CBESS Discussion Paper 16-04

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The Implications of Attachment through Choice and Order Effects on the Willingness-to-Accept- Willingness-to-Pay Disparity

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The Implications of Attachment through Choice and Order Effects on the Willingness-to-Accept- Willingness-to-Pay Disparity[*]

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March 18th, 2016

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PsycINFO Classification: 2346; 3920

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1. Introduction

It is a well-established finding that a consumer's minimum willingness-to-accept (WTA) to give up a good often exceeds their maximum willingness-to-pay (WTP) to purchase the same good. This is inconsistent with a fundamental principle of consumer theory, that an individual's indifference curves may be drawn independently of their budget constraint or endowment. Thaler (1980) presented this as an endowment effect, as a manifestation of loss aversion (Kahneman and Tversky, 1979); the idea that being endowed with the good leads to a reluctance to part with it, manifesting a willingness-to-accept that is higher than the willingness-to-pay of a prospective owner not endowed.

Extensive study of the WTA-WTP disparity has found a number of factors, such as type of good (Horowitz and McConnell, 2002), experience or repetition (Shogren *et al*., 1994, List, 2003, Loomes *et al*., 2010) or level of exposure to the good (Knetsch and Wong, 2009), which can impact on the size and scale of the disparity. Positive emotional states lead to an increase in the size of the endowment effect, relative to negative states (Lin *et al*., 2006) and in studying the effects of possession and valence on loss aversion, negative endowments (goods causing disutility) are more willingly traded than kept (Brenner *et al*., 2007), suggesting that emotional considerations of the type of good can also affect the WTA-WTP disparity. More recent research presents attentional biases as an alternative explanation of the disparity, whereby buyers and sellers assign different thought processes to transactions of this type (Carmon and Ariely, 2000, Nayakankuppam and Mishra, 2005, Johnson *et al.*, 2007, Pachur and Scheibehenne, 2012) that extend beyond simply whether one is or is not endowed with a good. In general, however, these differences in attentional focuses are solely attributed to the role of the participant, whether a buyer or seller, and not to the specific differences of the attributes of the goods themselves.

Typically, goods in these experiments are simply given arbitrarily to subjects to buy or sell; the type of good is pre-determined and subjects have no involvement in determining which goods they might buy or sell. This too is true of other experiments testing the WTA-WTP disparity, using typical goods such as mugs (Kahneman *et al*., 1990, Nayakankuppam and Mishra, 2005, Reb and Connolly, 2007). There are two potential issues with this style of experimental procedure that this paper hopes to address. Firstly, in almost every consumer market, choice plays a big part in personal decision making, spending and consumption. This represents a fundamental difference between the decision problems used in most experimental studies of the WTA-WTP disparity and those faced by consumers in the real world. Secondly, whilst such experimental procedure is fairly common within the WTA-WTP literature, the average experimental participant is unlikely to be familiar with the concept of buying and selling real goods within experiments. Whether subjects are affected by their prior exposure to such goods within an experimental setting (or their prior expectation of experimental procedure) might also be an influential factor in determining consequent valuations.

An experimental investigation of the attentional biases of buyers and sellers has found evidence that individuals in each of these roles focus on what they stand to forgo- expenditure in the case of buyers, and the good, or experience, in the case of sellers. Selling and buying prices were found to be affected by different manipulations of aspects of the experience of a good (Carmon and Ariely, 2000). If choice is an important aspect of individual decision making and "…close consideration of choice options may lead consumers to become attached to them…" (Carmon

et al., 2003, p.16), then this manipulation of good experience may be influential in determining buying and selling prices. Similarly, if subjects are experiencing a good for the first time within an experimental setting, a sense of novelty (either towards the good specifically or more generally to the experimental procedure of buying or selling real goods) might also increase a focus of attention towards the novel attributes of the goods. Berlyne (1951, pp.272-273) suggests that novelty increases attention towards a stimulus, but that this diminishes over time.

This paper seeks to investigate the impact that attachment towards a good, formed through the act of choice, has on the WTA-WTP disparity. If subjects chose which goods they had the opportunity to buy or sell, a theory of pure endowment would predict little effect on the disparity (as the experimental endowment itself is not affected). If choosing a good generates attachment towards the chosen good, and so alters the consumer's experience of the good, then a theory of attentional bias instead predicts that this affects the focus of attention toward the chosen good, suggesting that buying and selling prices (and so the overall disparity) could be affected by the formation of attachment through choice. If sellers focus on the experience of a good more than buyers, it could be expected that sellers would be more affected by manipulations of attachment towards a good, implying an increase in the overall WTA-WTP disparity. This paper also reports a surprising finding of order effects as a result of experimental design, whereby the first observed good is often most highly valued, and suggests increased attention towards the novel attributes and experience of these first observed goods as a possible explanation.

Within experimental settings the act of choice has often been attributed to the formation of a sense of attachment towards the chosen object (Carmon *et al.*, 2003, Gawronski *et al.*, 2007, Morewedge *et al*., 2009). Indeed it has been found that those who are offered some choice are more satisfied with a good than those simply given the same goods (Iyengar and Lepper, 2000). Whilst this attachment through choice has been argued to be a result of cognitive dissonance (Brehm, 1956, Cooper, 2007) - the idea that one must value something precisely because one has chosen it, this does not detract from the fact that the act of choosing is an effective method of creating a form of attachment toward the chosen good.

Conventional economic theory implies that adding choice options to an existing choice set should have a weakly positive effect on welfare. Choice overload, where too much choice causes disutility, forms a part of consumer literature, however. A meta-analysis of choice overload experiments cites factors such as diminishing perceived differences between options, time-cost of comparisons, and the increase of expectation with choice as reasons behind the cause of such a phenomenon (Scheibehenne *et al*., 2010). However, in real field data, even when samples were large (averaging 34 choices) the vast majority of reduced choice conditions yield reduced sales or little change (p.411) and overall there exists a lack of robustness in the evidence of choice overload (p.421), suggesting it is unlikely that experiments with small choice samples would be affected by any negative effects of excess choice options.

If attachment through choice transpires to have implications on individuals' monetary valuations of goods, and the potential for this occurrence is overlooked, then general preferences of individuals may be incorrectly determined, which is of both academic and practical concern. In terms of consumer research, such an effect on the valuations of buyers would have implications of behaviour in retail markets, and could be a powerful tool for marketing. In public policy, with willingness-to-pay and contingent valuation a common

method of valuation elicitation (Hanemann, 1994, for example), better understanding the motivations that help individuals derive their valuations could help to improve the accuracy of the valuation of public goods. The findings of strong order effects, and the possible role of novelty, is potentially important for stated preference methods of eliciting valuations of non-market goods and for experimental methodology.

This paper proceeds as follows: section two discusses the influence of choice, random lottery incentives and order effects in determining the experimental design. Section three provides a detailed outline of the experimental design, section four addresses the key hypotheses of the paper and section five gives both raw data and statistical analysis of the results. Section six provides a discussion of these findings and section seven concludes.

2. Issues of Experimental Design

An effective experimental comparison of willingness-to-pay and willingness-to-accept valuations with and without the formation of attachment through choice needs to satisfy certain design requirements. Comparing the valuation of a good obtained through the act of choice with that of a good that has simply been given to an individual is one possible method (Iyengar and Lepper, 2000). However, for a controlled test this seems insufficient. A controlled test would compare valuations of goods that had actually been chosen with the valuations of the goods that subjects *would have* chosen, had they been given the opportunity to choose. Simply giving subjects one of a selection of goods would not provide such a comparison, as the good given might not be the good that the subject most preferred.

An alternative design would be to ask non-choosing subjects to value each good individually, using a random lottery design to incentivise truthful revelation of valuations. This design would have the desired effect of not providing an opportunity for the formation of attachment through choice, as no choice is made by the subject. A comparison could then be made between the *explicitl*y most preferred chosen good of choosers and the *implicitly* most preferred (that is, most highly valued) good of the non-choosers.

