Essays on Attention in Individual Decision Making

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Abstract

A growing economic and psychology literature considers how changes in the attention given to situations and goods can affect consumers' evaluations of these things. This thesis employs an experimental methodology to investigate the role of attention in explaining a number of irregularities in individual consumer-related decision making that have been established in the behavioural and experimental economic literature: choice effects and order effects.

Chapter One presents a novel experimental design to test the different role of choice effects on the valuation of consumable goods when participants assume the roles of buyers and sellers, measuring the effect of choice on the well-known willingness-to-accept- willingness-to-pay disparity. Chapter Two utilises an experimental design to disentangle a number of potential attention-based order effects to explain surprising findings of order effects in Chapter One, where valuations for goods in earlier tasks were significantly higher than for goods valued in later tasks. Chapter Three presents another novel experimental design to capture the effect of choice on willingness-to-donate to charitable causes. Chapter Four takes this experimental design, and measures the effect of choice on consumable goods.

A number of interesting results are found within these papers. Evidence of the positive effect of choice is found in Chapters One, Three and Four, consistent with theories of attention. Chapter Two finds evidence that participants give reduced attention to both the general experimental design of later tasks and the specific goods they value in these later tasks. This provides a novel explanation of the causes of order effects. Together, the papers of this thesis show that attention can explain how individuals evaluate goods differently in different consumer-related decision making situations, and that carefully considered experimental methodology can be used to better isolate these effects in laboratory settings.

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I dedicate this body of work to my grandfather, JP. I know you are celebrating this achievement with me.

Introduction

The use of experimental methodology to test economic theories has grown substantially in the past several decades. In consumer-related decision problems, economics experiments can place participants in the role of a consumer and compare their financial and consumption decisions across a variety of incentivised scenarios. However, the validity of these types of experiments can be predicated on the assumption that participants behave in a way that is comparable to their actions in the real world. This doctoral thesis uses experimental methodology to test a number of established consumer-related decision making problems, adapting previous experimental designs in such a way to improve on their external validity, by designing experiments to better isolate target variables and creating decision making scenarios more in line with situations faced in the real world.

An important challenge of these consumer-related experiments is to better understand how participants evaluate the goods they face in these experimental scenarios. There is a growing literature which considers that the evaluation of a good may be largely influenced by the attention given to the attributes of that good, and that such attention may be influenced by the general construct of a scenario (Bordalo *et al*., 2013, 2016, Basu and Savani, 2017), or the specific role a participant plays in that scenario (Carmon and Ariely, 2000, Nayakankuppam and Mishra, 2005, Johnson *et al*., 2007). A common theme of the chapters of this thesis is that experimental outcomes are analysed from the perspective of these theories of attention. This thesis considers the potential issues of attention across two well established effects in behavioural and experimental economics: choice effects and order effects.

Members of most modern societies enjoy a freedom to choose the decisions they make across a wide spectrum of activities in day-to-day life. As such, choice effects have been examined experimentally across a number of differing situations, and across economic, psychology and food science literature. Prior research has suggested that comparing and choosing amongst goods may lead to consumers focusing their attention on the specific attributes of goods (Basu and Savani, 2017).

Choice has been found to increase intrinsic motivation to complete tasks (Zuckerman et al., 1978), increase satisfaction and purchase likelihood for food products (Iyengar and Lepper, 2000, Botti and Iyengar, 2004) and increase real food consumption decisions (King *et al*., 2008, Zeinstra *et al*., 2010, Dominguez *et al*., 2013, de Wild *et al*., 2015, Parizel *et al*., 2017). The attraction, or lure, of choice, may lead individuals to choose something they might otherwise

not have chosen (Bown *et al*., 2003) and too much choice, a choice overload, may instead reduce satisfaction levels (e.g. Scheibehenne *et al*., 2010). Evidently, both the specific construct of a scenario involving choice, and the differences in goods being chosen, may influence any choice decision and affect overall satisfaction of the choice outcome.

Whilst there exists an extensive experimental literature on the effect of choice, there has been little effort to quantify a monetary value of the act of choice, when compared to no choice. The metrics of measurement of choice effects in prior experiments have typically involved satisfaction ratings or binary consumption decisions (i.e. a single choice of whether to consume or purchase, or not). To know if we value the goods we choose more, precisely because we have chosen them, is an important finding for implications of the role of choice in the domains of both private and public goods. As such, this thesis considers the potential of the positive act of choice on financial and consumption decisions across both private goods, in the valuation of consumable goods, patterned mugs, and public goods, in the willingness-to-donate towards charitable causes.

As well as attempting to quantify a monetary effect of choice, the individual chapters in this thesis attempt to improve on potential limitations of prior experiments of choice, when comparing outcomes to a no-choice treatment. The fundamental way in which this is achieved is by adapting the experimental conditions of a no-choice treatment. In a typical experimental design, a choice treatment will consist of participants receiving a set of choice options and being asked to choose one of these. Different methods have been used to distribute a good to non-choosers, with the simplest method to randomly distribute the choice options to non-choosers. Whilst other methods have been employed, they all fail to address the potential issue that choosers are free to choose the good they prefer, whereas non-choosers may not necessarily receive the good they prefer from the set of available options. This difference in preferences of received goods may be generating an overstating of choice effects in the prior literature. The experimental designs in this thesis attempt to control for these potential preference effects by creating conditions of no-choice treatments which allow non-choosers to receive the good that they *would have* chosen, had they instead been given the opportunity to choose.

Chapter One considers the role of attention in explaining the effect of choice on the well-known willingness-to-accept- willingness-to-pay disparity. A number of studies have considered this disparity in buying and selling prices as a result of differences in attention that buyers and sellers give to money and goods, with sellers giving relatively greater attention to the positive

attributes of the goods they own than do prospective buyers (Carmon and Ariely, 2000, Nayakankuppam and Mishra, 2005, Johnson *et al*., 2007). Typically, goods in these experiments are given arbitrarily to participants, yet in reality, consumers generally are free to choose the goods they wish to buy and sell. This study tests the impact of attachment, formed through the act of choice, on the willingness-to-accept- willingness-to-pay disparity for valuations of consumable goods (patterned mugs). A positive effect of choice should generate an increase in valuations for both buyers and sellers. If sellers focus their attention on the attributes of goods more than do buyers, then choosing may have a greater effect on sellers than buyers, increasing the disparity between the two valuations. Chapter One attempts to address the above issue of differences in preferences of choosers and non-choosers by allowing non-choosers to value all available goods, and takes the highest valued good as implied as the most preferred good, to compare with the valuation of the chosen goods of choosers.

Findings from this chapter suggest that choice can increase buying and selling valuations. Whilst selling valuations appear to increase by a greater amount, implying an increase in the overall willingness-to-accept- willingness-to-pay disparity, such an effect is not statistically significant.

A particularly interesting, albeit surprising, finding from Chapter One was the discovery of order effects in the valuation of sequences of subjectively-differing goods (despite extensive efforts to control for these), which potentially distorted controlled measurements of choice effects. Independent of which specific good was valued first, it was extremely common for this first valued good to be valued more than later goods, and this was especially true for sellers.

An experimental study to further understand these surprising findings of order effects formed the basis of Chapter Two. This paper considers the impact of attention and novelty in explaining order effects in multiple-task valuation decisions. Asking participants to complete multiple tasks in an experiment is a common method employed for the valuation of both market and non-market goods, but the occurrence of order effects (whereby the order in which tasks are presented can affect responses) casts doubts as to the effectiveness of these designs.

Whilst these types of designs are often a fundamental aspect of methodologies which aim to elicit preferences for non-market goods, such as contingent valuations studies and discrete choice experiments, this effect of order is also problematic for experiments which utilise multiple-task experiments to generate multiple data points from participants as a way of reducing experimental costs. However, if the intention of multiple-task designs is to collect

meaningful data from each task independent of preceding responses, then any systematic changes in preferences brought about by the order in which tasks are presented represents a problem for users of this methodology.

Findings from the psychology and economic literature imply a positive relationship between novelty and attention given (Berlyne, 1951, Scitovsky, 1992). Whilst early tasks may be novel to participants, as task order progresses, such novelty falls, and so too the attention given to these later tasks. Such reduced attention may explain the common finding of reduced valuations for goods in later tasks in multiple-task valuation decisions. This paper considers this effect of attention from both the perspective of experimental novelty, the novelty of completing the task itself, and good-specific novelty, the novelty of valuing specific goods within each task.

Chapter Two finds evidence of both novelty effects, suggesting that participants give less attention to later tasks and for later observed goods. In this study two different types of goods were used (patterned mugs and luxury chocolates). The findings of Chapter One, that first valued goods were valued significantly more highly, was replicated in this study, using both the same and different types of goods to the previous study, suggesting that such a finding was not a coincidental occurrence. A particularly novel finding from this chapter was that whilst valuations tended to decrease as task order progressed, goods which were of the same type as the first valued good were valued significantly higher than goods which were of a different type. The chapter discusses a possible explanation of these findings related to a combined effect of attention and anchoring effects.

Chapter Three returns to the impact of choice on consumer decision-making. This paper tests whether choosing between different charitable causes can lead to an increase in donations to a chosen charity, relative to simply being assigned that charity. Due to the surprising finding of order effects in the design of Chapter One, the experimental design to keep preferences constant between choosers and non-choosers in this chapter was modified. Chapter Three attempts to address potential differences in preferences of choosers and non-choosers through a series of preference elicitation tasks. In these tasks participants responded to a number of preference ranking tasks for real charities. The charities distributed to non-choosers were determined from their preference decisions in these tasks, to ensure that they receive a charity they would have chosen, had they instead been able to choose. Since the vast majority of prior experiments consider the effect of choice on consumable goods, Chapter Four tests this novel experimental design for consumable goods (patterned mugs), also.

Chapter Three finds evidence that choice can significantly increase both the likelihood that participants will donate something to charity and increase the actual amount donated amongst those who do donate something. The chapter discusses the potential of choice to be used both in terms of charity marketing strategies, but also of its potential for use in public policy. Particularly, the chapter discusses the possibility of utilising citizen choice in determining taxation spending. Chapter Four finds some evidence that this choice can increase valuation towards consumable goods. Specifically it finds that choice has no effect in increasing the likelihood that participants will value a good at some positive amount, but amongst those who do value a good at some positive amount, choice significantly increases valuations.

The main contribution of this thesis is twofold. First, the findings of these papers offer novel explanations of two consumer-related experimental findings, choice effects and order effects. It utilises a multidisciplinary approach by applying research of attention and novelty from both economic and psychology literature, and applies this to explain experimental findings of choice and order effects. Second, the thesis applies a rigorous experimental methodology, aimed to improve on limitations of previous designs. In the domain of choice effects it attempts to remove potential preference effects of non-choosers that may previously have inflated a positive effect of choice. In terms of order effects, it employs an experimental design to isolate multiple potential mechanisms driving this effect; experimental and good-specific novelty effects. By creating robust experimental designs that better isolate target variables, the papers in this thesis have generated findings which better inform the effects of choice and order in consumer-related decision making problems, and as such has increased the scope for these findings to be applied to more general settings beyond the laboratory.

Chapters One, Three and Four of this thesis are single-authored papers. Chapter Two is a jointly authored paper with Prof. Robert Sugden. The majority of the contribution to this paper was made by me. I was wholly responsible for the programming and running of the experiment as well as the analysis of the results. I was also responsible for the majority of the writing of the paper, with the co-author providing assistance in the style and structure when required.

In general, findings from these papers offer support for the importance of the consideration of attention in individual consumer-related decision making problems. Whilst understanding how consumers evaluate decisions in economics experiments remains a challenge for users of consumer-decision experimental methodology, this thesis demonstrates the importance of carefully designed experiments in both affecting the way in which participants focus their

attention within an experiment, and in maximising the potential external validity of an experiment.

Chapter 1

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

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 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 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) that extend beyond simply whether one is or is not endowed with a good.

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). 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. Goods in these experiments are typically given arbitrarily; participants have no involvement in determining which types of goods they might buy or sell. This too is true of other experiments testing the WTA-WTP disparity, using typical goods such as mugs or pens (Kahneman *et al*., 1990, Nayakankuppam and Mishra, 2005).

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. If choice is an important aspect of individual decision making and choosing can generate a sense of association and attachment between a good and an individual (Gawronski *et al*., 2007), then this manipulation of good experience may be influential in determining buying and selling prices.

This paper seeks to investigate the impact that attachment towards a good, formed through choice, has on the WTA-WTP disparity, considering the effect of both WTP and WTA valuations individually, as well their relative effects and its consequent effect on the size of the disparity. If participants 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 could be affected. Given the assumption that goods would be seen as positive to participants, this increase in attention should yield increases in valuations for buyers and sellers. 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.

Within experimental settings the act of choice has often been found to induce a sense of attachment towards the chosen object (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, Botti and Iyengar, 2004). Whilst this attachment through choice has been argued to be a result of cognitive dissonance (Brehm,

1956) - 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.

When choice options are increased, the phenomenon of choice overload suggests that an excess of choice options could lead to decreased satisfaction (Scheibehenne *et al*., 2010, for example). If this were to persist, then choosing a good may in fact reduce feelings of attachment towards that chosen good and so instead reduce valuations. However, even with large choice sets (averaging 34 choices), reducing choice conditions yielded reduced sales or little changes (p.411), suggesting it is unlikely that experiments with small choice samples would be affected by any negative effects of excess choice options. The finding of this paper, presenting some evidence of positive effects of attachment through choice, suggests that choosing among relatively few options does indeed lead to an increase in valuation of the chosen good. This has potential implications both in terms of consumer research, and in the use of contingent valuation procedures (Hanemann, 1994, for example) for public good valuations.

The second issue this paper hopes to investigate is, 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 participants are affected by their prior exposure to such goods within an experimental setting might be an influential factor in determining consequent valuations. This paper also tests for the potential of order effects when participants value multiple goods within an experiment.

If participants 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 increase a focus of attention towards the novel attributes of the first goods. Berlyne (1951, pp.272-273) suggests that novelty increases attention towards a stimulus, but that this diminishes over time. Again, if sellers attend more to attributes of the good, then it is possible that sellers would be more likely affected by effects of the order of presentation of these goods, suggesting implications for both the size and scale of the WTA-WTP disparity. The findings in this paper, of strong order effects among sellers, 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 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 participants *would have* chosen, had they been given the opportunity to choose. Simply giving participants one of a selection of goods would not provide such a comparison, as the good given might not be the good that the participant most preferred.

An alternative design would be to ask non-choosing participants 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 participant. 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.

It is possible that recording only the most highly valued good of a number of goods for non-choosers may have some impact on valuations. For both choosers and non-choosers, it is possible that participants 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. For choosers, only one good is valued, and it is assumed that any errors are equally likely to lead to over- or under-valuation. The effects for non-choosers are twofold. First, there are multiple good valuations, thus increasing 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 non-choosers but leave the valuations of choosers on average unaffected, which

suggests that the results from this design could be a conservative estimate of any effect on valuations of attachment through choice.

Given that non-choosers would value multiple goods, the order in which these goods are presented to participants may also affect their consequent valuations. In hypothetical contingent valuation studies, order effects (whereby earlier observed goods are preferred to later ones) have been recorded (Bateman *et al.,* 2004, 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) and Clark and Friesen (2008) observe order effects in nested bundles of private goods which differ in objective value.

In attempting to isolate the impact of attachment, formed through choice, a key aspect of this experimental design is to minimise any preconceptions about which goods participants might choose; the goods themselves must differ only in subjective value. To the author's knowledge, no study has previously found order effects for private goods that differ only in terms of their subjective value, and 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 could imply that buying and selling valuations in this experimental design might be affected by this. It is therefore important to attempt to identify such effects if they do indeed occur, by counterbalancing the order in which goods are valued and by testing for possible effects within such an experimental design.

To attempt to ascertain to what extent participants' decisions within the experiment represent coherence and consistency in preferences of individuals, a post-experimental preference revelation exercise will be used. This would offer a comparison of *ex-post* preference decisions with the explicit choice preferences and implicit preferences by valuation during the experiment. In particular, whilst the highest valuation of non-choosers might be subject to overvaluation bias or order effects, the preference decisions in the post-experimental questionnaire ought not to be subject to such effects. If this were to be the case, then it would be of interest to compare preferences based on valuations and by preference revelation exercise, and to explore to what extent these differences impact on valuations.

Findings from Loewenstein and Adler (1995) suggest that the random lottery design of non-choosers could be likely to result in reduced valuations. They 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. 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.

3. Experimental Design

This experiment aimed to control and measure the impact of attachment, formed through choice, on the WTA-WTP disparity. A 3x2 between-subject experimental design was used. The three treatments, Random, Diluted Choice and Chosen, 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 provisionally 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.

The number and type of tasks completed by participants differed across treatments. In Random, participants completed three *given-goods* tasks and two cognitive distraction *lottery* tasks. In Diluted Choice, participants completed one *choice-goods* task and four cognitive distraction *lottery* tasks. In Chosen, participants completed one *choice-goods* task.

The goods used in each of these goods tasks were different patterned mugs (see Figure 1.1). Within the WTA-WTP literature the use of mugs as goods to be traded is well established. 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 pilot study and pre-experimental surveys found that the three shape patterns 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 (see Appendix 1.1). In the experiment, photographs were presented in colour. The mugs were white ceramic with black shapes.

Figure 1.1. Photographs of goods used in experiment[1]

3.1. Treatments

In the Chosen treatment, participants completed one choice-goods task. This treatment was designed to allow for the formation of attachment through choice. The experiment began with a choice between three goods, shown on the participants' computer screens. Participants were asked to select which good 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, participants received their chosen goods before valuations were elicited for that good.

In the Random treatment, there was no such opportunity for the formation of attachment through choice. Participants in this treatment completed five tasks, three given-goods tasks, key to the question this paper addresses, and two cognitive distraction lottery tasks (the nature of the lottery tasks will be explained in section 3.3). In each of the three given-goods tasks, participants were simply given one of the three possible goods before a valuation was elicited for each of these goods. By comparing the three valuations elicited for each good for each participant, it was possible to identify that participant's implicitly most preferred good; the highest valued good. This implicitly most preferred good was considered the good that participants would have chosen, had they instead been given the opportunity to choose. After participants had completed all five tasks, one was selected at random to be played for real, and participants' decisions in the selected task were made binding. All participants were made aware of this in the experimental instructions.

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

[1] See Appendix 1.2 for descriptions of goods.

cognitive distraction lottery tasks. As in Chosen, in the choice-goods task participants 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. 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, participants had a 20% chance of their implicitly or explicitly most preferred good being selected to be the task played for real and so this controlled for random lottery effects by holding them constant across the two treatments.

Task order in both Diluted Choice and Random treatments was counterbalanced to control for order effects. In Random, lottery tasks (which were always the second and fourth task) interspersed given-goods tasks (which were always the first, third and fifth task), and so to mimic this in Diluted Choice, the choice-goods task was always either the first, third or fifth task. Specific goods or lotteries were counterbalanced throughout.

3.2. Goods Tasks

Whilst the type of goods task (either choice- or given-) differed across treatments, once participants had received their good for that task, the tasks continued in an identical manner. In the goods tasks it was important to ensure an incentive-compatible elicitation device was used to encourage truthful valuation elicitation, and so a Becker-DeGroot-Marschak (henceforth BDM) mechanism (1964) was used.

In the goods task, buyers were informed they would have the opportunity to buy that good from the experimenter and take it away with them. Once buyers received their good, they were shown a set of possible prices, ranging from £0.20 to £6.00, in £0.20 increments, and were asked whether, at each of these prices, they wished to buy the good at this price, or not. In the goods tasks, sellers were informed that they owned (or conditionally owned in Random and Diluted Choice) the good, but that they could sell the good back to the experimenter. Once sellers received their good, they were shown a set of possible prices, ranging from £0.20 to £6.00, in £0.20 increments, and were asked whether, at each of these prices, they wished to sell the good back to the experimenter at that price, or not to sell.

This is in keeping with the designs used in other experiments that have investigated the WTA-WTP disparity. The fact that truthful valuations were the optimal response in the valuation mechanism was made clear to participants in the written instructions and the pre-experimental quizzes. The £6.00 upper limit was chosen in the expectation that most participants would value

the goods less than this. This design choice reduced the possibility that participants' valuations might be framed by the upper and lower bounds of the BDM mechanism (Bohm *et al*., 1997).

A buyer (seller) who acted on consistent preferences between money and goods would report at most one *preference switch*, from 'buy' ('not sell') at relatively low prices to 'not buy' ('sell') at relatively high prices. There would be no preference switch for a participant who would 'buy' ('not sell') at every price or would 'not buy' ('sell') at every price. If a participant reported no more than one preference switch for a good, the location of that switch (or its absence) locates one of thirty-one points on an ordinal valuation scale.

For a buyer with exactly one preference switch, their *valuation* of that good will be defined as the mean of the highest price at which they would 'buy' and the lowest price at which they would 'not buy' (or, equivalently, the highest price at which they would 'buy', *plus* £0.10). Participants who would 'not buy' at every price will be defined to have a valuation of £0.10 (henceforth defined as 'min-valuation'), and those who would 'buy' at every price to have a valuation of £6.10.

For a seller with exactly one preference switch, their *valuation* of that good will be defined as the mean of the highest price at which they would 'not sell' and the lowest price at which they would 'sell' (or, equivalently, the lowest price at which they would 'sell', *minus* £0.10). Participants who would 'sell' at every price will be defined to have a valuation of £0.10 (henceforth defined as 'min-valuation'), and those who would 'not sell' at every price to have a valuation of £6.10.

In any statistical analysis of results, these extreme parameters must be accounted for. First, non-parametric tests (such as Wilcoxon rank-sum tests) only require a ranking of values, and so this actual upper valuation does not affect statistical outcomes. In any regression analysis, censoring at lower and upper limits (using a Tobit model) accounts for any valuation above this upper limit, so this actual valuation does not affect statistical outcomes.

Using this valuation elicitation, 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. Whilst it is acknowledged that approximating both buying and selling valuations might not reveal a participants' specific valuation, most statistical tests of results concern the ranking of valuations, and so approximating valuations within an incremental range is sufficient.

3.3. Cognitive Distraction Tasks

For all four possible lottery tasks participants were asked to choose which of two monetary lotteries they would prefer to play, with payoffs determined by the roll of a die. Figure 1.2 illustrates the presentation of lottery task (ii), as it would have been shown on participants' screens, as an example. Whilst primarily designed to act as a cognitive distraction, the lottery task responses allow an investigation of whether attitudes to risk and propensities to violate Expected Utility Theory impact on valuation decisions in the goods tasks.

Participants were made aware that they were choosing between the two lotteries A and B, and that their payoff from their chosen lottery would be determined by the outcome of a roll of a six-sided die, illustrated on each lottery, if that lottery task was the randomly selected task. In lottery (ii), lottery B weakly dominates lottery A; thus a choice of A would violate the principle that preferences over lotteries respect stochastic dominance. This is the only lottery task where responses significantly impact valuations in the goods tasks. As such, it is the only lottery discussed in the paper (descriptions of the remaining lottery tasks can be found in Appendix 1.3).

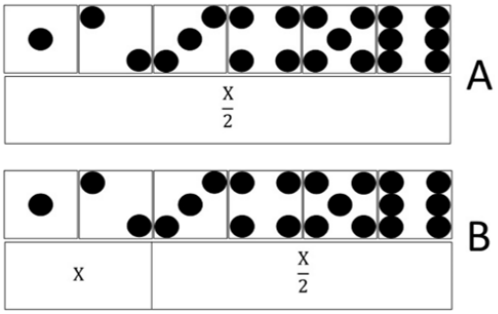

Figure 1.2. Example lottery task (lottery (ii))

During the experiment, the possible outcomes of the lotteries were shown as fractions of *X*, with participants aware that *X* could take one of thirty values, from £0.20 to £6.00 in £0.20 increments. Notice that the set of possible values of *X* is the same as the set of possible prices in the goods tasks. This *X*-value lottery design ensures that the two types of tasks involve the same reference points, so that comparisons of goods valuations are not distorted by values used in the lottery tasks.

3.4. Payment to Participants

Once all tasks were completed, a random lottery procedure was implemented in Random and Diluted Choice, where one of five task numbers was drawn at random to determine which of

the five tasks would be played for real (as only one task was completed in Chosen, this was always the task played for real). This was achieved by one participant selecting one of five sealed envelopes. The task number inside the selected envelope was determined as the task number for all participants. After this, one of the thirty values (from £0.20 to £6.00) was drawn at random, and this was determined to be either the price of the good or the value of *X* in the lotteries. This value was selected in the same manner as the task number.

If the task drawn for a participant was a goods task, the participant's decision about whether to buy or sell the good at the drawn price was made binding. If the participant *was* willing to buy the good at the drawn price, they would receive the good and take away a £6.00 participation fee *minus* the drawn price. If the participant *was not* willing the buy the good at the drawn price they did not receive the good but would take away the full £6.00 participation fee.

If the participant *was not* willing to sell the good at the drawn price, they kept the good and took it away with them, in addition to a £6 participation fee. If the participant *was* willing to sell the good at that price, they did not take the good away but instead received the drawn price in addition to their £6 participation fee.

In Random and Diluted Choice, if the task drawn for a participant was a lottery task, the participant was then shown the lottery they had chosen in that task with the value of *X* equal to the drawn value. An experimenter visited them with a die to determine their final payoff, in addition to the £6 participation fee. Within Diluted Choice it was possible for one participant 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.

3.5. Implementation

The experiment took place in early 2015 at the University of East Anglia's Centre for Behavioural and Experimental Social Science (CBESS). All participants were recruited through the Centre's online recruitment system and had no prior experience of experiments of this type. Participants were seated in isolated booths as instructions were read aloud to outline the nature of the experiment (and 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 treatments included pre-experimental quizzes, in which participants were tested on their understanding of the experimental procedures and decision-making mechanisms. Participants who answered incorrectly were directed back to the relevant instructions before being informed of the correct answer, and were encouraged to ask the experimenter if they were still unsure. 80.8% of questions were answered in this first attempt, suggesting that, in general, participants understood the mechanisms of the experimental design.

4. Hypotheses

Table 1.1 outlines the six possible sub-treatments of the experimental design. To define treatment notation, V_{ij} is the valuation that an individual recorded in treatment '*i*', sub-treatment '*j*'. Participants only participated in one of the six possible sub-treatments, in a between-subject design, with comparisons of valuations across sub-treatments made across the population of participants.

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

Notes: Valuation of Random is the highest valued good across the three *given* goods tasks

Table 1.1. Experimental design

Null Hypothesis (H0) - Neoclassical Preferences

$$V_{RB} = V_{DB} = V_{CB} = V_{RS} = V_{DS} = V_{CS}$$

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

$$V_{RB} < V_{RS} \quad \text{and} \quad V_{DB} < V_{DS} \quad \text{and} \quad V_{CB} < V_{CS}$$

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

$$V_{RB} < V_{DB} \qquad \text{and} \qquad V_{RS} < V_{DS}$$

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.

Alternative Hypothesis (H3) - Random Lottery Effect

$$V_{DB} < V_{CB} \qquad \text{and} \qquad V_{DS} < V_{CS}$$

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, although possibly to a differing extent. 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 the one task Chosen.

Alternative Hypothesis (H4) – Attachment Effect on the WTA-WTP Disparity

$$(V_{RS} - V_{RB}) < (V_{DS} - V_{DB})$$

Prior hypothesis (H2) predicts that attachment effects will have a positive effect on valuation for both buyers and sellers. This increase in valuations may occur in two ways- increasing in absolute terms (i.e. attachment effects have some constant monetary increase for both buyers and sellers alike) or increasing in relative terms (i.e. attachment effects cause a percentage increase in valuations for both buyers and sellers alike). However, it is important to consider whether both buying and selling valuations are affected equally by attachment effects. This hypothesis, following Carmon and Ariely (2000), predicts that sellers ought to be more affected by any attachment effects than buyers, and as such, the WTA-WTP disparity would be significantly greater in Diluted Choice than the WTA-WTP disparity in Random.

Since both buying and selling valuations in Diluted Choice are predicted to be greater than in Random (as per (H2)) then a significant effect of an increased disparity in Diluted Choice is more likely to be observed when considering an attachment effect in terms of absolute increases

in valuations (as this requires a smaller increase than a relative increase). Thus, in an effort to capture any significant effect of increase in disparity between Diluted Choice and Random, this hypothesis predicts an increase in disparity size to be greater in absolute terms.

Alternative Hypothesis (H5) - Random Lottery Effect on the WTA-WTP Disparity

$$(V_{DS} - V_{DB}) < (V_{CS} - V_{CB})$$

It is possible that loss aversion might contribute to a difference in the impact of random lottery effects for buyers and sellers. In the case of sellers in Chosen, participants know they own the good with certainty; the goods task is the only task they complete, and so sellers would feel a reluctance to part with this owned good. In Diluted Choice, whilst participants make decisions within the goods task *as if* it were being played out for real, actual potential ownership of the good is reduced to a one-in-five chance. As a result, one might expect 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.

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. If removing random lottery effects does increase feelings of loss aversion in Chosen, and this affects sellers more than buyers, then we would expect the absolute disparity in Chosen to be greater than the disparity in Diluted Choice.

Order Effects

A unique feature of Random is the act of valuing all three mugs. 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 participant valuations. Testing for evidence of order effects in Random, i.e. whether the order in which these, subjectively differing, shaped goods are presented have any effect on participants' valuations, requires within-participant comparison, simply each participant'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. If individuals acted on neoclassical preferences, then individual valuation decisions in Random should not be influenced by the order in which goods are presented for valuation.

In analysing order effects, the following notation is used. For a given treatment and participant, let O_i be the participant's valuation of whichever good appeared in order i (i = 1, 2, 3) in the

sequence of goods tasks. Absent any order effects, we would expect the distribution of most preferred goods to be the same across the three possible orders, apart from sampling error. For any pair i, j of orders ($i, j = 1, 2, 3$), let p_{ij} be the probability that the valuations of a random participant 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 participant, each O_i is a random draw from a single distribution, the same for all i, leading to a testable hypothesis.

Null Hypothesis (H6)

$$p_{ij} = p_{ji}$$

That is, the probability that pair i, j of orders satisfies the property $O_i > O_j$ should be equally as likely as to satisfy $O_j > O_i$. It would also be expected that $q_{12} = q_{13} = q_{23}$, i.e. the probability of pairs of orders being valued equally should be the same for all pairs of orders. If order effects were to be present, we would expect earlier order i to be valued more highly than later order j, such that $p_{ij} > p_{ji}$.

5. Results

A total of 262 participants took part in the experiment, but 8 participants reported inconsistent preferences in their valuation decisions, meaning that unambiguous intended valuations could not be inferred. That is, these participants revealed more than one *preference switch* in their valuation decisions[2]. These participants were dropped from the analysis, leaving 254 participants in total with usable data.

5.1. Summary Statistics

Table 1.2 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 buying prices (£0.93) were exceeded by selling prices (£1.88) by a ratio of 1:2.01.

[2] If participants revealed inconsistency, efforts were made to allow for human error and still record an intended valuation. This was achieved through the following rule: if consistency could be achieved through the rectification of *one* valuation decision, and it was obvious which valuation decision was erroneous, then this one valuation decision was rectified and valuation was inferred from these consistent valuation decisions. The valuations of eleven participants were amended using this rule. The remaining eight participants dropped from the analysis either had required more than one valuation decision rectified to become consistent, or it was not obvious which valuation decision was erroneous.

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.

		Random	**Diluted Choice**	**Chosen**
	Average Valuation	**£0.83**	**£0.94**	**£1.02**
	Median Valuation	£0.50	£0.90	£0.90
Buyer	***No. of Participants***	***42***	***45***	***46***
	No. of cases of min-valuation	*15*	*11*	*5*
	Average Valuation	**£1.59**	**£1.78**	**£2.24**
	Median Valuation	£1.10	£1.50	£2.10
Seller	***No. of Participants***	***36***	***45***	***40***
	No. of cases of min-valuation	*7*	*8*	*3*

Notes: Min-valuation: buyer 'not buy' at any values, or seller 'sell' at every value.

Table 1.2. Summary statistics by treatment

Before considering tests for specific hypotheses H1-H5, it is of interest to first observe if there is a general trend of increase in valuations across the three treatments, Random, Diluted Choice and Chosen, separately for buyers and sellers. A non-parametric (two-tailed) test (Cuzick, 1985) shows that there is a statistically significant and increasing trend in valuations from Random, to Diluted Choice, to Chosen for both buyers (z= 1.960, p= 0.050) and sellers (z= 2.061, p= 0.031). This is an extension of a Wilcoxon rank-sum test, testing for a consistent trend in differences in the value of a variable across multiple sets of observations, where these sets have a natural ordinal ranking. This provides initial evidence that allowing the formation of attachment effects and the removal of random lottery effects increases valuations for both buyers and sellers.

Of all participants, 49/254 (19.3%) would not buy at any of the thirty given values (buyers), or would sell at every given value (sellers), suggesting the minimum possible valuation of £0.10 (min-valuation). Whilst the reasoning behind min-valuations remains ambiguous, there are legitimate reasons as to why participants might not want the good at any interval valuations, and so these valuations are appropriate to keep in the analysis of results. 31/133 buyers (23.3%) declared such a valuation, compared to 18/121 sellers (14.9%), which differ at a statistically significant level ($\chi^2(1)$= 2.893, p= 0.089). Min-valuations appear to decrease across treatments,

with 22/78 participants in Random, 19/90 in Diluted Choice and 8/86 in Chosen declaring such a valuation, and these differ significantly across treatments ($\chi^2(2)= 9.683$, $p= 0.008$).

5.2. Statistical Analysis

Further analysis utilises more controlled tests for evidence in favour of Alternative Hypotheses (H1-H5). The non-parametric results of Alternative Hypotheses (H1-H3) are summarised in Table 1.3. The use of parametric analysis allows 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 and formal Economics study) and responses to lottery tasks[3]. Whilst valuations in this experiment were restricted such that they could not be less than £0.10 or greater than £6.10, actual valuations could be less than or greater than these. To address this econometrically, a Tobit model is used, with lower and upper bounds set at £0.10 and £6.10 respectively.

Hypothesis	**Difference of Average Valuation**	**Wilcoxon Rank-Sum (*p*-value)**	**Ratio of Average Valuation**
Endowment Effect (H1)			
$V_{RB} < V_{RS}$	+ £0.76	**0.007*** **	1 : 1.91
$V_{DB} < V_{DS}$	+ £0.84	**0.009*** **	1 : 1.90
$V_{CB} < V_{CS}$	+ £1.22	**<0.001*** **	1 : 2.20
Attachment Effect (H2)			
$V_{RB} < V_{DB}$	+ £0.11	0.196	1 : 1.13
$V_{RS} < V_{DS}$	+ £0.19	0.565	1 : 1.12
Random Lottery Effect (H3)			
$V_{DB} < V_{CB}$	+ £0.08	0.567	1 : 1.08
$V_{DS} < V_{CS}$	+ £0.45	**0.092***	1 : 1.25

Table 1.3. Statistical tests of Alternative Hypotheses (H1-H3)

Alternative Hypothesis (H1) - Endowment Effect

$$V_{RB} < V_{RS} \quad \text{and} \quad V_{DB} < V_{DS} \quad \text{and} \quad V_{CB} < V_{CS}$$

For Random, Diluted Choice and Chosen treatments selling prices exceed buying prices by approximately £0.76, £0.84 and £1.22 (a relative difference of £91%, 90% and 120%)

[3] As answering the questionnaire was optional, one subject in Random did not disclose their demographic information and so were omitted from any regression analysis including demographic information.

respectively. A Wilcoxon rank-sum test provides significant evidence of a higher willingness-to-accept in sellers than willingness-to-pay in buyers for all three treatments. Random (z= 2.716, p= 0.007), Diluted Choice (z= 2.630, p= 0.009) and Chosen (z= 3.916, p< 0.001) all suggest very strong support for Alternative Hypothesis (H1).

