in lottery valuations than mug valuations when PZ's procedures

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<sup>25</sup> Köszegi and Rabin (2006) and DellaVigna (2009) discuss how different expectations and probabilistic beliefs may have resulted in different reference states for dealers and experienced nondealers of sports memorabilia versus inexperienced nondealers or visitors to a sportscard show, thus accounting for their different tendency to trade in the field experiments reported by List (2003).

<sup>26</sup> ILS themselves suggest two structural differences between mug and lottery valuation tasks. First, in the lottery tasks but not the mug task, "the response-mode units are also used in specifying the objects that are being valued" and so "lottery tasks may prompt respondents to use 'anchoring' heuristics that are not applicable to other tasks," such as the mug task. Second, tasks involving lotteries with monetary outcomes induce additional types of misconceptions, for which the PZ procedures do not control. Of course, these two structural differences may very well be the reasons why valuation disparities remain in the lottery tasks but not the mug task when PZ's procedures are used.

are used? 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 PZ's and ILS's experiments, anticipatory regrets (Loomes and Sugden, 1982) may be more salient for sellers who have sold the lotteries they are endowed with than for buyers who have not bought the lotteries. Anticipated regret aversion may interact with loss aversion, resulting in more significant WTA-WTP disparities in lottery valuations than in mug valuations.<sup>27</sup>

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, Beattie, van der Pligt and de Vries, 1996)<sup>28</sup> and a higher WTA for sellers of lottery (Humphrey, Mann, and Starmer, 2005).<sup>29</sup> Moreover, there is also some evidence that neither physical possession nor actual ownership of the lottery is necessary to evoke anticipated regret. What is needed, in addition to feedback on lottery outcomes for nonparticipants, is some sense of entitlement to the lottery even when one is not playing. Specifically, Zeelenberg and Pieters (2004) show that 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 in the Netherlands (in which one has to buy a ticket with a number printed on it).

The strength and saliency of the reference states are naturally a function of the economic environment and its complexity. Thus, procedurally-induced weak reference states may be an especially important issue in laboratory experiments where the experimenter gives ownership *arbitrarily* and *instantaneously* without creating

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<sup>27</sup> While subjects are generally not allowed to communicate with each other during the value elicitation exercise, it does not seem too difficult for a seller to imagine that some of his neighbors have kept their lotteries when the lottery outcome turns out to be favorable. Social comparison could accentuate anticipated regrets.

<sup>28</sup> Zeelenberg, Beattie, van der Pligt and de Vries (1996) show that when given a choice between two gambles, one of which will be openly resolved regardless of whether subjects choose it and the other will only be resolved for the subjects who choose it, subjects tend to choose the gamble that will be resolved for certain regardless of whether it is the safer gamble to minimize anticipated regrets.

<sup>29</sup> Humphrey, Mann, and Starmer (2005) endowed subjects with a real world lottery in the form of a UK National Lottery scratchcard (where lottery outcome is revealed by scratching off six latex panels) and used the BDM random price mechanism to elicit the subjects' WTA to sell the lottery at 30 prices ranging between £0.05 and £1.5. They find that for 26 out of the 30 prices, the proportion of subjects keeping the lottery in the group that received complete outcome feedback despite having sold the lottery was greater than the corresponding proportion in the group that did not receive outcome feedback if they sold the lottery.

any sense of entitlement or possession to the newly owned good. Because the reference state and its saliency may depend on a number of factors such as ownership, status quo, and expectations, which generally, but not always, coincide with the establishment of the same reference state, it may be more accurate to think of the endowment effect as a “reference effect” rather than as an “ownership effect”. Finally, it is generally difficult to specify a set of sufficient conditions for saliency; what suffices in a simple and intuitive environment may be inadequate in a complicated environment that bears little resemblance to real life situations, as more complicated procedural controls are included to rule out potential confounding factors. But this difficulty only 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.