However, Loewenstein and Adler (1995) discover that random lottery devices can result in an underestimation of valuation compared to when a good is actually received. Using just selling prices, they found that goods owned conditional on some random device to determine ownership (the flip of a coin, in their case) were valued significantly less than the same goods that were owned with certainty. Offering subjects the opportunity to revise their selling prices once ownership was established led to a significant upward revision of selling prices. The random lottery design of non-choosers would therefore be likely to result in reduced valuations as a result of *conditional ownership*. To control for this effect, the likelihood of actually receiving the most preferred good (either chosen or implied through valuation) would need to be constant across treatments.

Another potential problem of this design is that non-choosers' valuations of the various goods might be affected by the order in which these goods are presented. In hypothetical contingent valuation studies, order effects have been observed (Bateman and Langford, 1997, Payne *et al*., 2000). It has been argued that this observation might result from embedding effects (Kahneman and Knetsch, 1992), whereby when different bundles of goods vary in objective size or value, the absolute valuations of goods can depend on whether larger or smaller goods

were valued first. There is some evidence that embedding effects also occur for private goods and when decisions are potentially binding (Bateman *et al*., 1997). Clark and Friesen (2008) observe order effects in nested bundles of private goods which differ in objective value and Ariely *et al*. (2003) discover order effects in the willingness-to-accept to listen to objectively differing unpleasant sounds. The investigation of order effects in valuations of goods which differ only in subjective value is, to the author's knowledge, novel. Such goods should not be subject to embedding effects.

However, if order effects are caused by some manifestation of reduced attention towards goods that are confronted later in an experiment, then theories of attentional bias predict both buying and selling valuations might be affected by this. It is therefore important to attempt to control for such effects by counterbalancing the order in which goods are valued and by testing for possible effects within such an experimental design. A post-experimental preference revelation exercise would also offer a comparison of ex-post preference decisions with the explicit choice preferences and implicit preferences by valuation during the experiment, to test whether treatment variables of the experimental design (treatment type or buyer/ seller) were influencing preferences and consequent valuations.

3. Experimental Design

This experiment aimed to control and measure the impact of attachment on the WTA-WTP disparity. The experiment took place in early 2015 at the University of East Anglia's Centre for Behavioural and Experimental Social Science (CBESS). All subjects were recruited through the Centre's online recruitment system and had no prior experience of experiments of this type. In all experiments, subjects were seated in isolated booths as instructions were read aloud to outline the nature of the experiment (subjects were all given written sets of instructions to follow along with). The experiment was conducted using experimental software package z-Tree (Zurich Toolbox for Ready-made Economic Experiments) (Fischbacher, 2007). All subjects received a show-up fee of £6.00, plus or minus what they acquired in the experiment itself.

A 3x2 between-subject experimental design was used. The three treatments, Chosen, Diluted Choice and Random were sub-divided into two sub-treatments, Buyer and Seller. In general, buyers were presented with a good and asked at different prices whether they would be willing to pay that price to buy the good from the experimenter and take the good away. Sellers, on the other hand, were given a good and told it was theirs to keep, before being asked at different prices whether they would be willing to accept that price to sell the good back to the experimenter. This is in keeping with the designs used in other experiments that have investigated the WTA-WTP disparity. All treatments included pre-experimental quizzes, which took place after experimental instructions were read aloud, in which subjects were tested on their understanding of the experimental procedures and decision-making mechanisms. (Subjects who answered incorrectly were directed back to the relevant instructions before being able to continue.)

In the Chosen treatment, subjects completed one *choice* goods task. This treatment was designed to allow for the formation of attachment through the act of choice. The experiment began with a choice between three mugs shown on the subjects' computer screens (see Figure

1). Subjects were asked to select which mug they wished to choose; buyers were informed this would be the good they would have the opportunity to buy from the experimenter, and sellers were informed that this would be the good they owned and would have the opportunity to sell back to the experimenter. Once choices were made, subjects received their chosen goods before making their valuation decision.

--- Figure 1 near here ---

In the Random treatment, there was no such opportunity for the formation of attachment through choice. Subjects in this treatment completed five tasks, three *given* goods tasks and two lottery tasks (see Figure 2). After subjects had completed all five tasks, one was selected at random to be played for real, and subjects' decisions in the selected task were made binding. All subjects were made aware of this in the experimental instructions. In each of the three *given* goods tasks, subjects were simply given one of the three possible mugs before making their valuation decision. By comparing the three valuations made by each subject, it was possible to identify that subject's implicitly most preferred mug.

To account for the potential random lottery effects in Random, a third treatment, Diluted Choice, was introduced. Diluted Choice consisted of five tasks, one *choice* goods task and four lottery tasks. As in Chosen, in the *choice* goods task subjects were shown three pictures of the possible goods and asked to choose their preferred good to have the opportunity to buy or sell, allowing again for the formation of attachment through choice, and subjects received their chosen good before making their valuation. This task was completed alongside four lottery tasks, and after all five tasks were completed, one was selected at random, to be played out for real. This mimicked the random lottery design in Random. In both Random and Diluted Choice, subjects had a 20% chance of their implicitly or explicitly most preferred mug being selected to be the task played for real and so this controlled for the issue of random lottery effects by holding them constant across the two treatments.

In the goods tasks it was important to ensure an incentive-compatible elicitation device was used to encourage truthful valuation, and so a Becker-DeGroot-Marschak (henceforth BDM) mechanism (1964) was used. At increments of £0.20, from £0.20-£6.00, subjects were asked if they were willing to buy/ not buy (or sell/ not sell) at each of these thirty values. Once this was completed, at the end of the experiment one of the thirty values was drawn at random, to be determined as the price of the good, and subjects' decisions at that price were made binding. The fact that truthful valuations were the optimal response in the valuation mechanism was made clear to subjects in both the written instructions and the pre-experimental quizzes.

There is evidence to suggest that the format of upper and lower value boundaries of a BDM mechanism may result in a framing bias (Bohm *et al*., 1997, Bardsley, 2008). As the BDM mechanism limits were held constant across treatments, absolute valuations are not of particular interest; rather the relative comparisons across treatments are important. There were two reasons for choosing an upper limit of £6.00: i) the market value of the good (unknown to subjects) was £2.25 so this gave scope to value higher and lower without being too hindered by the concerns of insufficiently set value boundaries and ii) at the time of the experiment, mugs of a higher quality were on sale at the university retailing at a price of £5.50, so it seemed unlikely that subjects would typically want to reach beyond this upper limit.

To ascertain implied valuations from the BDM mechanism requires a different approach for buyers and sellers. A buyer with consistent preferences would be expected to have a preference reversal from 'buy' to 'not buy' once the value in the BDM mechanism exceeded their own personal valuation. As the specific buying valuation may be anywhere within the £0.20 incremental range of two valuations, the estimated valuation was at the midpoint between the two values, i.e. valuation of buyers was determined as the highest value at which they declared they would 'buy', *plus* £0.10. For sellers with consistent preferences, there would be one preference reversal from 'not sell' to 'sell' once the value in the BDM mechanism exceeded their own personal valuation. As the specific selling valuation may be anywhere within the £0.20 incremental range of the two valuations, the estimated valuation was at the midpoint between the two values, i.e. valuation of sellers was determined as the lowest value at which they declared they would 'sell', *minus* £0.10. Subjects who declared they would 'buy' at no value or 'sell' at every value were determined to have a value of £0.10 (as this implies their personal valuation lies somewhere between £0.00 and £0.20) and subjects who declared they would 'buy' at every value or 'sell' at no value were determined to have a value of £6.10 (as this implies their personal valuation exceeds £6.00). In this way, the calculation of valuations for maximum buying price, and minimum selling price, of buyers and sellers were made comparable, and so hypotheses of equality between the two may be conducted.