This too is supported by Tobit regression analysis in Table 1.4, which includes the effects of demographic information and lottery task decisions (the effects of which will be discussed in section 5.5). In each of the three models, the dependent variable is the valuation for a mug reported by a participant (the highest valuation for a mug in Random, or the valuation of the chosen mug in Diluted Choice and Chosen). The following independent variables are used:

Seller: takes the value 1 if the participant was in a Seller sub-treatment, 0 otherwise.

Age: takes the value of the participant's reported age, with the minimum reported age standardised to 0 (i.e. the participants reported age, minus 18).

Female: takes the value 1 if the participant's reported gender is female, 0 otherwise.

Economics: takes the value 1 if the participant's reported field of study is Economics, 0 otherwise.

Risk Averse: takes the value 1 if participant chose Choice A in Lottery Task (i), 0 otherwise.

Dominance: takes the value 1 if participant chose Choice A in Lottery Task (ii), 0 otherwise.

Strong Risk Averse: takes the value 1 if participant chose Choice A in Lottery Task (iii), 0= otherwise.

Common Ratio: takes the value 1 if participant chose Choice A in Lottery Task (i) *and* Choice B in Lottery Task (iv), takes the value 0 if any other pairwise choice in (i) and (iv)).

Opposite CR: takes the value 1 if participant chose Choice B in Lottery Task (i) *and* Choice A in Lottery Task (iv), takes the value 0 if any other pairwise choice in (i) and (iv)).

All models in Table 1.4 find positive and significant increases in valuations for sellers in Random (p= 0.023), Diluted Choice (p= 0.001) and Chosen (p< 0.001).

Valuation	Endowment Effect		
	Model 1	Model 2	Model 3
	Random **(H1)**	Diluted Choice **(H1)**	Chosen **(H1)**
Seller	0.9568**	1.1348***	1.2648***
	(0.411)	*(0.325)*	*(0.292)*
Age	0.0209	0.0437	0.0129
	(0.051)	*(0.063)*	*(0.027)*
Female	-0.5529	-0.7383**	0.3153
	(0.414)	*(0.353)*	*(0.305)*
Economics	0.1891	-0.1368	1.0379**
	(0.678)	*(0.483)*	*(0.462)*
Risk Averse	0.1095	0.0234	-------
	(0.502)	*(0.539)*	-------
Dominance	0.9753	0.3976	-------
	(0.628)	*(0.545)*	-------
Strong Risk Averse	-------	-0.0490	-------
	-------	*(0.351)*	-------
Common Ratio	-------	-0.7344*	-------
	-------	*(0.373)*	-------
Opposite CR	-------	-0.1774	-------
	-------	*(0.819)*	-------
Constant	0.4293	1.1856**	0.5828**
	(0.661)	*(0.533)*	*(0.282)*
Sigma	1.6718***	1.4309***	1.2790***
	(0.174)	*(0.125)*	*(0.107)*
# Obs	77	90	86

Standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1

Table 1.4. Tobit models of endowment effects, demographic effects and lottery task decisions on valuations

Tables 1.5 and 1.6 include models to test alternative hypotheses H2-H5. In these models, the dependent variable is the valuation for a mug reported by a participant (the highest valuation for a mug in Random, or the valuation of the chosen mug in Diluted Choice and Chosen). In addition to prior independent variables in Table 1.4, the following independent variables are also used:

Diluted Choice: takes the value 1 if the participant was in a Diluted Choice treatment, 0 otherwise (including only Random and Diluted Choice testing for an attachment effect).

*Diluted Choice*Seller*: takes the value 1 if the participant was in Diluted Choice-Seller sub-treatment, 0 otherwise (including only Random and Diluted Choice testing for an attachment effect).

Chosen: takes the value 1 if the participant was in a Chosen treatment, 0 otherwise (including only Diluted Choice and Chosen testing for a random lottery effect).

*Diluted Choice*Seller*: takes the value 1 if the participant was in Chosen-Seller sub-treatment, 0 otherwise (including only Diluted Choice and Chosen testing for a random lottery effect).

Alternative Hypothesis (H2) - Attachment Effect

$$V_{RB} < V_{DB} \quad \text{and} \quad V_{RS} < V_{DS}$$

When testing for a pure effect of attachment on both buyers and sellers, differences between valuations across Random and Diluted Choice indicate a presence of an *attachment premium*. For buyers, an approximation of such an *attachment premium* is a 13% increase (roughly £0.11), and for sellers a 12% increase (roughly £0.19). Wilcoxon rank-sum tests fail to find significant differences for buyers (z= 1.292, p= 0.196) or sellers (z= 0.576, p= 0.565). Tobit regression analysis, controlling for additional demographic and lottery task information, presents evidence of significant effects (p= 0.065) of a positive *attachment premium* for sellers, but a non-significant effect (p= 0.324) for buyers.

Alternative Hypothesis (H3) - Random Lottery Effect

$$V_{DB} < V_{CB} \quad \text{and} \quad V_{DS} < V_{CS}$$

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 a relatively modest 8% (approximately £0.08). For sellers, however, average valuation increased by a much larger 25% increase (approximately £0.45). Wilcoxon rank-sum tests also show no significant difference for buyers (z= 0.573, p= 0.567) but a significant difference for sellers (z= 1.686, p= 0.092). Tobit regression analysis also finds significant differences for sellers (p= 0.088) but not for buyers (p= 0.491).

Alternative Hypothesis (H4) - Attachment Effect on the WTA-WTP Disparity

$$(V_{RS} - V_{RB}) < (V_{DS} - V_{DB})$$

In Models 1 and 2 in Table 1.5, regression analysis tests for an attachment effect separately for buyers and sellers, and observed only a significant effect for sellers, suggesting an increase in overall WTA-WTP disparity in Diluted Choice when compared to Random.

Pooling both buyers and sellers in one model in Model 3, the interaction variable *Diluted*

*Choice*Seller* indicates the absolute increase in size of the WTA-WTP disparity in Diluted Choice as implied by the two separate buyer and sellers models. A positive coefficient (0.2279) implies that in absolute terms, WTA-WTP disparity is greater in Diluted Choice, than in Random. This increase in disparity is not statistically significant (p= 0.701). This suggests that choice effects are unlikely to significantly increase the overall absolute size of the WTA-WTP disparity.

Valuation	**Attachment Effect**		
	Model 1 Buyer **(H2)**	Model 2 Seller **(H2)**	Model 3 Buyer vs. Seller **(H4)**
Seller	-------	-------	0.8900**
	-------	-------	*(0.377)*
Diluted Choice	0.3006	0.7451*	0.1395
	(0.303)	*(0.398)*	*(0.363)*
DC*Seller	-------	-------	0.2279
	-------	-------	*(0.515)*
Age	0.0314	0.0891	0.0178
	(0.040)	*(0.070)*	*(0.039)*
Gender	0.1913	-1.7479***	-0.6462**
	(0.302)	*(0.409)*	*(0.266)*
Economics	0.1383	-0.9144	-0.1554
	(0.466)	*(0.564)*	*(0.391)*
Risk Averse	-0.0294	0.0179	-0.1446
	(0.384)	*(0.457)*	*(0.321)*
Dominance	-0.2110	1.7465***	0.5710
	(0.467)	*(0.617)*	*(0.405)*
Constant	0.2818	1.7478***	0.8381*
	(0.506)	*(0.565)*	*(0.462)*
Sigma	1.2777***	1.5978***	1.5594***
	(0.126)	*(0.146)*	*(0.104)*
# Obs	86	81	167

Table 1.5. Tobit models of attachment effects, demographic effects and lottery task decision on valuation

Alternative Hypothesis (H5) – Random Lottery Effects on the WTA-WTP Disparity

$$(V_{DS} - V_{DB}) < (V_{CS} - V_{CB})$$

Non-parametric and regression analysis, in Models 1 and 2 in Table 1.6, shows that whilst there were some increase in valuations with the removal of random lottery effects for both buyers and sellers, this effect was only statistically significant in the case of sellers. Again, this implies that WTA-WTP disparity ought to be greater in Chosen than Diluted Choice.

Indeed, pooling both buyers and sellers in Model 3 allows for interaction variable *'Chosen*Seller'*, which indicates the additional increase in disparity in Chosen, relative to Diluted Choice. A positive coefficient (0.3729) implies that the WTA-WTP disparity in Chosen is greater than the disparity in Diluted Choice, although this increase is not statistically significant (p= 0.397). This lack of statistical significance implies that although the implementation of random lottery effects might be reducing valuations (and significantly so for sellers) this is unlikely to have significant implications for the WTA-WTP disparity.

Valuation	**Random Lottery Effect**		
	Model 4 Buyer **(H3)**	Model 5 Seller **(H3)**	Model 6 Buyer vs. Seller **(H5)**
Seller	-------	-------	0.9441***
	-------	-------	*(0.310)*
Chosen	0.1390	0.6996*	0.2071
	(0.201)	*(0.405)*	*(0.305)*
Chosen*Seller	-------	-------	0.3729
	-------	-------	*(0.439)*
Age	0.0250	0.0045	0.0137
	(0.031)	*(0.041)*	*(0.027)*
Gender	0.1516	-0.4204	-0.0980
	(0.205)	*(0.430)*	*(0.230)*
Economics	0.0634	0.7642	0.4421
	(0.304)	*(0.594)*	*(0.330)*
Constant	0.6464***	1.7265***	0.6587**
	(0.213)	*(0.438)*	*(0.270)*
Sigma	0.9362***	1.7638***	1.4127***
	(0.080)	*(0.153)*	*(0.085)*
# Obs	91	85	176

Table 1.6. Tobit models of random lottery effects, demographic effects and lottery task decision on valuation

5.3. Order Effects

Within Random, testing for order effects requires a within-participants comparison of implicit preferences for goods, based on their valuations. Of the 78 participants in Random, 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 greater than min-valuation and common to two or three goods. The remaining 22 were those who assigned min-valuation to all three goods.

Table 1.7 reports the numbers of participants 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. 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 ($\chi^2(2)= 30.216$, $p< 0.001$).

Highest Valuation(s)	**Number of Participants**
O₁	28
O₂	6
O₃	3
O₁, O₂	7
O₁, O₃	1
O₂, O₃	3
O₁, O₂, O₃	8
Min-valuation	22
n	78

Table 1.7. Highest valuations in Random treatment by order

Null Hypotheses (H6)

$$p_{ij} = p_{ji}$$

As shown in Table 1.8, 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. As such, the null hypothesis of $p_{ij} = p_{ji}$ is clearly rejected.

When comparing the prevalence of equal valuation between pairings, the notion that all three possible equal pairings occur with the same frequency, $q_{12} = q_{13} = q_{23,}$ is also rejected for all participants (when excluding participants with equality in valuations for all three goods, or with min-valuations for all goods) ($\chi^2(2)= 11.400$, p-value= 0.003). There appears to be significantly more equality in second and third ordered preferences, again suggesting evidence of first valued goods being valued more highly. Both effects are much stronger in sellers than buyers, suggesting that order effects are more prevalent for sellers.

	Number of Participants		
Relative Valuations	**All (n=48)**	**Buyer (n=20)**	**Seller (n=28)**
($O_1 > O_2$)	29	9	20
($O_1 < O_2$)	10	6	4
($O_1 = O_2$)	9	5	4
($O_1 > O_3$)	37	14	23
($O_1 < O_3$)	8	4	4
($O_1 = O_3$)	3	2	1
($O_2 > O_3$)	20	10	10
($O_2 < O_3$)	10	4	6
($O_2 = O_3$)	18	6	12
	p-value of χ^2 test		
Null Hypothesis	**All**	**Buyer**	**Seller**
$p_{12} = p_{21}$	**0.002*** **	0.439	**0.001*** **
$p_{13} = p_{31}$	**<0.001*** **	**0.018** **	**<0.001*** **
$p_{23} = p_{32}$	**0.068***	0.109	0.317

Table 1.8. Relative valuations in Random treatment by order

5.4. Shape Effects

As explained in section 3, the three mug designs were chosen with the intention that the distribution of participants' 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 participants' 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 1.9 reports, 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.

It is also useful to consider whether participants' explicit or implicit preferences between shapes were randomly distributed. Table 1.10 reports, for each shape, the number of participants who chose it (Choice and Diluted Choice) or for whom it was uniquely valued most highly (Random).

It is clear that participants in Chosen and Diluted Choice reveal a much greater skew of preferences for shapes than do participants 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 participant's highest-valued shape to be whichever shape was seen first. That effect will tend to mask underlying (order-independent) preferences for shapes.

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	£1.43	£1.26	£0.67	$\chi^2(2)= 2.715$	0.257

Table 1.9. Mean valuation of buyers and sellers by good type

Shape Preference	**(n)**	**Squares**	**Circles**	**Triangles**	**χ^2 test**	***p*-value**
Chosen and Diluted Choice	176	71	70	35	$\chi^2(2)= 14.330$	**<0.001*** **
Random (Strict Preference)	37	18	9	10	$\chi^2(2)=3.946$	0.139

Table 1.10. Shape preference by good type

5.5. Demographic Effects and Lottery Task Decisions

Variables describing participants' demographic information and their responses to lottery tasks were included in the parametric regression models. When considering demographic information, there is some evidence to suggest that females value goods less than their male counterparts, and this is significantly so for sellers in Random and Diluted Choice in Table 1.5 ($p< 0.001$). Indeed, Wilcoxon rank-sum tests indicate that female valuations are significantly lower than males ($z= 1.964$, $p= 0.050$).

The number of min-valuations of participants is significantly greater for females than males ($\chi^2(1)= 4.185$, $p= 0.041$). When participants who indicate min-valuation are removed,

valuations by gender do not differ significantly (z= 0.216, p= 0.829). This suggests that this finding may be driven by a greater general disinterest in the goods by females, rather than different strategic motivations that differ by gender (see Appendix 1.4 for derivations of statistical tests for gender effects).

The lottery task response variable that proved to be notably significant was the dummy variable representing direct violation of dominance in lottery (ii), where participants who violated dominance reported significantly higher valuations for goods, and this effect was found only for sellers (p= 0.006) in Table 1.5. Whilst any interpretation of this occurrence is speculative, it may suggest that participants 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 participants who chose dominated lotteries (only 11.3% of possible participants) does not affect the results of the key hypotheses however, and this fact gives insufficient reason to remove these participants from overall analysis (see Appendix 1.5.1).

Given the somewhat unusual nature of the lottery tasks, it is possible that participants 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, 81.5% of all lottery questions were answered correctly at the first attempt. Clearly, most participants were able to quickly understand the mechanism and payoff structure of the lottery tasks. Separating participants into those who answered all lottery quizzes correctly, and those who did not reveals no difference in likelihood to directly violate dominance ($\chi^2(1)$= 0.509, p= 0.475, see Appendix 1.5.3).

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 participants' non-incentivised judgements about the desirability of each of the three mugs. Participants 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 participants, within- and between-treatments. As different participants may interpret these scores differently, scores were used only to elicit within-participant ordinal comparisons. As the questionnaire asked about potentially sensitive demographic information it was optional; two participants chose not to answer completely the goods appeal aspect of the questionnaire, and so were omitted from the analysis described below.

Each participant'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 participant'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 1.11 shows the numbers of participants with consistent responses, separated by treatment and by Buyer or Seller.

Participants with Consistent Preferences/ Number of Participants	**Random**	**Diluted Choice**	**Chosen**	**χ^2 test**	***p*-value**
All	62/76	84/90	83/86	$\chi^2(2)= 11.869$	**0.003*** **
Buyer	34/40	41/45	44/46	$\chi^2(2)= 2.924$	0.232
Seller	28/36	43/45	39/40	$\chi^2(2)= 10.789$	**0.005*** **

Table 1.11. Preference consistency of participants by treatment

These results show that, in general, and particularly amongst sellers, there was much less consistency in Random than in Chosen or Diluted Choice. Of the 14 participants 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. Of the remaining two participants, both valued the second good they valued within the experiment most highly. Absent any confounding factors, one would expect these inconsistent preference valuations to be randomly distributed across all three possible orders. This non-random nature of the order in which these most highly valued goods are observed ($\chi^2(2)= 17.714$, $p< 0.001$) provides further evidence of order effects.

If order effects are affecting valuations within the goods task, causing valuations to suggest preferences to differ from those in the post-experimental questionnaire, then it may be that the post-experimental questionnaire better reveals unimpeded preferences. Valuations for Random could instead be defined by the good that participants report as most desirable in the post-experimental questionnaire, and it will be interesting to observe whether valuations based on these preferences instead affect the results of this paper. The results, and their consequent effects on the Alternative Hypotheses (H1-H5) can be found in Appendix 1.6.

When determining preferences by reporting outcomes from the post-experimental questionnaire for Random treatment, there are significant outcomes for the tested hypotheses

in this paper. On average, valuations in Random fall, a consequence of taking questionnaire based preferences rather than preferences by highest valuation when the two differ. Questionnaire based preferences should not be subject to the order effects affecting valuations. Indeed, of participants that report a 'strict preference', where one good is rated more desirable than the other two in the post-experimental questionnaire, there is no evidence to suggest that these preferences are skewed in favour of first valued goods ($\chi^2(2)= 0.143$, $p= 0.931$, see Appendix 1.6.4).

Using this alternative definition of valuation, Wilcoxon rank-sum tests find evidence of a persistence of an endowment effect in Random ($z= 2.138$, $p= 0.033$). Wilcoxon rank-sum tests also find evidence that valuations differ significantly between Random and Diluted Choice for buyers ($z= 1.686$, $p= 0.092$) but not sellers ($z= 1.538$, $p= 0.124$), though this difference for sellers is greater than the results of the original reported attachment effect using conventional valuations ($z= 0.576$, $p= 0.565$). Regression analysis using post-experimental questionnaire responses and including demographic information suggests that sellers are more strongly affected by attachment effects ($p= 0.003$) than buyers ($p= 0.188$). When comparing the resulting impact of attachment effects on the WTA-WTP disparity, the interaction variable in regression analysis *(Diluted Choice*Seller*) yields a positive coefficient (0.4720) suggesting an increase in the disparity, though this effect is still not statistically significant ($p= 0.354$).

Such a high level of consistency in Diluted Choice and Chosen suggests that this non-incentivised questionnaire was successful in revealing accurate preferences. It is possible however that such consistency may be a manifestation of cognitive dissonance – that participants 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 participants' 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, participants experience no such creation of an attachment effect, nor such cognitive dissonance.

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 some evidence to suggest that the formation of attachment through choice can increase valuations of a good for both buyers and sellers, resulting in an *attachment premium* of roughly 13%, but that this is only statistically significant for sellers. Whilst allowing for attachment effects on average increases the size of the WTA-WTP disparity this difference is not statistically significant. Thirdly, there is evidence to suggest that random lottery effects, and removing the certainty of ownership, can have significant negative effects on selling prices, but non-significant effects for buyers. Despite this difference, the increase in size of the WTA-WTP disparity is not statistically significant. Finally, there is strong evidence of a tendency in Random for the first good presented to participants to be valued more highly than goods presented later, particularly for sellers.

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 choice between relatively few goods, when options are readily understood and goods are perceived as desirable, increases the satisfaction towards the chosen item. Using preferences for Random based on post-experimental questionnaire responses as an alternative, and arguably more accurate, measure of preferences, suggests that once the potential implications of order effects are removed, there is increased evidence of attachment effects for buyers and sellers, and once controlling for demographic information, this effect is significant and much stronger in sellers.

This experiment has shown that offering participants a simple choice of very generic household goods could elicit an *attachment premium*. If attachment itself is part of the experience of a good or service, then it ought not to be dismissed in the evaluation of a product. For buyers, an awareness of the impact of attachment in their intrinsic valuation may lead to better informed valuation decision of goods and services. Of course, for sellers, manipulating a sense of attachment may be utilised to charge a premium or gain a competitive advantage.

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. 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. Individuals who are willing to pay toward the provision of publicly provided goods, services or charitable causes, may do so because of feelings of attachment towards these, and so attempting to maximise this, whether from attachment through choice or more personal feelings of attachment, may be a strategy for increasing contributions.

6.2. Random Lottery Effects

The *conditional ownership* of Diluted Choice revealed reduced valuations compared to the certain ownership in Chosen for sellers. Whilst increasing the WTA-WTP disparity overall, this was not statistically significant. This difference between buyers and sellers may be a result of loss aversion. 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. Order Effects

The order effects found in Random have implications in a number of ways. Firstly, it suggests that the valuations of goods differing only in subjective value are affected by similar effects to those in previous literature, concerning hypothetical public goods or goods of differing objective value. This raises future questions as to why these persist, given that many prior explanations of order effects ought not to hold for subjectively differing goods. The fact that order effects persist more significantly for sellers than buyers suggests that the process by which order effects are formed may be dependent on some difference between buyers and sellers, and this finding may be important for stated preference methodology.

6.4. 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; there are diminishing returns to novelty. Novelty has been used as a potential explanation in a variety of economic scenarios, from the increased valuation of new coloured

foodstuffs (Stevens and Winter-Nelson, 2008), to an increase in affinity effects (Tom, 2004, p.168) or appeal of novel goods (Tom *et al*., 2007, p.123).

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. As sellers focus much more readily than buyers on these positive attributes (Carmon and Ariely, 2000, Nayakankuppam and Mishra, 2005), then this may be driving order effects in this paper.

The potential to take away a consumable good is undoubtedly a novel experience for participants who participate in WTA-WTP experiments. Whilst participants in the Random treatment were made aware that they would value three goods in this experiment, they did not know what these goods would be until they were distributed in the first task. Though participants were encouraged to look, touch and think about their good before completing their valuation decisions, these were made very shortly after seeing the good for the first time. 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, along with the attention given, could have dissipated. This loss of novelty, and attention, would be perpetuated (though at a diminished rate) once the third good was revealed. Of course, any particularly strong preferences for the new shaped pattern might override such a fall in novel value.

The results also find the strongest difference between preferences for novel goods occurs after the first novel good has been revealed, also found in contingent valuation of public provisions (Payne *et al*., 2000). Whilst that may be a result of failure to consider the valuation of future hypothetical provisions until a second provision is observed (p.13), the incentivised structure of this experiment, using real goods, should not exhibit this effect.

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.

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 *et al.*, 2004) and also removes the novelty of experiencing new goods during the experiment. In this experiment, participants 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 absent 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 some evidence of an attachment effect was found. Although the effect was greater for sellers than buyers, this did not significantly increase the size of the disparity in Diluted Choice. Evidence of an attachment effect is increased when using measures of preferences based on a post-experimental questionnaire, which attempted to eliminate some of the potential confounding factors associated with the experimental design, such as order effects. In removing potential random lottery effects a significant positive 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.

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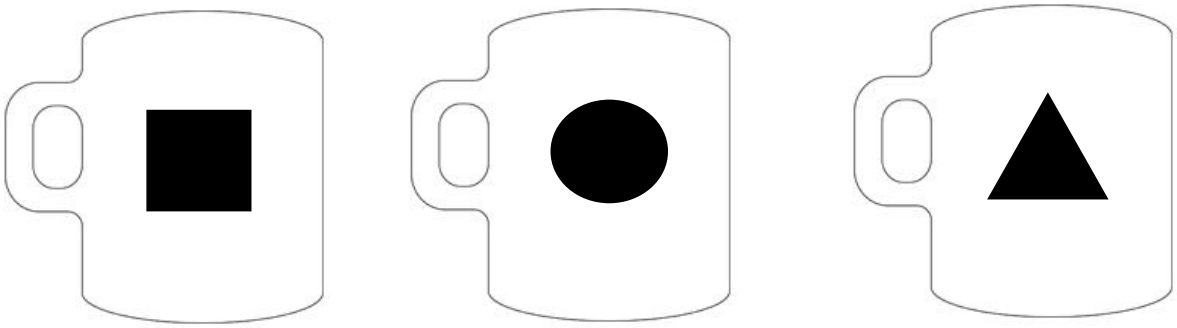
Appendices

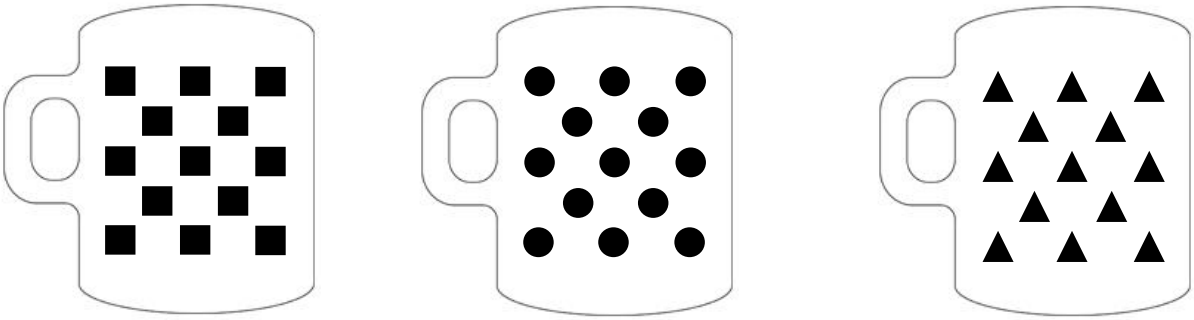
Appendix 1.1. Pre-experimental online pilot survey of mug designs

1.1.1. Potential mug designs

Individual



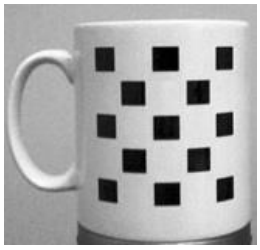
Many



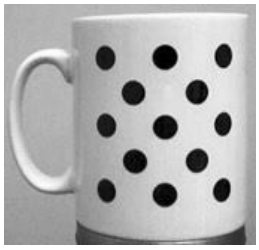
1.1.2. Wilcoxon rank-sum tests of differences in preference scores of different potential mug designs

Average Preference Score (0-7)	**Individual** ***(n=79)***	**Many** ***(n=69)***	**Wilcoxon Rank-Sum** ***z*-stat**	***p*-value**
Squares	1.38	2.80	4.748	**< 0.001*** **
Circles	1.47	2.83	4.625	**< 0.001*** **
Triangles	1.91	2.10	0.968	0.333
Set of Three	2.68	3.09	1.130	0.258

Appendix 1.2. Descriptions of goods used



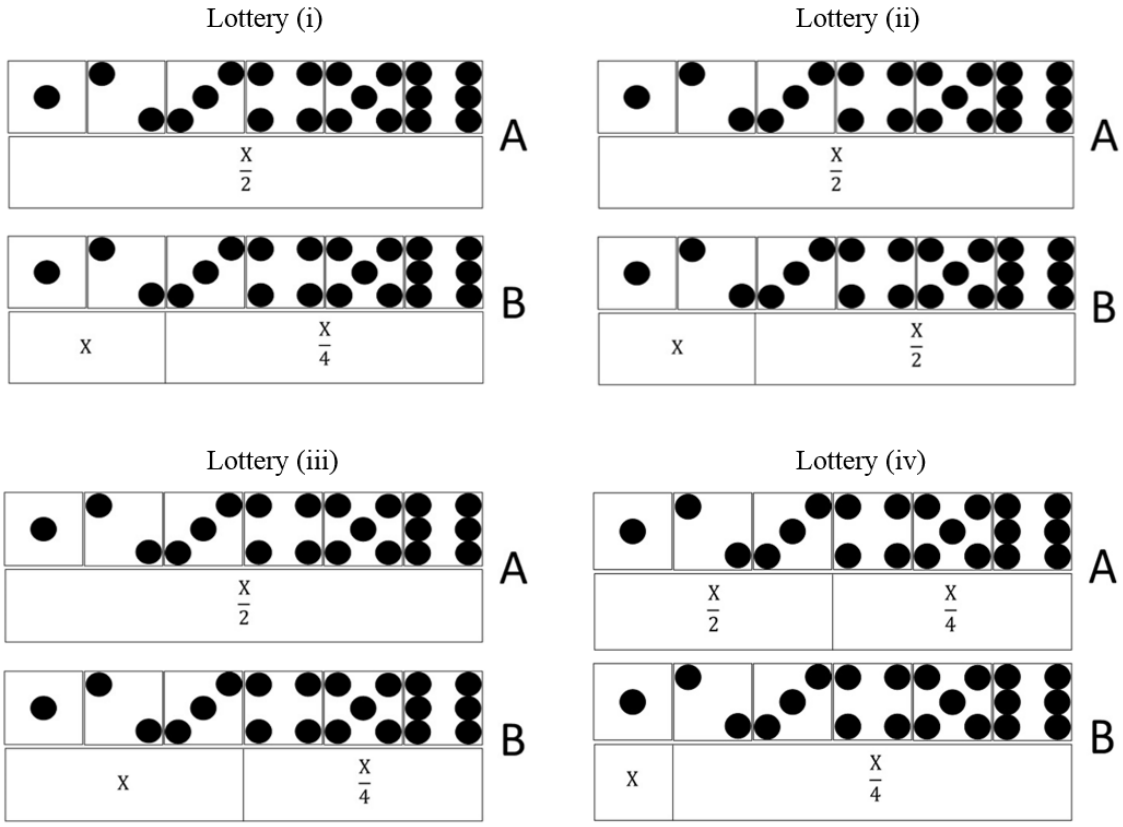
A plain white ceramic mug with printed black squares. Dishwasher and microwave safe.



A plain white ceramic mug with printed black circles. Dishwasher and microwave safe.

A plain white ceramic mug with printed black triangles. Dishwasher and microwave safe.

Appendix 1.3. Lottery task descriptions and results



The four lottery tasks above were designed to test for attitudes of risk and for possible violations of Expected Utility Theory.

Risk Aversion: Lottery task (i)

Both options have the same expected payoff, but B involves risk and A does not. Thus, a risk averse participant would choose A and a risk loving participant would choose B. 81.0% of participants revealed risk aversion.

Dominance: 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. 11.3% of participants directly violated dominance.

Strong Risk Aversion: Lottery task (iii)

As the expected payoff of the risky lottery B exceeds the certain value of A, any participant choosing A could be seen as strongly risk averse. 34.4% of participants revealed strong risk aversion.

Implied Dominance: Lottery task (i) and lottery task (iii)

Any participant choosing B in lottery task (i), but A in lottery task (iii) would indirectly violate dominance. No participants indirectly violated dominance.

Independence Axiom- Common Ratio Effect: Lottery task (i) and lottery task (iv)

As lottery task (iv) is simply a scaled down version of lottery task (i), that axiom of independence 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 (Cubitt *et al*. (1998a), for example). 44.4% of participants revealed the common ratio effect. 5.6% violated independence in the opposite direction.

Appendix 1.4. Effects of demographic information on valuations

1.4.1. Occurrence of min-valuations by gender

	Gender		
	Male	**Female**	**(n)**
> £0.10	91	113	204
£0.10	14	35	49
(n)	105	148	**253**

Chi-squared test of relationship between tendency of min-valuation and gender:

$$\chi^2(1)= 4.185,\ p= 0.041$$

1.4.2. Average valuation by gender

Average Valuation	**Male**	**Female**	**Wilcoxon Rank-Sum**	
			***z*-stat**	***p*-value**
All	£1.50	£1.12	1.964	**0.050****
Buyer	£0.81	£0.95	-0.159	0.873
Seller	£2.27	£1.29	3.272	**0.001*****

1.4.3. Average valuation by gender, excluding min-valuations

Average Valuation	**Male**	**Female**	**Wilcoxon Rank-Sum**	
			***z*-stat**	***p*-value**
All	£1.75	£1.59	0.216	0.829
Buyer	£1.06	£1.38	-1.227	0.220
Seller	£2.34	£1.79	1.611	0.107

Appendix 1.5. Effects of lottery task responses on valuations

1.5.1. Tobit models of endowment effects, demographic effects and lottery task decision on valuation, with participants who violated dominance in lottery tasks removed

P-EQ Valuation	**Endowment Effect**	
	Model 1 Random **(H1)**	Model 2 Diluted Choice **(H1)**
Seller	0.7351* (0.436)	1.1085*** (0.322)
Age	0.0264 (0.056)	0.1343* (0.072)
Female	-0.4673 (0.440)	-0.9812*** (0.351)
Economics	0.1849 (0.675)	-0.2877 (0.482)
Risk Averse	0.1726 (0.550)	0.0161 (0.533)
Dominance	------- -------	------- -------
Strong Risk Averse	------- -------	-0.0347 (0.351)
Common Ratio	------- -------	-0.7309** (0.365)
Opposite CR	------- -------	-1.7674* (0.973)
Constant	0.4192 (0.713)	1.1694** (0.512)
Sigma	1.6625*** (0.186)	1.3470*** (0.125)
# Obs	68	80

1.5.2. Tobit models of attachment effects, demographic effects and lottery task decision on valuation, with participants who violated dominance in lottery tasks removed

P-EQ Valuation	Attachment Effect		
	Model 3 **(H2)** Buyer	Model 4 **(H2)** Seller	Model 5 **(H4)** Buyer vs. Seller
Seller	-------	-------	0.6576*
	-------	-------	(0.393)
Diluted Choice	0.3168	0.8610**	0.1982
	(0.335)	(0.393)	(0.377)
Diluted Choice*Seller	-------	-------	0.3186
	-------	-------	(0.535)
Age	0.0027	0.1269*	0.0399
	(0.047)	(0.069)	(0.043)
Female	0.3422	-1.8800***	-0.6610**
	(0.336)	(0.401)	(0.276)
Economics	0.1816	-1.0255*	-0.1812
	(0.488)	(0.557)	(0.395)
Risk Averse	0.0406	0.3320	0.0578
	(0.443)	(0.462)	(0.343)
Dominance	-------	-------	-------
	-------	-------	-------
Constant	0.2108	1.4031**	0.6739
	(0.574)	(0.559)	(0.482)
Sigma	1.3339***	1.4869***	1.5264***
	(0.142)	(0.143)	(0.109)
# Obs	75	73	148

1.5.3. Performance in lottery task-related pre-experimental quizzes and likelihood to violate dominance in lottery tasks, in Random and Diluted Choice

	(n)	**All Correct**	**One or More Incorrect**
All	168	137	31
Dominance	19	15	4
No Dominance	149	127	22

Chi-squared test of relationship between violation of dominance and correct pre-experimental quiz answers:

$$\chi^2(1)= 0.509,\ p= 0.475$$

Appendix 1.6. Post-experimental questionnaire preferences in Random

1.6.1. Summary statistics by post-experimental questionnaire preferences in Random

		Random	Diluted Choice	Chosen
Buyer	**Average P-EQ Valuation**	**£0.78**	**£0.94**	**£1.02**
	Median P-EQ Valuation	£0.30	£0.90	£0.90
	No. of Participants	***40***	***45***	***46***
	No. of cases of min-valuation	*17*	*11*	*5*
Seller	**Average P-EQ Valuation**	**£1.28**	**£1.78**	**£2.24**
	Median P-EQ Valuation	£0.90	£1.50	£2.10
	No. of Participants	***36***	***45***	***40***
	No. of cases of min-valuation	*8*	*8*	*3*

1.6.2. Statistical tests of alternative hypotheses (H1-H2) by post-experimental questionnaire preferences in Random

Hypothesis	Difference of Average P-EQ Valuation	Wilcoxon Rank-Sum (*p*-value)	Ratio of Average P-EQ Valuation
Endowment Effect (H1)			
$V_{RB} < V_{RS}$	+ £0.50	**0.033****	1 : 1.638
Attachment Effect (H2)			
$V_{RB} < V_{DB}$	+ £0.16	**0.092***	1 : 1.205
$V_{RS} < V_{DS}$	+ £0.51	0.124	1 : 1.397

1.6.3. Tobit models of endowment and attachment effects by post-experimental questionnaire preferences in Random

P-EQ Valuation	**Endowment Effect**	**Attachment Effect**		
	Model 1 Random **(H1)**	Model 2 Buyer **(H2)**	Model 3 Seller **(H2)**	Model 4 Buyer vs. Seller **(H4)**
Seller	0.6851*	-------	-------	0.6400*
	(0.396)	-------	-------	*(0.374)*
Diluted Choice	-------	0.4212	1.1277***	0.2742
	-------	*(0.317)*	*(0.374)*	*(0.358)*
Diluted Choice*Seller	-------	-------	-------	0.4720
	-------	-------	-------	*(0.508)*
Age	0.0247	0.0299	0.0957	0.0189
	(0.051)	*(0.043)*	*(0.066)*	*(0.039)*
Female	-0.5293	0.1392	-1.6368***	-0.6280**
	(0.399)	*(0.316)*	*(0.383)*	*(0.261)*
Economics	0.3130	0.1393	-0.8055	-0.1132
	(0.646)	*(0.484)*	*(0.525)*	*(0.381)*
Risk Averse	-0.0622	-0.0383	-0.1501	-0.2316
	(0.483)	*(0.402)*	*(0.426)*	*(0.314)*
Dominance	1.1521*	-0.2832	1.8780***	0.6182
	(0.625)	*(0.498)*	*(0.575)*	*(0.403)*
Constant	0.4032	0.1981	1.3975***	0.7588*
	(0.640)	*(0.531)*	*(0.527)*	*(0.454)*
Sigma	1.5869***	1.3229***	1.4867***	1.5225***
	(0.170)	*(0.135)*	*(0.135)*	*(0.103)*
# Obs	76	85	81	166

1.6.4. Relationship between (strict) good preference by post-experimental questionnaire and experimental order of good presentation

(n)	**Experimental Order of Good Presentation**		
	First	**Second**	**Third**
All	13	15	14
Squares	5	5	6
Circles	5	7	5
Triangles	3	3	3

Chi-squared test of non-random distribution of (strict) preference by post-experimental questionnaire across experimental order of good presentation:

$$\chi^2(2)= 0.143,\ p= 0.931$$

Appendix 1.7. Experimental instructions

Part One

Introduction

Welcome to this experiment on decision making. Thank you for coming.