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

Treatment	PZ (Replication)	KW (Manipulation)
Statistically significant WTA-WTP gap?	No	Yes
Buying Task	N=31 (Session 1)	N=30 (Session 3)
Selling Task	N=29 (Session 2)	N=30 (Session 4)
Possession at time of decision	Yes for sellers Yes for buyers	Yes for sellers No for buyers
Framing or reference formation	Use “the mug” for both buyers and sellers.  Sometimes frame selling as “giving up” their mugs but not always.	Use “the mug” for buyers, but use possessive determiners for sellers (e.g., “my mug”).  Always frame selling as “giving up” their mugs.
Possible anchoring or suggestion of value	Examples use numerical values that are plausible for a coffee mug	Examples use numerical values that are absurd for a coffee mug and emphasize that they are hypothetical
Controls for Misconceptions		
1. Incentive-compatible elicitation device	Yes	Yes
2. Numerical examples explaining why it is not optimal to underbid or overbid	Yes	Yes
3. Anonymity in decision	Yes	Yes
4. Use of lotteries	No	No
5. Buyers and sellers are different subjects	Yes	Yes

**Table 2 – Individual Subject Data and Summary Statistics**

Session	Treatment	Individual responses (in S\$)	Mean	Median	Std. dev.
1	PZ.Buyer WTP N=31	0, 0, 0.45, 1, 1, 1, 1.2, 1.5, 1.8, 2, 2, 2, 2, 2.5, 2.5, 2.5, 2.5, 2.5, 3, 3, 3.45, 3.5, 3.55, 3.9, 4, 4, 4, 4, 4.5, 4.5, 5.75	2.57	2.50	1.42
2	PZ.Seller WTA N=29	0, 0, 0.5, 1, 1, 1, 1.5, 2, 2, 2, 2, 2.5, 2.5, 3, 3, 3.5, 4, 4.5, 4.5, 4.5, 5, 5.5, 5.5, 6, 6, 8.5, 10, 12.5, 15	4.10	3.00	3.60
3	KW.Buyer WTP N=30	0, 0, 0, 0.5, 0.5, 1, 1, 1, 1, 1, 1.5, 1.5, 2, 2, 2, 2, 2, 2, 2.5, 2.5, 2.5, 2.5, 2.5, 3, 3.5, 4.5, 5.5, 8, 8.9, 10	2.56	2.00	2.51
4	KW.Seller WTA N=30	0, 0, 0, 0.5, 1.5, 2, 2, 2, 2.5, 2.5, 2.5, 3, 3, 3.5, 3.5, 4.5, 4.5, 4.5, 4.5, 5, 5, 5, 5, 5.5, 5.5, 5.5, 6.5, 7, 7.5, 10	3.80	4.00	2.36

**Table 3 – Statistical Test Results**

	Wilcoxon-Mann-Whitney rank sum test (Null hypothesis: identical distributions)			Median test (Null hypothesis: populations have identical medians)		
<u>Panel A:</u> All Offers	<i>z</i>	<i>p</i> -value	Conclusion ( $\alpha = .05$ )	Pearson $X^2$	<i>p</i> -value	Conclusion ( $\alpha = .05$ )
PZ treatment (Sessions 1 and 2)	-1.484	0.1378	Can't reject null	0.5880	0.443	Can't reject null
KW treatment (Sessions 3 and 4)	-2.495	0.0126	Reject null	8.2127	0.004	Reject null
<u>Panel B:</u> Exclude Offers>10	<i>z</i>	<i>p</i> -value	Conclusion ( $\alpha = .05$ )	Pearson $X^2$	<i>p</i> -value	Conclusion ( $\alpha = .05$ )
PZ treatment (Sessions 1 and 2)	-1.080	0.2803	Can't reject null	0.2414	0.623	Can't reject null

Note: The Pearson  $X^2$  statistics were corrected for continuity.

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### Appendix: Summary of Experimental Instructions

This appendix contains a summary of the experimental instructions to facilitate easy comparison. Actual experimental instructions used by the experimenters and written instructions received by the subjects are available upon request.

The instructions for PZ replication (Treatments 1 and 2) are shown in black, whereas the instructions for our manipulation treatments (Treatments 3 and 4) are shown in square brackets and in red, e.g., [red], when they deviate from the instructions for PZ replication. 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

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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.

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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.