For all lottery tasks (see Figure 2) subjects were asked simply to choose which of two possible monetary lotteries they would prefer to play. At the point at which subjects made their decisions, the possible outcomes of the lotteries were shown in fractions of *X*. Subjects were made aware that the value of *X* could be one of thirty values, from £0.20 to £6.00, in £0.20 increments. If a lottery task was chosen, then at the end of the experiment one of the thirty values was drawn at random to be the value of *X*. Subjects were then shown which lottery they chose in that task and an experimenter visited them with a dice to determine their final payoff, in addition to the full show-up fee. This *X*-value lottery design ensured that goods tasks and lottery tasks had the same reference points, so that comparisons of goods valuations across treatments were not distorted by reference points induced by lottery tasks. Within Diluted Choice it was possible for one subject to play a lottery task for real and another to play a goods task for real. This style of *X*-value lottery task meant that both the price of the good and the value of *X* in the lottery could be determined in the same valuation draw.

--- Figure 2 near here ---

The four lottery tasks were designed to test for attitudes of risk and for possible violations of Expected Utility Theory. Figure 2 illustrates the four pairs of lotteries. In both Random and Diluted Choice, subjects chose between pairs of lotteries in lottery tasks (i) and (ii). In lottery task (i), both options have the same expected payoff, but B involves risk and A does not. Thus, a risk averse subject would choose A and a risk loving subject would choose B. In lottery task (ii), lottery B weakly dominates lottery A; thus a choice of A would violate the principle that preferences over lotteries respect stochastic dominance. In Diluted Choice, additional lottery tasks (iii) and (iv) were undertaken and were designed to test for further possible violations of Expected Utility Theory. Lottery task (iii) tests for a stronger aversion to risk, as the expected payoff of the risky lottery B exceeds the certain value of A. Any subject choosing B in lottery task (i), but A in lottery task (iii) would indirectly violate dominance. Finally, a comparison between lottery tasks (i) and (iv) tests for the common ratio effect- a violation of the axiom of independence (Cubitt *et al*. (1998), for example). As lottery task (iv) is simply a scaled down

version of lottery task (i), that axiom implies that choices between A and B should be consistent across the two tasks. The choice of A in task (i) and B in task (iv) would represent the common ratio effect. The lottery tasks allow an investigation of whether attitudes to risk and propensities to violate Expected Utility Theory impact on valuation decisions in the goods tasks.

Task order in both Diluted Choice and Random treatments was counterbalanced to control for order effects. In Random, the order of tasks by type was held constant (lottery tasks were always task 2 and task 4 to act as a cognitive distraction between the three *given* goods tasks), but the specific good or lottery in these tasks was counterbalanced. To best mimic this in Diluted Choice, the *choice* goods task was always task 1, task 3 or task 5, but again, the goods task was counterbalanced across subjects (as were the four lottery tasks). Lottery tasks (i) and (ii) were always played as the two lottery tasks in Random, and all four, (i) – (iv), were played in Diluted Choice. The two choice options within each lottery task were also counterbalanced across subjects. Within the choice part of Chosen and Diluted Choice treatments *choice* goods tasks, the order of the presentation of the pictures of possible goods was counterbalanced too.

Within the WTA-WTP literature the use of mugs as goods to be traded is well established. The use of shapes as patterns was designed to limit the effects of pre-experimental preferences (it seems unlikely that any individual should have substantial preferences between shapes). In deciding on the pattern of shapes two aspects were considered- maximising the perceived value of the mugs and minimising perceived differences in their attractiveness while making them clearly distinct. A previous study has found increases in valuations between plain white mugs and custom-designed university insignia mugs (Tom, 2004), but whether this is a result of attachment to the institution or the comparison of two distinct and separate goods remains ambiguous, and so did not seem an appropriate method to emulate. A pilot study and pre-experimental surveys found that the three shape patterns in Figure 1 yielded higher perceived value of the three mugs and less difference in valuations across the three mugs than other potential patterns, and so were determined to be the most appropriate patterns for use.

4. Key Hypotheses

Table 1 below outlines the six possible sub-treatments of the experimental design.

--- Table 1 near here ---

Null Hypothesis (H0) - Neoclassical Preferences

$$V_{RB} = V_{DCB} = V_{CB} = V_{RS} = V_{DCS} = V_{CS}$$

If individuals acted on neoclassical preferences, their valuations would not be affected by the distinction between buyer and seller, or by attachment or random lottery effects. Under this null hypothesis, and on the assumption of negligible income effects, the distribution of valuations would be the same across all sub-treatments.

Alternative Hypothesis (H1) - Endowment Effect

$$V_{RB} < V_{RS} \quad \text{and} \quad V_{DCB} < V_{DCS} \quad \text{and} \quad V_{CB} < V_{CS}$$

As a manifestation of loss aversion, the hypothesis of the endowment effect predicts that selling prices exceed buying prices. As support of this hypothesis is so wide in the literature, this acts as a test for the validity of the experimental design, which should support a result of this nature.

Alternative Hypothesis (H2) - Attachment Effect

$$V_{RB} < V_{DCB} \quad \text{and} \quad V_{RS} < V_{DCS}$$

As the act of choice has been seen to result in association between an individual and a chosen object (Gawronski *et al*., 2007; p.221) this hypothesis predicts that allowing for attachment, through choice, should result in increased valuations. As attachment is induced through the act of choice in Diluted Choice, any increase in valuation as a result of attachment will be captured through this comparison of Random and Diluted Choice. The hypothesis makes no prediction as to the differences in the size of effects between buyers and sellers.

Alternative Hypothesis (H3) - Random Lottery Effect

$$V_{DCB} < V_{CB} \quad \text{and} \quad V_{DCS} < V_{CS}$$

The results of Loewenstein and Adler (1995) suggest the use of a random lottery design reduces sellers' valuations and it is reasonable to believe that this would apply to buyers' valuations also. Given the uncertainty of only one of five tasks being selected to be played for real in Diluted Choice, if random lottery designs do indeed reduce valuations, then this treatment would be expected to yield lower valuations than in Chosen. This is a result of the comparison of a 20% chance of the one *choice* goods task being selected in the five task Diluted Choice, with a 100% chance of the one *choice* goods task being selected in the one task Chosen. As both treatments allow for the formation of attachment, this is a pure test of the impact of any random lottery effects.

5. Results

A total of 262 subjects took part in the experiment, but 8 subjects reported inconsistent preferences in their valuation decisions such that logical intended valuations could not be deduced, leaving 254 subjects in total with usable data.

5.1 Summary Statistics

--- Table 2 near here ---

--- Figure 3 near here ---

Table 2 provides summary statistics for all treatments. Average valuation across all treatments was £1.38, but there was substantial variation across treatments, from £0.83 (in Random-Buyer) to £2.24 (in Chosen-Seller). When separating into buyers and sellers, on average selling prices (£1.88) exceeded buying prices (£0.93) by 102.2%.Willingness-to-accept exceeds willingness-to-pay by approximately the 'roughly double' found in Kahneman *et al*. (1990) and other experiments of its type, providing strong evidence of an endowment effect. Within each treatment, selling prices were approximately double the value of buying prices, implying that all three treatments were subject to a robust and significant endowment effect. A box plot graph, in Figure 3, shows that median valuations in general are increasing from Random to

Diluted Choice to Chosen and from buyer to seller, and statistical testing confirms that these medians differ significantly (Kruskal-Wallis, $\chi^2(2)= 34.895$, $p< 0.01$***[2]). Dispersion of valuations within the interquartile range and overall range of valuations is generally larger in seller treatments, and the degree of skew differs inconsistently across treatments and between buyer and seller. From this simple analysis it appears that incidences of non-valuations and extremely high valuations relative to treatment type may have some influence in this, and further analysis of the results will shed light on these differences in the distribution of valuations.

Of all subjects, 49 out of 254 (19.3%) would not buy at any of the thirty given values (buyers), or would sell at every given value (sellers). Whilst the reasoning behind such 'non-valuations' remain ambiguous, there are legitimate reasons as to why subjects might not want the good at any valuation, and so these valuations are appropriate to keep in the analysis of results. 31 of the 133 buyers (23.3%) declared such a valuation, compared to 18 out of 121 sellers (14.9%), which differ at a statistically significant level ($\chi^2(1)= 2.893$, $p= 0.089$*). There is little evidence to suggest that the distribution of non-valuations differs across treatments.