Please follow along as I read through the instructions. If you have a question, please raise your hand and I will come to answer your question privately.

The following instructions are simple, and if you follow them carefully you will have the chance to earn money, and other things. What you take away from the experiment will be determined by your decisions and by chance. After you have completed the experiment you will receive a £6 participation fee, plus or minus the amount resulting from your decisions in the experiment.

Your decisions in this experiment are private, and we ask you not to communicate with others during this experiment. It is also important you do not react verbally to outcomes during the experiment. If you have any questions during the experiment please raise your hand and an experimenter will come to assist you. Please keep to these simple rules, as anyone breaking them may be asked to leave without payment.

The experiment consists of five tasks – one *goods task* and four *lottery tasks*. After you have completed all five tasks, one of the tasks will be selected at random. What you take away from the experiment will be determined by the decisions you made in that task, and only by those decisions. As any one of the five tasks might be selected to determine your earnings, you should think about each task as if it were for real, and as if it were the only task in the experiment.

I will now describe the two types of task.

Goods Task

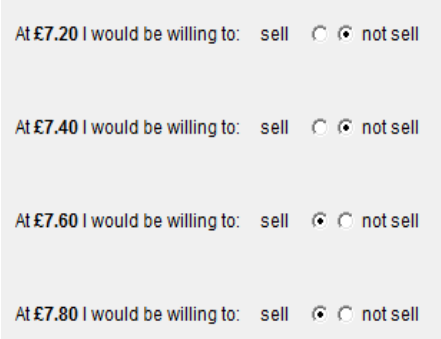
At the start of the goods task, you will be given the opportunity to choose one of three items, which you then own. You will have the opportunity to choose whether to keep the item or to sell it back to the experimenter. You will be shown a list of prices. For each of these prices you will be asked if you would be willing to sell the item at this given price or not.

At the end of the experiment, if the goods task is selected to determine your earnings, one of the prices listed will be selected at random. This will be the price that the experimenter offers for the item in the goods task. If you have indicated you would be willing to sell the item at this price, you will give up the item and receive that price in addition to the £6 participation fee. If you have indicated you would not be willing to sell the item at this price, you will keep the item and receive no additional money.

Please note that your decisions in the task cannot affect which price the experimenter offers. So when deciding whether or not you are willing to sell at the listed prices, it is in your interest to think about each price separately.

To assist in the explanation of the goods task, here is an example.

Suppose, after being given an item, a participant in the experiment is asked whether or not they would be willing to sell it at the following prices:



Here the participant has indicated that they are not willing to sell at prices £7.20 and £7.40, but that they are willing to sell at prices £7.60 and £7.80. If this task was selected to determine the participant's earnings, and if one of the four prices was selected at random to be the offer made by the experimenter, the participant's decision at that price would be made binding.

Lottery Task

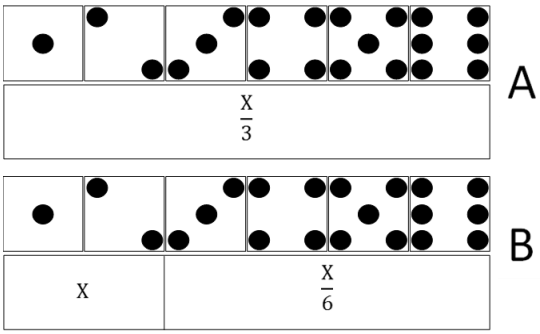
In each lottery task you will be asked to choose one of two possible lotteries.

At the end of the experiment, if a lottery task is selected to determine your earnings, you will play the lottery that you chose in that task.

The payoff of each lottery will be determined by a roll of a six-sided dice, with each number on the dice corresponding to a payoff. The money values of the payoffs will not be known until the end of the experiment. At the time at which you are making your choice, all the payoffs will be described as fractions of X, where X is some amount of money in the range from £0.20 to £6.00 (in £0.20 intervals). At the end of the experiment, if a lottery task is selected to determine your earnings, one of these amounts of money will be selected at random, and this

will then be the value of X in the lottery you have chosen. A roll of the dice will then determine the payoff you receive, in addition to the £6 participation fee.

To assist in the explanation of the lottery tasks, here is an example. Suppose a participant in the experiment is asked to choose between the following lotteries:



Suppose the participant chooses Lottery A. If the dice rolls a 1, 2, 3, 4, 5 or 6, the payoff is equal to the value of (X/3). Suppose the participant chooses Lottery B. If the dice rolls a 1 or 2, the payoff is equal to the value of X. If the dice rolls a 3, 4, 5 or 6, the payoff is equal to the value of (X/6). Remember, the value of X is not determined until the end of the experiment.

Before we proceed, I ask you to answer the short quiz that will follow shortly on your screens, to ensure you understand the tasks in the experiment. Please attempt these and feel free to re-read the instructions as you do so. If you have any queries please raise your hand and an experimenter will come to assist you.

Now we are ready to start the experiment.

Part Two

Determining the Price

We will now select the price offered in the goods task and the value of X in the lottery tasks. We explained that X might be any amount of money in the range from £0.20 to £6.00 in £0.20 intervals. You may have noticed that in the goods task, the possible offers were amounts of money in the same range. We will now select one amount of money in this range. If the task that determines your earnings is the goods task, this amount will be the price offered. If the task that determines your earnings is a lottery task, this amount will be the value of X. To ensure a randomly drawn value we will ask one of you to draw out at random one envelope from this bag. There are 30 envelopes in this bag. Each envelope contains a card showing a different value in the range from £0.20-£6.00 in £0.20 increments.

The value drawn is ___. Please wait whilst this is uploaded to your screen. Once you have seen this, please press Continue.

Determining the Stage

We will now select the task which will be used to determine what you take away from the experiment. To ensure that the task is selected at random, we will ask one of you to draw out at random one envelope from this bag. There are 5 envelopes in this bag. Each envelope contains a card with a different number in the range from 1 to 5, representing the five tasks in the order in which you completed them.

The task drawn is ___. Please wait whilst this is uploaded to your screen. Once you have seen this, please press Continue.

You will now be shown on your screen which task corresponds to the number selected. Once you have seen this, please press Continue.

If your task is the **goods task** then ___ is now the selling price of the item. On your screen it should show you which item you chose, and your decision at that price. If you have indicated you were willing to sell the item at this price then, on leaving the experiment, you will receive ___ in addition to your £6 participation fee and will not keep the item. If you have indicated you were not willing to sell the item at this price then, on leaving the experiment, you will receive the item and your £6 participation fee.

If your task is a **lottery task** then ___ is now the value of X in the lotteries. On your screen it should show you which lottery you chose in this task and the values of the possible payoffs. An experimenter will visit you shortly with a dice to determine which payoff you receive, in addition to your £6 participation fee.

Please wait whilst the experimenter assists in determining final payments individually. A questionnaire will be given to you, and it is requested you complete this before taking your payment receipt and questionnaire with you to the payment desk upon leaving. Final payments and the giving of any goods you are owed will take place at the payment desk on your way out.

Thank you for taking part in this experiment.

Notes: These experimental instructions were for participants in sub-treatment 'Diluted Choice-Seller'. For buyer sub-treatments, relevant terminology was changed, and for Random and Chosen sub-treatments, relevant instructions and terminology was changed accordingly.

Appendix 1.8. Pre-experimental quiz (correct answer in **bold**)

Question 1: When is the value of the good price / lottery payoff determined?

a) Before any tasks are completed

b) Before each task is completed

c) After all tasks have been completed

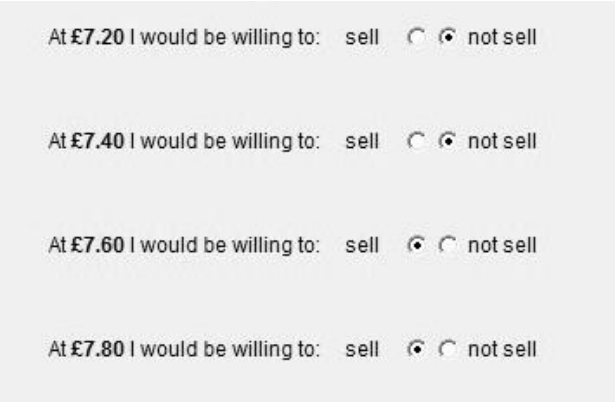
Question 2: Of the five tasks, how many will be played out for real?

a) 1

b) 2

c) 5

Goods Task



Question 3: Using the example given in the instructions suppose the value of £7.20 is drawn. What does the person do?

a) Keep the item and pay £7.20

b) Keep the item and receive nothing

c) Give up the item and receive £7.20

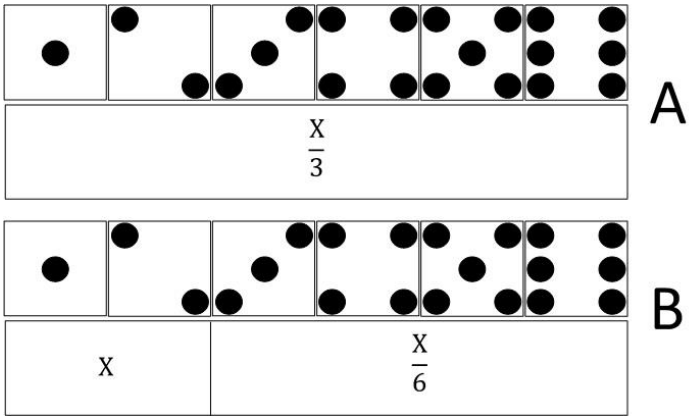
Question 4: Using the example given in the instructions suppose the value of £7.60 is drawn. What does the person do?

a) Give up the item and receive £7.60

b) Give up the item and receive £7.80

c) Keep the item and pay £7.60

Lotteries Task



Question 5: Using the example given in the instructions, suppose Lottery B is chosen and the value drawn is £9.00. If a dice rolls a 4, what is the payoff?

a) (X) = £9.00

b) (X/6)= (£9.00/6) = £1.50

c) Something else

Question 6: Using the example given in the instructions, suppose Lottery A is chosen and the value drawn is £9.00. If a dice rolls a 2, what is the payoff?

a) £9.00

b) £4.50

c) £3.00

Notes: The pre-experimental quiz was completed on the participant's computer screens. These questions were for sub-treatments 'Random-Seller' 'Diluted Choice-Seller'. For buyer sub-treatments, relevant terminology and images were changed. For sub-treatments 'Chosen', questions 2) 5) and 6) were removed and question 1) was adapted to acknowledge only one task.

Appendix 1.9. Post-experimental questionnaire

Thank you for your participation. Please answer this short questionnaire.

1) Age:

_____

2) Gender:

_____

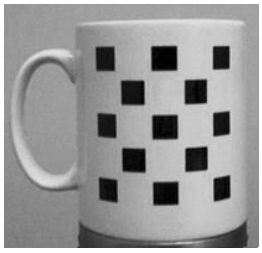
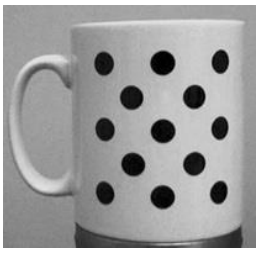
3) Course of Study:

_____

4) Please indicate which type of task was selected for you in this experiment. (*please tick*)

○ Goods Task ○ Lottery Task

5) Please look at the following pictures of the possible items you could have taken away with you in this experiment:


Please indicate below how desirable each of these mugs is for you. (*please tick*)
(where 0 = *completely undesirable* and 10 = *completely desirable*)

Squares:	○ 0	○ 1	○ 2	○ 3	○ 4	○ 5	○ 6	○ 7	○ 8	○ 9	○ 10
Circles:	○ 0	○ 1	○ 2	○ 3	○ 4	○ 5	○ 6	○ 7	○ 8	○ 9	○ 10
Triangles:	○ 0	○ 1	○ 2	○ 3	○ 4	○ 5	○ 6	○ 7	○ 8	○ 9	○ 10

6) Please indicate which of the following statements applies to you in this experiment. (*please tick*)

◯ I was willing to sell the mug I chose at all of the listed prices and **will not** take the mug away with me.

◯ I was not willing to sell the mug I chose at some of the listed prices and **will not** take the mug away with me.

◯ I was not willing to sell the mug I chose at some of the listed prices and **will** take the mug away with me.

7) Please only answer the question relevant to your answer in **6)**.

If you **will not** take away a mug with you, please think about the mug you chose, and indicate how much you think you would have enjoyed taking it away with you and owning? (*please tick*)
(where 0 = *not enjoy at all* and 10 = *enjoy greatly*)

◯	◯	◯	◯	◯	◯	◯	◯	◯	◯	◯
0	1	2	3	4	5	6	7	8	9	10

or

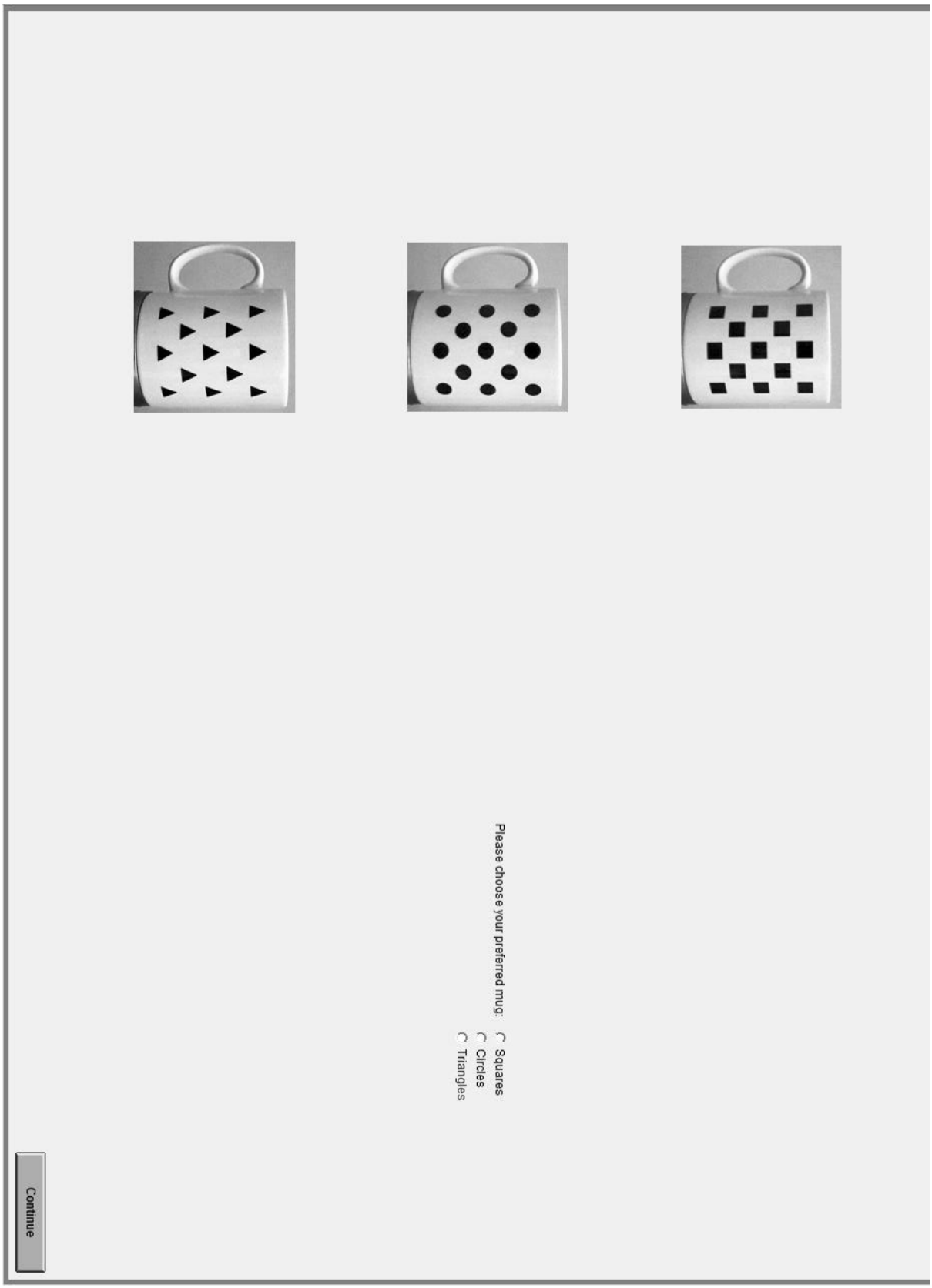
If you **will** take a mug away with you, please indicate how much you think you will enjoy taking it away with you and owning? (*please tick*)
(where 0 = *not enjoy at all* and 10 = *enjoy greatly*)

◯	◯	◯	◯	◯	◯	◯	◯	◯	◯	◯
0	1	2	3	4	5	6	7	8	9	10

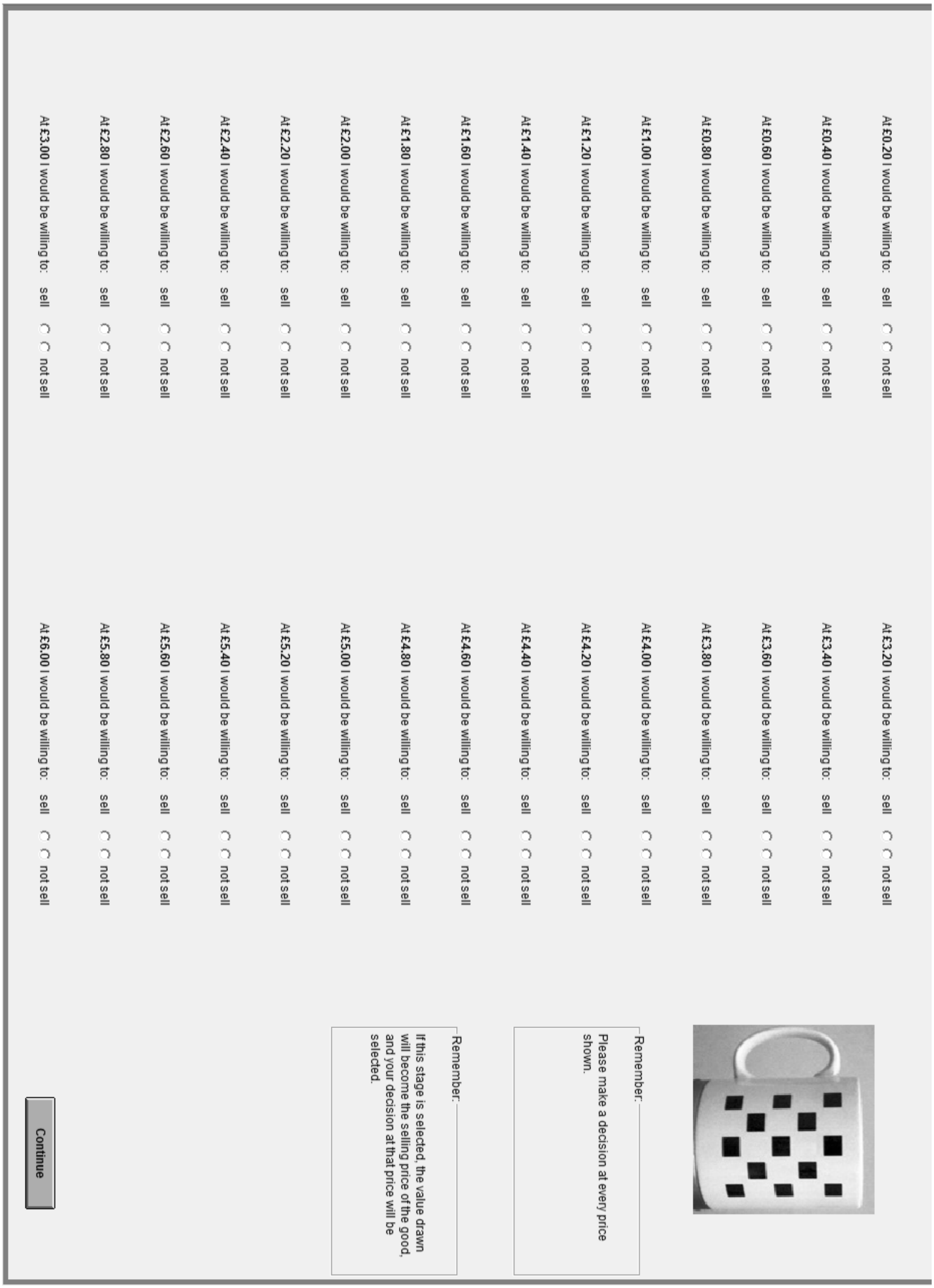
Notes: This post-experimental questionnaire was for participants in sub-treatment 'Diluted Choice-Seller' and 'Random-Seller'. For buyer sub-treatments, relevant terminology was changed, and for Chosen sub-treatments, question 4) was removed. The order of presentation of goods in question 5) was counterbalanced across participants.

Appendix 1.10. Experimental screenshots

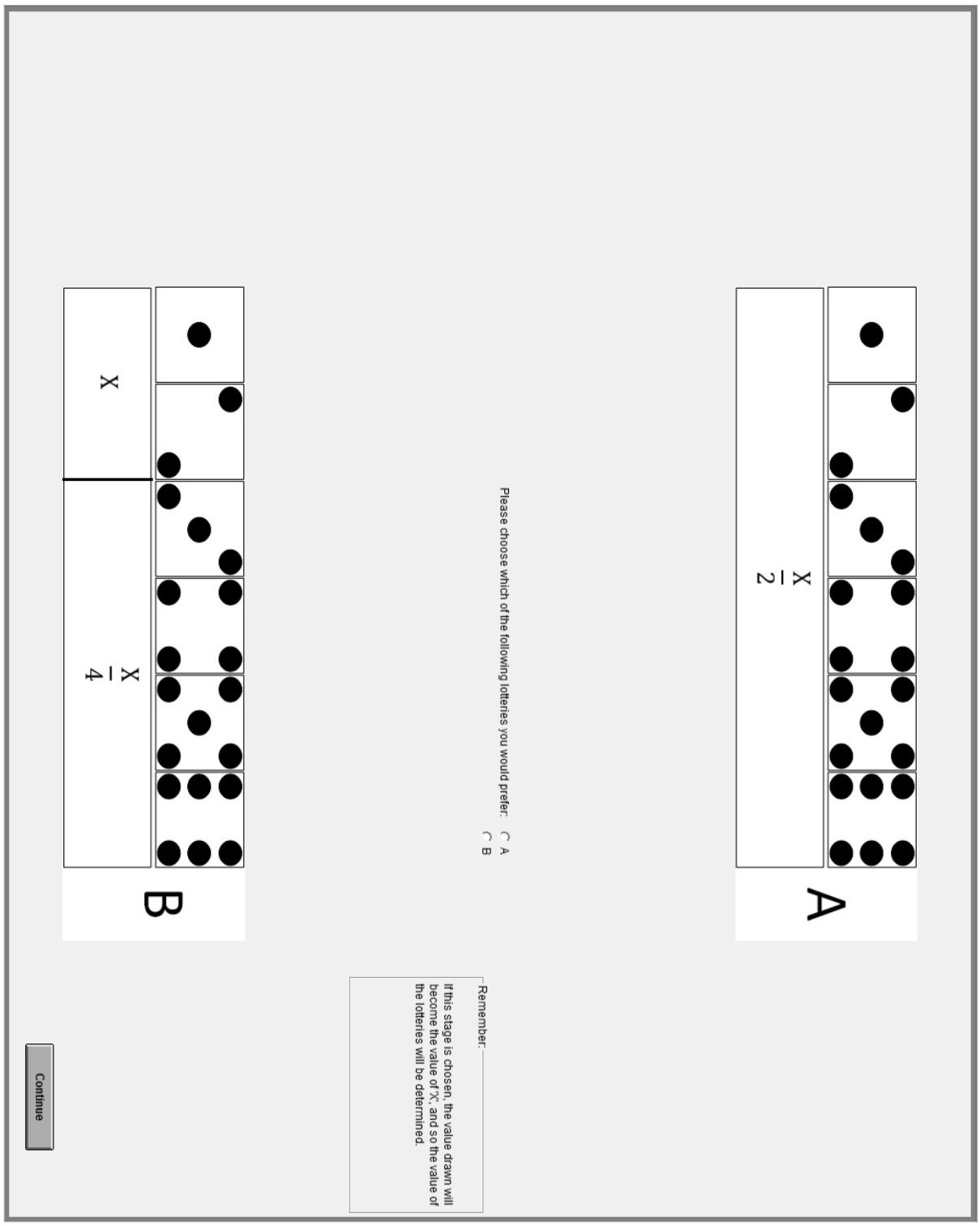
1.10.1. *Choice-goods* task choice page in Diluted Choice and Chosen treatments



1.10.2. BDM valuation elicitation page for goods task (squares mug)

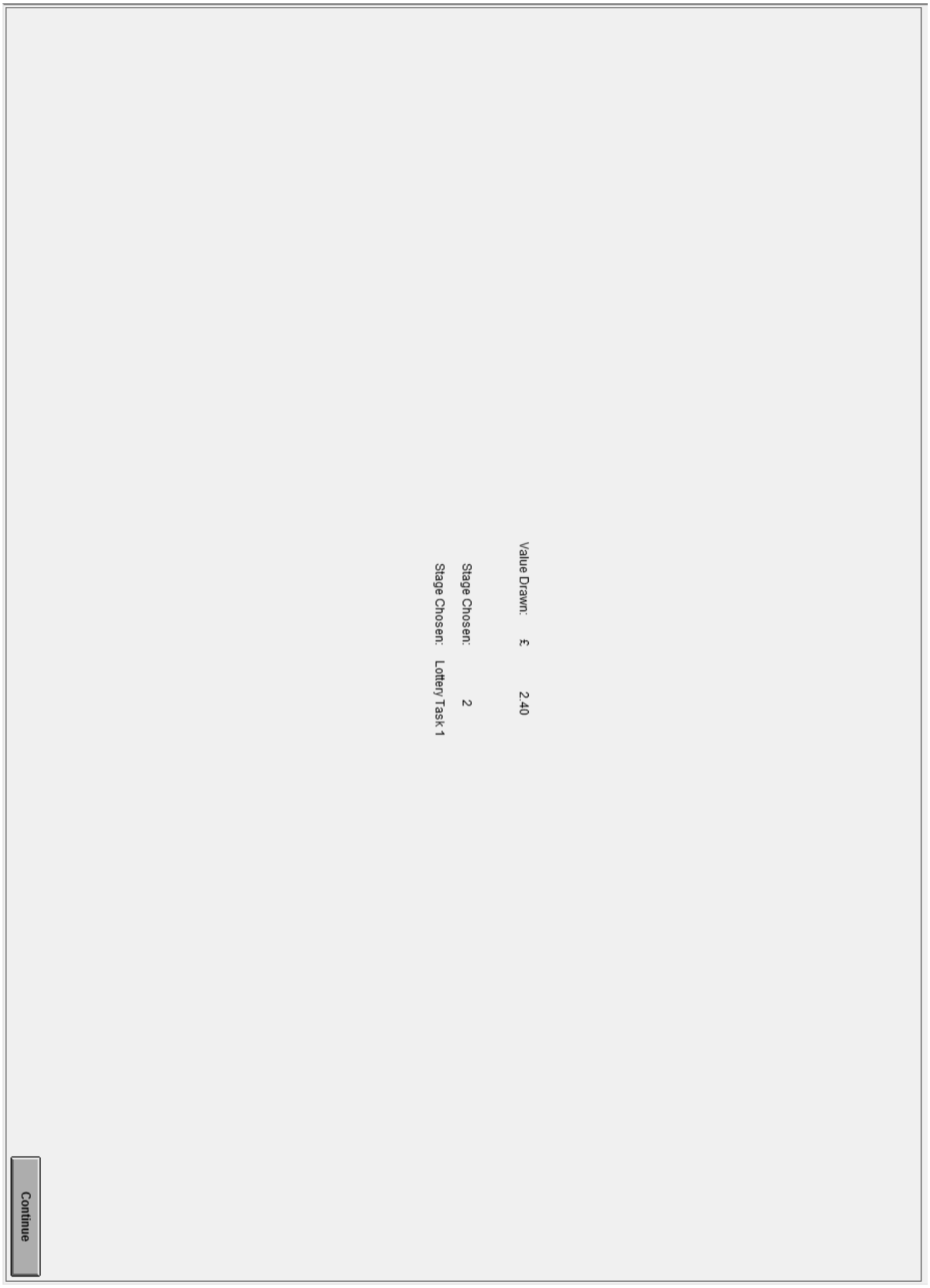


1.10.3. Lottery task decision page in Random and Diluted Choice treatments (lottery task (i))

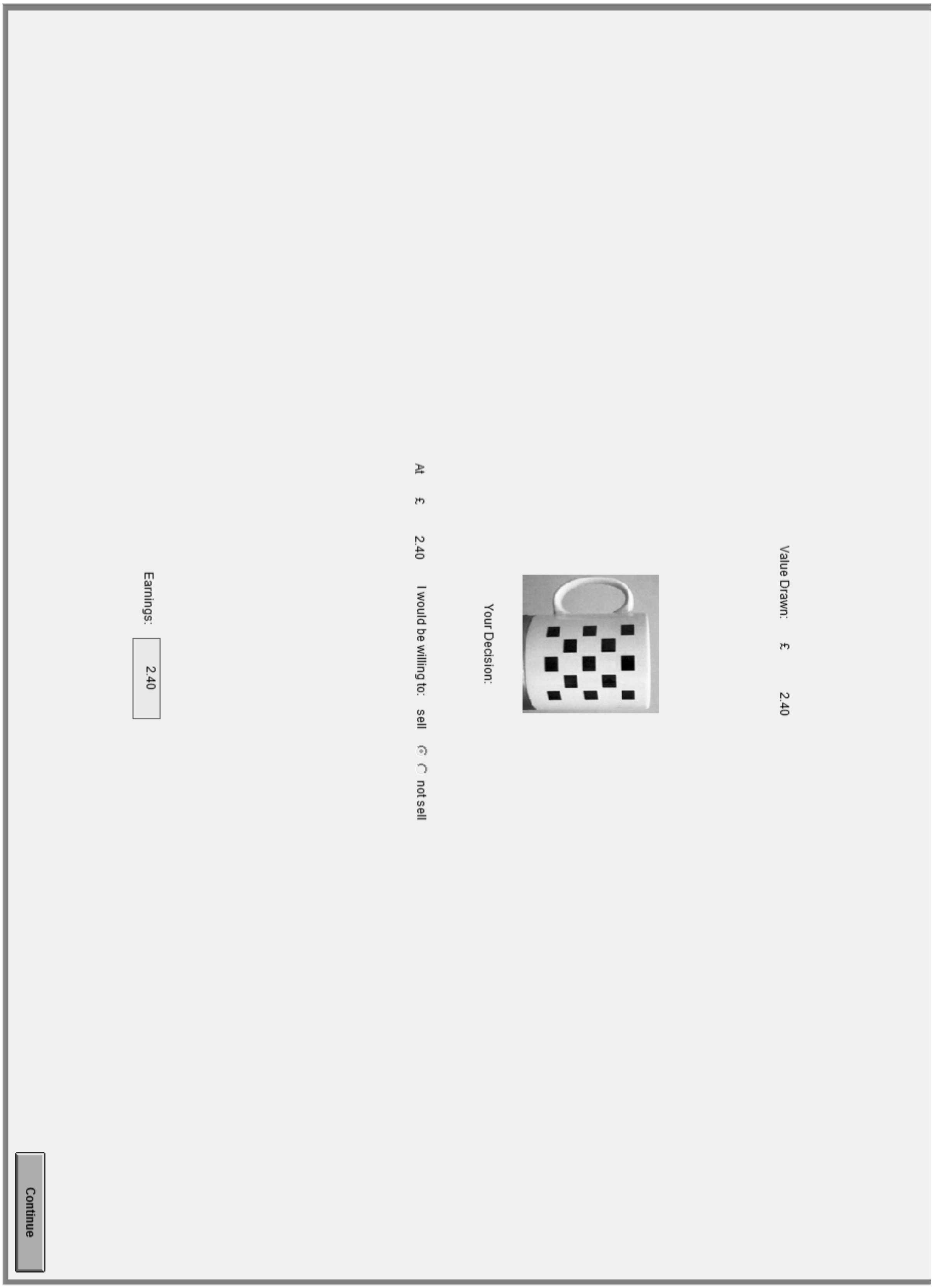


1.10.4. Summary page valuation and task outcome- goods task

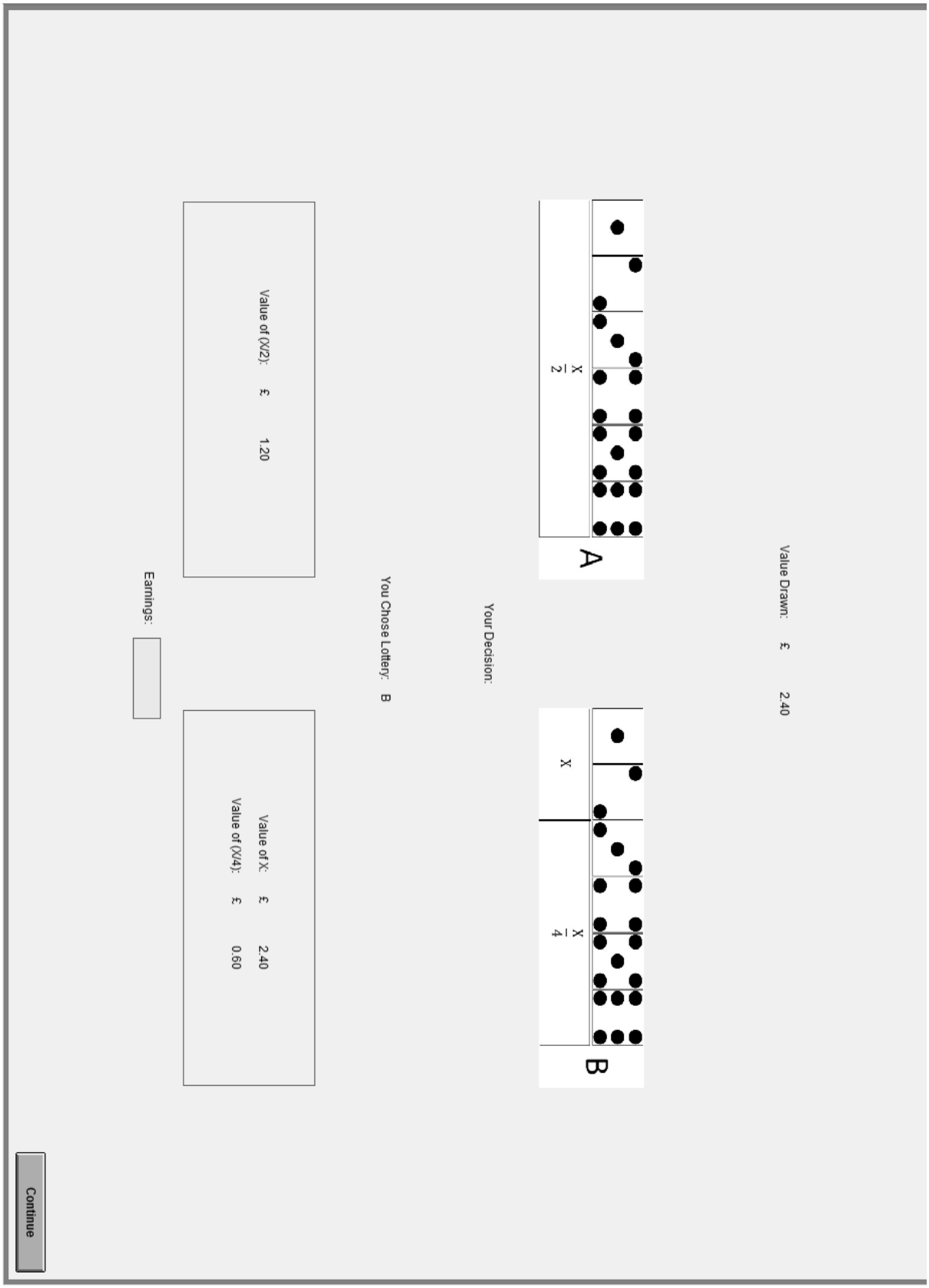
1.10.5. Summary page valuation and task outcome- lottery task (i)