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

# Record Sheet

ID Number: \_\_\_\_\_

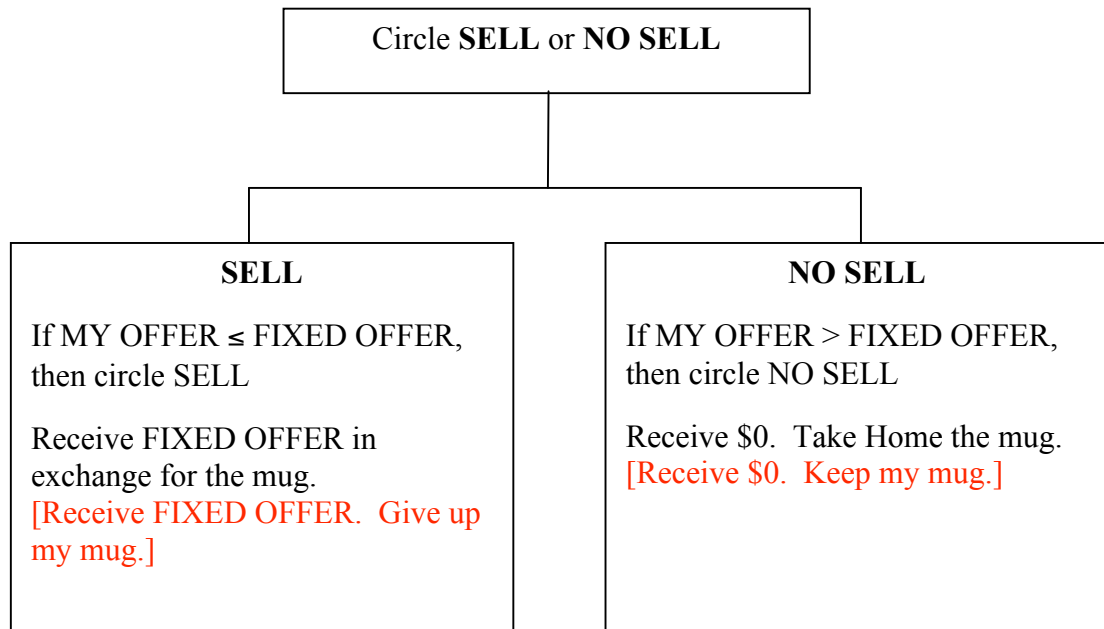
Step 1: decide on my offer.

MY OFFER : \$\$ \_\_\_\_\_

Step 2: listen for fixed offer announcement

FIXED OFFER : \$\$ \_\_\_\_\_

Step 3:



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

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### 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.

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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.”

## Not for Publication

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**

# Record Sheet

ID Number: \_\_\_\_\_

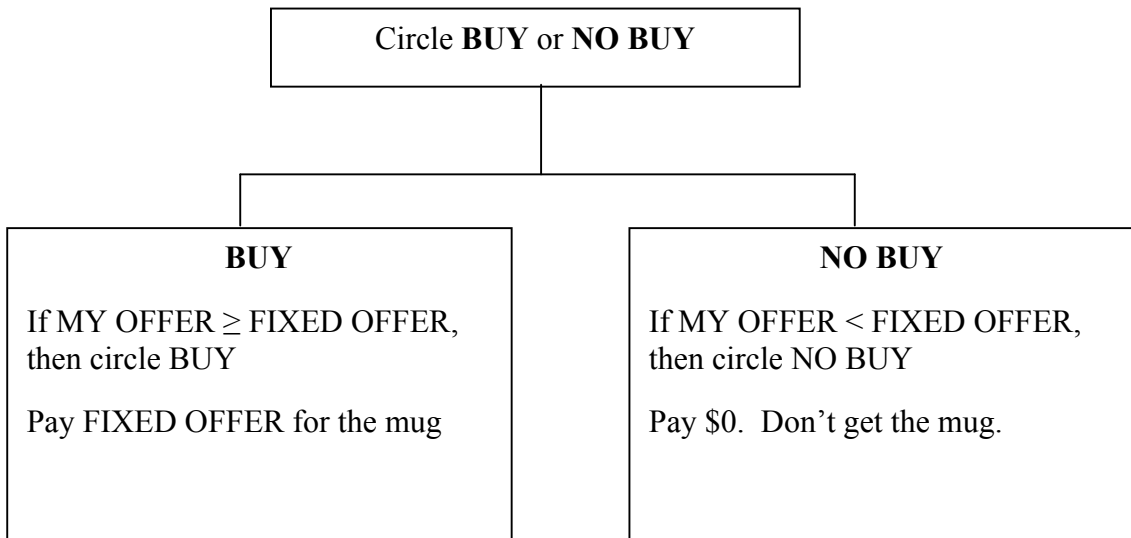
Step 1: decide on my offer.

MY OFFER : S\$ \_\_\_\_\_

Step 2: listen for fixed offer announcement

FIXED OFFER : S\$ \_\_\_\_\_

Step 3:



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