5.2 Statistical Analysis

Further analysis tests for evidence in favour of Alternative Hypotheses (H1-H3). The key results of these tests are summarised in Table 3. The use of OLS regressions allowed the use of dummy variables for relevant characteristics of the data, to statistically analyse differences across buyers and sellers, as well as allowing for the control of a number of possible confounding effects, such as demographic information (age, gender, formal Economics study). However, there existed little significant or robust demographic patterns and so only non-parametric statistical analysis is reported here (full regression analysis can be found in Appendix A).

---Table 3 near here---

Alternative Hypothesis (H1) - Endowment Effect

$$V_{RB} < V_{RS} \quad \text{and} \quad V_{DCB} < V_{DCS} \quad \text{and} \quad V_{CB} < V_{CS}$$

For Random, Diluted Choice and Chosen treatments selling prices exceed buying prices by approximately £0.76, £0.84 and £1.22 respectively. A Wilcoxon-Mann-Whitney (WMW) rank-sum test provides strong evidence of a higher willingness-to-accept in sellers than willingness-to-pay in buyers for all three treatments (all significant in a one-tailed test at the 1% level[3]), suggesting very strong support for Alternative Hypothesis (H1) that there is an endowment effect in all three treatments.

Alternative Hypotheses (H2 + H3) - Combined Effect

A first step is to ascertain the total difference in valuation caused by both attachment and random lottery effects; a comparison between the two extreme treatments, Random and Chosen. Buying valuations in Chosen exceed those in Random by approximately £0.18, a 22.1% increase. For sellers, a difference of approximately £0.64 for sellers represents a 40.2% increase between Random and Chosen valuations. A Wilcoxon-Mann-Whitney rank-sum test

[2]Level of statistical significance: *- 10%, **- 5%, ***- 1%.

[3] Unless otherwise stated, all non-parametric tests will present results as a two-tailed test.

of Random vs. Chosen (one-tailed, due to the combined direction of deviation from the null hypothesis) when separating into buyers (WMW, z= 2.022, p= 0.022**) and sellers (WMW, z= 2.117, p= 0.017**) suggests that both are influenced by some degree of attachment and/ or random lottery effects.

Alternative Hypothesis (H2) - Attachment Effect

$$V_{RB} < V_{DCB} \quad \text{and} \quad V_{RS} < V_{DCS}$$

When testing for a pure effect of attachment, differences between valuations across Random and Diluted Choice indicate a presence of an *attachment premium*. For buyers, the value of such an *attachment premium* is approximately £0.11, a 12.8% increase, and for sellers £0.19, an 11.9% increase. Wilcoxon-Mann-Whitney (one-tailed) rank-sum tests fail to find significant differences for sellers (WMW, z= 0.576, p=0.282), with buyers (WMW, z= 1.292, p= 0.098*) appearing modestly affected by an attachment effect.

Alternative Hypothesis (H3) - Random Lottery Effect

$$V_{DCB} < V_{CB} \quad \text{and} \quad V_{DCS} < V_{CS}$$

A comparison between valuations in Diluted Choice and Chosen allows the presence and magnitude of random lottery effects to be isolated. Knowing that the chosen good task is certain to be played out for real as in Chosen (as opposed to the 1 in 5 chance *conditional ownership* of Diluted Choice) results in buyers increasing average valuation by approximately £0.08 (a relatively modest 8.2%). For sellers, however, average valuation increased by approximately £0.45, a much larger 25.2% increase. Wilcoxon-Mann-Whitney (one-tailed) rank-sum tests show no significant difference for buyers (WMW, z= 0.573, p= 0.283) but significant differences for sellers (WMW, z= 1.686, p= 0.046**).

5.3 Random Treatment

A distinctive feature of Random was the act of valuing all three mugs, as opposed to just one in Diluted Choice and Chosen. Though this was determined to be the most effective way to ascertain the valuation of a preferred mug (to be consistent with the other two treatments) it is important to consider whether this difference has any other implications in determining subject valuations.

5.3.1 Random Treatment- Preference Valuation

Recording only the most highly valued good in Random may have had some impact on average valuations. In any treatment, it is possible that subjects may erroneously value the good, either through initial misperception of its worth or through genuine mistake or disinterest, or simply through stochastic variation of preferences. Such 'errors' may result in overvaluation or undervaluation. In Diluted Choice and Chosen, only one good is valued, and it is assumed that any errors are equally likely to lead to over- or under-valuation. What this means for Random valuations is twofold. First, there are three good valuations, and so there are three times the opportunity for such erroneous valuations. Secondly, and more crucially, as only the most valued good is taken as a valuation, there is an upward bias of the impact of such errors. Overvaluations would be more frequently recorded (as undervalued goods are much less likely to still be the most valued good). This could shift upwards the average valuation of Random but leave Diluted Choice and Chosen largely unaffected, which suggests the impact of any

attachment effect and the size of an *attachment premium* might actually be larger than is recorded in these results.

5.3.2 Random Treatment- Order Effects

In Random, preferences for goods was entirely subjective and subjects were held accountable to their decisions, by the use of a BDM valuation mechanism and random lottery design. Thus, there was little prior reason to expect order effects to occur. Regardless, in keeping with standard experimental procedure, care was taken across all treatments to control for any possible order effects. Presentation of images in choice and lottery tasks, lottery task type order and order of goods type were all counterbalanced to control for this.

Testing for evidence of order effects in Random, i.e. whether the order in which shaped goods were presented had any effect on subjects' valuations, requires within-subject comparison, so absolute values of goods are of little interest as these are likely to vary stochastically between subjects. Instead, what is important is simply each subject's implicit preference ranking of the goods. Each goods task can appear in one of three *orders* in the sequence of these tasks: first, second or third.

Within Random, of the 78 subjects, 37 had 'strict favourite' implicit preferences, where one good was valued more highly than the other two and 19 'joint favourite' implicit preferences, where the highest valuation was positive and common to two or three goods. The remaining 22 were those who assigned non-valuation to all three goods. In analysing order effects, the following notation is used. For a given treatment and subject, let O_i be the subject's valuation of whichever good appeared in order i (i = 1, 2, 3) in the sequence of goods tasks. Table 4 below reports the numbers of subjects for whom each of O_1, O_2, O_3 is the strictly highest valuation, and for whom each combination of these valuations is jointly highest.

---Table 4 near here---

In the absence of any confounding effects, one would expect the most preferred goods to be distributed randomly across the three orders. In general, however, there appears to be a tendency for O_1, the valuation of the first observed mug, to be higher than O_2 or O_3. Amongst those with 'strict favourite' implicit preferences, this is particularly prevalent and this finding is strongly statistically significant ($\chi^2(2)= 30.216$, $p< 0.01$***).

For any pair i, j of orders (i, j = 1, 2, 3), let p_{ij} be the probability that the valuations of a random subject have the property $O_i > O_j$, and define $q_{ij} \equiv q_{ji} \equiv 1 - p_{ij} - p_{ji}$, i.e. the probability that $O_i = O_j$. Given that the assignment of the three goods to the three orders has been counterbalanced, the null hypothesis of no order effects implies that, for any given subject, each O_i is a random draw from a single distribution, the same for all i. This leads to two testable hypotheses. The first is that $p_{ij} = p_{ji}$ for all i, j. The second is that $q_{12} = q_{13} = q_{23}$.

---Table 5 near here---

Table 5 reports the results of non-parametric tests of these two hypotheses. In general, valuations decrease with order (i.e. $O_1 > O_2 > O_3$). This effect is much stronger between O_1 and O_2 than between O_2 and O_3, and is much stronger in sellers than buyers. When comparing the prevalence of equal valuation between pairings, the null hypothesis that all three possible equal pairings occur with the same frequency is rejected, and there appears to be significantly more equality in second and third ordered preferences (where equality translates to equal

valuation) than both first and second, and first and third. The implications of these findings both within this experiment and externally will be discussed.