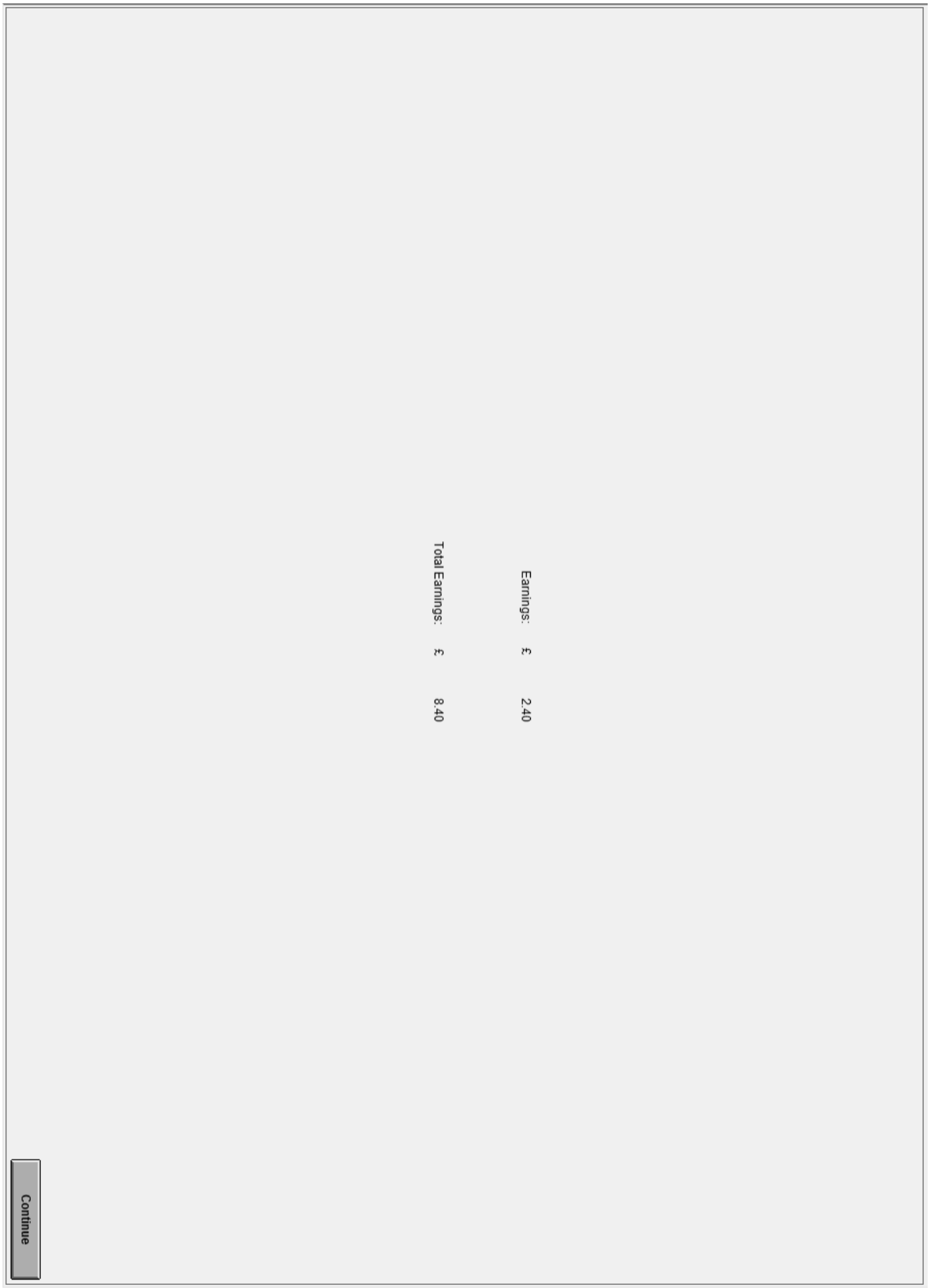
1.10.6. Experiment outcome summary page- goods task



1.10.7. Experiment outcome summary page- lottery task (i)



1.10.8. Final earnings page



Chapter 2

Attention and Novelty: An Experimental Investigation of Order Effects in Multiple Valuation Tasks

1. Introduction

In experimental economics and in stated preference studies, it is common to use designs that generate multiple responses from individuals. Multiple responses are an intrinsic part of within-subject experimental designs where the aim is to compare individuals' responses to different tasks. They are also essential for the methodology of discrete choice experiments, which elicit many binary decisions from each respondent as a means of estimating individuals' valuations of non-marketed goods or services. Multiple-response designs are also a useful way of reducing the costs of experimentation by generating multiple data points from each participant. However, there is a growing literature which suggests that multiple-response designs are vulnerable to *order effects*: the order in which tasks are presented can affect the outcomes of these decisions. These effects are particularly problematic when, as in stated preference studies, the aim is to estimate the distribution of preferences in some population. Order effects have been found in a number of stated preference methodologies, including choice experiments for non-marketed goods (Day and Pinto Prades, 2010; Day *et al*., 2012), contingent valuation surveys for hypothetical environmental goods (Bateman *et al*., 2004; Payne *et al*., 2000), and multiple-task valuation exercises for real goods (Ariely *et al*., 2003; Clark and Friesen, 2008). Order effects pose a less direct problem when within-subject designs are used to make qualitative comparisons between behaviour in different experimental treatments, since in these cases, counterbalancing of the order of tasks can be used as a control. Even so, the presence of order effects casts doubt on whether individuals possess stable preferences that can be elicited through surveys or experiments. Understanding why order effects occur is a fundamental problem for experimental economics.

The existing literature has identified a wide range of mechanisms by which, when an individual faces a sequence of valuation elicitation tasks, earlier tasks may induce the formation of reference points which then affect responses to later tasks. Such mechanisms include anchoring

effects (Ariely *et al*. 2003), embedding (or part-whole) effects (Kahneman and Knetsch, 1992; Bateman *et al*. 1997), and shaping effects (Loomes *et al*. 2003). We will refer to these effects as *indirect* order effects.

This paper looks at order effects from a new perspective. It investigates the possibility that there is a more direct causal mechanism which induces *attention-based* order effects. Such an effect would be one which causes a systematic trend in responses as task order progresses, simply as a result of the reduced attention that participants give to later tasks. Specifically, this paper tests for evidence of reduced attention generating a systematic decrease in valuations across multiple selling valuation tasks, for different real, consumable goods.

Previous experimental studies have found systematic declines in the valuations of sellers across repeated valuation tasks for the same good (Shogren *et al*., 1994; Loomes *et al*., 2010). Loomes *et al*. (2010, p. 385) suggest this may be a result of a reduced sense of loss aversion. Their hypothesis is that sellers are reluctant to give up a good in earlier tasks (generating relatively high selling valuations), but that repeating the selling task for the same good dulls the sense of loss aversion in later tasks and so reduces valuation, as the salience of not selling weakens.

Previous economic research has found that attention given to a good may be influenced by the general situational setting (Bordalo *et al*., 2013, 2016, Basu and Savani, 2017) or by the role a participant plays in that situation (Carmon and Ariely, 2000; Nayakankuppam and Mishra, 2005). In particular, an increase in attention toward the positive attributes of a good can result in an increase in valuations (Carmon and Ariely, 2000; Nayakankuppam and Mishra, 2005). This paper argues that such a decline in valuations in later tasks may be a consequence of the reduced attention that participants give to later tasks. From a psychological perspective, attention toward a novel stimulus diminishes over time (Berlyne, 1951). While being asked to value goods or services in earlier tasks is originally a novel exercise for participants, as task order progresses, such novelty value falls.

These findings suggest that, as novelty diminishes, so too does the attention given to that less novel stimulus, either as a result of familiarity with the act of completing the task itself, or as the specific details within the task become less novel, or a combination of the two. The role of diminishing attention is therefore an important one for experimental methodology, as it implies that participants may think less or more about tasks depending on the order in which they complete them.

In previous experiments that have found systematic declines in the valuations of sellers across repeated valuation tasks, the market or non-market goods (Shogren *et al*., 1994) or lotteries (Loomes *et al*., 2010) were identical in every task. In such a design it is not possible to determine whether the decline in valuations is a result of reduced attention to the act of completing tasks in general or to the particular good that is being valued, since both are repeated identically in each task. The experiment reported in the present paper attempts to disentangle these effects by manipulating the type of goods valued in each task in such a way that the effects of different kinds of novelty can be separated.

In the experiment, each of the six selling tasks faced by each participant involved a different good. The goods used in the experiment were chosen to allow the effects of different degrees of novelty to be investigated. The six goods can be partitioned into two subsets, one containing three distinct but similar patterned mugs, the other containing three distinct but similar luxury chocolate bars. In this design, changes in attention induced by the relative novelty of tasks and goods may generate order effects in three ways, each of which can be investigated in isolation. A task-specific *experimental novelty effect* may occur as a result of reduced attention to the act of completing later tasks, independent of the specific goods used. Two kinds of good-specific novelty effects are possible. A *within-subset novelty effect* may occur if participants give reduced attention to goods that are similar to goods that have featured in previous tasks. A *between-subset novelty effect* may occur if participants give more attention to the first type of good (mugs or chocolates) they confront than to the second.

The results of the experiment show a general tendency for a decline in valuations over the six selling tasks. This decline is especially strong immediately after the first task – evidence of an experimental novelty effect. There is also evidence that valuations are higher for the first type of good that a participant confronts than for the second – consistent with a between-subset novelty effect. These findings suggest that both the general completion of tasks and the specific goods used for valuation may be responsible for generating attention-based order effects.

The rest of the paper proceeds as follows. Section 2 discusses current theories of indirect order effects, and the role of attention as a possible cause of order effects. Section 3 describes the experimental design and section 4 outlines the key hypotheses to be investigated. Section 5 presents the results and section 6 provides a discussion of the implications of these findings. Section 7 concludes.

2. Explanations of Order Effects

2.1. Indirect Order Effects

The persistence of order effects across different stated preference methodologies that have been used to elicit valuations of marketed and non-marketed goods has resulted in a number of theories explaining this occurrence. (For a detailed review of these theories, see Day *et al*., 2012.)

Anchoring effects, whereby responses to earlier questions are used as cues to shape responses to later ones, have been observed in stated preference exercises as starting point bias (for example, Herriges and Shogren, 1996). The impact of anchoring has been shown with even arbitrary anchors, such as a spin of a roulette wheel (Tversky and Kahneman, 1974) or respondents' social security numbers (Ariely *et al*., 2003), influencing consequent and unrelated decision-making exercises. When comparing the differing effects of arbitrary anchoring on buying and selling prices, existing research presents conflicting accounts, finding that selling prices may be more (Sugden *et al*., 2013) or less (Simonson and Drolet, 2004) affected by anchoring than buyers, under differing conditions.

When respondents are asked to value bundles of goods and when the bundles involved in different questions differ in size or objective value, embedding effects (or part-whole bias) may occur. For example, if one question elicits a valuation of the set of goods {A, B, C} while another elicits a valuation of {A, B}, the valuation of {A, B} tends to be higher if it elicited before that of {A, B, C}. One possible explanation of embedding in hypothetical contingent valuation surveys is that valuation is a proxy of purchase of moral satisfaction (Kahneman and Knetsch, 1992), suggesting that the moral satisfaction of contributing doesn't depend on what is actually achieved. However, research on nested bundles of objectively differing private goods provides evidence of embedding effects, even with incentivised valuation elicitation devices (Bateman *et al*., 1997; Ariely *et al*., 2003; Clark and Friesen, 2008), suggesting that the purchase of moral satisfaction is not a complete explanation of embedding effects.

Shaping effects may induce order effects in certain kinds of experimental market. When valuations of a given good are elicited in repeated markets, the valuations that are implicit in participants' bids to buy or sell tend to move towards prices set in previous markets (Loomes *et al*, 2003), possibly because participants are unsure about the true value of the good to them. In a repeated Vickrey second price auction (such as in Shogren *et al*., 2001), the selling price is equal to the second lowest bid, and so shaping effects can induce downward trends in sellers'

valuations. Knetsch *et al*. (2001) find evidence that manipulating the design of a Vickrey second price auction may generate different shaping effects (potentially eliminating any decay in valuations), and so reveal concerns about the demand-revealing properties of these auction designs.

In some instances, order effects may be generated by strategic bidding which diminishes over time (Shogren *et al*., 1994, p.266). In surveys which elicit valuations for non-marketed commodities, repeated questions involving different levels or costs of public good provision may reduce the credibility of any given level or cost being actually implemented (Carson and Groves, 2007, p.185). Fatigue effects have also been shown to present potential issues for stated preference choice experiments (e.g. Savage and Waldman, 2008), creating tendencies for favouring the status quo or increased randomness in responses (Day *et al*., 2012, p.75).

There is evidence from psychological experiments that when participants carry out a sequence of tasks, each of which requires them to rate the desirability of pairs of trivially differing images, the first viewed images tend to be judged more favourably than later ones (Pandelaere *et al*., 2010). Possible explanations for this finding include the strength of memory retrieval of first goods (Bruce and Papay, 1970), the perceived legitimacy of first goods as the 'original', or the positive mental imprint first viewed goods leave relative to consequent goods (Pandelaere *et al*., 2010, pp.447-448).

2.2. Attention-Based Order Effects

In studies of the effects of experience on the well-known disparity between willingness-to-pay of buyers and willingness-to-accept of sellers, it is common to find a systematic decline in the valuations of sellers, but not in those of buyers (e.g. Shogren *et al*., 1994; Loomes *et al*., 2010). As noted in section 1, the decline in sellers' valuations might be the result of reductions in loss aversion. Loss aversion with respect to the good would be irrelevant for buyers[4].

Attention-based order effects offer an alternative explanation of why such a decline in valuations is found for sellers but not buyers. Theories of attentional bias (Carmon and Ariely, 2000) suggest that individuals tend to give more attention to what they stand to forgo in a transaction than to what they stand to gain. Since sellers stand to forgo a good, it is sellers who focus more on the attributes of goods. If the more an individual focuses their attention on the

[4]If buyers' valuations were affected by loss aversion with respect to money, declining loss aversion would induce an increase in those valuations. Whether loss aversion for money has quantitatively significant effects on decisions involving low-value goods is an unresolved question (Bateman *et al*., 2005).

(positive) attributes of a good, the more desirable the good is perceived to be, then this may lead to higher valuations by sellers than by buyers. If attention falls as task order progresses, then it follows that later goods are perceived as less desirable, and so their valuations decline.

Research from psychology suggests that attention is also positively related to the novelty value of a stimulus (Berlyne, 1951; Betsch *et al*. 1998). The potential significance of novelty for economics was noticed by Scitovsky (1992) but, to date, there has been relatively little investigation of the role of novelty in economic decision making. There is some evidence that novelty increases the appeal of certain kinds of goods (Tom, 2004; Tom *et al*., 2007) and is a cause of increased willingness-to-pay for new variants of recognisable foods (Jaeger and Harker, 2005; Stevens and Winter-Nelson, 2008; Meenakshi *et al*., 2012), suggesting it can be responsible for tangible effects on the way that goods are perceived. Research on consumer product trials find that trial periods, where consumers are able to experience a good prior to valuation, may result in reduced valuations for that good (De Groot *et al*., 2009). Such an effect may be related to ideas of novelty and attention, that what was once novel is no longer, and so upon valuation, such novel attributes are no longer given such attention and so valuations are reduced.

There are at least three mechanisms by which variations in novelty, and corresponding variations in attention, might generate order effects in multiple-task surveys and experiments which elicit selling valuations.

The first mechanism is an experimental novelty effect – the reduced novelty of the specific experimental methodology as an experiment or survey progresses. With typical lab experiments using student participants, or contingent valuation experiments using telephone or face-to-face interviews with members of the public, the average participant is unlikely to have taken part in many multiple-task studies previously. Thus, earlier tasks are undoubtedly more novel to participants, but this novelty may quickly dissipate once participants assimilate to the nature of the tasks. The greater novelty of earlier tasks might induce more attention and thereby (because of attentional bias towards what is forgone) higher selling valuations. This mechanism would tend to induce a general decline in selling valuations, independent of changes in the specifics of the task.

In an experiment eliciting multiple valuations of hypothetical environmental goods, a *primacy effect* has been found: the reduction in valuation between the first and second valuation task is greater than that between other adjacent tasks (Payne *et al*., 2000). That experiment elicited

willingness-to-pay for public contributions to a variety of environmental goods, but if experimental novelty effects do drive declines in valuations across tasks, it may be that primacy effects are even stronger in selling tasks.

The second mechanism is a within-subset novelty effect. If the goods valued in different tasks are similar to one another, then the novelty of the good that a participant is valuing will fall as the number of similar goods previously valued increases. This mechanism would tend to induce a decline in selling valuations within any given subset of similar goods.

The third mechanism is a between-subset novelty effect. If the goods in an experiment are naturally thought of as belonging to distinct types that differ substantially from one another, then the introduction of a new type of good may be perceived as a shift from one type of task (for example, 'valuing chocolates') to another type ('valuing mugs'). Since the second type of good to be introduced would no longer be associated with the initial novelty of the experiment, this mechanism would tend to induce lower selling valuations for later types of good than for earlier types.

In contingent valuation studies, one attempt to rectify issues of order is to employ a method of advanced disclosure whereby survey participants are fully informed as to the type of goods to be valued before any valuations are elicited. This procedure has been found to dissipate some order effects (Bateman *et al*., 2004; Day *et al*., 2012). If good-specific (within- or between-subset) novelty effects were driving a downward trend in valuations, then it is possible that advanced disclosure would reduce this effect, as the initial information about the goods to be valued might remove any good-specific novelty that would otherwise be revealed at the start of each new valuation task.

3. Experimental Design

The experiment reported in this paper was designed to test for attention-based order effects when valuations are elicited in selling tasks. An important feature of the design is its ability to isolate the different attention-based mechanisms described in section 2.2, while controlling for the effects of the other mechanisms discussed in section 2.1.

The experiment used a 4x2 between-subject design. In each of the eight sub-treatments, participants completed eleven tasks. These were made up of six *goods tasks*, where participants were presented with a good and a valuation was elicited for that good, and five cognitive distraction *lottery tasks*, which occurred between each pair of goods tasks.

3.1. Goods Tasks

The goods tasks involved the valuation of six different goods separated into two distinct subsets, three mugs and three chocolate bars. The use of two different subsets of goods allowed within-subset and between-subset novelty effects to be disentangled.

The three mugs were all white, ceramic mugs, and differed only in the type of pattern of shapes printed on the mug ('Squares', 'Circles' or 'Triangles'). The three chocolate products were all the same luxury brand chocolate bar, differing only by the type of biscuit topping on each chocolate ('Rocky Road', 'Milk and Cookies' or 'Mississippi Mud Pie').[5] Thus, within each subset, the three goods differed only in subjective value. That is, they differed only on a dimension that was clearly a matter of personal taste, with no connotation of any difference in objective quality or market value.

In real world individual consumption decision making it is common for goods compared for purchase to be identical in terms of price and quality, but to differ in terms of some dimension of personal taste, such as colour for items of clothing. This feature of the experiment was used to control for embedding effects that might occur if one good was perceived as objectively larger than or better than another, and to minimize the possibility that participants would use beliefs about market prices as cues for relative valuations within each subset. To the authors' knowledge, there has been no research looking exclusively at order effects in valuations of private goods that differ only in terms of their subjective value.

The goods used in the experiment were determined in light of a pilot survey (see Appendix 2.2), with the objectives that: i) within each subset, there should be considerable cross-participant variation in preference rankings of the three goods; ii) for most individual participants, differences in valuations between goods in the same subset should be relatively small, iii) the types of goods used in the two subsets should be substantially different; iv) average valuations of goods in the two subsets should be similar to one another.

Once the experiment began, the procedure was identical across all treatments. Participants were told in each goods task that they would be given a good which they then conditionally owned, but that they would be able to sell back to the experimenter at a price that would be determined at the end of the experiment. A good was distributed to participants, and they were encouraged

[5]The three patterns printed on the mugs were custom-designed for the experiment. The three chocolate bars were from a luxury British confectioners (Hotel Chocolat). Detailed descriptions of the goods, which were shown to the participants when receiving the goods, can be found in Appendix 2.1.

to look at (and pick up) the good to assess it, before making a series of binary decisions designed to elicit the minimum price at which participants were willing to sell that good.

Eliciting selling, rather than buying, valuations was intentional. The experiment aimed to manipulate dimensions of the attributes of the goods and their relative novelty value to affect attention levels. As noted in section 2.2, previous research suggests that sellers give more attention than buyers to the attributes of goods (Carmon and Ariely, 2000, Nayakankuppam and Mishra, 2005). Thus, attention- and novelty-based effects are more likely to be found in selling tasks.

It has previously been acknowledged that the growth of online second-hand markets has increased the frequency with which consumers take the role of sellers, as opposed to the more conventional role of buyers (Simonson and Drolet, 2004, pp.681-682), suggesting that such a position would not be an unfamiliar one to participants.

A common feature of experiments that exhibit shaping effects, or evidence of strategic bidding, is the occurrence of feedback about the outcome of each task (for example, about whether the participant has sold the good and if so, at what price) before the next task begins. In this experiment, there was no such feedback. A random lottery incentive system was used: one of the numbers 1 to 11 was randomly selected, at the end of the experiment. Each participant's earnings were determined by their response to the task that had that number for them. This meant that the selected task would either be a goods task or a lottery task for all participants, but the counterbalancing of specific tasks within each task type meant that the specific good or lottery selected would differ across participants.

If the selected task was a goods task, the price at which the good could be sold would only then be revealed. Thus, there was no possibility for participants' responses to later tasks to be influenced by prices revealed in earlier tasks. Because valuations for each good were elicited immediately after the good was shown to participants, order effects could not be caused by differences in the strength of memory retrieval over time.

The downward trends in selling valuations found by Shogren *et al*. (1994) and Loomes *et al*. (2010) may be the result of a reduced feeling of ownership (and so reduced loss aversion) for later goods. In these studies, a market valuation was determined after each round. Participants knew the outcome of their decision in each round, and so may have 'sold' goods in multiple rounds. If participants were frequently 'selling' their goods after completing goods tasks, this might reduce loss aversion in later tasks.

This effect cannot occur in the present design because of the absence of feedback. Since no selling decision is realised until the end of the experiment, a sense of conditional ownership is maintained for every good. Whilst it is possible that there remains a diminished loss aversion that stems from participants simply completing multiple goods tasks and thinking about selling, such an effect can be classified as an experimental novelty effect – a tendency for participants to give less attention to losses as the experiment progresses.

As noted in section 2.1, order effects in contingent valuation studies have sometimes been attributed to the lower credibility of scenarios that appear later in an experiment or survey. Effects of this kind are unlikely in the present design, in which participants report valuations for real, private goods and in which the incentive system is clearly defined.

If participants fail to behave in accordance with expected utility theory, responses elicited using a random lottery incentive system may differ from those elicited in single-task designs. Existing evidence suggests that this does not induce systematic bias for simple choices between lotteries (Cubitt *et al*., 1998b), but that selling valuations tend to be lower in random-lottery designs than in single-task designs (Loewenstein and Adler, 1995). However, this potential bias is not a problem for the present study, which is concerned only with the relative valuations of goods across tasks.

In order to maximise the likelihood of honest valuation decisions of participants, a Becker-DeGroot-Marschak (henceforth BDM) mechanism (1964) was used. In each goods task, participants were shown a set of possible prices, ranging from £0.20 to £6.00, in £0.20 increments, and were asked whether, at each of these prices, they wished to sell the good back to the experimenter at that price, or not to sell. Once all participants had completed the valuation decisions for that task, all goods were collected and the experiment continued. This was kept the same for all goods tasks.

The £6.00 upper limit was chosen in the expectation that most participants would value the goods less than this. (The market values of the goods, which were not revealed to participants, were considerably less than £6.00.[6]) This design choice reduced the possibility that participants' valuations might be framed by the upper and lower bounds of the BDM mechanism (Bohm *et al*., 1997).

[6]The retail values of the goods were: mugs- £2.25 and chocolates- £3.15, although pre-experimental surveys suggested that participants' willingness-to-pay for the two were both approximately £1.50.

A participant who acted on consistent preferences between money and goods would report at most one *preference switch*, from 'not sell' at relatively low prices to 'sell' at relatively high prices. (There would be no preference switch for a participant who would 'not sell' at every price or would 'sell' at every price). If a participant's decisions implied more than one preference switch, these switches were highlighted on the participant's screen, together with the relevant material from the experimental instructions which reminded them of the workings of the valuation mechanism, and gave them the opportunity to revise their decision, if they wished to. Participants were free to resubmit decisions with two or more preference switches, but this revision stage allowed errors to be corrected.

If a participant reported no more than one preference switch for a good, the location of that switch (or its absence) locates one of thirty-one points on an ordinal valuation scale. For a participant with exactly one preference switch, their *valuation* of that good will be defined as the mean of the highest price at which they would 'not sell' and the lowest price at which they would 'sell' (or, equivalently, the lowest price at which they would 'sell', *minus* £0.10). Participants who would 'sell' at every price will be defined to have a valuation of £0.10, and those who would 'not sell' at every price to have a valuation of £6.10.

In any statistical analysis of results, these extreme parameters must be accounted for. First, non-parametric tests (such as Wilcoxon rank-sum tests) only require a ranking of values, and so this actual upper valuation does not affect statistical outcomes. In any regression analysis, censoring at lower and upper limits (using a Tobit model) accounts for any valuation above this upper limit, so this actual valuation does not affect statistical outcomes.

3.2. Cognitive Distraction Tasks

In the five lottery tasks, participants were asked to choose which of two monetary lotteries they would prefer to play, with payoffs determined by the roll of a die. Lottery task (i) is shown as an example in Figure 2.1, in the form in which it was presented on participants' computer screens. During the experiment, the possible outcomes of the lotteries were shown as fractions of X, with participants aware that X could take one of thirty values, from £0.20 to £6.00 in £0.20 increments. Notice that the set of possible values of X is the same as the set of possible prices in the goods tasks. This X-value lottery design ensures that the two types of tasks involve the same reference points, so that comparisons of goods valuations are not distorted by values used in the lottery tasks.

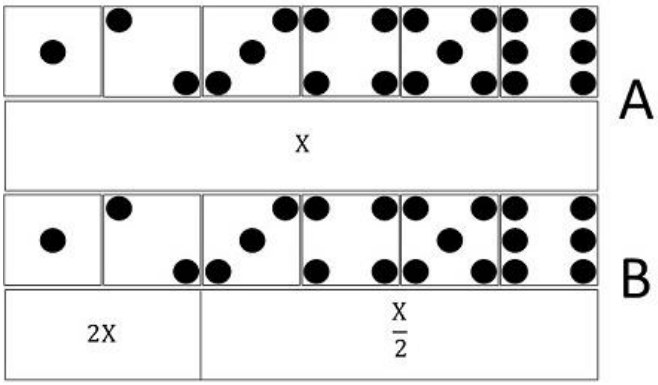


Figure 2.1. An example of a lottery task (lottery (i))

Whilst the primary function of the lottery tasks was to act as a cognitive distraction between goods tasks, the responses of participants to the lottery tasks revealed risk attitudes and possible violations of expected utility theory. The order in which participants completed the lottery tasks was counterbalanced across participants, and the position of the lotteries within each task was counterbalanced also. All lottery task descriptions can be found in Appendix 2.3.

3.3. Payments to Participants

Once all tasks were completed, one of the eleven task numbers was then drawn at random to determine the task for which each participant would play for real. This was achieved by one participant selecting one of eleven sealed envelopes. The task number inside the selected envelope was determined as the task number for all participants. After this, one of the thirty values (from £0.20 to £6.00) was drawn at random, and this was determined to be either the price of the good or the value of *X* in the lotteries. This value was selected in the same manner as the task number.

If the task drawn for a participant was a goods task, the participant's decision about whether to sell the good at the drawn price was made binding. If the participant *was not* willing to sell the good at the drawn price, they kept the good and took it away with them (in addition to a £6 participation fee). If the participant *was* willing to sell the good at that price, they did not take the good away but instead received the drawn price in addition to their £6 participation fee. The fact that truthful valuations were the optimal response in the valuation mechanism was made clear to participants in both the instructions and the pre-experimental quizzes.

If the task drawn for a participant was a lottery task, the participant was then shown the lottery they had chosen in that task with the value of *X* equal to the drawn value. An experimenter visited them with a die to determine their final payoff, in addition to the £6 participation fee.

3.4. Treatments

The experiment had four treatments (A–D), each of which was subdivided into two sub-treatments (e.g. A1 and A2). Treatments A, B and C, which will be called the *main treatments*, differ only in the order in which goods in the goods tasks were presented. Treatment D uses the same order of goods as Treatment A, but adds a control designed to remove good-specific novelty from the experimental design. This control uses a method of *advanced disclosure* that has been used in some contingent valuation surveys, in which respondents are fully informed about the types of goods to be valued before any valuations are elicited (Bateman *et al*., 2004). In Treatment D, before facing any of the tasks, participants were shown on their screens images of the six goods to be valued, and were informed these would be the goods for valuation in the six goods tasks. The order in which these tasks would be presented was not revealed at this stage. (In fact the order was counterbalanced such that, for half the participants, the task order reproduced the top-to-bottom-left-to-right order in which the goods were initially presented; for the other half, this task order was reversed.)

In all treatments, the same three different chocolates and three different mugs were used as the goods for valuation. The order in which the goods were presented in each sub-treatment is shown in Table 2.1. M_1, M_2 and M_3 respectively denote the first, second and third mug task faced by a participant; C_1, C_2 and C_3 denote the first, second and third chocolate tasks. Within each treatment, which of the three mugs (Squares, Circles or Triangles) appeared in which of the positions M_1, M_2 and M_3 was counterbalanced, and similarly for the three chocolates. Thus, for example, Table 2.1 shows that in Sub-Treatment A1, participants faced three tasks involving mugs followed by three tasks involving chocolate. Notice that, within each treatment, the two sub-treatments counterbalance mugs and chocolates. Thus, for example, Treatment A can be interpreted as a treatment in which participants face three tasks involving one type of good followed by three tasks involving the other; whether mugs are faced first or second is counterbalanced.

The main treatments share the feature that there is either one transition between good types (Treatment A) or two transitions (Treatments B and C). All task orders that are compatible with this constraint are included in the design. By imposing this constraint, rather than counterbalancing all possible task orders, the design increases the power of tests of within-subset novelty. (For example, it ensures that for two-thirds of all main-treatment participants, the three mug tasks are faced in succession.)

Considering only the main treatments, each task can be described by four characteristics: *order* (*i*), *within-subset novelty* (*j*), *between-subset novelty* (*k*), and *good type* (*m*). Order takes the form $i \in \{1, \ldots, 6\}$, with $i=1, \ldots, 6$ referring to the first, …, sixth goods task faced. Within-subset novelty takes the form $j \in \{1, 2, 3\}$, with $j = 1, 2, 3$ referring to the whether the task is the first, second and third involving a good of the relevant type. Between-subset novelty takes the form $k \in \{1, 2\}$, with $k=1$ referring to a task in which the good is of the same type as the good faced in the first task, and $k=2$ to a case in which it is not. Good type takes the form $m \in \{0, 1\}$, where $m=0$ refers to a task involving a chocolate and $m=1$ refers to a task involving a mug.

Thus, for example, the second task in Treatment A1 is described by $i=2$, $j=2$, $k=1$, $m=1$; the fifth task in Treatment C2 is described by $i=5$, $j=2$, $k=1$, $m=0$. As a matter of definition, these four characteristics are not completely independent of one another (for example, $i=1$ necessarily implies $j=1$ and $k=1$). However, the design ensures that the impact on valuations of variation in each characteristic can be captured in isolation, holding other characteristics constant.

		Task Order					
		1	2	3	4	5	6
Treatment	**A1**	M_1	M_2	M_3	C_1	C_2	C_3
	A2	C_1	C_2	C_3	M_1	M_2	M_3
	B1	M_1	M_2	C_1	C_2	C_3	M_3
	B2	C_1	C_2	M_1	M_2	M_3	C_3
	C1	M_1	C_1	C_2	C_3	M_2	M_3
	C2	C_1	M_1	M_2	M_3	C_2	C_3
	D1	M_1	M_2	M_3	C_1	C_2	C_3
	D2	C_1	C_2	C_3	M_1	M_2	M_3

Table 2.1. Order of goods tasks by sub-treatments

3.5. Implementation

The experiment took place in early 2016 at University of East Anglia's Centre for Behavioural and Experimental Social Science (CBESS). The 243 participants were recruited through the centre's online recruitment system and had no experience of experiments of this type. The experiment was conducted using the experimental software package z-Tree (Zurich Toolbox for Ready-made Economic Experiments) (Fischbacher, 2007).

The experimental instructions were read aloud and participants had the opportunity to ask any questions. Before facing the goods and lottery tasks, participants were asked to answer a set of

multiple-choice questions to test their understanding of the experimental procedure, including questions regarding the BDM valuation mechanism in the goods tasks and possible lottery outcomes in the lottery tasks. If a participant answered incorrectly on the first attempt, they were asked to review the relevant instructions and attempt the question again. 83.5% of questions were answered correctly at the first attempt, and 96.0% were answered correctly by the second attempt, suggesting participants in general understood the mechanisms of the experimental design.

4. Hypotheses

The hypotheses that refer to Treatments A, B and C can be formulated in terms of the distribution, within the population of potential participants, of valuations $V_{i,j,k,m}$, where $V_{i,j,k,m}$ denotes a valuation that is conditional on order i, within-subset novelty j, between-subset novelty k, and good type m. (Within either good type, the specific good to which the valuation refers is to be interpreted as a random draw from the relevant set of three goods.) The assumption that participants act on neoclassical preferences implies the null hypothesis that the distribution of $V_{i,j,k,m}$ is independent of the values of i, j and k. As explained in sections 2.2 and 3.4, the experiment was designed to test the following hypotheses about attention-based order effects:

Alternative Hypothesis H1: Experimental novelty effects

(a) For all feasible j,k,m: $V_{1,j,k,m} > V_{2,j,k,m} > V_{3,j,k,m} > V_{4,j,k,m} > V_{5,j,k,m} > V_{6,j,k,m}$

(b) For all feasible j,k,m, and for all $s \in \{2, 3, 4, 5\}$: $(V_{1,j,k,m} - V_{2,j,k,m}) > (V_{s,j,k,m} - V_{(s+1),j,k,m})$

Part (a) of this hypothesis predicts that, holding all other factors constant, the later in the series of tasks a good is valued, the lower its valuation. Part (b) predicts a *primacy effect*: holding all other factors constant, experimental novelty effects are stronger between the first and second goods task than between other pairs of adjacent goods tasks. For example, consider Sub-Treatments A2 and B2. In B2, the first mug task, M_1, is the third goods task, whereas in A2, the first mug task, M_1, is the fourth goods task. Hypothesis H1(a) predicts that M_1 elicits a higher valuation in Sub-Treatment B2 than in A2.