5.4 Shape Effects

As explained in Section 3, the designs of the three mugs were chosen with the intention that the distribution of subjects' valuations would be similar for each design (characterised by the shapes *squares*, *circles* and *triangles*). Because of counterbalancing, the hypothesis tests reported in this paper do not depend on this similarity property, but it is useful to check how far it was satisfied. Since the main tests are concerned with subjects' valuations of their *most preferred* mugs (i.e. the chosen mug in Chosen and Diluted Choice, or the mug that was uniquely or jointly most highly valued in Random), it is particularly relevant to consider whether these valuations were different for different mugs. Table 6 reports, separately for buyers and sellers, the mean valuation of each mug, conditional on that mug being most preferred. This table also reports Kruskal-Wallis tests which show that valuations of most preferred mugs did not differ significantly according to which mug was most preferred.

---Table 6 near here---

It is also useful to consider whether subjects' explicit or implicit preferences between shapes were randomly distributed. Table 7 reports, for each shape, the number of subjects who chose it (in Choice and Diluted Choice) or for whom it was uniquely valued most highly (in Random).

---Table 7 near here---

It is clear that subjects in Chosen and Diluted Choice reveal a much greater skew of preferences for shapes than do subjects in Random, manifested by a much lower propensity to choose triangle mugs. This disparity may be a consequence of order effects. Since valuations in Random decrease with order, there is a tendency for any subject's highest-valued shape to be whichever shape was seen first. That effect will tend to mask underlying (order-independent) preferences for shapes.

5.5 Lottery Effects

The lottery tasks in Random and Diluted Choice provide information about subjects' attitudes to risk, propensities to choose dominated lotteries, and propensities to violate the independence axiom. Variables describing subjects' responses to lottery tasks were included in the OLS regressions (reported in Appendix A). The only such variable that proved to be significant was the dummy variable representing direct violation of dominance in lottery (ii), and this effect was found only for sellers. The average valuation of a seller's most preferred mug was higher by an average of £1.57 for subjects who chose the dominated lottery A in lottery task (ii) than for subjects who chose B (OLS regression, $p< 0.01$***). Whilst any interpretation of this occurrence is speculative, it may suggest that subjects who did not understand the lottery task (and so chose the weakly dominated option) may also have been confused with the goods task. Simply omitting valuations of subjects who chose dominated lotteries (only 11.3% of possible subjects) does not affect the results of the key hypotheses however, and this fact gives insufficient reason to remove these subjects from overall analysis. No subjects indirectly violated dominance (choosing B in lottery task (i), but A in lottery task (iii)).

Given the somewhat unusual nature of the lottery tasks, it is possible that subjects simply did not understand the tasks themselves, and perhaps this might be influencing a lack of correlation

between risk preferences and goods task valuation. However, in pre-experimental quizzes, 81.2% of all lottery questions were answered correctly at the first attempt. Clearly, most subjects were able to quickly understand the mechanism and payoff structure of the lottery tasks. Separating subjects into those who answered all lottery quizzes correctly, and those who did not reveals no difference in likelihood to directly violate dominance (10.9% of those who answered correctly, and 12.9% of those who did not, chose the dominated lottery).

Of the subjects who faced lottery tasks (i) and (iv), 50.0% violated the independence axiom. As in many other experiments, these violations were not random. 44.4% of subjects revealed the common ratio effect by choosing A in lottery task (i) and B in lottery task (iv)) while only 5.6% violated independence in the opposite direction. This difference was strongly significant ($\chi^2(1)= 36.296$, $p< 0.01$***). The fact that these choices show the same systematic patterns as have been found in other experiments is further evidence that subjects understood the lottery tasks.

5.6 Preference Consistency- Post-Experimental Questionnaire

A post-experimental questionnaire, identical for all three treatments, enabled the collection of demographic information. The questionnaire also elicited subjects' non-incentivised judgements about the desirability of each of the three mugs. Subjects were shown images of the three goods on a printed questionnaire, and asked to score the appeal of each one from 0 (completely undesirable) to 10 (completely desirable). The order of the three images was counterbalanced across subjects, within- and between-treatments. As different subjects may interpret these scores differently, scores were used only to elicit within-subject ordinal comparisons. As the questionnaire asked about potentially sensitive demographic information it was optional; two subjects chose not to answer completely the goods appeal aspect of the questionnaire, and so were omitted from the analysis described below.

Each subject's ranking of mugs by desirability can be compared for consistency with his or her chosen mug (in Chosen or Diluted Choice) or most highly valued mug (in Random). A subject's questionnaire responses are defined to be *consistent* with his or her decisions in the experiment if the mug that was rated most desirable (or one of the mugs rated jointly most desirable) was also the chosen mug (in Chosen or Diluted Choice) or one of the most highly valued mugs (in Random). Table 8 shows the numbers of subjects with consistent responses, separated by treatment and by buyer or seller.

---Table 8 near here---

These results show that, in general, and particularly amongst sellers, there was much less consistency in Random than in Chosen or Diluted Choice. This difference may be a manifestation of cognitive dissonance – that subjects believe that they must find a good desirable precisely because they have chosen it. Alternatively, the attachment effect, created through an act of choice might make a chosen good more salient. On the natural assumption that the attributes of a free mug are positive, such salience could be expected to have a positive impact on subjects' judgements about the appeal of a chosen mug. If, as suggested by the evidence reported by Carmon and Ariely (2000), sellers attend more than buyers to the attributes of a good, it is natural that they would be able recall feelings towards such attributes more easily than buyers. In Random, subjects experience no such creation of an attachment effect, nor such cognitive dissonance.

Of the 14 subjects in Random who did not reveal consistency in preferences, 12 valued the first good they valued within the experiment most highly, despite rating a different good more appealing in the post-experimental questionnaire. Absent any confounding factors, one would expect these inconsistent preference valuations to be randomly distributed across all the possible orders. This non-random nature of the order in which these most highly valued goods are observed ($\chi^2(2)= 17.714$, $p< 0.01$***) provides further evidence of order effects.

6. Discussion

The results in this experiment can be categorised into four main findings. First, there is significant and robust evidence of an endowment effect, consistent with previous experiments of this type. Second, there is evidence to suggest that the formation of attachment through the act of choice can increase valuations of a good for both buyers and sellers, resulting in an *attachment premium* of roughly 15%. Thirdly, there is evidence to suggest that random lottery effects, and removing the certainty of ownership, can have negative effects on both buying and selling prices, but more significantly for sellers than buyers. Finally, there is strong evidence of a tendency in Random for the first good presented to subjects to be valued more highly than goods presented later.

6.1 Attachment Effects

The modest attachment effects that are created through the act of choosing a preferred good have potential implications beyond the experimental literature. Attachment through the act of choice between relatively few goods, when options are readily understood and goods are perceived as desirable, increases the satisfaction towards the chosen item. For buyers, in a practical setting an awareness of the impact of attachment on intrinsic valuation would be useful in the evaluation of a product, and may lead to better informed valuation decisions of goods and services. That is not to say that any attachment towards a good should be dismissed as part of the valuation process, as attachment itself may be part of the experience of a good or service, but certainly an acknowledgement of the effect could stand to improve buyers' decision making. For sellers of goods and services, by contrast, such attachment can be utilised to charge a premium or gain a competitive advantage. This experiment has shown that offering subjects a simple choice of very generic household goods is sufficient in eliciting an *attachment premium*, and so this suggests that research in attachment remains a potentially important and lucrative aspect of marketing.

A third implication of attachment effects lies in the valuation of publicly provided goods and services. Methods such as contingent valuation are designed to elicit individuals' valuations of a public good or service, often by asking individuals to state their willingness-to-pay for a good or service and summing the totals within a population. This experiment suggests that attachment is pushing valuations upwards, and whilst by small amounts at the individual level, this could result in much larger effects for public provisions. Whilst this experiment has provided evidence that suggests there is a financial premium associated with an individual's attachment to something, the question still remains whether, and by how much, attachment should be included in the intrinsic valuation of a public provision, and answering this may better shape methods of public good valuation.

6.2 Random Lottery Effects

The *conditional ownership* of Diluted Choice revealed reduced valuations compared to the certain ownership in Chosen, and this effect was much stronger in sellers. This difference between buyers and sellers may be a result of loss aversion. In the case of sellers in Chosen, subjects knew they owned the good with certainty; the goods task was the only task they completed. In Diluted Choice, whilst subjects made decisions within the goods task *as if* it were being played out for real, actual potential ownership of the good was reduced to a one-in-five chance across the five tasks completed.

It is no surprise, then, that sellers in Diluted Choice feel a lessened sense of ownership towards the good, and so a reduced loss aversion, resulting in a smaller willingness-to-accept. This follows the pattern of results in Loewenstein and Adler (1995). For buyers, on the other hand, there is no ownership throughout either treatments in the experiment, and buyers are not expected to feel such loss aversion towards the goods they do not initially own. As well as ratifying Loewenstein and Adler's results, this finding also provides potentially important methodological implications for the role of random lottery devices, particularly in the design of future WTA-WTP experiments.

6.3 Novelty: An Explanation of Order Effects in Random Treatment

The evidence of such prominent order effects in Random leaves questions unanswered as to why they persist so strongly. One potentially interesting interpretation is the role that novelty plays in determining increased preference for the first good presented for valuation. Novelty, the idea of something being appealing by its different or unusual nature, is argued as being an 'inverted-U' shape (Scitovsky, 1976, 1992). The total lack of novelty may be seen as dull or uninteresting, whereas total novelty may be too much of a separation from the norm and so unappealing. Some novelty is good as it keeps things interesting, but too much and it becomes unattractive; there are diminishing returns to novelty.

In valuation of new foodstuffs, the novelty of experiencing different coloured versions of recognisable food is a potential cause of increased willingness-to-pay (Stevens and Winter-Nelson, 2008, Meenakshi *et al*., 2012). Novelty possibly increases affinity effects when specifically designed mugs are emblazoned with the college logo of which the subjects were members (Tom, 2004, p.168) and subliminal exposure of a novel item retains its novelty value and so is rated more appealing than when given extended supraliminal exposure (Tom *et al*., 2007, p.123).

With typical experiments using only monetary incentives, the potential to take away a consumable good is undoubtedly a novel experience for subjects who participate in WTA-WTP experiments. Whilst subjects in the Random treatment were made aware that they would value three goods in the experiment, they did not know what these goods would be until they were distributed in the first task. Though subjects were encouraged to look, touch and think about their good before completing their valuation decisions, these were made very shortly after seeing the good- one of the patterned mugs, for the first time.

Following the finding that attention towards a novel stimulus diminishes over time, (Berlyne, 1951, pp.272-273), an increase in attention to the positive attributes of the goods can translate to an increase in valuation, and sellers focus much more readily than buyers on these positive attributes (Carmon and Ariely, 2000, Nayakankuppam and Mishra, 2005). Though subjects might prefer different shaped patterns, it seems unlikely that subjects would consider this after

having seen only one type of patterned good, as they would have no reason to believe that the remaining two goods were alternative patterned mugs. Once the second good was revealed, as an almost identical good differing only in terms of the shape used to decorate it, much of the novelty of having the opportunity to value, and take away, a mug could have dissipated. Any of the novel (and so appealing) aspects of the first mug would be less novel in the second mug, and so potentially less appealing. Of course, any strong preferences for the new shaped pattern might override such a fall in novel value, but this in itself might explain why some discrepancy in order preference still remains. This loss of novelty would be perpetuated (though at a diminished rate) once the third good was revealed.

Within this experiment, order effects occurred in a way that fits the ideas of novelty and the resultant increase in attention towards a novel good. It also holds that the strongest difference between preferences for novel goods occurs after the first novel good has been revealed. The difference between first and second valuations being largest has also been found in contingent valuation of public provisions (Payne *et al.*, 2000) and also when using private 'bads' as goods, when testing for the willingness-to-accept for listening to unpleasant sounds (Ariely *et al*., 2003, pp.80-84). This suggests attention towards negative attributes of novel goods is heightened, but that this too diminishes when repetition reduces the novelty of something.

In studying the effects of experience on the WTA-WTP disparity, there is much evidence to suggest that convergence between the two valuations occurs with repetition (Shogren *et al*., 1994, List, 2003, Loomes *et al*., 2010) and such convergence appears more characterised by a reduction in WTA values (as opposed to increased WTP) (Shogren *et al*., 1994, pp.260-266, Loomes *et al*., 2010, pp.381-382). This too may be a manifestation of a reduced attention to novel features of the goods or lotteries used in these experiments, which subsides (more greatly amongst sellers) with repetition.

If the order effects found in Random compromise the validity of reported valuations as indicators of individuals' true preferences, then it is possible that this impacts on the core research of this paper, namely the effect of attachment through choice on the willingness-to-accept- willingness-to-pay disparity. If order effects affected processes such as the focus of attention toward a good, and if these were processes which Random was designed to control, then it may be that the difference between Random and Diluted Choice is not an entirely clean difference for calculating an *attachment premium*. Any existence of overlapping cognitive processes in order and attachment effects would reduce the evidence of a true *attachment premium*.

In contingent valuation studies, the use of 'advanced disclosure', where all goods or services to be valued are disclosed in full prior to any valuations, has eliminated order effects (Bateman and Langford, 1997) and also removes the novelty of experiencing new goods during the experiment. In this experiment, subjects in Diluted Choice and Chosen observed the good they were to value before receiving it (and so removing any novelty value), akin to advanced disclosure, which was not the case in Random. If attention towards these novel elements in Random has a positive effect on valuations, then it suggests that there was overvaluation in Random, implying that the *attachment premium* presented in this paper is underestimated. To test the robustness of this claim, future research might test whether advanced disclosure does indeed remove order effects for incentivised, subjectively differing, private goods.

7. Conclusion

This research aimed to control and measure the impact of attachment through choice on the willingness-to-accept-willingness-to-pay disparity. The much recorded disparity between buyers and sellers was present across all treatments, and an attachment effect was found. Controlling for the formation of attachment through the act of choice (encouraging subjects to decide upon their preference towards a good) does appear to result in an increased valuation when compared to valuations where no such formation of attachment is enabled. Whilst further research is needed to determine the extent of this effect, this result has the potential to offer guidance in both consumer research and in public policy valuations. In controlling for potential random lottery effects a significant effect was discovered among sellers, echoing Loewenstein and Adler's (1995) findings.

A particularly novel finding within the analysis of these results was the seeming impact of order on preference for valuations (specifically the tendency for first valued goods to be most preferred). Whilst order effects have long been acknowledged in contingent valuations for public goods, much less work has been done to assess the impact of order effects on private goods. The findings from these results offer support to the existence of order effects, as well as their tendency to be stronger in sellers. Further research will help to verify the robustness of both these attachment and order effects, and stands to have important implications for the methodology of stated preference.

Acknowledgements:

The author would like to give thanks to Robert Sugden, Subhasish M. Chowdhury and Anders Poulsen for their guidance, comments and suggestions, and to the Centre for Behavioural and Experimental Social Science (CBESS) and the University of East Anglia for financial support. Thanks are also due to Mengjie Wang and Anwesha Mukherjee for their competent experimental assistance. The usual disclaimer applies.