Alternative Hypothesis H2: Within-subset novelty effects

For all feasible i,k,m: $V_{i,1,k,m} > V_{i,2,k,m} > V_{i,3,k,m}$

This hypothesis predicts that, holding all other factors constant, the less novel a good within a subset, i.e. the more goods of that subset that have already been valued, the lower its valuation. For example, consider Sub-Treatments A2 and C2. In A2, the fourth goods task is M_1, i.e. the first task to involve a mug. In C2, the fourth goods task is M_3, i.e. the third task to involve a mug. Hypothesis H2 predicts that the valuation elicited in the fourth goods task of A2 is greater than that elicited in the fourth goods task of C2.

Alternative Hypothesis H3: Between-subset novelty effects

For all feasible i,j,m: $V_{i,j,1,m} > V_{i,j,2,m}$

This hypothesis predicts that, holding all other factors constant, the first good type presented has a higher valuation than the second good type. For example, consider Sub-Treatments A2 and C1. In both sub-treatments, the fifth goods task is M_2, i.e. the second task to involve a mug. In C1, the first good type presented was mugs; in A2, it was chocolates. Hypothesis H3 predicts that the valuation elicited in the fifth goods task of C1 is greater than that elicited in A2.

Alternative Hypothesis H4: Advanced disclosure

Whilst this is not the primary objective of this paper, it is also of interest to consider how far advanced disclosure reduces attention-based order effects. Hypothesis H4 predicts that within-subset and between-subset novelty effects, as predicted by Hypotheses H2 and H3, are less strong in Treatment D than in the main treatments.

5. Results

A total of 243 participants took part in the experiment, but 11 participants reported inconsistent valuation decisions such that unambiguous intended valuations could not be inferred, and so were dropped from the analysis[7]. This left 232 participants in total with usable data.

[7]Recall that participants were given an opportunity to rectify apparently inconsistent decisions in each goods task. If participants still revealed inconsistency after this, efforts were made to allow for human error and still record an intended valuation. This was achieved through the following rule: if consistency could be achieved through the rectification of *one* valuation decision, and it was obvious which valuation decision was erroneous, then this one valuation decision was rectified and valuation was inferred from these consistent valuation decisions. The valuations of two participants (each with inconsistent valuation decisions for two of six goods) were amended using this rule. The 11 participants dropped from the analysis either had at least one goods task which required more than one valuation decision to become consistent, or it was not obvious which valuation decision was erroneous.

5.1. Summary Statistics

It is first of interest to observe valuations across order by treatment. Table 2.2 reports the mean valuation in each of the four treatments, pooling across the two component sub-treatments. Figure 2.2 presents the same data graphically.

Valuation (£)		*n*	Task Order 1	2	3	4	5	6
Treatment	**A**	58	2.53	2.32	2.37	2.36	2.40	2.35
	B	60	2.60	2.56	1.96	1.93	2.06	2.38
	C	60	2.29	1.87	1.84	1.74	1.87	1.91
	Pooled A-C	178	2.47	2.25	2.05	2.01	2.11	2.21
	D	54	2.41	2.32	2.20	2.09	2.00	1.98

Table 2.2. Mean valuations by treatment and task order

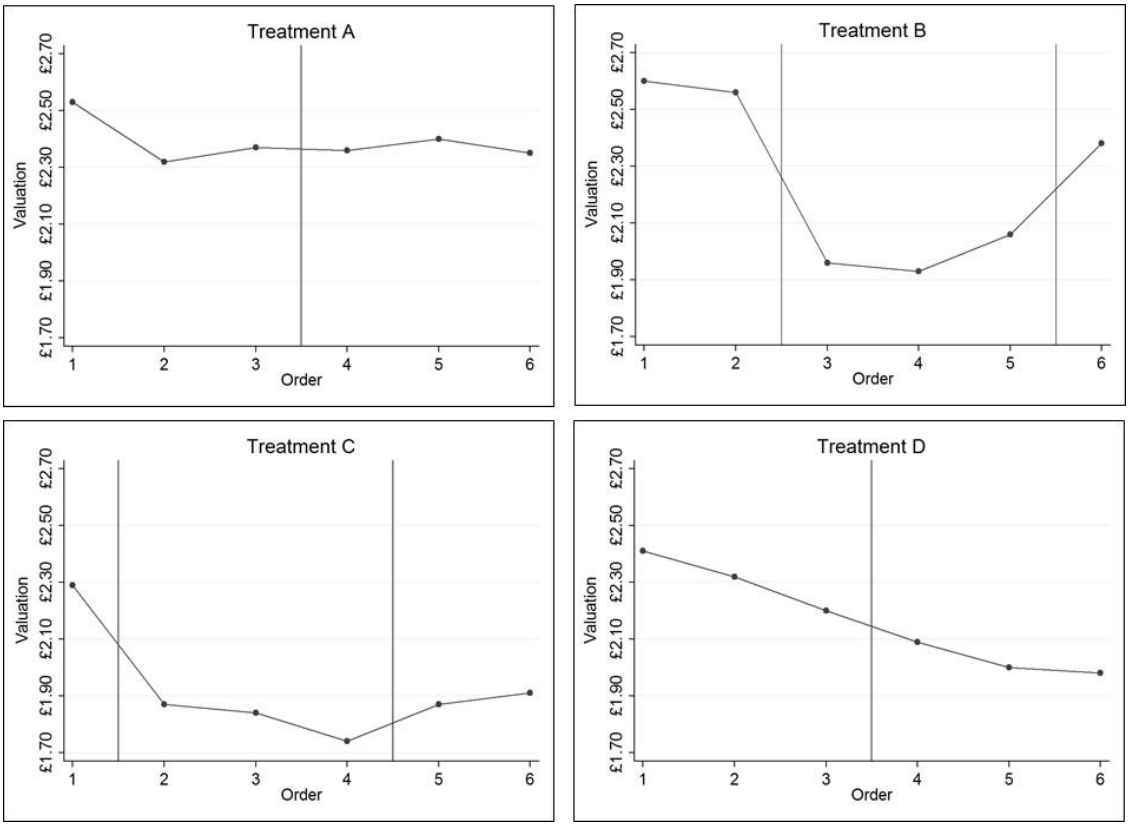


Figure 2.2. Graphs of valuations by treatment and task order

Notes: Vertical bars indicate a transition from one good type (m), to another.

Pooling Treatments A to C, there is an overall downward trend in valuations. A non-parametric (two-tailed) test (Cuzick, 1985) shows that this trend is statistically significant (z= 2.050, p=0.040). This is an extension of a Wilcoxon rank-sum test, testing for a consistent trend in

differences in the value of a variable across multiple sets of observations, where these sets have a natural ordinal ranking. Treatment D shows a similar trend (z= 1.750, p=0.080).

5.2. Results: Tests of Hypotheses H1-H3

Whilst non-parametric analysis enabled a test for overall trends in valuations, parametric regression analysis is required to disentangle the different novelty effects predicted by Hypotheses H1–H3. Regression analysis also allows for the effects on valuations of demographic variables and of responses in lottery tasks to be included (regression results including these variables can be found in Appendix 2.4).

Whilst valuations in this experiment were restricted to not be less than £0.10 or greater than £6.10, actual valuations could be less than or greater than these limit valuations (though there were only 6 participants who chose to not sell one or more of their goods at £6.00). To address this econometrically, a Tobit model will be used, with lower and upper bounds set at £0.10 and £6.10 respectively.

Given the potential for the non-independence of valuations at the participant level, a random effects Tobit regression model is used. Table 2.3 reports the results of three estimated models pooling Treatments A-C. In each model, the dependent variable is the valuation $V_{i,j,k,m}$ reported by a participant in a task with order i, within-subset novelty j, between-subset novelty k and good type m. The following independent variables are used:

Order: takes the value i-1 (i.e. 0, 1, …, 5 for tasks that appear in order 1, 2, …, 6).

First Task: takes the value 1 when i=1 (i.e. when the task is the first to be faced by the participant), 0 otherwise.

$Novelty_2$: takes the value 1 when j=2 (i.e. when the task is the second to involve a good of the relevant type), 0 otherwise.

$Novelty_3$: takes the value 1 when j=3, (i.e. when the task is the third to involve a good of the relevant type), 0 otherwise.

First Good Type: takes the value 1 when k=1 (i.e. when the task involves the first good to be seen by the participant), 0 when k=2.

Mug: takes the value m (i.e. 1 if the good type is ‘mug’, 0 if it is ‘chocolate’).

In all three models, valuations are estimated to be significantly higher for chocolates than for mugs ($p< 0.001$).

Valuation	Model 1 **Hypothesis H1(a)**	Model 2 **Hypothesis H1(a)** **Hypothesis H1(b)**	Model 3 **Hypothesis H1(a)** **Hypothesis H1(b)** **Hypothesis H2** **Hypothesis H3**
Order	-0.0614***	-0.0090	-0.0083
	(0.016)	*(0.021)*	*(0.026)*
First Task	-------	0.3646***	0.2229*
	-------	*(0.094)*	*(0.119)*
Novelty$_2$	-------	-------	-0.0061
	-------	-------	*(0.088)*
Novelty$_3$	-------	-------	-0.0127
	-------	-------	*(0.104)*
First Good Type	-------	-------	0.2291***
	-------	-------	*(0.066)*
Mug	-0.4712***	-0.4706***	-0.4747***
	(0.054)	*(0.053)*	*(0.053)*
Constant	2.5069***	2.3151***	2.2301***
	(0.112)	*(0.122)*	*(0.131)*
Sigma(u)	1.3048***	1.3058***	1.3080***
	(0.076)	*(0.076)*	*(0.076)*
Sigma(e)	0.8614***	0.8535***	0.8469***
	(0.022)	*(0.022)*	*(0.021)*
# Obs	1,068	1,068	1,068
# Groups	178	178	178

Standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1

Table 2.3. Random effects Tobit models of order, primacy, *between*- and *within-subset* novelty effects on valuation in Treatments A-C (lower limit: £0.10, upper limit: £6.10)

Model 1 tests for overall order effects (i.e. general trends in valuations). In line with the non-parametric test reported in section 5.1, there is strongly significant evidence of an order effect ($p< 0.001$), with valuations declining in each successive task.

Model 2 drops the assumption that order effects are linear, by including the variable *First Task* which picks up primacy effects. This variable has a strongly significant positive effect on valuations ($p< 0.001$), but its inclusion makes *Order* insignificant.

Model 3 includes variables capable of separately identifying the attention-based effects predicted by Hypotheses H1–H3. After controlling for other effects, *First Task* remains positive and significant ($p= 0.060$); valuations in the first task are estimated to enjoy a financial

premium. *Order* remains insignificant; its estimated effect on valuations is virtually zero. $Novelty_2$ and $Novelty_3$, which would pick up the within-subset novelty effects predicted by Hypothesis H2, are insignificant and have estimated values close to zero. *First Good Type*, which picks up the between-subset novelty effects predicted by H3, is positive and strongly significant (p= 0.001). Valuations of the first good type to be faced are estimated to enjoy a financial premium.

This analysis supports the following conclusions:

Result 1 (experimental novelty): Holding other factors constant, there is strong evidence of a primacy effect: valuations are higher in the first task than in all subsequent tasks. There is no evidence of a decline in valuations after the second task.

Result 2 (within-subset novelty): There is no evidence of the within-subset novelty effects predicted by Hypothesis H2.

Result 3 (between-subset novelty): There is strong evidence of the between-subset novelty effect predicted by Hypothesis H3.

5.3. Results: Tests of Hypothesis H4

Models 1, 2 and 3 were also estimated using only data from Treatment D, the advanced disclosure treatment. Table 2.4 reports the results of these estimations. Whilst order, primacy, between-subset novelty and good type variables are defined as before, $Novelty_2$ and $Novelty_3$ are pooled as $Novelty_{23}$ (i.e. takes the value 1 when j=2 or j=3, 0 otherwise) to avoid over-identification[8]. In comparing these results with those in Table 2.3, it should be noticed that the sample size is much smaller (54 rather than 178) and that statistical tests are correspondingly less powerful.[9]

Model 1 shows a strong and highly significant overall order effect (p< 0.001); valuations are estimated to fall in each successive task.

As one would expect from a glance at the graph for Treatment D in Figure 2.2, Model 2 shows no significant primacy effect, and the estimated premium for the first task is very small. The overall order effect remains strong and significant (p= 0.002). In Model 3, none of the order or

[8]The problem of over-identification arises because, within Treatment D, there is insufficient counterbalancing of order and novelty variables.

[9]The two orders in which goods were presented at the advanced disclosure stage were counterbalanced across participants (see section 3.4). The regressions reported in Table 2.4 pool these sub-treatments. There were no systematic differences in valuations between them (see Appendix 2.5).

novelty variables are significant, perhaps reflecting the small sample size and the high degree of positive correlation between the variables. Given the absence of any obvious discontinuities in the downward trend in the Figure 2.2 graph, it is natural to draw the following conclusion, which gives limited support for Hypothesis 4:

Result 4 (advanced disclosure). Under advanced disclosure, there is no evidence of good-specific (within-subset or between-subset) novelty effects.

Valuation	Model 1 **Hypothesis H1(a)**	Model 2 **Hypothesis H1(a)** **Hypothesis H1(b)**	Model 3 **Hypothesis H1(a)** **Hypothesis H1(b)** **Hypothesis H2** **Hypothesis H3**
Order	-0.1056***	-0.1008***	-0.0662
	(0.024)	*(0.032)*	*(0.100)*
First Task	-------	0.0337	0.0605
	-------	*(0.145)*	*(0.174)*
Novelty$_{23}$	-------	-------	-0.0099
	-------	-------	*(0.194)*
First Good Type	-------	-------	0.1153
	-------	-------	*(0.317)*
Mug	-0.4417***	-0.4415***	-0.4429***
	(0.082)	*(0.082)*	*(0.082)*
Constant	2.6176***	2.5998***	2.4584***
	(0.193)	*(0.208)*	*(0.363)*
Sigma(u)	1.2704***	1.2705***	1.2705***
	(0.130)	*(0.130)*	*(0.130)*
Sigma(e)	0.7289***	0.7289***	0.7285***
	(0.033)	*(0.033)*	*(0.033)*
# Obs	324	324	324
# Groups	54	54	54

Table 2.4. Random effects Tobit models of order, primacy, *between*- and *within-subset* novelty effects on valuation in Treatment D (lower limit: £0.10, upper limit: £6.10)

5.4. Other Findings

Whilst the primary purpose of the lottery tasks was to act as a cognitive distraction between goods tasks, they elicited some information about participants' degrees of risk aversion and their propensities to violate principles of expected utility theory (EUT). Summary data about responses to these tasks can be found in Appendix 2.3. A large majority of participants revealed risk aversion. 53.0 per cent of participants revealed the common ratio effect, one of the most commonly-observed violations of the independence axiom of EUT (see, for example, Cubitt *et al*., 1998a), while only 4.7 per cent violated that axiom in the opposite direction, a discrepancy

consistent with other experiments (e.g. Starmer and Sugden, 1989). There were also very few violations of the dominance axiom, suggesting that participants understood the *X*-value lottery design. Participants' valuations in the goods tasks were not significantly affected by their degree of risk aversion or by their propensities to violate EUT (see Appendix 2.4).

Demographic information, collected in a post-experimental questionnaire, was included in additional regression analyses to test if any demographic factors consistently influenced participants' valuations. These questionnaires were optional. Six participants did not answer all the questions and so were omitted from the analysis, reported in Appendix 2.4. Gender, nationality and previous formal study of economics had no significant effects, but there is some evidence that older participants reported lower valuations. The most interesting finding was that, in the main treatments, experience of having taken part in previous economics experiments had a strong and significant negative effect on valuations (p= 0.008).[10] Valuations were estimated to be significantly higher for participants who were taking part in an experiment for the first time. Adding variables that interacted experience with order and novelty revealed no obvious patterns. Viewed in the perspective of an attention-based theory, this effect of experience may suggest that, the more frequently participants take part in any experiment, the less novel future experiments become, and so the attention participants give to the nature of them diminishes, reducing valuations in general.

Because of the counterbalancing of the order of tasks (see section 3.4), any systematic differences between the valuations of the three goods in each subset would not affect the tests of Hypotheses H1–H4. Nevertheless, it is of interest to assess whether there were any such differences. Two relevant tests are reported in Appendix 2.6. These tests use data only from those participants who implicitly reported a 'most preferred' good in a subset (i.e. who gave one good a strictly higher valuation than each of the other two). The first test is of whether the distribution of first preferences is non-random across goods. The second test is of whether the absolute valuations of participants' most-preferred goods differs according to which good is most preferred. No significant differences are found for either test, applied to either subset.

6. Discussion

The results reported in section 5 provide evidence that, when experimental participants face a series of tasks designed to elicit selling valuations *for different goods*, valuations tend to fall

[10]In Treatment D, for which the sample size was much smaller, no significant effects were found.

over the course of the experiment. This evidence parallels previous experimental findings about the effects of repeating *the same* selling task.

By using goods with different degrees of similarity to one another in different orders, the experiment was able to disentangle different attention-based mechanisms that might induce a fall in valuations. The treatments without advanced disclosure produce three main results. First, there is a strong tendency for participants' valuations to be higher in the first task they face than in subsequent tasks – an *experimental* novelty effect. Second, in a setting in which goods fall into two dissimilar types, there is a strong tendency for valuations to be higher for the first type of good faced than for the second – a *between-subset* novelty effect. Third, there is no evidence of *within-subset* novelty effects.

This combination of results may seem surprising. Given the known tendency for valuations of identical goods to decline as tasks are repeated, one might have expected to find the same tendency when goods are very similar to one another. The absence of a within-subset novelty effect suggests that apparently small differences between goods – slightly different patterns on otherwise identical mugs, different biscuit toppings on otherwise identical chocolate bars – can maintain participants' attention in a sequence of goods tasks. It may be significant that, for each good type, these 'small' differences were restricted to one attribute of the good. The fact that this was the *only* attribute that varied between the relevant tasks would have made it particularly salient to participants. A participant whose attention is focused on biscuit toppings on chocolate bars (and who is anticipating the possibility of having one to eat) can experience a sequence of 'Rocky Road', 'Milk and Cookies' and 'Mississippi Mud Pie' as three distinct novelties.

In contrast, the between-subset novelty effect implies that the transition from valuing mugs to valuing chocolate bars (or vice versa) induced a reduction in attention. It seems that some of the novelty of the first task carried over to later tasks in which the same type of good appeared, but not to tasks involving a different type. A possible explanation of this effect is that it is a form of anchoring, analogous with the finding of Payne *et al*. (2000) that when respondents sequentially report valuations for each of a given set of public projects, the sum of these valuations is influenced by the order in which valuations are made: the higher the *relative* valuation of the good faced first, the higher is the sum of valuations.

This effect might work through anchoring *on valuations*: participants might use valuations that they have reported in earlier tasks as anchors when subsequently valuing similar goods. But,

given that the effect does not apply across good types (while, as noted in section 2.1, even arbitrary numbers can be anchors for valuations) it is more plausible to conjecture that anchoring is *on attributes*. As explained in section 2.2, theories of attentional bias explain differences between buying and selling valuations as a result of sellers giving more attention than buyers to the (positive) attributes of goods. Relatedly, it is possible that the positive attention devoted to the attributes of the first valued good then acts as an anchor for subsequent valuations. Participants may attend more to the attributes that are possessed by that good than to attributes that they experience later. Thus, the initial focus of attention on these attributes may spill over to different goods of the same type – that is, goods that share many of the attributes of the first good.

These conjectures receive some support from the results of the advanced disclosure treatment. The distinguishing feature of this treatment was that participants saw all six goods before facing the first goods task: individual goods tasks were not associated with the novelty of learning about a new good. In this treatment, unlike the other treatments but in line with previous experiments using identical goods, there was a consistent decline in valuations over the six tasks. It may be that advanced knowledge of all the goods made later tasks less interesting, reducing the attention that participants gave to them, independent of the good to be valued in any particular task. One must be careful not to over-interpret the results of this treatment, given its small sample size, but the absence of significant primacy and between-subset novelty effects in this treatment is consistent with the conjecture that advanced disclosure dampened participants' sense of novelty when facing successive goods tasks.

7. Conclusion

In designing and interpreting experiments and stated preference methodologies that elicit individuals' valuations of goods, it is important to understand the mechanisms by which participants' responses to tasks can be affected by the order in which those tasks are faced. The experiment reported in this paper is a contribution to this under-researched area. Its findings highlight the importance of attention in mediating order effects, and the potentially complex relationships between the novelty of a task and the attention that it receives. In economics experiments, the quality of the data typically depends on participants' engagement with and attention to the tasks they face. A fuller understanding of the role of novelty in maintaining attention may lead to more effective experimental designs.

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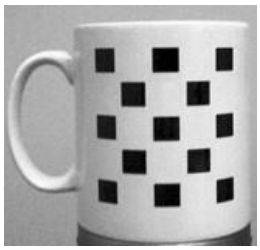
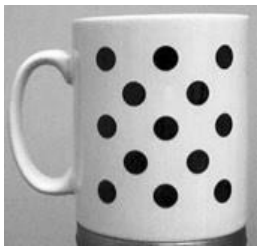
Appendices

Appendix 2.1. Descriptions of goods used



A plain white ceramic mug with printed black squares. Dishwasher and microwave safe.



A plain white ceramic mug with printed black circles. Dishwasher and microwave safe.

A plain white ceramic mug with printed black triangles. Dishwasher and microwave safe.

Rocky Road: Cookies and puffed rice set in milk and white chocolate.

Milk and Cookies: Shortcake and cocoa biscuits set in milk and white chocolate.

Mississippi Mud Pie: Cocoa biscuit crunch set in milk and white chocolate.

Appendix 2.2. Pre-experimental online pilot survey of chocolate bar types

2.2.1. Kruskal-Wallis test of differences in preference score between different chocolate bar types

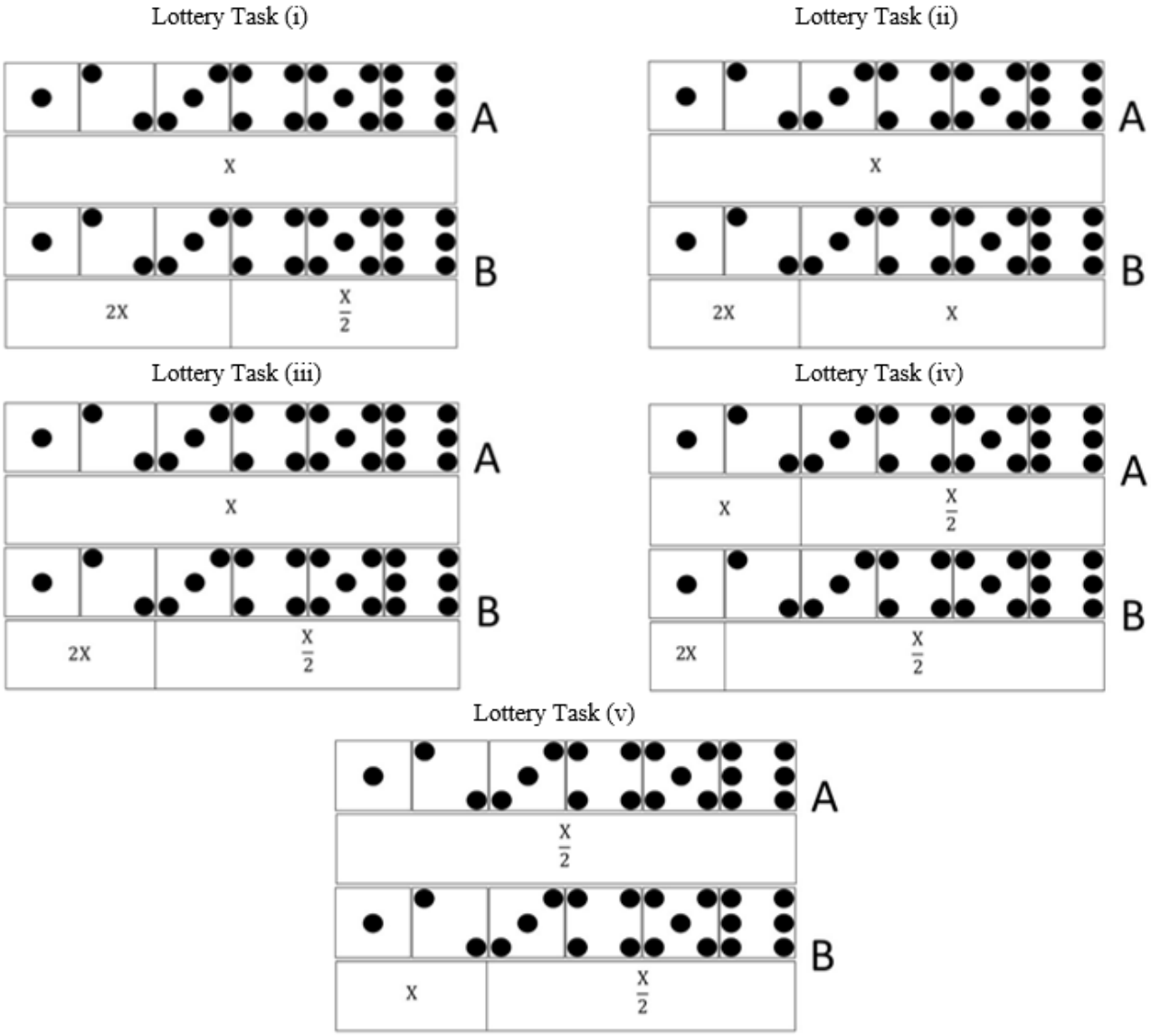
Average Preference Score (0-7)	**Rocky Road**	**Milk and Chocolate**	**Mississippi Mud Pie**	**Kruskal-Wallis Test**	***p*-value**
Hotel Chocolat (*n=49*)	3.41	4.10	3.84	$\chi^2(2)= 2.506$	0.286

2.2.2. Average difference in preference score between most and least preferred chocolate bar types within-participant

Average difference in preference score between most and least preferred
2.29

Notes: The mugs used were the same as in Chapter 1. Details of a pre-experimental online pilot survey that directed the use of these mug designs can be found in Appendix 1.1 of Chapter 1.

Appendix 2.3. Lottery task descriptions and results



Risk Aversion: 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. 77.6% of participants revealed risk aversion.

Strong Risk Aversion: Lottery task (iii)

As the expected payoff of the risky lottery B exceeds the certain value of A, any subject choosing A could be seen as strongly risk averse. 28.4% of participants revealed strong risk aversion.

Dominance: Lottery task (ii) and lottery task (v)

In both tasks, Lottery B weakly dominates lottery A; thus a choice of A would violate the principle that preferences over lotteries respect stochastic dominance. In lottery task (ii), 5.6% of participants violated dominance, and lottery task (v), simply a scaled down version of (ii), 2.2% violated dominance. 0.9% of participants violated dominance in both lottery tasks.

Implied Dominance: Lottery task (i) and lottery task (iii)

Any subject choosing B in lottery task (i), but A in lottery task (iii) would indirectly violate dominance. 1.7% of participants indirectly violated dominance.

Independence Axiom- Common Ratio Effect: Lottery task (i) and lottery task (iv)

As lottery task (iv) is simply a scaled down version of lottery task (i), the axiom of independence 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 (Cubitt *et al*. (1998a), for example). 53.0% of participants revealed the common ratio effect. 4.7% violated independence in the opposite direction.

Appendix 2.4. Random effects Tobit model of order, primacy, *between-* and *within-subset* novelty effects on valuation by Treatments *A-C* and *D* (lower limit: £0.10, upper limit: £6.10), including lottery task, demographic effects, interaction variables for non-experience

Valuation *Treatments*	**Lottery/ Dem.** ***A-C***	**Lottery/ Dem.** ***D***	**Lottery/ Dem. Non-Experience** ***A-C***	**Lottery/ Dem. Non-Experience** ***D***
Order	-0.0084	-0.0570	-0.0104	-0.0625
	(0.027)	*(0.101)*	*(0.028)*	*(0.100)*
First Task	0.2284*	0.0300	0.2287*	0.0575
	(0.122)	*(0.174)*	*(0.128)*	*(0.173)*
Novelty$_2$	-0.0088	-------	-0.0007	-------
	(0.090)	-------	*(0.095)*	-------
Novelty$_3$	-0.0162	-------	-0.0282	-------
	(0.106)	-------	*(0.112)*	-------
Novelty$_{23}$	-------	-0.0296	-------	-0.0287
	-------	*(0.195)*	-------	*(0.194)*
First Good Type	0.2284***	0.1266	0.2560***	0.2082
	(0.067)	*(0.318)*	*(0.071)*	*(0.316)*
Mug	-0.4753***	-0.4682***	-0.4685***	-0.4753***
	(0.054)	*(0.082)*	*(0.057)*	*(0.082)*
Risk Averse	-0.0106	-0.2120	-0.0096	-0.2103
	(0.318)	*(0.549)*	*(0.318)*	*(0.547)*
Strong Risk Averse	0.1806	-0.0680	0.1797	-0.0681
	(0.232)	*(0.440)*	*(0.232)*	*(0.440)*
Dominance	0.5839	-0.3960	0.5820	-0.3971
	(0.356)	*(0.939)*	*(0.356)*	*(0.937)*
Implied Dominance	-0.0114	1.1197	-0.0105	1.1187
	(0.801)	*(1.404)*	*(0.801)*	*(1.401)*
Common Ratio Effect (CRE)	-0.3318	-0.0946	-0.3321	-0.0976
	(0.230)	*(0.466)*	*(0.230)*	*(0.465)*
Opposite CRE	0.0603	-0.0646	0.0608	-0.0660
	(0.516)	*(0.872)*	*(0.516)*	*(0.870)*
Age	0.0997	-0.2530	0.0996	-0.2527
	(0.079)	*(0.219)*	*(0.079)*	*(0.219)*
Age^2	-0.0140**	0.0180	-0.0140**	0.0180
	(0.006)	*(0.022)*	*(0.006)*	*(0.022)*
Female	-0.1254	0.1993	-0.1255	0.2010
	(0.204)	*(0.367)*	*(0.204)*	*(0.366)*
Economics	0.2354	-0.6167	0.2353	-0.6175
	(0.263)	*(0.445)*	*(0.263)*	*(0.444)*
UK/ Irish Nationality	-0.2773	-0.1813	-0.2770	-0.1791
	(0.218)	*(0.453)*	*(0.218)*	*(0.452)*
Non-Experience (NE)	0.8358***	0.2757	1.0247**	0.6748
	(0.317)	*(0.674)*	*(0.467)*	*(1.312)*
NE*Order	-------	-------	0.0137	0.0625
	-------	-------	*(0.110)*	*(0.356)*
NE *First Task	-------	-------	-0.0123	-0.3575
	-------	-------	*(0.429)*	*(0.617)*
NE *Novelty$_2$	-------	-------	-0.0744	-------
	-------	-------	*(0.294)*	-------
NE *Novelty$_3$	-------	-------	0.1195	-------
	-------	-------	*(0.385)*	-------
NE *Novelty$_{23}$	-------	-------	-------	0.0037
	-------	-------	-------	*(0.690)*
NE *First Good Type	-------	-------	-0.3208	-1.0582
	-------	-------	*(0.224)*	*(1.126)*
NE *Mug	-------	-------	-0.1556	0.0586
	-------	-------	*(0.211)*	*(0.291)*
Constant	2.3993***	3.1788***	2.3880***	3.1492***
	(0.369)	*(0.761)*	*(0.370)*	*(0.759)*
Sigma(u)	1.1952***	1.1626***	1.1953***	1.1635***
	(0.071)	*(0.122)*	*(0.071)*	*(0.121)*
Sigma(e)	0.8568***	0.7160***	0.8543***	0.6836***
	(0.022)	*(0.033)*	*(0.022)*	*(0.031)*
# Obs	1,044	312	1,044	312
# Groups	174	52	174	52

Appendix 2.5. Random effects Tobit model of order, primacy, *between*- and *within-subset* novelty effects on valuation by Treatment *D* (lower limit: £0.10, upper limit: £6.10), including interaction variables for all effects on reverse presentation of advanced disclosure screen

Valuation	Model 1 **Hypothesis H1(a)**	Model 2 **Hypothesis H1(a)** **Hypothesis H1(b)**	Model 3 **Hypothesis H1(a)** **Hypothesis H1(b)** **Hypothesis H2** **Hypothesis H3**
Order	-0.1257***	-0.1155***	-0.0878
	(0.033)	*(0.044)*	*(0.139)*
First Task	-------	0.0703	0.1065
	-------	*(0.201)*	*(0.240)*
Novelty$_{23}$	-------	-------	0.0105
	-------	-------	*(0.269)*
First Good Type (FGT)	-------	-------	0.0925
	-------	-------	*(0.440)*
Mug	-0.3687***	-0.3679***	-0.3708***
	(0.114)	*(0.114)*	*(0.114)*
Reverse AD	-0.0747	-0.0342	-0.0511
	(0.386)	*(0.415)*	*(0.726)*
Reverse AD*Order	0.0448	0.0339	0.0454
	(0.048)	*(0.064)*	*(0.200)*
Reverse AD*First Task	-------	-0.0757	-0.0985
	-------	*(0.290)*	*(0.347)*
Reverse AD*Novelty$_{23}$	-------	-------	-0.0429
	-------	-------	*(0.388)*
Reverse AD*FGT	-------	-------	0.0380
	-------	-------	*(0.633)*
Reverse AD*Mug	-0.1609	-0.1617	-0.1588
	(0.164)	*(0.164)*	*(0.164)*
Constant	2.6514***	2.6138***	2.4868***
	(0.269)	*(0.289)*	*(0.503)*
Sigma(u)	1.2715***	1.2717***	1.2717***
	(0.130)	*(0.130)*	*(0.130)*
Sigma(e)	0.7268***	0.7267***	0.7264***
	(0.033)	*(0.033)*	*(0.033)*
# Obs	324	324	324
# Groups	54	54	54

Appendix 2.6. Tests for systematic preferences or valuations of specific goods

2.6.1. Distribution of good type preferences of participants who valued one good within a subset uniquely more highly than the other two (thus excluding participants who value more than one good equally most highly).

The table pools all treatments *A-D* and also separates preferences by first subset of goods.

Mugs Preference	**(n)**	**Squares**	**Circles**	**Triangles**	**χ^2 test**	***p*-value**
All	109	32	38	39	$\chi^2(2)= 0.790$	0.674
Mug First	68	21	24	23	$\chi^2(2)= 0.210$	0.902
Chocolate First	41	11	14	16	$\chi^2(2)= 0.930$	0.629
Chocolates Preference	**(n)**	**RR**	**MC**	**MM**	**χ^2 test**	***p*-value**
All	115	31	36	48	$\chi^2(2)= 3.980$	0.137
Mug First	52	12	17	23	$\chi^2(2)= 3.500$	0.174
Chocolate First	63	19	19	25	$\chi^2(2)= 1.140$	0.565

2.6.2. Mean valuations of the (uniquely) most highly valued goods, by subset

The table pools all treatments *A-D* and also separates preferences by first subset of goods.

Mug Valuation	**Squares**	**Circles**	**Triangles**	**Kruskal-Wallis Test**	***p*-value**
All	£3.28	£2.54	£2.81	$\chi^2(2)= 2.701$	0.259
Mug First	£3.51	£2.48	£3.02	$\chi^2(2)= 3.463$	0.177
Chocolate First	£2.85	£2.66	£2.50	$\chi^2(2)= 0.119$	0.942
Chocolates Valuation	**RR**	**MC**	**MM**	**Kruskal-Wallis Test**	***p*-value**
All	£2.99	£3.00	£3.02	$\chi^2(2)= 0.006$	0.997
Mug First	£3.02	£2.79	£3.20	$\chi^2(2)= 1.045$	0.593
Chocolate First	£2.97	£3.18	£2.86	$\chi^2(2)= 0.481$	0.786

Appendix 2.7. Experimental instructions

Part One

Introduction

Welcome to this experiment on decision making. Thank you for coming. Please follow along as I read through the instructions. If you have a question, please raise your hand and I will come to answer your question privately. The following instructions are simple, and if you follow them carefully you will have the chance to earn money, and other things. What you take away from the experiment will be determined by your decisions and by chance. After you have completed the experiment you will receive a £6 participation fee, plus the amount resulting from your decisions in the experiment.

Your decisions in this experiment are private, and we ask you not to communicate with others during this experiment. It is also important you do not react verbally to outcomes during the experiment. If you have any questions during the experiment please raise your hand and an experimenter will come to assist you. Please keep to these simple rules, as anyone breaking them may be asked to leave without payment.

The experiment consists of eleven tasks – six *goods* tasks and five *lottery* tasks. After you have completed all eleven tasks, one of the tasks will be selected at random. What you take away from the experiment will be determined by the decisions you made in that task, and only by those decisions. As any one of the eleven tasks might be selected to determine your earnings, you should think about each task as if it were for real, and as if it were the only task in the experiment. I will now describe the two types of task.

Goods Task

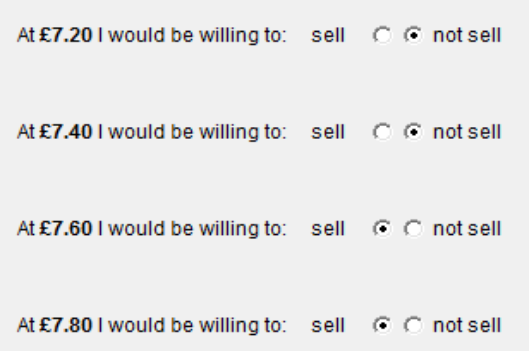
At the start of each goods task, you will be given an item which you then own. Before the experiment begins you will be shown on your screens all six goods for the six goods tasks. You will have the opportunity to choose whether to keep the item or to sell it back to the experimenter. You will be shown a list of prices. For each of these prices you will be asked if you would be willing to sell the item at this given price or not.

At the end of the experiment, if a goods task is selected to determine your earnings, one of the prices listed will be selected at random. This will be the price that the experimenter offers for the item in that goods task. If you have indicated you would be willing to sell the item at this price, you will give up the item and receive that price in addition to the £6 participation fee. If

you have indicated you would not be willing to sell the item at this price, you will keep the item and receive no additional money.

Please note that your decisions in the task cannot affect which price the experimenter offers. So when deciding whether or not you are willing to sell at the listed prices, it is in your interest to think about each price separately.

To assist in the explanation of the goods tasks, here is an example. Suppose, after being given an item, a participant in the experiment is asked whether or not they would be willing to sell it at the following prices:



An individual with consistent preferences would switch from 'not sell' to 'sell' no more than once. This is because they would not want to sell at any price less than their personal valuation of the good and would want to sell at prices greater than this. Here the participant has indicated that they are not willing to sell at prices £7.20 and £7.40, but that they are willing to sell at prices £7.60 and £7.80. This suggests that the personal valuation of the good by the participant is somewhere greater than £7.40, but less than £7.60. If this task was selected to determine the participant's earnings, and if one of the four prices was selected at random to be the offer made by the experimenter, the participant's decision at that price would be made binding.

Lottery Task

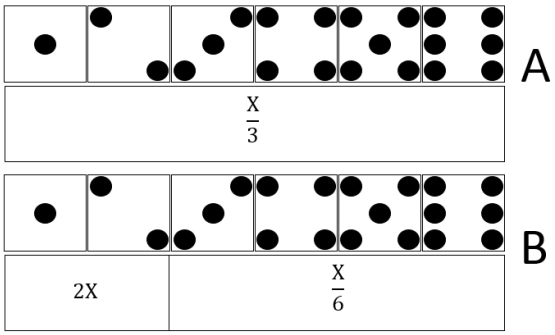
In each lottery task you will be asked to choose one of two possible lotteries.

At the end of the experiment, if a lottery task is selected to determine your earnings, you will play the lottery that you chose in that task.

The payoff of each lottery will be determined by a roll of a six-sided dice, with each number on the dice corresponding to a payoff. The money values of the payoffs will not be known until the end of the experiment. At the time at which you are making your choice, all the payoffs

will be described as fractions of X, where X is some amount of money in the range from £0.20 to £6.00 (in £0.20 intervals). At the end of the experiment, if a lottery task is selected to determine your earnings, one of these amounts of money will be selected at random, and this will then be the value of X in the lottery you have chosen. A roll of the dice will then determine the payoff you receive, in addition to the £6 participation fee.

To assist in the explanation of the lottery tasks, here is an example. Suppose a participant in the experiment is asked to choose between the following lotteries:



Suppose the participant chooses Lottery A. If the dice rolls a 1, 2, 3, 4, 5 or 6, the payoff is equal to the value of (X/3) (i.e. X divided by 3). Suppose the participant chooses Lottery B. If the dice rolls a 1 or 2, the payoff is equal to the value of (2*X) (i.e. X multiplied by 2). If the dice rolls a 3, 4, 5 or 6, the payoff is equal to the value of (X/6) (i.e. X divided by 6). Remember, the value of X is not determined until the end of the experiment.

Before we proceed, I ask you to answer the short quiz that will follow shortly on your screens, to ensure you understand the tasks in the experiment. Please attempt these and feel free to re-read the instructions as you do so. If you have any queries please raise your hand and an experimenter will come to assist you.

Part Two

Determining the Price

We will now select the price offered in the goods tasks and the value of X in the lottery tasks. We explained that X might be any amount of money in the range from £0.20 to £6.00 in £0.20 intervals. You may have noticed that in the goods tasks, the possible offers were amounts of money in the same range. We will now select one amount of money in this range. If the task that determines your earnings is a goods task, this amount will be the price offered. If the task that determines your earnings is a lottery task, this amount will be the value of X. To ensure a randomly drawn value we will ask one of you to draw out at random one envelope from this bag. There are 30 envelopes in this bag. Each envelope contains a card showing a different value in the range from £0.20-£6.00 in £0.20 increments.

The value drawn is ___. Please wait whilst this is uploaded to your screen. Once you have seen this, please press Continue.

Determining the Task

We will now select the task which will be used to determine what you take away from the experiment. To ensure that the task is selected at random, we will ask one of you to draw out at random one envelope from this bag. There are eleven envelopes in this bag. Each envelope contains a card with a different number in the range from 1 to 11, representing the eleven tasks in the order in which you completed them.

The task drawn is ___. Please wait whilst this is uploaded to your screen. Once you have seen this, please press Continue.

You will now be shown on your screen which task corresponds to the number selected. As the task drawn is ___, the task type for all participants should be a ___ task. Once you have seen this, please press Continue.

If your task is a **goods task** then ___ is now the selling price of the item. On your screen it should show you which item corresponds to this task, and your decision at that price. If you have indicated you were willing to sell the item at this price then, on leaving the experiment, you will receive ___ in addition to your £6 participation fee and will not keep the item. If you have indicated you were not willing to sell the item at this price then, on leaving the experiment, you will receive the item and your £6 participation fee.

If your task is a **lottery task** then ___ is now the value of X in the lotteries. On your screen it should show you which lottery you chose in this task and the values of the possible payoffs. An experimenter will visit you shortly with a dice to determine which payoff you receive, in addition to your £6 participation fee.

Please wait whilst the experimenter assists in determining final payments individually. A questionnaire will be given to you, and it is requested you complete this before taking your payment receipt and questionnaire with you to the payment desk upon leaving. Final payments and the giving of any goods you are owed will take place at the payment desk on your way out.

Appendix 2.8. Pre-experimental quiz (correct answer in **bold**)

Question 1: When is the value of the good price / lottery payoff determined?

a) Before any tasks are completed

b) Before each task is completed

c) After all tasks have been completed

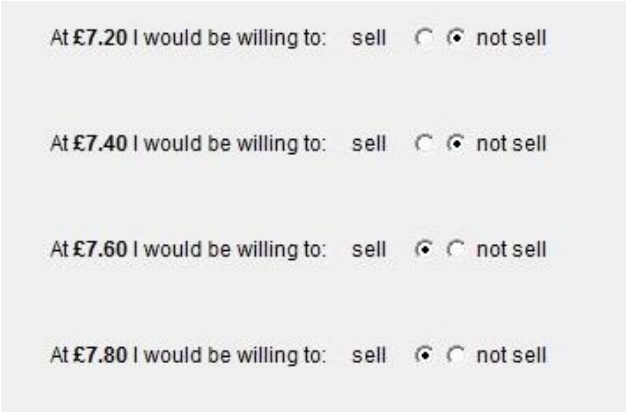
Question 2: Of the eleven tasks, how many will be played out for real?

a) 1

b) 6

c) 11

Goods Tasks



Question 3: Using the example given in the instructions, and above, suppose the value of £7.20 is drawn. What does the participant do?

a) Keep the item and pay £7.20

b) Keep the item and receive nothing

c) Give up the item and receive £7.20

Question 4: Using the example given in the instructions, and above, suppose the value of £7.60 is drawn. What does the participant do?

a) Give up the item and receive £7.60

b) Give up the item and receive £7.80

c) Keep the item and pay £7.60

Question 5: Using the example given above, what can we say about the preferences of this participant?

a) The participant has consistent preferences

b) The participant has inconsistent preferences

c) We cannot tell at this stage

Question 6: Using the example given above, what can we say about the preferences of this participant?

a) The participant has consistent preferences

b) The participant has inconsistent preferences

c) We cannot tell at this stage

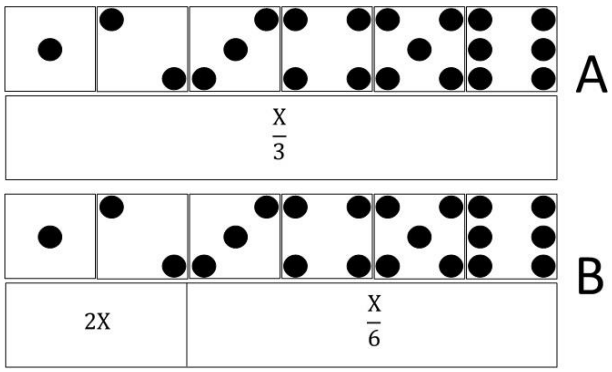
Question 7: Using the example given above, what can we say about the preferences of this participant?

a) The participant has consistent preferences

b) The participant has inconsistent preferences

c) We cannot tell at this stage

Lottery Tasks



Question 8: Using the example given in the instructions, and above, suppose Lottery B is chosen and the value drawn is £9.00. If a dice rolls a 2, what is the payoff?

a) (X) = £9.00

b) (X/3)= (£9.00/3) = £3.00

c) (2X)= (2*£9.00)= £18.00

Question 9: Using the example given in the instructions, and above, suppose Lottery A is chosen and the value drawn is £9.00. If a dice rolls a 4, what is the payoff?

a) £9.00

b) £3.00

c) £18.00

Notes: The pre-experimental quiz was completed on the participant's computer screens.

Appendix 2.9. Post-experimental questionnaire

Thank you for your participation. Please answer this short questionnaire.

1) Age:

_____

2) Gender:

_____

3) Course of Study:

_____

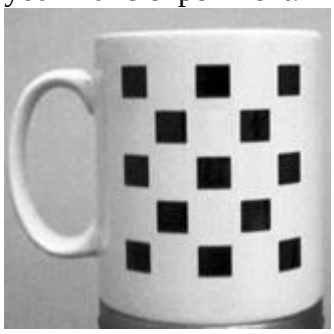
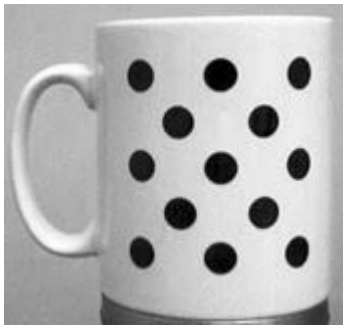
4) Nationality:

_____

5) Please indicate the number of CBESS experiments you have previously taken part in:

○	○	○	○
This is my first	2-5	6-10	More than 10

6) Please look at the following pictures of the possible items you could have taken away with you in this experiment:


Please indicate below how desirable each of these mugs is for you. (*please tick*)
(where 0 = *completely undesirable* and 10 = *completely desirable*)

Squares:	○ 0	○ 1	○ 2	○ 3	○ 4	○ 5	○ 6	○ 7	○ 8	○ 9	○ 10
Circles:	○ 0	○ 1	○ 2	○ 3	○ 4	○ 5	○ 6	○ 7	○ 8	○ 9	○ 10
Triangles:	○ 0	○ 1	○ 2	○ 3	○ 4	○ 5	○ 6	○ 7	○ 8	○ 9	○ 10

7) Please look at the following pictures of the possible items you could have taken away with you in this experiment:

Please indicate below how desirable each of these chocolates is for you. (*please tick*)
(where 0 = *completely undesirable* and 10 = *completely desirable*)

Rocky Road:	○ 0	○ 1	○ 2	○ 3	○ 4	○ 5	○ 6	○ 7	○ 8	○ 9	○ 10
Milk and Cookies:	○ 0	○ 1	○ 2	○ 3	○ 4	○ 5	○ 6	○ 7	○ 8	○ 9	○ 10
Mississippi Mud Pie:	○ 0	○ 1	○ 2	○ 3	○ 4	○ 5	○ 6	○ 7	○ 8	○ 9	○ 10

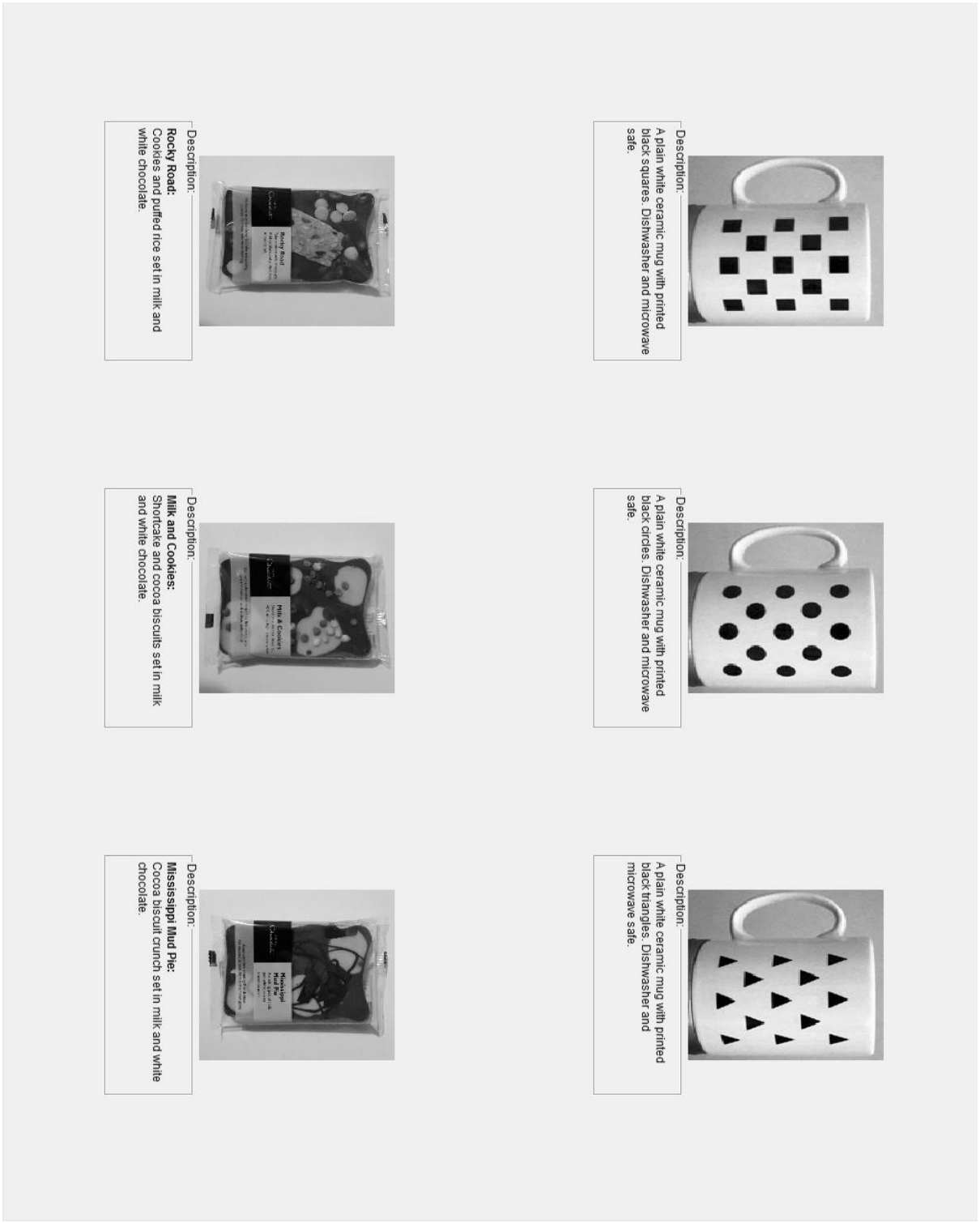
8) Please feel free to use this space to comment on how you determined your valuation decisions in the experiment:

Thank you for answering this questionnaire.

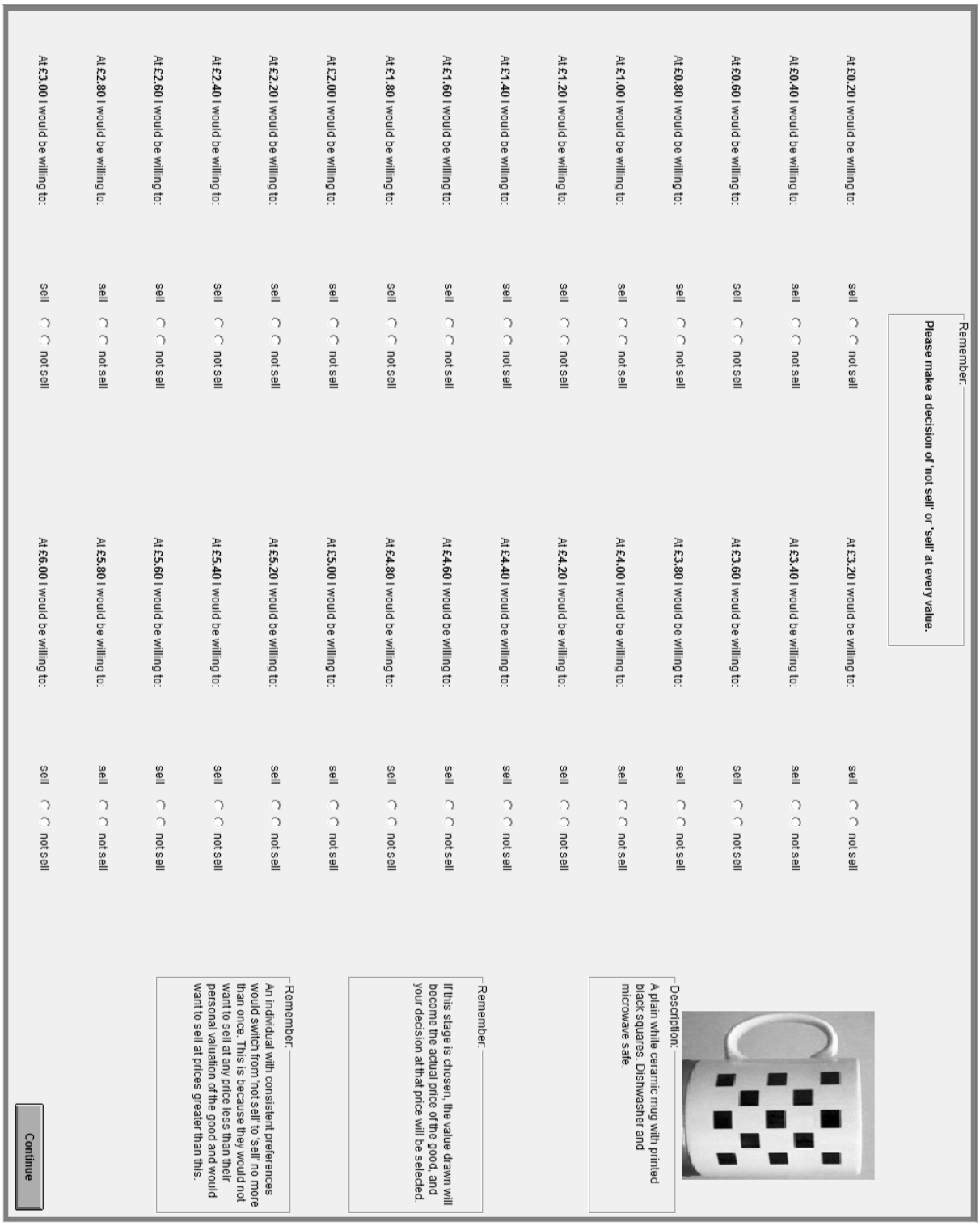
Notes: The order of questions 6) and 7) and the presentation of goods within these questions were counterbalanced across participants.

Appendix 2.10. Experimental screenshots

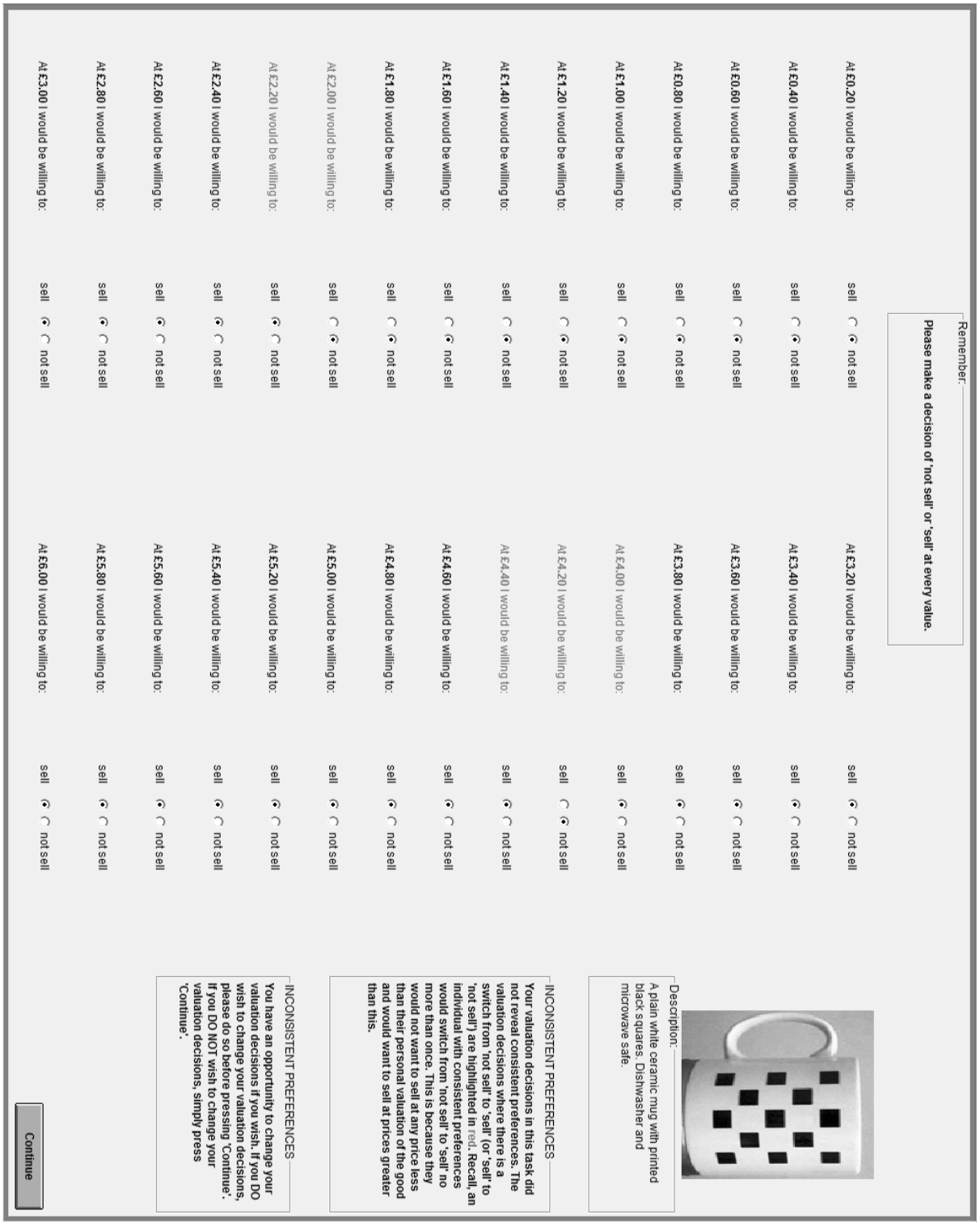
2.10.1. Advanced disclosure of goods in experiment (in Treatment D only)



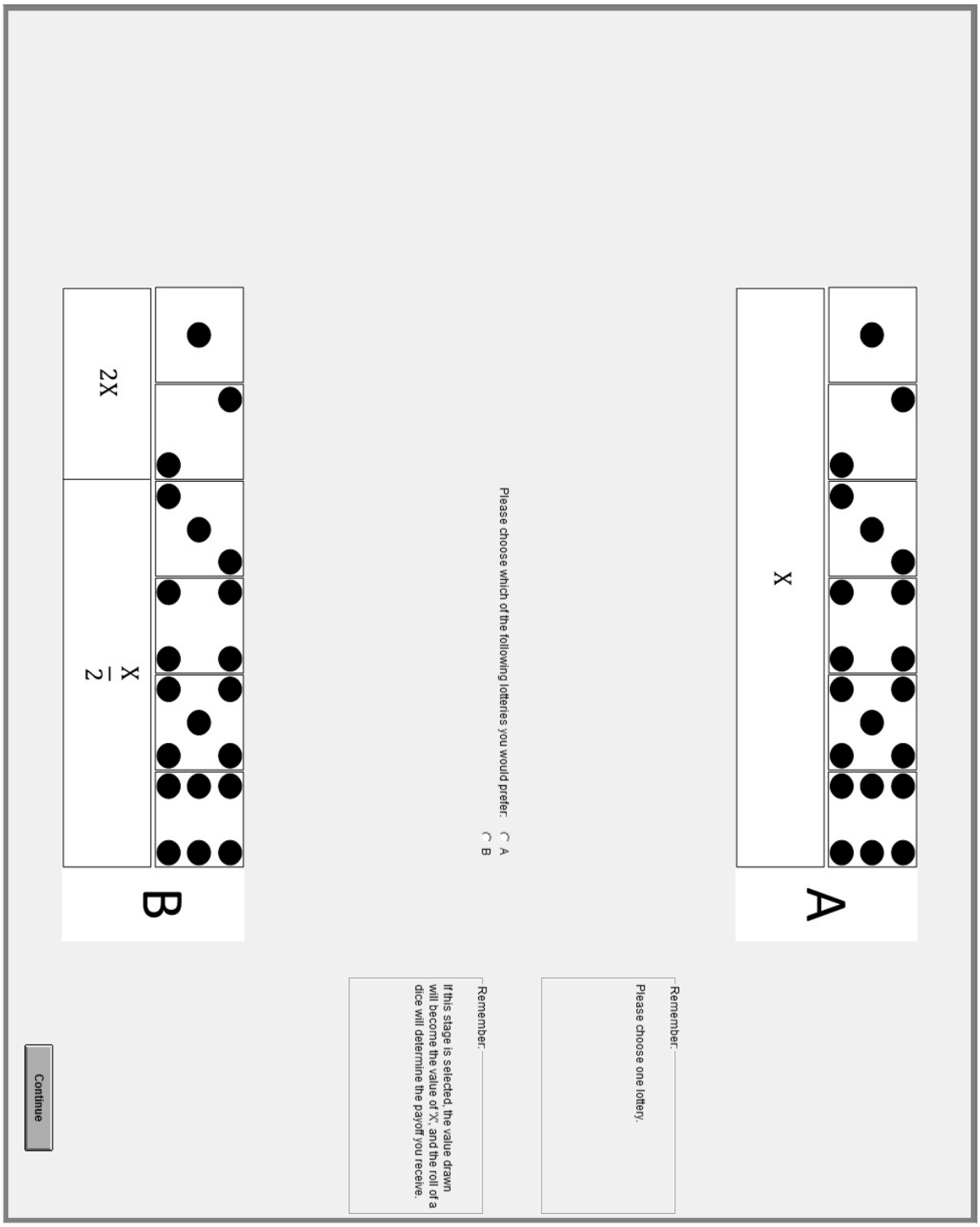
2.10.2. BDM valuation elicitation page for goods task (squares mug)



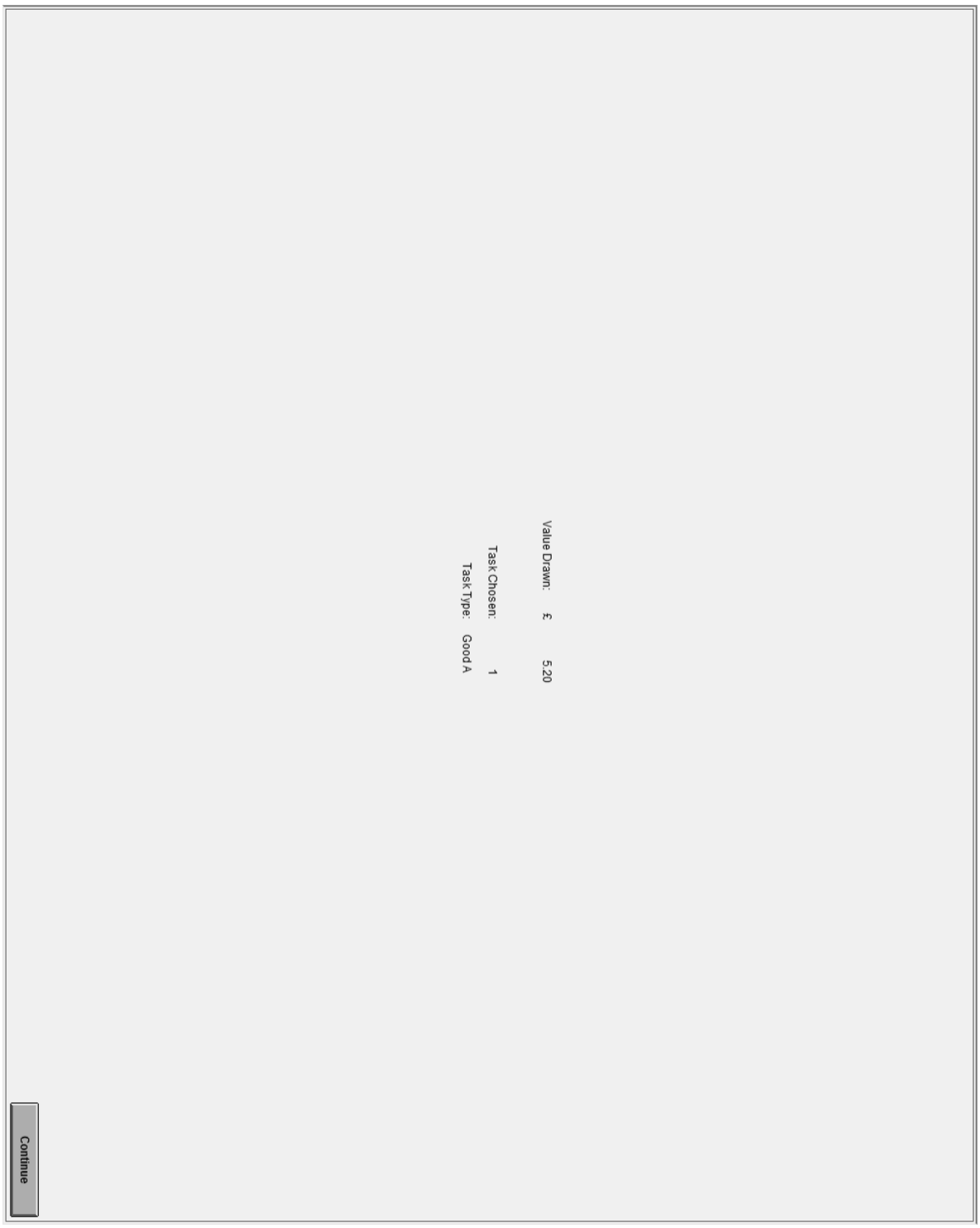
2.10.3. BDM valuation elicitation page for goods task (squares mug)- inconsistent preferences revision opportunity



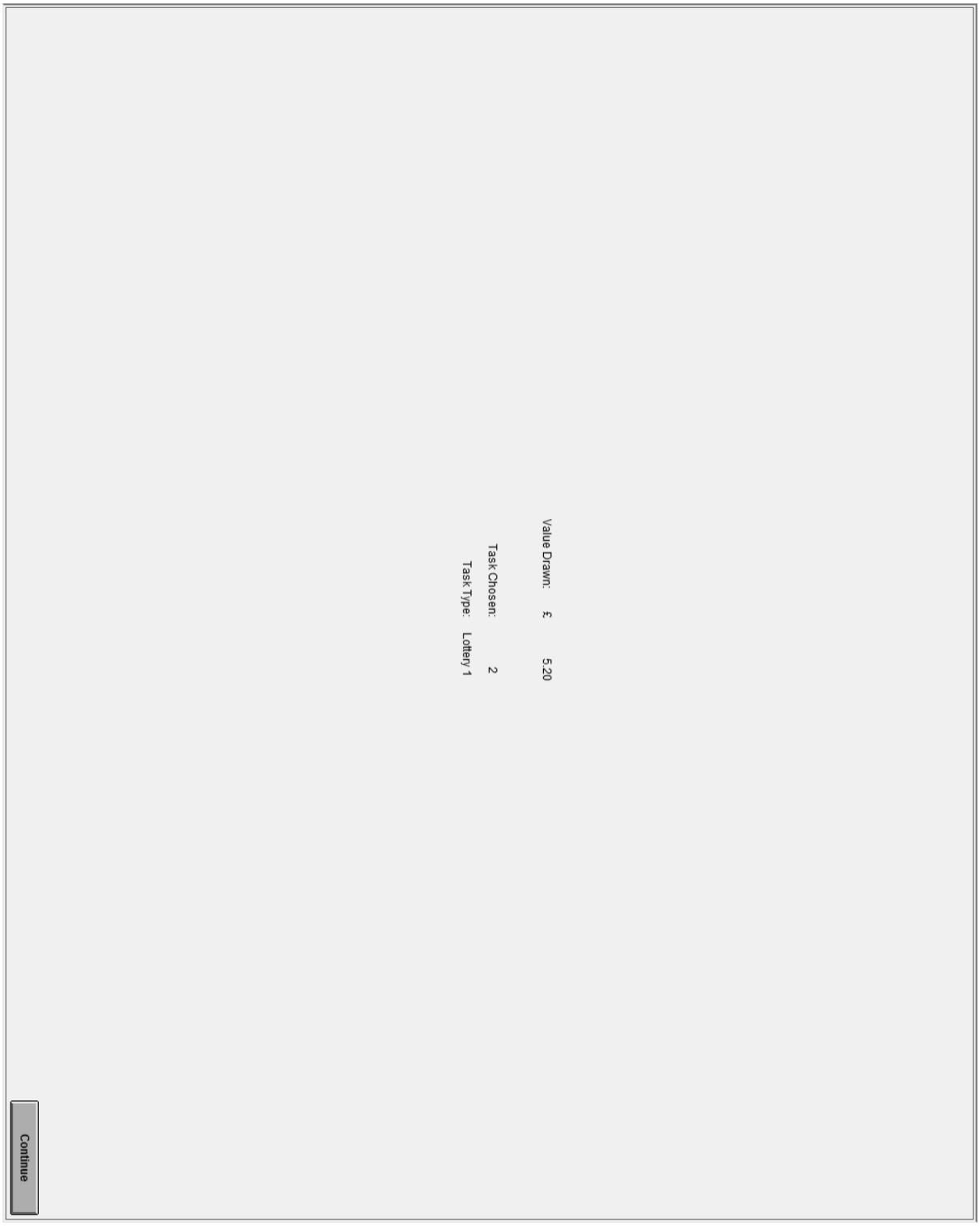
2.10.4. Lottery task decision page (lottery task (i))