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Figures and Tables

Figure 1. Photographs of goods used in experiment[4]

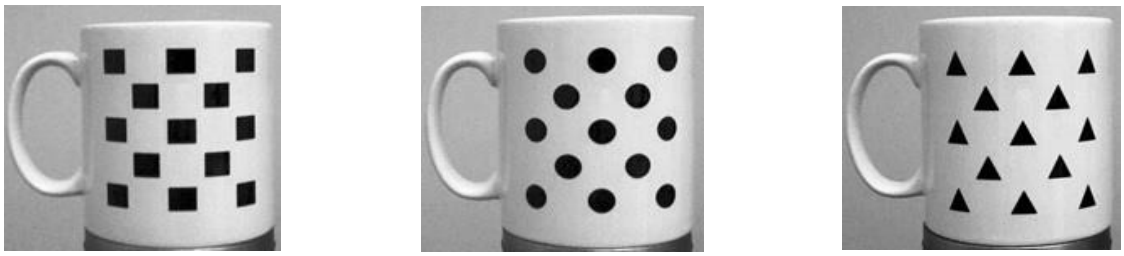
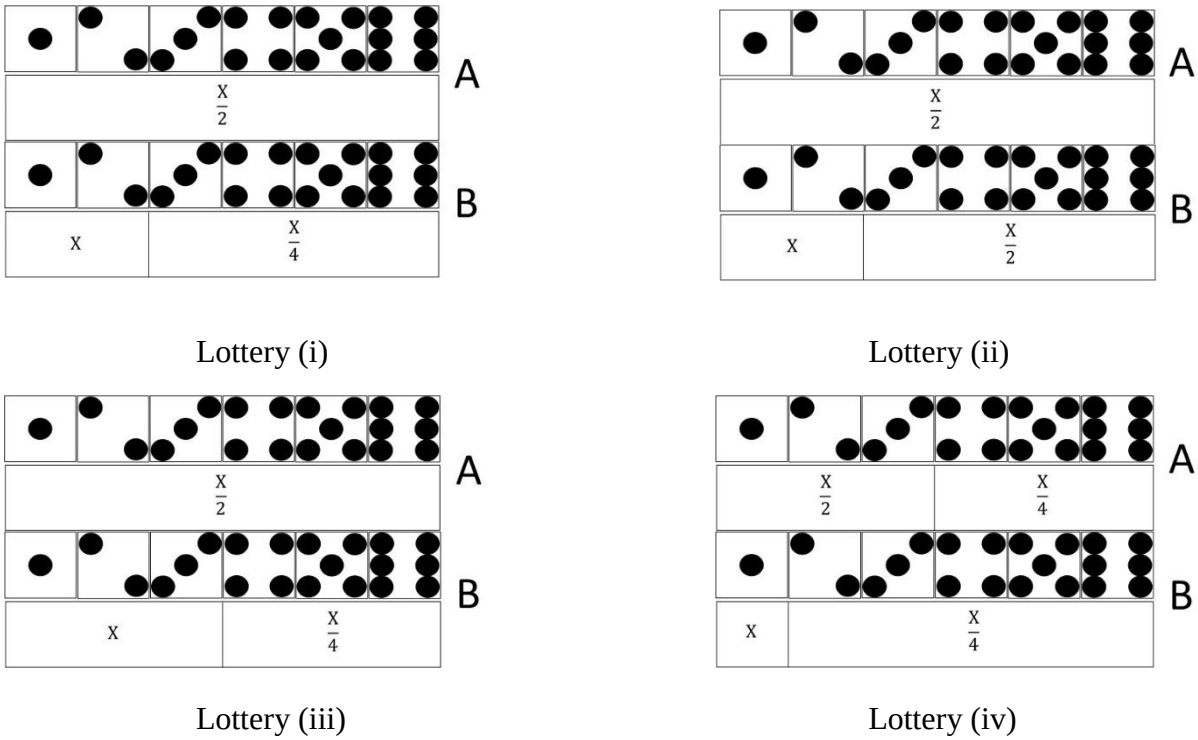


Figure 2. The four possible lottery tasks



[4] In the experiment, photographs were presented in colour. The mugs were white ceramic with black shapes.

Figure 3. Box plot of valuation by treatment

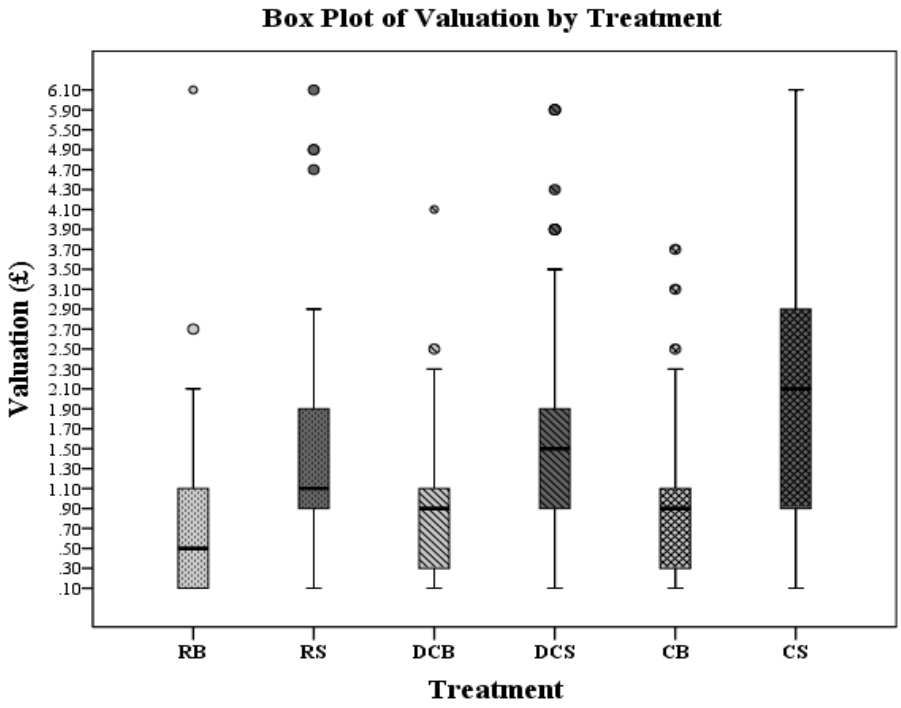


Table 1. Experimental design

	Random	**Diluted Choice**	**Chosen**
	-3 *given* goods tasks-	-1 *choice* goods task-	-1 *choice* goods task-
	-2 lottery tasks-	-4 lottery tasks-	
Buyer (WTP in goods task)	V_{RB}	V_{DCB}	V_{CB}
Seller (WTA in goods task)	V_{RS}	V_{DCS}	V_{CS}

Notes: Where V_{ij}= average valuation of treatment 'i', sub-treatment 'j'.
Valuation of Random is the highest valued good across the three *given* goods tasks.

Table 2. Summary Statistics

Average Valuation ***No. of Subjects*** *No. of cases of non-valuation*	**Random**	**Diluted Choice**	**Chosen**
Buyer	**£0.83** ***42*** *15*	**£0.94** ***45*** *11*	**£1.02** ***46*** *5*
Seller	**£1.59** ***36*** *7*	**£1.78** ***45*** *8*	**£2.24** ***40*** *3*

Notes: Non-valuation occurs when a buyers would 'not buy' at any values or a sellers would 'sell' at every value.

Table 3. Statistical tests of Alternative Hypotheses (H1-H3)

Hypothesis	**Average Difference (£)**	**Wilcoxon-Mann-Whitney (*p*-value)**[5]
Endowment Effect (H1)		
WTP (R) = WTA (R)	+ £0.76	**<0.01*** **
WTP (DC) = WTA (DC)	+ £0.84	**<0.01*** **
WTP (C) = WTA (C)	+ £1.22	**<0.01*** **
Combined Effect (H2 + H3)		
WTP (R) = WTP (C)	+ £0.18	**0.022****
WTA (R) = WTA (C)	+ £0.64	**0.017****
Attachment Effect (H2)		
WTP (R) = WTP (DC)	+ £0.11	**0.098***
WTA (R) = WTA (DC)	+ £0.19	0.282
Random Lottery Effect (H3)		
WTP (DC) = WTP (C)	+ £0.08	0.283
WTA (DC) = WTA (C)	+ £0.45	**0.046****

[5] One-tailed test.