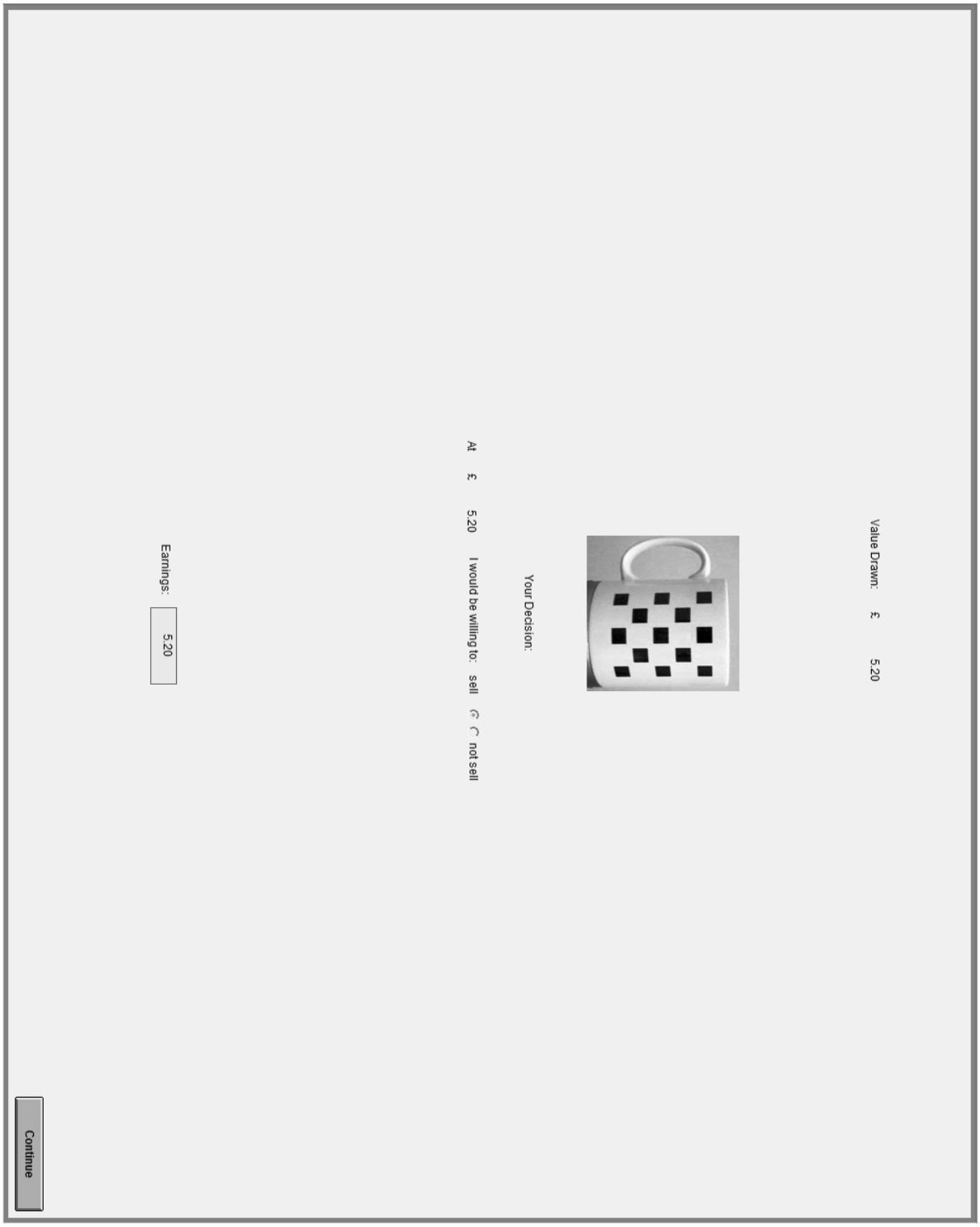
2.10.5. Summary page valuation and task outcome- goods task (squares)



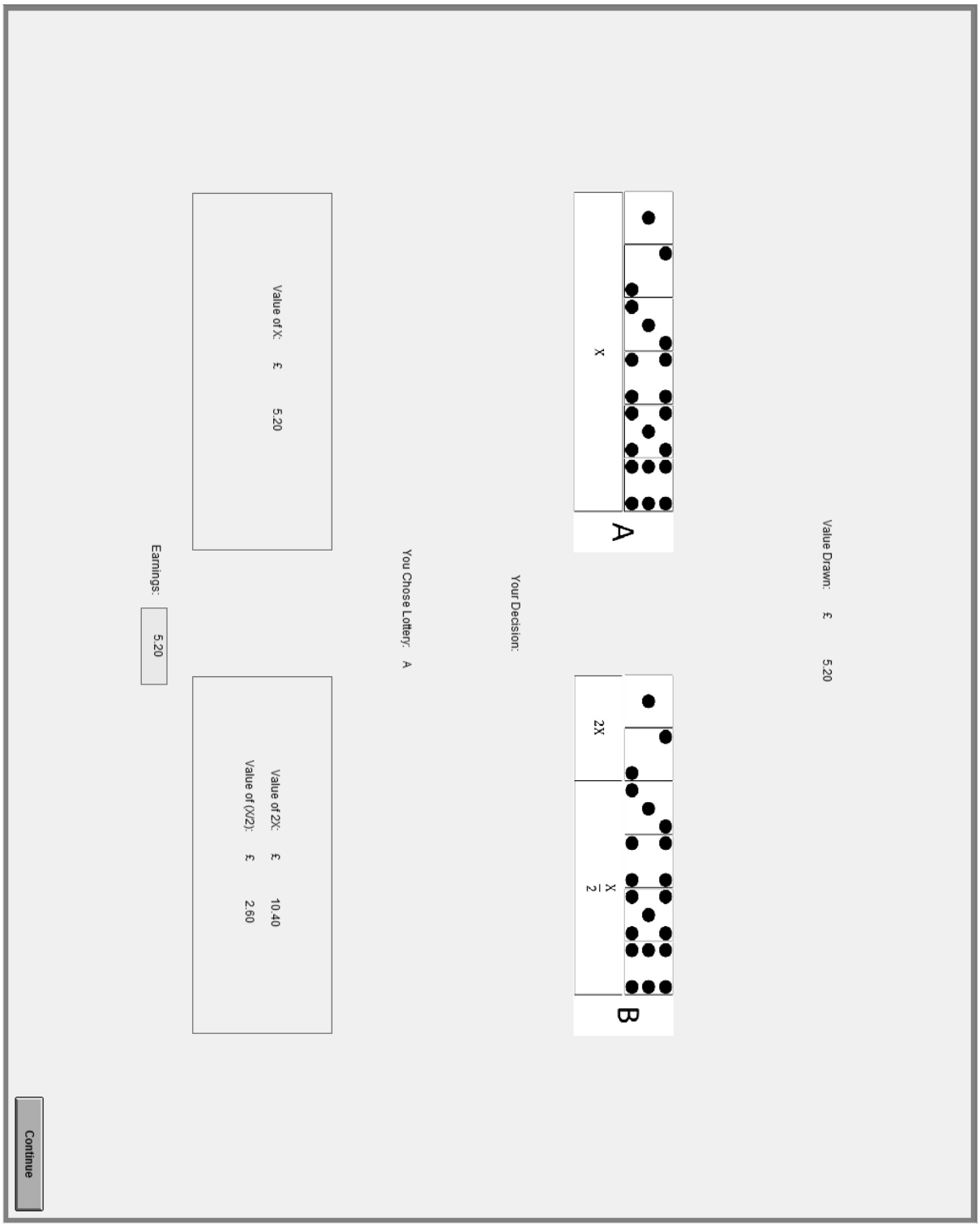
2.10.6. Summary page valuation and task outcome- lottery task (i)



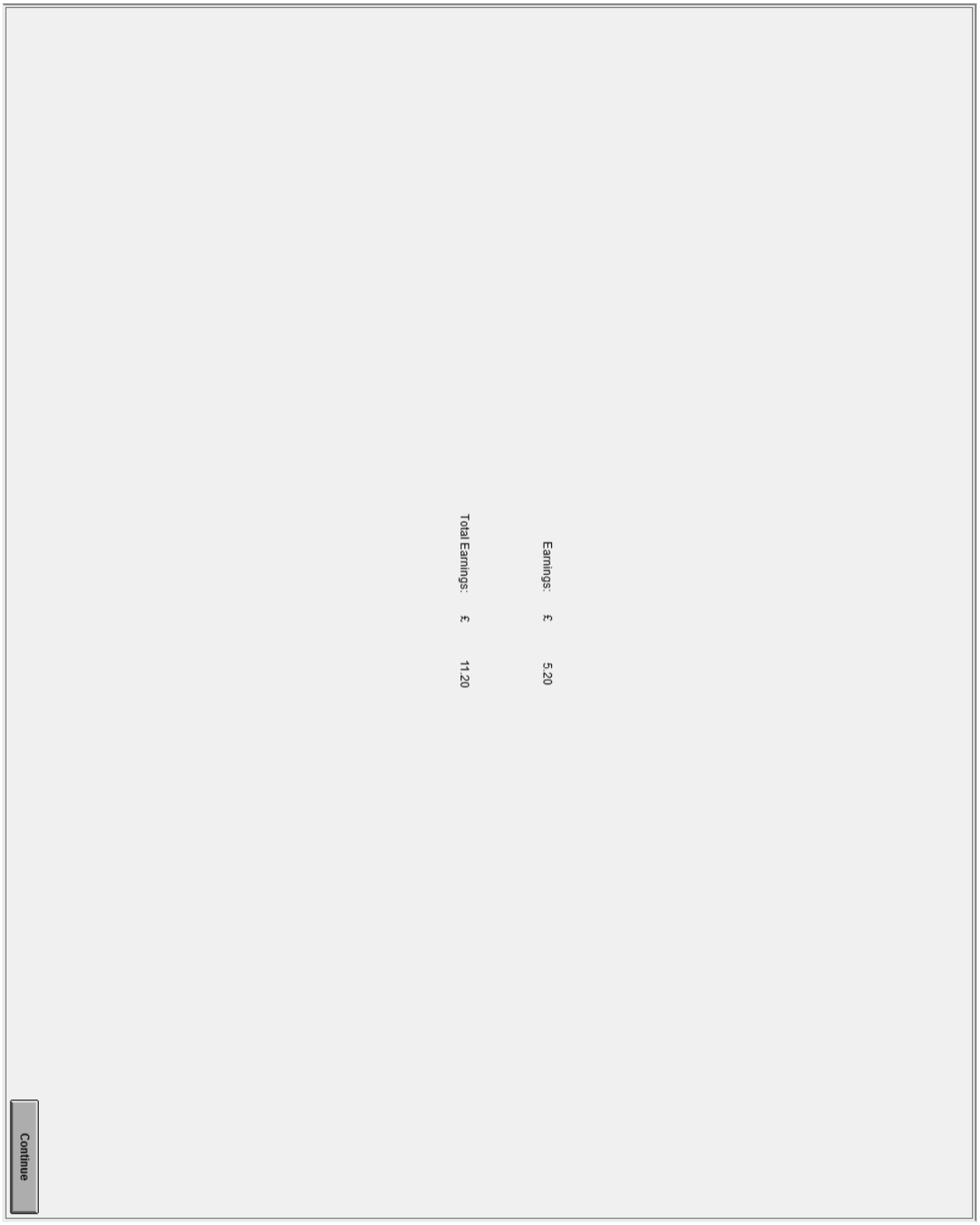
2.10.7. Experiment outcome summary page- goods task (squares)



2.10.8. Experiment outcome summary page- lottery task (i)



2.10.9. Final earnings page



Chapter 3

The Value of Choice in Charitable Giving: A Novel Experimental Design

1. Introduction

It is an unavoidable fact that choice plays an enormous part in the decisions of economic, social and political activity in our day-to-day lives. The clothes we buy, the friendship circles we mix in and the political parties with whom we associate ourselves with are determined largely through our freedom to choose such things. The extent to which we value this freedom to choose, however, has been subject to debate in the economic, psychology and philosophy literature. This paper reports the results of an experiment that uses a novel experimental method to attempt to capture a real monetary value on the positive effect of choice on the levels of donations to charitable causes.

The potential benefits of choice (relative to no-choice) are well documented in the literature (see Zuckerman *et al*., 1978, Iyengar and Lepper 2000, Botti and Iyengar, 2004, Eckel *et al*., 2017, Helms *et al*., 2012, 2013, Mulder and Joireman, 2016, for example), and may be categorised into two distinct categories. The first is that the act of choosing itself yields a positive intrinsic value (I refer to this as a *pure choice* effect), and this is the effect cited commonly in the literature. The second is that allowing choice among a set of goods, rather than simply giving one good, increases the likelihood that a preferred alternative may be selected (I refer to this as a *preference* effect).

Whilst the prior experiments of choice versus no-choice suggest their findings of a positive choice effect are the result of a pure choice effect, I argue the design of these experiments make it impossible to determine whether preference effects are inflating these findings. This experiment attempts to improve on these designs by holding preference effects constant across both choice and no-choice treatments, allowing any remaining difference between treatments to be more confidently attributed to a pure choice effect.

The fundamental way that this is achieved is by implementing a novel design which attempts to ensure a comparison is made between a chosen charity in the choice treatment, and a charity a participant *would have* chosen in the no-choice treatment, had they instead been given the opportunity to choose between the same alternatives.

Explaining what motivates individuals to donate to charities has been well-studied in the past, not least because very simple models of individual rationality could predict that no one would ever willingly donate money to charity (e.g. Sugden, 1982, Dawes and Thaler, 1988). Private donations to charitable causes however is big business, with £9.7bn being donated in 2016 in the UK alone[11]. In the same year, there were over 167,000 registered charities in the UK eligible to receive such donations[12]. Choice, therefore, is inherent in deciding which causes to donate to.

The results of this paper suggest evidence in favour of a pure choice effect for charitable donations. That is, allowing participants to explicitly choose the charity they wish to donate to significantly increases both the likelihood to donate and actual amount donated, when controlling for preferences across choice and no-choice treatments. These findings present exciting potential for the role of choice not only in the domain of improving donation contributions to charitable causes, but also in public policy formation.

The rest of the paper proceeds as follows. Section 2 discusses the existing literature that considers experimental tests of both choice effects and willingness-to-donate to charities, and discusses potential issues of these prior designs. Section 3 describes the experimental design in this paper, and section 4 outlines the key hypothesis to be investigated. Section 5 presents the results and section 6 provides a discussion of the implications of these findings in terms of charitable giving and in public policy formation. Section 7 concludes.

2. Choice Effects and Charitable Giving in the Literature

Imagine you are shopping for a new shirt. There are two Stores, A and B. Store A only offers a shirt in one colour (say, white), whilst Store B offers the same shirt in white *and* blue. Store A caters just fine to those who prefer white shirts, but clearly it falls short for those who prefer

[11] Charity Aid Foundation, 2017, *CAF UK Giving*, [online], Charity Aid Foundation, Available at: <https://www.cafonline.org/docs/default-source/about-us-publications/caf-uk-giving-web.pdf> [Accessed 21/04/2017]

[12] Charity Commission, 2016, *Find Charities: Sector Overview*, [online], Charity Commission, Available at <http://apps.charitycommission.gov.uk/ShowCharity/RegisterOfCharities/SectorData/SectorOverview.aspx> [Accessed 21/04/2017]

their clothing to be colourful. If I prefer white shirts, I am indifferent between the white shirt offered in Store A, and the same white shirt I would choose in Store B. If I prefer blue shirts, however, I would be much happier to be able to choose my preferred blue shirt in Store B than if I was offered only a white shirt in Store A.

If consumers have heterogeneous preferences, then choice in Store B will increase the possibility that more consumers are able to purchase the shirt they prefer, compared with offering only one of these options; a preference effect. Dowding and John (2009) suggest that the very nature of competitive consumer markets, offering heterogeneous services and specialising in different types of products, quality and price, promotes the effectiveness of choice in exactly this way.

The second benefit of choice is more commonly cited in the literature- that allowing a participant to make an active choice in Store B yields some positive outcome, a pure choice effect, and this can be attributed to a number of social psychology, philosophical and economic findings. Choice has been hypothesised to increase intrinsic motivation toward task completion (Deci, 1975, Zuckerman *et al.*, 1978, Deci and Ryan, 2000), increase satisfaction towards chosen goods (Iyengar and Lepper, 2000, Botti and Iyengar, 2004), or generate a sense of emotional attachment towards chosen goods (Gawronski *et al*., 2007).

Potential explanation of this positive effect of choice include an improved feeling of autonomy in decision making (Sugden, 1998, 2003, Mulder and Joireman, 2016), i.e. the more a person chooses, "the more his life is *his*" (Sugden, 1998, p.311), the positive effects of using the mental faculties required to choose (Sugden, 1998), or as an avoidance of the unpleasant sensation of cognitive dissonance, and so to reinforce the choice decision as the correct decision (Brehm, 1956, Festinger, 1957).

Another possible explanation of the positive effect of choice, is that this results from the increased attention given to chosen options. The effect of attention has previously been given as an alternative explanation for the well-known endowment effect (Carmon and Ariely, 2000, Nayakankuppam and Mishra, 2005), whereby when potential sellers are asked to value the goods they stand to lose, they focus their attention on the attributes of what they stand to lose- the good, and so increase their valuation of these goods relative to buyers, who do not focus their attention on these attributes.

In terms of the effect of attention on choice, being presented with a set of choice options encourages individuals to consider the relative positive and negative attributes associated with

each potential choice, as part of the decision-making process (Basu and Savani, 2017). Given a utility maximising individual, a chosen good can be assumed to be the good which presents the greatest positive attributes. In Store B, I am forced to consider the differences between the two shirts- their colour. If I prefer the colour blue over white, all things being equal, I choose the blue shirt.

When asking subjects to value this chosen good, the salience of such positive attributes should be prominent- as participants have spent some mental exertion in determining that these attributes are the most positive, and so this may increase valuation. Because I have considered my preferences for shirt colours, it is salient to me that I like blue shirts, therefore I particularly value that the shirt is blue.

Compare this to a scenario in which the same good is simply given to participants; suppose I am only offered the blue shirt in Store A. Despite the good being the same, without an active choice, the prominence of these same positive attributes is less significant as no comparison and choice was required. The attention given to the attributes of this good, and so its valuation, does not increase. Since I have not been asked to consider the potential for different coloured shirts, I have not had to consider that I prefer blue shirts to white, and so I do not value so highly being able to purchase a blue shirt.

Conflicting findings in the literature suggest that attitudes to choice might not always be positive. Differences between choice options must be both meaningful (Sugden, 1998) and amongst attractive alternatives (Botti and Iyengar, 2004) in order for choice to be beneficial to consumers. The lure of choice may lead participants to choose something they might otherwise have not selected (Bown *et al*., 2003). Excessive choice has reduced satisfaction and purchase likelihood, relative to limited choice (evidence of choice overload, e.g. Scheibehenne *et al*., 2010), and such findings have been attributed to increased information costs (Dowding and John, 2009), dissatisfaction and regret about non-chosen alternatives (Iyengar and Lepper, 2000), increased choice complexity (Mogilner *et al*., 2008), or a requirement of choice justification (Scheibehenne *et al*., 2009). When choice involves unequal outcomes for multiple parties or outcomes depend on chance, it may be preferred that others make the choice (Beattie *et al*., 1994), and if option similarity increases, an opportunity to avoid choice may be preferred (Dhar, 1997).

2.1. Potential Issues of Experimental Studies of Choice Effects

A positive effect of choice has been measured in the prior literature across a number of dimensions, including its effect on motivation for completing puzzles (Zuckerman *et al*., 1978), satisfaction and purchasing behaviour for chocolates (Iyengar and Lepper, 2000) or yoghurt flavours (Botti and Iyengar, 2004), hypothetical insurance purchases (Szrek and Baron, 2007) and real food consumption decisions across children (Zeinstra *et al*., 2010, Dominguez *et al*., 2013, de Wild *et al*., 2015) and adults (King *et al*., 2008, Parizel *et al*., 2017).

In these experiments, a number of different outcomes are used to measure an effect of choice, relative to some defined 'no-choice' condition. Any difference in outcomes across treatments has typically been attributed as a pure choice effect. I argue that these studies do not properly take into account the potential preference effects confounding these findings in their experimental designs. Whilst a choice treatment is relatively straightforward in these studies (i.e. participants are presented with a selection of potential options, and asked to choose from these), the specific experimental protocol of determining the 'good' a participant receives in the no-choice treatment generally differs. The simplest strategy is to randomly distribute to non-choosers the potential goods in the choice treatment (King *et al*., 2008) but this does not take into account the preferences of non-choosers.

Findings of choice effects in this design could simply be attributed to the fact that choosers receive the good they most prefer, whilst non-choosers receive something that they might not have chosen for themselves, if they were instead choosers. Think back to our consumer shopping for shirts, and imagine consumers are being asked to place a value on their potential purchases. Store B mimics a choice treatment, allowing consumers to choose between a white or blue shirt. It is assumed, given this freedom to choose, that consumers in Store B will select their preferred coloured shirt. Store A mimics a no-choice treatment, where one colour shirt is arbitrarily assigned to a consumer. Suppose the shirt assigned to a consumer in Store A is not the colour they prefer (e.g. they are given a white shirt, where instead they would prefer to choose a blue shirt).

Any differences between the valuations of a chooser and a non-chooser may be a combination of both the positive effect of the act of choice (a pure choice effect) and the fact that choosers have received their preferred shirt, and non-choosers may not (a preference effect). It is therefore an oversight to conclude that differences between choice and no-choice treatments in experiments utilising this design is purely an effect of the positive act of choosing.

A common strategy, argued to improve on simply random distribution of options to non-choosers, is the use of a yoked design (Zuckerman *et al*., 1978, Iyengar and Lepper, 2000, Botti and Iyengar, 2004). In a yoked design, non-choosers receive the same good selected by a randomly matched chooser in the choice treatment. This has an advantage over purely random distribution of goods for non-choosers, as it ensure that the distribution of goods selected is identical across the two treatment populations. That is, if 75% of choosers chose a blue shirt, 75% of non-choosers are given a blue shirt. However, at the individual level it is still possible that the good a non-chooser receives is not the good that he or she personally would have chosen if they had instead been given the opportunity to choose.

The use of pre-experimental preference surveys have been used in other designs to attempt to mitigate this issue. Choice options were determined as a subset of goods that were ranked similarly in a larger set, either at the individual (Zeinstra *et al*., 2010) or population (Dominguez *et al*., 2013, Parizel *et al*., 2017) level. However, the goods distributed for non-choosers were still randomly determined among these smaller subsets of similarly preferred options.

Other possible strategies have included differing the specific goods available in choice and no-choice conditions across different days, to econometrically control for any differences in preferences for specific goods (de Wild *et al*., 2015), although this does not remove potential preference effects across any given choice and no-choice comparison in isolation. Another strategy is to attempt to pre-determine the choice that non-choosers will make, by making the choice decision between an almost-dominant and almost-dominated option. Here, rational choosers are expected to select the almost-dominant option, and this is compared with the decisions of non-choosers toward this option (Szrek and Baron, 2007). This trivialisation of choice, however, is not reflective of the idea that choice is meaningful and subjective to any individual consumer.

The experiment in this paper is designed to hold constant any preference effects between a no-choice and choice treatment, to determine whether a difference remains which may be more confidently attributed to a pure choice effect. If a pure choice effect does indeed exist, the prior literature directs the conditions required to maximise the possibility of observing it, in a number of ways: i) there must exist meaningful differences in attributes between choice options, ii) these different attributes must be largely viewed as positives, iii) choice sets must not be excessively large and iv) a comparison must be of the good a chooser chooses from a set of

options, with the good a non-chooser receives being the good they would have chosen if they had instead had the opportunity to choose from the same options.

2.2. Motivations of Charitable Giving

Whilst the research surrounding charity and choice effects is relatively sparse, there has been a great deal of research that considers different effects on charitable giving, including the 'warm-glow' of giving (or impure altruism (Andreoni, 1990), level of earning (Erkal *et al*. 2011), effect of guilt arousal (Hibbert *et al*., 2007), engagement with fundraisers (Andreoni *et al*., 2017) physical or emotional closeness to a cause (Eckel *et al*., 2007) or level of matched donations by an external source (Karlan and List, 2007).

Recent developments in donation marketing have utilised choice in charity appeal strategies. Many UK supermarket stores now allow consumers to determine which of a number of local charities they wish that store to donate a fixed donation to. Shoppers receive a token with their purchases and then vote for their preferred charity by placing their token in the box of that charity. After a determined period of time, the store splits a fixed donation amount according to the proportion of tokens in each charity's box- that is, the more tokens a charity receives, the larger the share of donation they receive. These charity boxes have been argued to increase a closeness of relationship between supermarkets and their consumers (Shaw, 2012).

Another relatively new method of charity giving is through the use of charity gift-cards, where recipients receive a pre-determined amount of money as a gift, which they can then choose to donate to a wide range of charities. Such choice through gift-cards may increase feelings of relationship between donators and those helped (Mulder and Joireman, 2016).

In both of the above scenarios, donation amount and choice are made exogenously; the supermarket (or the gift-giver) determine the amount to be donated, and the consumer (or the gift-recipient) make the charity choice. In a study unrelated to the effect of choice on donations, Reinstein and Riener (2012) find little difference in donation rates across treatments which allow participants to choose to donate between either two or three different charities. This paper aims to test whether positive effects of choice, when compared to no-choice, can translate into quantifiable monetary increases in willingness-to-donate.

Research on restricted gift-giving offers some insights into choice effects on charitable giving (e.g. Eckel *et al*., 2017, Helms *et al*., 2012, 2013, Mulder and Joireman, 2016). In these experiments, a comparison is made between donating to a general charity or cause (unrestricted

giving), versus donating to a more specific cause within that general charity or cause (restricted giving). Relative to unrestricted giving, restricted giving has been shown to increase willingness-to-donate (Eckel *et al*., 2017, Helms *et al*., 2012, 2013) and increase feeling of autonomy towards the chosen cause (Mulder and Joireman, 2016). Since what exactly is being donated to by definition differs across treatments, this is not a measure of an exclusive choice effect across treatments, however.

2.3. Charitable Giving and Public Policy

In an analysis of attitudes towards different forms of the recently popularised *nudging* (i.e. "choice architecture that alters people's behaviour in a predictable way without forbidding any options or significantly changing their economic incentives" (Thaler and Sunstein, 2008, p.6)), the use of 'one-click donations' (where online retailers invite consumers to make a donation to a certain charity at the click of a button- often when payment information is already stored) was least supported amongst thirteen popular methods of nudging (Jung and Mellers, 2016). This could at least be in part to the removal of autonomy and choice in deciding whether or not to engage with the act of donating to a certain charity. If so, finding strategies which increase the feeling of autonomy and ability to choose which charity to donate to, may increase the acceptance and take-up rate of these donation strategies in real world environments.

Evidence of the positive effects of choice on charitable giving is not only beneficial to fundraising organisations, but also in the domain of public policy. First, since many charitable causes concern issues that are also of concern to government (e.g. social welfare or overseas aid), discovering ways to increase willingness-to-contribute to these charitable causes may reduce the financial burden of governments seeking this funding through mandatory taxation.

Second, charitable donations mirror many aspects of giving to a public good. A significant difference between donating to charity and payment towards public goods (typically through taxation), is that charitable giving is a largely voluntary act, whilst payment of taxes is generally compulsory. If increasing opportunities to choose in the domain of charitable donations increases willingness-to-donate, then this may encourage the role of choice in public policy. The potential benefits of this are numerous. Financially, a positive effect of choosing may generate an increase in willingness-to-pay taxes, and similarly reduce the tendency of attempted evasion of paying taxes. Additionally, increasing a feeling of autonomy in taxation decisions may leave citizens happier with paying taxes by contributing more to the decision in how public finances are spent.

One existing example of choice in public good giving is of the historic Italian tax "*otto per mille*" (meaning eight-per-thousand)- whereby citizens choose to give 0.8% of their taxes to either a specific religious body of their choosing, or to give it to the state for general use in humanitarian projects (Introvigne and Stark, 2005, pp.5-6). Such a strategy is likely to increase the impact a public policy has on an individual's well-being, since individuals are directly able to influence the recipient of taxation revenue (or at least their proportion of that revenue) based on their preferences and beliefs.

3. Experimental Design

This experiment was designed to test for a pure choice effect on donation decisions for real charities. This experiment measured differences in real charitable donations when manipulating the level of choice of charities participants could donate to. Participants were separated into two treatments, *Choice* or *No-Choice*, in a between-subject design. Each treatment consisted of two distinct stages, Part One, the preference-elicitation stage, and Part Two, the donation stage.

A total of nine different, real charities were selected. These were selected following a pre-experimental pilot survey which aimed to find charities that would be relatively well-liked but with differences in individual preferences for each charity[13]. These differed across two dimensions, *area of specialism* (health, social, humanitarian/ environmental) and *locality* (regional, national, global) meaning each charity was unique across a 3x3 specialism-locality matrix. Since the experiment was undertaken at University of East Anglia, located in Norfolk, UK, the regional charities were focused on activities in Norfolk, and the national charities were focused on activities in the UK. Details of each charity, and the information shown to participants, may be found in Appendix 3.2.

3.1. Part One – Preference Elicitation Stage

For both treatments, the process of Part One was identical. Part One consisted of thirteen tasks. In each of these thirteen tasks, participants completed ranking tasks. In these ranking tasks, participants were shown the logos and descriptions of three real charities, and were asked to rank these three charities based on how likely they believed they would be to donate to these charities, relative to one another (by selecting one of three ranking statements, "*most*" "*second most*" and "*least*" likely to donate, for each charity). These ranking decisions were hypothetical

[13] Details of the pre-experimental survey can be found in Appendix 3.1.

decisions and particular responses were not incentivised. For completing Part One, participants received a flat rate of £5, irrespective of the answers they gave. Not incentivising specific responses was deliberate in an attempt to generate truthful attitudes of participants and remove any potential strategic behaviours to try and give a 'correct' answer. Indeed the experimental instructions for Part One read "*[w]e are interested in your attitudes towards different charitable causes…there are no right or wrong answers to these…"*. However, participants did have to make a ranking decision for each charity in each task, and each ranking statement had to be unique in each task, i.e. there could be no ties of ranking statements.

Whilst it could be argued that a lack of incentives gave participants little reason to take these decisions seriously, participants were encouraged to take these decisions seriously. A series of consistency measures throughout the experiment enabled a measure of the degree to which participants were consistent across these decisions. The results from these (discussed in section 5.2) suggest participants did indeed take these decision tasks seriously, and so in general, preferences can be determined from these decisions in Part One.

These ranking exercises completed in Part One were fundamental in generating the preference rankings of participants. With nine separate charities, in the first twelve tasks each charity appeared in four tasks, meaning every charity was ranked once against each and every of the other eight charities. The order of these twelve tasks were randomised across participants. The thirteenth task was a randomly repeated version of one of the first twelve tasks, as a test of consistency between ranking preferences across these identical tasks. Whilst it was possible that the thirteenth task might have been a repeat of the immediately previous twelfth task, the experimental instructions informed participants that they may see the same charities in multiple tasks, and as such, this unlikely occurrence should not have appeared untoward for participants.

3.2. Part Two – Donation Stage

Once Part One was complete, Part Two was explained, and differed fundamentally depending on whether a Choice or No-Choice treatment. At the beginning of Part Two, participants in both Choice and No-Choice treatments were informed they were to be allocated £5, of which they could donate as much or as little as they wished to their chosen (Choice) or given (No-Choice) charity. Recall participants had already earned £5 from Part One, but donations could only come from the £5 allocated in Part Two.

3.2.1. Choice Treatment

In the Choice treatment, Part Two began by one of the previous tasks in Part One being randomly selected and repeated for each participant[14], with the three charities being shown on participants' computer screens. However, instead of ranking the three charities, in this instance participants were asked to choose which charity they wished to have the opportunity to donate to. This allowed for the act of choice in the Choice treatment. Once participants had made their choice, the relevant donation page for their chosen charity was shown. Participants were not informed this was a repeated task, and the design of Part One, with twelve unique ranking tasks, was designed to be sufficiently varied such that participants would not recall their actions in this task, meaning decisions in Part Two would be independent to decisions in Part One, but following consistent preferences. The experiment made no attempt to uncover the specific reasons as to why a participant would choose one charity over another, but it relied on an assumption made here that participants would choose the charity which they ranked most highly in the equivalent task in Part One.

3.2.2. No-Choice Treatment

In the No-Choice treatment, recall the aim was to remove the act of choice, but still give participants the opportunity to donate to the charity they would have chosen, if they had instead been given the option to choose from a given set of charities. This was achieved in the following way. As in the Choice treatment, one of the previous tasks in Part One was randomly selected for each participant. However, this was not revealed to participants. Instead, the charity from that randomly selected task which participants had ranked most highly in that task in Part One was automatically assigned to that participant. Participants were simply informed that this charity had been selected for them. Once participants were made aware of this selected charity the relevant donation page was shown.

What is of crucial importance in the No-Choice treatment is that the charity selected for participants is the one that their responses in Part One indicated they would have chosen, had they instead been given the opportunity to choose from this equivalent task. Note, if that is the case, then the individual preferences of participants are respected across both the Choice and

[14] Of the twelve unique tasks in Part One, one was repeated as the thirteenth task, to be a consistency check across ranking tasks. This repeated task in Part One was omitted from the possible tasks to be randomly selected as the repeated task in Part Two.

No-Choice treatment. Thus, any differences in valuations between these two treatments can therefore be more accurately attributed to that of a pure choice effect.

One potential concern in the No-Choice treatment was that participants would remember that they had ranked highly the charity selected for them in Part Two, when completing the ranking tasks in Part One. Since participants completed twelve unique ranking tasks in Part One (with any one charity only featuring in four of these), it seems unlikely that participants would be able to recall their ranking decisions for all specific charities, however.

3.2.3. Donation Page

In both Choice and No-Choice treatments, once a charity was determined as the one the participant could donate to, that charity's donation page was shown to participants. On this donation page were 27 possible donation amounts; ranging from £0.00 to £5, meaning that participants could donate none, some or all of their allocated £5 to charity. It was made clear to participants that whatever they chose to not donate would be added to the £5 earned in Part One to form their final earnings.

One might expect a purely self-interested participant to be inclined to donate nothing and earn a maximum £10. Whilst previous experimental literature and a pilot study suggested participants would donate to some degree, in order to encourage donations, and following evidence of the positive impact of a donation matching protocol on donation levels (Karlan and List, 2007) a £0.10:£0.10 donation matching protocol was implemented. This meant that for every £0.10 a participant donated, the experimenter would match that donation to the same charity. When participants were making their donation decisions, full information at each donation amount (i.e. the amount donated to charity by the participant, the amount matched by the experimenter and the amount the participant would take away with them) was given to participants on their screens. Donation decisions were made electronically at participants' isolated computer booths by selecting and confirming the desired donation amount on their screens. Once the experiment was completed, particular care was taken to ensure participants were not able to observe the donation amounts by other participants, to maintain the anonymity of participants' decisions. Upon receiving payment, participants were also given instructions of a website and email link from which they would be able to request the confirmation of donations made by the experimenters once all experiments were complete, if they so desired, to ensure the authenticity of their donation decisions.