Table 4. Highest valuations in Random treatment by order

Highest valuation(s)	Number of subjects
O_1	28
O_2	6
O_3	3
O_1, O_2	7
O_1, O_3	1
O_2, O_3	3
O_1, O_2, O_3	8
Non-valuation	22

Table 5. Relative valuations in Random treatment by order

	Number of Subjects		
Relative Valuations	**All (n=56)**	**Buyer (n=27)**	**Seller (n=29)**
$(O_1 > O_2)$	29	9	20
$(O_1 < O_2)$	10	6	4
$(O_1 = O_2)$	17	12	5
$(O_1 > O_3)$	37	14	23
$(O_1 < O_3)$	8	4	4
$(O_1 = O_3)$	11	9	2
$(O_2 > O_3)$	20	10	10
$(O_2 < O_3)$	10	4	6
$(O_2 = O_3)$	26	13	13
	***p*-value of χ^2 test**		
Null Hypothesis	**All**	**Buyer**	**Seller**
$p_{12} = p_{21}$	**<0.01*** **	0.439	**<0.01*** **
$p_{13} = p_{31}$	**<0.01*** **	**0.018** **	**<0.01*** **
$p_{23} = p_{32}$	**0.068***	0.109	0.317
$q_{12} = q_{13}$	0.257	0.513	0.257
$q_{12} = q_{23}$	0.170	0.842	**0.059***
$q_{13} = q_{23}$	**0.013** **	0.394	**<0.01*** **
$q_{12} = q_{13} = q_{23}$	**0.042** **	0.682	**<0.01*** **

Table 6. Mean valuation of buyers and sellers by good type

Valuation - Buyer (£)	**Squares**	**Circles**	**Triangles**	**Kruskal-Wallis test**	***p*-value**
Chosen and Diluted Choice	£0.99	£0.88	£1.19	$\chi^2(2)= 2.164$	0.339
Random	£0.63	£0.45	£0.67	$\chi^2(2)= 0.678$	0.713
Valuation - Seller (£)	**Squares**	**Circles**	**Triangles**	**Kruskal-Wallis test**	***p*-value**
Chosen and Diluted Choice	£1.84	£1.95	£2.38	$\chi^2(2)= 2.132$	0.344
Random	£1.42	£1.26	£0.67	$\chi^2(2)= 2.715$	0.257

Table 7. Shape preference by good type

Shape Preference	(n)	Squares	Circles	Triangles	χ^2 test	*p*-value
Chosen and Diluted Choice	176	71	70	35	$\chi^2(2)= 14.330$	**<0.01*****
Random (Strict Preference)	37	18	9	10	$\chi^2(2)=3.946$	0.139

Table 8. Preference consistency of subjects by treatment

Subjects with Consistent Preferences/ Number of Subjects	Random	Diluted Choice	Chosen	χ^2 test	*p*-value
All	62/76	84/90	83/86	$\chi^2(2)= 11.869$	**<0.01*****
Buyer	34/40	41/45	44/46	$\chi^2(2)= 2.924$	0.232
Seller	28/36	43/45	39/40	$\chi^2(2)= 10.789$	**<0.01*****

Appendix A - OLS Regression Analyses

A1.

Transaction Price	Random Buyer vs. Seller	Diluted Choice Buyer vs. Seller	Chosen Buyer vs. Seller
(intercept)	**1.82519*** **	**1.74357*** **	**0.69818*** **
(se)	*0.57167*	*0.63234*	*0.25912*
Seller	**0.72244** **	**0.99929*** **	**1.19272*** **
(se)	*0.30652*	*0.27905*	*0.26938*
Age	**0.00102**	**0.02040**	**0.01170**
(se)	*0.03963*	*0.05467*	*0.02561*
Female	**-0.32376**	**-0.62330** **	**0.91153**
(se)	*0.30992*	*0.30525*	*0.42750*
Economics	**-0.17963**	**-0.30950**	**0.69818** **
(se)	*0.52736*	*0.42251*	*0.42720*
Risk Averse	**0.11070**	**-0.04669**	-------
(se)	*0.37884*	*0.46968*	-------
Dominance	**0.84696***	**0.32589**	-------
(se)	*0.47785*	*0.46443*	-------
Strong Risk Averse	-------	**-0.10905**	-------
(se)	-------	*0.30078*	-------
Common Ratio	-------	**-0.48874**	-------
(se)	-------	*0.31726*	-------
Opposite CR	-------	**0.01998**	-------
(se)	-------	*0.70852*	-------
# Obs	**77**	**90**	**86**
Adj. R^2	**0.0592**	**0.0972**	**0.2273**

Notes: Description of regression variables
Seller (1= Seller, 0= Buyer)
Age (Age of subject, assuming baseline of 18)
Female (1= Female, 0= Male)
Economics (1= Formal Economics study, 0= No formal Economics study)
Risk Averse (1= Choice A in Lottery Task (i), 0= Choice B in Lottery Task (i))
Dominance (1= Choice A in Lottery Task (ii), 0= Choice B in Lottery Task (ii))
Strong Risk Averse (1= Choice A in Lottery Task (iii), 0= Choice B in Lottery Task (iii))
Common Ratio (1= Choice A in Lottery Task (i) and Choice B in Lottery Task (iv), 0= Any other pairwise choice in (i) and (iv))
Opposite CR (1=Choice B in Lottery Task (i) and Choice A in Lottery Task (iv), 0= Any other pairwise choice in (i) and (iv))

A2.

Transaction Price	**Combined Effect**		**Attachment Effect**		**Random Lottery Effect**	
	Random vs. Chosen Buyer	**Random vs. Chosen Seller**	**Random vs. DC Buyer**	**Random vs. DC Seller**	**DC vs. Chosen Buyer**	**DC vs. Chosen Seller**
(intercept)	**0.59531****	**1.60287*****	**0.49332**	**1.94551*****	**0.81109*****	**1.85594*****
(se)	*0.23616*	*0.37606*	*0.44634*	*0.39566*	*0.18296*	*0.39195*
Diluted Choice	-------	-------	**0.16042**	**0.58299***	-------	-------
(se)	-------	-------	*0.22232*	*0.33910*	-------	-------
Chosen	**0.23192**	**0.68177***	-------	-------	**0.06563**	**0.53394**
(se)	*0.21129*	*0.36522*	-------	-------	*0.17333*	*0.35784*
Age	**0.02513**	**-0.01054**	**0.01519**	**0.05829**	**0.02023**	**0.00355**
(se)	*0.02474*	*0.03582*	*0.03053*	*0.06099*	*0.02675*	*0.03664*
Female	**0.18563**	**-0.11461**	**0.17228**	**-1.3763*****	**0.14210**	**-0.30306**
(se)	*0.21305*	*0.38541*	*0.22299*	*0.35110*	*0.17662*	*0.38379*
Economics	**0.03267**	**1.10662***	**-0.16429**	**-0.85189***	**-0.03943**	**0.64177**
(se)	*0.32677*	*0.65619*	0.35636	0.49589	*0.26651*	*0.52713*
Risk Averse	-------	-------	**-0.02996**	**0.01151**	-------	-------
(se)	-------	-------	0.28530	0.39667	-------	-------
Dominance	-------	-------	**-0.19442**	**1.54154*****	-------	-------
(se)	-------	-------	0.34345	0.53823	-------	-------
# Obs	**87**	**76**	**86**	**81**	**91**	**85**
Adj. R^2	**-0.0168**	**0.0298**	**-0.0562**	**0.1687**	**-0.0312**	**0.0037**

Notes: Description of regression variables
Combined Effect: Chosen (1= Chosen, 0= Random)
Attachment Effect: Diluted Choice (1= Diluted Choice, 0= Random)
Random Lottery Effect: Chosen (1= Chosen, 0= Diluted Choice)