3.3. Implementation

The experiment took place in late 2016- early 2017 at the University of East Anglia's Centre for Behavioural and Experimental Social Science (CBESS). Participants were recruited through the centre's online recruitment system and were relatively inexperienced; due to concerns that participants who had taken part in many prior experiments would be less likely to donate, all participants had participated in three or fewer experiments, and none had previously participated in experiments of this type. Due to the types of certain charities used (regional charities in Norfolk, and national charities in the UK), it was determined that all participants recruited would be students at the University of East Anglia (who therefore had at least some association with the regional charities) who identified as UK nationals (so would have at least some association with the national charities). The experiment was conducted using experimental software package z-Tree (Zurich Toolbox for Ready-made Economic Experiments) (Fischbacher, 2007), and participants undertook the experiment in isolated computer booths.

Both treatments included pre-experimental quizzes, where participants were tested on their understanding of experimental procedures. Participants who answered incorrectly were asked to review the relevant instructions before attempting the question again. 93.1% of all questions were answered correctly at the first attempt, and 99.7% were answered correctly after two attempts, suggesting participants understood the mechanisms of the experimental design.

4. Hypotheses

The design of these experiments allows for a simple measure of choice effect, perceived as differences in valuations across Choice and No-Choice treatments. The hypothesis of a positive effect of choice is not a new one, although the design of this experiment is likely to yield a more conservative estimate than previous findings, given the control in design.

This paper does not attempt to isolate one specific cause of a choice effect, but a null hypothesis predicts no difference between donations with or without an effect of choice, given an experimental design that ensures all participants receive the charity they would have (or actually had) chosen from a given set. An alternative hypothesis predicts the following difference in average donations of individual (i) (D_i) for treatments No-Choice (NC) and Choice (C):

Alternative Hypothesis (H1) – Choice Effect - Charitable Donations

For all i $D_{iNC} < D_{iC}$

5. Results

A total of 120 participants took part in this experiment. Table 3.1 below outlines the summary statistics for both treatments.

Mean donations were greater in Choice than No-Choice treatments. Charity donations in No-Choice were on average £2.12, and in Choice donations were on average £2.83, an increase of approximately 33.5%, and this difference in average donations was statistically significant (*t*-stat= 2.104, *p*= 0.038). Median results report similar findings and significance levels. It was possible for donations to be £0.00, £5.00, or somewhere in between. If choice were to have a positive effect on responses, then it would be expected that incidences of £0.00 donations ought to be less common in Choice than in No-Choice. This is the case, where 7 participants donate nothing in Choice, and 15 do so in No-Choice); a chi-squared test (*p*= 0.088) suggests that this difference in distribution of no-donation is non-random. Similarly, the positive effect expected of choice should yield more maximum £5.00 donations in the Choice treatment than No-Choice. 20 participants in Choice donated their full £5.00 allocation, compared to 11 participants in No-Choice, though this difference is not quite statistically significant (*p*= 0.106).

	Charity Donations	
	No-Choice	**Choice**
n	60	60
Mean (£)	**£2.12**	**£2.83**
t-Test (*t*-stat)	2.104	
p-value	**0.038****	
Median (£)	**£2.20**	**£2.50**
Mann-Whitney (*z*-stat)	1.998	
p-value	**0.046****	
Min (£0.00) (n)	**15/60**	**7/60**
Chi-Squared (χ^2-stat)	2.909	
p-value	**0.088***	
Max (£5.00) (n)	**11/60**	**20/60**
Chi-Squared (χ^2-stat)	2.613	
p-value	0.106	

Table 3.1. Summary statistics by treatment

5.1. Preference Score Effects

The nine charities used in this experiment were selected in the light of a pilot study which suggested that the charities would be generally well-liked, implying that there would not be

large and consistent difference in donation amounts between different charities. Whilst such an occurrence would not affect the overall experimental outcomes of choice effects, it is nonetheless of interest to observe to what extent this held true. Kruskal-Wallis tests reveal that, for both No-Choice ($\chi^2(8)= 7.032$, $p= 0.553$), and Choice ($\chi^2(8)= 12.128$, $p= 0.146$), actual amount donated did not significantly differ across specific charities (see Appendix 3.3).

The design of Part One allowed each charity to be ranked once with each and every alternative charity in pairwise comparisons in the first twelve tasks. From these pairwise comparisons, a preference score can be elicited. If a charity is preferred to all other eight options, it scores a preference score of eight, and if it is not preferred to any of the other eight options, it scores a preference score of zero, and so on- with a higher score implying a higher overall preference.

It would be reasonable to expect that, the higher the overall preference score, the greater the preference for the charity and so the higher the donation decision. Table 3.2 outlines, for each preference score, the average donations of all charities which were assigned that preference score by participants. An extension of a Wilcoxon rank-sum (two-tailed) test, a Cuzick Trend test tests for a consistent trend of donation decisions as preference score increases. A lack of significance for both treatments suggests that overall preference score has little effect on final donation decisions.

	No-Choice		**Choice**	
Score	Mean (£)	(n)	Mean (£)	(n)
0	----	0	----	0
1	----	0	----	0
2	----	0	£5.00	1
3	£3.50	1	£1.23	3
4	£1.54	5	£2.58	6
5	£2.69	10	£3.40	5
6	£2.16	11	£3.43	9
7	£1.76	22	£2.63	14
8	£2.42	11	£2.75	22
Mean (£)	**Cuzick Trend Test**		**Cuzick Trend Test**	
	z-stat	0.260	*z*-stat	-0.010
	p-value	0.799	*p*-value	0.991

Table 3.2. Mean donations by preference score

5.2. Consistency Effects

The viability of these experiments as an effective measure of a pure choice effect was predicated on an assumption that the choices made by participants in Part Two of the Choice treatment were consistent with the decisions made in the corresponding ranking task in Part

One. One potential concern with the design of this experiment was that participants' decisions in Part One tasks were not explicitly incentivised, and so they were free to answer these as truthfully, or consistently, as they wished.

Consistency Measures and Test Statistics	**Task Choice Consistency**	**Task 13 Consistency**	**Questionnaire Consistency**	**Transitivity Violations**
Actual Consistency / n	50/60	104/120	106/120	419/10080
Expected Consistency / n	*=20/60*	*=40/120*	*≈27/120*	*=2520/10080*
Actual Consistency %	83.3%	86.7%	88.3%	4.2%
Expected Consistency %	*33.3%*	*33.3%*	*22.2%*	*25.0%*
χ^2-statistic	χ^2= 67.500	χ^2= 153.603	χ^2= 303.450	χ^2= 2335.556
***p*-value**	***p*< 0.001***	***p*< 0.001***	***p*< 0.001***	***p*< 0.001***

Table 3.3. Results of tests for a number of experimental consistency measures

Table 3.3 outlines results of a number of consistency measures which tested whether participants were consistent in their ranking and choice decisions throughout the experiment. The table also reports the results of chi-squared tests for each consistency measure, which compare observed consistency levels with a baseline expected consistency, if participants simply responded randomly to all tasks in this experiment- a concern as a result of the lack of incentivised preference tasks in Part One.

Task Choice consistency measures the proportion of Choice participants who chose the charity in Part Two they ranked most highly in the corresponding ranking task in Part One. Over 80% of participants revealed such consistency. If it was assumed all participants answered each ranking and choice task randomly, a *Task Choice consistency* of 33.3% would be expected. This implies two things. First, participants did appear to take these tasks seriously. Second, this implies that in No-Choice, the charities assigned to participants in Part Two (based on the charity they had ranked most highly in a corresponding task in Part One) is an effective approximation of the charity they would have chosen, had they instead been given the opportunity to choose (since this is indeed what the vast majority of Choice participants did). Therefore we can conclude, with some confidence, that any difference between donation decisions between No-Choice and Choice, can be attributed to a pure choice effect.

For robustness, additional consistency measures were included in the experimental design. Recall Task 13 in Part One was simply a randomly repeated ranking task of one of the first twelve. This allowed a test of *Task 13 consistency*, testing if participants rank the same charity most highly when the same three charities were shown in a repeated task. If ranking decisions were random, a consistency of 33.3% would be expected- actual consistency was 86.7%. A

post-experimental questionnaire asked participants their two most preferred charities of the entire set of nine, intended to measure whether their perceived preferences across the set was consistent with their preference scores from the ranking tasks. If one of these two selected charities was consistent with the charity with the highest preference score, *Questionnaire consistency* was achieved. Random selection in this questionnaire would suggest a consistency of 22.2%- actual consistency was 88.3%.

By comparing individual participants' rankings across the first twelve tasks in Part One, it is possible to construct a measure of the tendency to violate transitivity. Transitivity implies that in the ranking of three options across ranking tasks (e.g. Charity A, B and C), then if $C_A > C_B$ and $C_B > C_C$ in the ranking tasks (i.e. Charity A ranked higher than Charity B, and Charity B ranked higher than Charity C), then it must be that $C_A > C_C$ (i.e. Charity A ranked higher than Charity C). In total, with nine options of charities, there existed 84 possible *ranking-trios* of charities for each participant. Given, for any ranking-trio, there are eight possible ranking orderings, two of these imply a violation of transitivity (see Appendix 3.4 for a proof of this). Frequency of *Transitivity violation* under random ranking decisions would be expected at 25.0%- actual violation frequency was 4.2%. Indeed 26.7% of participants revealed no transitivity violations across all 84 ranking-trios. All of the above consistency tests imply the same conclusion; that participants took these ranking tasks seriously, and were generally extremely consistent in the ranking decisions they made.

5.3. Regression Analysis

Whilst non-parametric analysis of the summary statistics suggest evidence of a choice effect on charitable donations, the use of parametric statistical analysis will enable the inclusion of a number of other factors, including demographic effects, preference score and consistency measures, to better understand the effect that choice has on donations. In running a regression analysis, it is important to recognise the effect that the upper and lower censoring of results may have on the dependent variable (donation amount). Given that participants could not donate less than £0.00 (a natural censoring) and could not donate more than £5.00 (an imposed censoring within this experiment), then those who donated at these limits may have true donation values that exceed or fall below these limits. Analysis of the distributions of donations (see Appendix 3.5) suggests that both left and right censoring has generated greater distributions at these limits in than would typically be expected by normal distribution. Thus any regression analysis would need to account for these censoring limits.

In Models 1 to 4 of Table 3.4, a Tobit model is used, allowing for censoring at both the lower and upper limits (i.e. at £0.00 and £5.00). Models 1 and 2 measure the effect of a variety of demographic and experimental effects for both No-Choice and Choice treatments separately. Models 3 and 4 pool both treatments to include the effect of choice on donation amount. In these models, the dependent variable is the donation reported by a participant. The following independent variables are used:

Choice: takes the value 1 if the participant was in Choice treatment, 0 if No-Choice.

Age: takes the value of the participant's reported age, with the minimum reported age standardised to 0 (i.e. the participants reported age, minus 18).

Age^2: takes the square of variable *Age*, to measure for a quadratic effect of age.

Female: takes the value 1 if the participant's reported gender is female, 0 otherwise.

Economics: takes the value 1 if the participant's reported field of study is Economics, 0 otherwise.

Preference Score: takes the value of the preference score (0, 1, …, 8) of the donated charity.

Task Choice Consistency (for Choice only): takes the value 1 if participants in Choice choose the charity they ranked most highly in the corresponding task in Part One, 0 otherwise.

All Consistency: takes the value 1 if participants are consistent in all other consistency measures (Task 13 consistency, Questionnaire consistency, no Transitivity violations), 0 otherwise.

*Economics*Choice*: takes the value 1 if the participant's reported field of study is Economics *and* if the participant was in Choice treatment, 0 otherwise.

In Models 3 and 4 in Table 3.4, in line with non-parametric analysis, the effect of choice on charity donations is pronounced; participants in the Choice treatment donated significantly more than those in No-Choice, when including other explanatory variables ($p= 0.002$, $p= 0.045$, respectively).

Charity Donations	**Tobit Model** (Lower Limit- £0.00, Upper Limit- £5.00)			
	Model 1 **No-Choice**	Model 2 **Choice**	Model 3 **All**	Model 4 **All**
Choice	------- -------	------- -------	1.9251*** *(0.592)*	1.3112** *(0.646)*
Age	0.1604 (0.337)	0.8765*** (0.326)	0.4625** *(0.215)*	0.4953** *(0.214)*
Age^2	-0.0177 (0.024)	-0.0456*** (0.016)	-0.0261** *(0.011)*	-0.0279** *(0.011)*
Female	0.0712 (0.783)	1.0488 (0.782)	0.7681 *(0.573)*	0.7117 *(0.564)*
Economics	-3.7721*** (1.215)	-0.3699 (0.798)	-1.5563** *(0.679)*	-3.4882*** *(1.213)*
Preference Score	-0.0864 (0.288)	0.0438 (0.281)	-0.1019 *(0.187)*	-0.0804 *(0.184)*
Task Choice Consistency	------- -------	-2.2624* (1.282)	------- -------	------- -------
All Consistency	-1.1437 (0.964)	1.8499* (0.943)	0.5908 *(0.680)*	0.5412 *(0.670)*
Economics*Choice	------- -------	------- -------	------- -------	2.8569** *(1.434)*
Constant	3.1130 (2.088)	3.0963* (1.558)	1.6558 *(1.305)*	1.7477 *(1.282)*
Sigma	2.6507*** (0.361)	2.6400*** (0.369)	2.8124*** *(0.276)*	2.7638*** *(0.271)*
# Obs	60	60	120	120

Standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1

Table 3.4. Tobit models of choice, demographics, consistency and preference score on charitable donations

It is possible that the effect of choice is being generated by different categories of experimental participants. Participants must first decide whether or not they wish to donate anything at all. Tables 3.5 and 3.6 report models which account for these differences. Table 3.5 utilises a Probit model, which observes the effect of a number of variables on the probability that participants will donate something (i.e. an amount greater than £0.00). The dependent variable is the probability that the participant donates a positive amount, greater than £0.00.

As can be seen from the model in Table 3.5, choice significantly increases the likelihood to donate something (p= 0.010).

(Likelihood of donation > £0.00)	Model 5 **Probit Model** **All**
Choice	0.8036** *(0.313)*
Age	0.1180 *(0.113)*
Age^2	-0.0056 *(0.005)*
Female	0.5708* *(0.315)*
Economics	-0.5352 *(0.332)*
Preference Score	-0.0839 *(0.108)*
All Consistency	-0.3083 *(0.330)*
Constant	0.9827 *(0.715)*
# Obs	120

Table 3.5. Probit model of choice, demographics, consistency and preference score on likelihood to donate

If participants do decide they wish to donate something, they then must decide how much to donate. Table 3.6 utilises a Truncated Regression model, truncated at £0.00 (i.e. it only includes in the model the participants who did donate a positive amount), and observes the effects of a number of additional variables, on the dependent variable donation amount, conditional on participants donating a positive amount.

As can be seen from the model in Table 3.6, choice also significantly increases the actual amount donated amongst those who choose to donate something (p= 0.054).

Demographic factors also appear to have significant effects on donations rates. Age has a significant and positive effect on donation levels as each additional year increases donation, although this does so at a diminishing rate. Economics study appears to significantly reduce donation levels. Models 1 and 2 suggest this effect is particularly strong for participants in No-Choice, and this is ratified by the significance of interaction variable *Economics*Choice* in Model 4 (p= 0.049). The coefficients for gender effects suggest that females on average are willing to donate more (though this difference is not statistically significant), and are significantly more likely to be willing to donate some positive amount in the Probit model.

Charity Donations (if donation > £0.00)	Model 6 **Truncated Regression All**
Choice	0.7581*
	(0.394)
Age	0.3103**
	(0.144)
Age^2	-0.0206**
	(0.009)
Female	0.1539
	(0.374)
Economics	-0.6833
	(0.501)
Preference Score	0.0176
	(0.119)
All Consistency	0.8694**
	(0.438)
Constant	1.8491**
	(0.863)
Sigma	1.6267***
	(0.152)
# Obs	98

Table 3.6. Truncated Regression model of choice, demographics, consistency and preference score on charitable donations

The lack of significant effects of preference score on donation, as seen in the summary statistics, is reflected in the econometric models. Given the high level of consistency in participants' preferences, it is perhaps surprising that increased overall preference does not translate into an increase in donations. One potential explanation of this might be related to specific charities used in this experiment. The types of charities used in this experiment were chosen so as to be relatively well-liked (following a separate, pre-experimental survey using different participants), and as such it is possible that absolute differences in preferences across charities were small. Clearly participants did have different preferences for different charities, but it is not necessarily the case that this should translate into to an increased desire to donate.

If goods are evaluated by considering the attributes of a good, then this is enhanced through choice by comparing these differences in attributes (Basu and Savani, 2017). It is possible that, for any unique charity, the vast majority of motivation to donate stems from attributes that are consistent across any charity, for example, the intrinsic motivation of feeling good about donating to a worthy cause. This could explain a lack of relationship between preference and donation, since the differences between charities only forms one small attribute involved in the evaluation of the charity.

When faced with three charity options to choose from Part Two in Choice, participants typically chose in a way consistent with their previous responses. Whilst this choice might form an increased focus on the attributes that differ (the specific charity cause), among those three possible alternatives presented to participants, they are choosing their most preferred relative to those three, irrespective of its absolute ranking amongst the full set of nine charities. This suggests that a pure choice effect is important in increasing donations by making salient to participants that that chosen good is the preferred option from a certain subset.

It is of interest to consider the effect of consistency on donation level. Only participants in Choice could indicate Task Choice consistency, and Model 2 suggests that the 10 participants who were not consistent donated significantly more than those who were. For the remaining consistency measures, all three were pooled, to measure the effect of being fully consistent (i.e. indicating Task 13 consistency, Questionnaire consistency and no occurrence of Transitivity violation). In total 25.0% of participants were fully consistent in these three measures, and pooling both treatments this caused an increase in donations on average, although this effect was not significant in all models.

6. Discussion

Whilst the vast majority of previous experiments test a choice effect in terms of consumable goods, the finding of a strongly significant and positive choice effect in terms of donations to charity is a particularly interesting finding. When participants are able to choose which charity they wish to donate to, their donation on average increases, when compared to those who are instead simply assigned the charity they had previously indicated they would have chosen had they been given the opportunity to choose. Evidently, the act of choice yields positive increases in donation levels.

The potential benefits of choice on charitable giving span a number of potential markets in reality. For fundraisers not particularly interested in which specific charity donations are channelled to, the act of choosing which specific charity yields an increase in donation. The use of cause-related marketing is a common marketing strategy employed by firms, and the types of charities that products tie themselves to can affect the effectiveness of this as a marketing strategy to increase sales (e.g. Strahilevitz, 1999, Strahilevitz and Myers, 1998). These findings suggest that the strategy may be even more successful if brands were able to incorporate an act of choice into this cause-related marketing.

A number of donation strategies currently do not utilise the potential positive effects of choosing. The use of 'one-click donations' or the similar 'check-out donations' (where, when making card payments to settle bills or make payment at retail check-outs, card machines prompt consumers to round-up their payment to the nearest whole number, with proceeds donated to a certain charity), may be strategies to benefit from choice. As highlighted in section 2.3, dislike of these donation strategies may be in part a result of a loss of autonomy; these encourage a shopper to part with their money to a charity exogenously determined by someone else. Offering consumers a variety of donation recipients could encourage an increase in consumer participation to these strategies, as they become more involved in determining to whom their money is directed. Whilst these pure choice effects might increase donations, this strategy might also benefit from preference effects, as allowing choice would increase the likelihood that any one consumer might be presented with a charity that they feel more inclined to donate to.

However, what must be considered is whether the increase in cognitive burden required to make a choice would make increased choice beneficial in these situations. Future experimental study would be beneficial to investigate the trade-off between the benefit of increased choice versus increased cognitive burden in choosing in these situations to measure the effectiveness of choice in real world scenarios.

A wider benefit of the findings of a choice effect on charitable giving is in relation to donations as a form of giving to a public good. Analogously with 'one-click donations', citizens of a given society might feel less engaged with paying their taxes if they have little say in where their personal contribution is spent. The results from this study suggest that choice is indeed valued in financial giving when the giver does not explicitly benefit from such a decision to give. To increase the possibility of choice in public policy formation could potentially be positive, based on these results. Such choice might increase a feeling of engagement in society as each citizen has the opportunity to increase their involvement in the way public money is spent. This might be achieved by allowing choice in just one, relatively small, element of public spending (as in the example of the Italian religion tax *otto per mille*), but this may generate a spill-over of positive feeling of involvement across public spending more generally, and so presents a potential low-cost and low-risk way to improve attitudes to public good contribution, or increase willingness-to-contribute or reduce attempts to evade taxation.

This study does not fully address the potential issues faced by public good giving, such as strategic motivations of who or what to give to. For example, the use of choice in Italian taxation policy *otto per mille*, appears vulnerable to these same potentially problematic nuances of conventional public policy valuation. In 1997, Waldensian-Methodist Churches claimed only roughly 25,000 members, but 127,585 Italians chose this religious faction to receive their *otto per mille* tax (Introvigne and Stark, 2005, pp.10). The reasons behind such a finding are not clear.

Provided Italian tax payers understand the mechanism of the choice design, such apparently inconsistent choices may still be argued as a revelation of preference in that decision, since there is little logical reason to choose something you would not prefer, whatever the reason for that preference. If these preferences include strategic motivations, such as directing your donation to a smaller religious faction because you favour equity, for example, then whether allowing such a choice mechanism yields benefit remains open to debate. This study demonstrates the effectiveness of choice in increasing giving to a public cause, but how this is effectively implemented remains a separate and ongoing issue.

7. Conclusion

The aim of this experiment was to implement a design in which a pure choice effect could be captured, across a scenario relevant to the real world- donations to charitable causes. This paper offered a new methodological design in isolating and measuring a pure choice effect, by attempting to ensure that the charities that non-choosers faced were the options they would have chosen, had they instead been given the opportunity to choose from a set of alternatives.

By isolating this effect, the results of this paper suggest there is evidence of a pure choice effect even when this design is implemented, and explicit choice significantly increases both likelihood to donate and actual amount donated. This paper introduces an attention-based hypothesis to explain choice effects, suggesting that choice encourages closer consideration of the attributes of different options, which in turn leads to a perception of greater charitable donations. The positive effect of choice encourages the increased attempt to utilise the power of choice not only in terms of charitable giving, but also in the formation of public good policies. Whilst more research is undoubtedly required to further the practical application of these findings, these results, under a controlled design, imply a substantial and significant positive association with the act of choosing.

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Appendices

Appendix 3.1. Pre-experimental online pilot survey

3.1.1. List of possible charities used in pre-experimental online pilot survey

Addaction

Amnesty International (Amnesty Int.)

Avert

Beat

Born Free

Break

Crisis

Dementia UK (Dementia)

Leeway

Marine Conservation (MC)

Norfolk Heart Trust (NHT)

PACT Animal Sanctuary (PACT)

Rainforest Concern (RC)

Stop the Traffik (StT)

Water Aid

War Child

3.1.2. Ranking across mixed ranking tasks

1 (best) – 4 (worst)	**Charity**			
Combination 1 (*n=36*)	**NHT**	**Leeway**	**Break**	**PACT**
Average Ranking	2.75	2.17	1.83	3.25
Modal Ranking	3	2	1	4
Combination 2 (*n=37*)	**Dementia**	**Addaction**	**StT**	**MC**
Average Ranking	1.76	2.41	2.32	3.51
Modal Ranking	1	3	3	4
Combination 3 (*n=32*)	**Beat**	**Crisis**	**Amnesty Int.**	**RC**
Average Ranking	2.94	1.67	2.28	3.13
Modal Ranking	4	1	2	4
Combination 4 (*n=33*)	**Avert**	**War Child**	**Water Aid**	**Born Free**
Average Ranking	2.94	1.82	1.73	3.52
Modal Ranking	3	1	1	4
Combination 5 (*n=29*)	**NHT**	**Dementia**	**Beat**	**Avert**
Average Ranking	2.52	1.52	3.34	2.62
Modal Ranking	2	1	4	3
Combination 6 (*n=28*)	**Leeway**	**Break**	**Crisis**	**Addaction**
Average Ranking	2.25	2.46	2.14	3.14
Modal Ranking	2	3	1	4
Combination 7 (*n=25*)	**Amnesty Int.**	**StT**	**War Child**	**Water Aid**
Average Ranking	2.88	2.88	2.2	2.04
Modal Ranking	3	4	2	1
Combination 8 (*n=25*)	**PACT**	**MC**	**RC**	**Born Free**
Average Ranking	2.6	2.72	2.24	2.44
Modal Ranking	4	4	1	3
Combination 9 (*n=17*)	**NHT**	**Crisis**	**War Child**	**Born Free**
Average Ranking	2.88	2.12	1.76	3.24
Modal Ranking	3	3	1	4
Combination 10 (*n=17*)	**Dementia**	**Addaction**	**Water Aid**	**PACT**
Average Ranking	1.53	2.76	2.12	3.59
Modal Ranking	1	3	1	4
Combination 11 (*n=17*)	**Beat**	**Leeway**	**StT**	**MC**
Average Ranking	2.88	2.00	1.71	3.41
Modal Ranking	3	1	1	4
Combination 12 (*n=16*)	**Avert**	**Break**	**War Child**	**RC**
Average Ranking	2.63	2.56	1.50	3.31
Modal Ranking	3	3	1	4

3.1.3. Measurement of extreme preferences in ranking tasks survey of 3.1.2. (by occurrence of modal ranking best (1st) or worst (4th))

Charity	Occurrence of Modal Ranking **1st**	**4th**
NHT	0	0
Break	1	0
Leeway	1	0
PACT	0	3
Dementia	3	0
Addaction	0	1
StT	1	1
MC	0	3
Beat	0	2
Crisis	2	0
Amnesty Int.	0	0
RC	1	2
Avert	0	0
War Child	3	0
Water Aid	3	0
Born Free	0	2

Notes: Shaded charities experienced most frequent extreme preferences and so were determined less appropriate for use. As such these charities were removed, keeping nine intended charities.

3.1.4. Final matrix of locality/ specialism of charities used in experiment

	Charity Name	**Locality** **Regional**	**National**	**Global**
Specialism	**Health**	NHT	Beat	Avert
	Social	Break	Addaction	StT
	Humanitarian/ Environment	Leeway	Amnesty Int.	RC

Appendix 3.2. Details of charities used

Addaction: A charity that supports adults and children suffering from alcohol or drug addictions and mental health issues in the UK.

A £100 donation could help to pay for up to 4 group sessions to provide peer support about alcohol abuse.

Amnesty International: A charity that campaigns for the improvements of human rights through action and education in the UK and across the world.

A £100 donation could help to allow experts to attend international government meetings over a year.

Avert: A charity that aims to increase knowledge and understanding of HIV/ AIDS to reduce infection and improve HIV programmes across the world.

A £100 donation could help to pay the monthly salary of a community nurse to care for people living with HIV in sub-Saharan Africa.

Beat: A charity that aims to improve support and treatment for people suffering from eating disorders and improving the education of eating disorders in the UK.

A £100 donation could help to fund a trained peer support helpline worker for a day.

Break: A charity that assists in the provision of children's homes, family support and fostering facilities for families in Norfolk.

A £100 donation could help to pay for 3 parents to attend a parent support group.

Leeway: A charity that offers advice and support, such as outreach services and drop-in centres, to those who suffer from domestic abuse in Norfolk.

A £100 donation could help to enable 10 women to receive outreach support.

Norfolk Heart Trust: A charity that helps improve the treatment and support for individuals and their families suffering from heart diseases in Norfolk.

A £100 donation could help to provide patients with specialist equipment in their homes.

Rainforest Concern: A charity that protects threatened rainforests, the biodiversity they contain and the indigenous people who rely on the rainforests across the world.

A £100 donation could help to sponsor 2 acres of rainforest to help protect it for a year.

Stop the Traffik: A charity that aims to disrupt and prevent human trafficking through campaigning and education in the UK and across the world.

A £100 donation could help to train volunteers to give lessons to educate students on campaigns that highlight human trafficking.

Appendix 3.3. Statistics of tasks and charities observed in Part Two

3.3.1. Tests of non-random distribution of mean donation and frequency by charity selected or chosen in Part Two, across treatments

	No-Choice		**Choice**	
Charity	Mean (£)	(n)	Mean (£)	(n)
Addaction	£1.74	10	£1.85	4
Amnesty Int.	£2.65	2	£3.18	5
Avert	£2.70	8	£4.07	13
Beat	£1.13	4	£1.30	3
Break	£3.25	6	£2.20	5
Leeway	£2.67	10	£2.91	7
NHT	£2.07	9	£1.96	8
RC	£0.80	4	£3.09	9
StT	£1.49	7	£2.42	6
Mean (£)	**Kruskal-Wallis Test**		**Kruskal-Wallis Test**	
	$\chi^2(8)$-stat	7.032	$\chi^2(8)$-stat	12.128
	p-value	0.553	*p*-value	0.146
(n)	**χ^2-Test**		**χ^2-Test**	
	$\chi^2(8)$-stat	9.900	$\chi^2(8)$-stat	11.100
	p-value	0.272	*p*-value	0.196

3.3.2. Tests of non-random distribution of mean donation and frequency by task selected in Part Two, across treatments

	No-Choice		**Choice**	
Task	Mean (£)	(n)	Mean (£)	(n)
1	£2.42	5	£2.16	5
2	£0.63	3	£2.95	2
3	£2.35	4	£3.80	5
4	£2.90	6	£2.90	7
5	£2.21	8	£2.35	4
6	£2.50	3	£3.13	3
7	£1.00	5	£1.38	10
8	£2.52	5	£2.10	5
9	£3.00	5	£4.20	5
10	£1.59	11	£2.63	4
11	£2.03	3	£3.20	5
12	£2.50	2	£4.58	5
Mean (£)	**Kruskal-Wallis Test**		**Kruskal-Wallis Test**	
	$\chi^2(11)$	8.004	$\chi^2(11)$	18.667
	p-value	0.713	*p*-value	**0.067***
(n)	**χ^2-Test**		**χ^2-Test**	
	$\chi^2(11)$-stat	13.600	$\chi^2(11)$-stat	8.800
	p-value	0.256	*p*-value	0.640

Appendix 3.4. Proof of possibility of transitivity violations in trios of pairwise ranking preferences

Given three charities (C_A, C_B, C_C) across all ranking tasks, there are eight possible ranking orders for pairs of these three charities:

	Pairwise Rankings			**Implied Ranking Preferences**
1)	$C_A > C_B$	$C_B > C_C$	$C_A > C_C$	$C_A > C_B > C_C$
2)	**$C_A > C_B$**	**$C_B > C_C$**	**$C_C > C_A$**	**N/A**
3)	$C_A > C_B$	$C_C > C_B$	$C_A > C_C$	$C_A > C_C > C_B$
4)	$C_A > C_B$	$C_C > C_B$	$C_C > C_A$	$C_C > C_A > C_B$
5)	$C_B > C_A$	$C_B > C_C$	$C_A > C_C$	$C_B > C_A > C_C$
6)	$C_B > C_A$	$C_B > C_C$	$C_C > C_A$	$C_B > C_C > C_A$
7)	**$C_B > C_A$**	**$C_C > C_B$**	**$C_A > C_C$**	**N/A**
8)	$C_B > C_A$	$C_C > C_B$	$C_C > C_A$	$C_C > C_B > C_A$

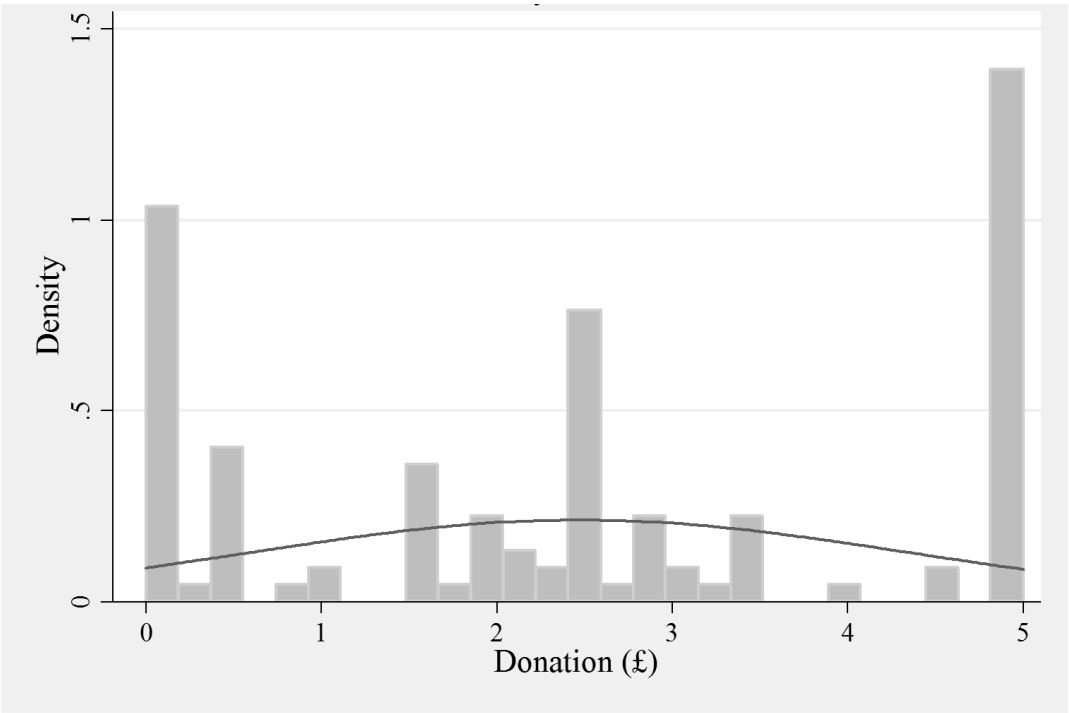
Of these eight possible pairwise ranking orders, two (2 and 7) violate transitivity. Thus, randomly selecting ranking decisions for each pairing containing two of these three charities would imply a probability of violation of transitivity of 25.0%.

Appendix 3.5. Distribution of donation amounts

3.5.1. Summary statistics of distribution of grouped donation amounts

Donation Amount (*n*)	**All**	**No-Choice**	**Choice**
£0.00	22	15	7
£0.10-£0.90	12	7	5
£1.10 - £1.90	16	5	11
£2.10 - £2.90	28	16	12
£3.10 - £3.90	9	5	4
£4.10 - £4.90	2	1	1
£5.00	31	11	20

3.5.2. Histogram of distribution of actual donation amounts



Appendix 3.6. List of ranking task combinations

Task Number	Task Name	Charity Type		
1	**Regional**	NHT	Break	Leeway
2	**National**	Beat	Addaction	Amnesty Int.
3	**Global**	Avert	StT	RC
4	**Health**	NHT	Beat	Avert
5	**Social**	Break	Addaction	StT
6	**Hum./ Env.**	Leeway	Amnesty Int.	RC
7	**Mix 1**	NHT	Addaction	RC
8	**Mix 2**	Beat	StT	Leeway
9	**Mix 3**	Avert	Break	Amnesty Int.
10	**Mix 4**	NHT	StT	Amnesty Int.
11	**Mix 5**	Beat	Break	RC
12	**Mix 6**	Avert	Addaction	Leeway

Notes: Whilst a task number was assigned to each specific ranking task, the randomisation of task order meant that participants completed these tasks in different orders.

Appendix 3.7. Experimental instructions

Introduction

Welcome to this experiment on decision making. Thank you for coming. Please follow along as I read through the instructions. If you have a question, please raise your hand and I will come to answer your question privately.

Your decisions in this experiment are private, and we ask you not to communicate with others or react verbally to outcomes during this experiment. If you have any questions during the experiment please raise your hand and an experimenter will come to assist you. Please do not eat or use any electronic devices whilst you are taking part in this experiment. Please keep to these simple rules, as anyone breaking them may be asked to leave without payment. Various agencies have provided funding for this experiment.

This experiment consists of two parts, Part 1 and Part 2. I will shortly read the instructions for Part 1. You will receive the instructions for Part 2 after Part 1 has finished. In completing Part 1, you will receive £5. What you will earn in Part 2 will be explained in more detail after Part 1 has finished. I will now read aloud the instructions for Part 1.

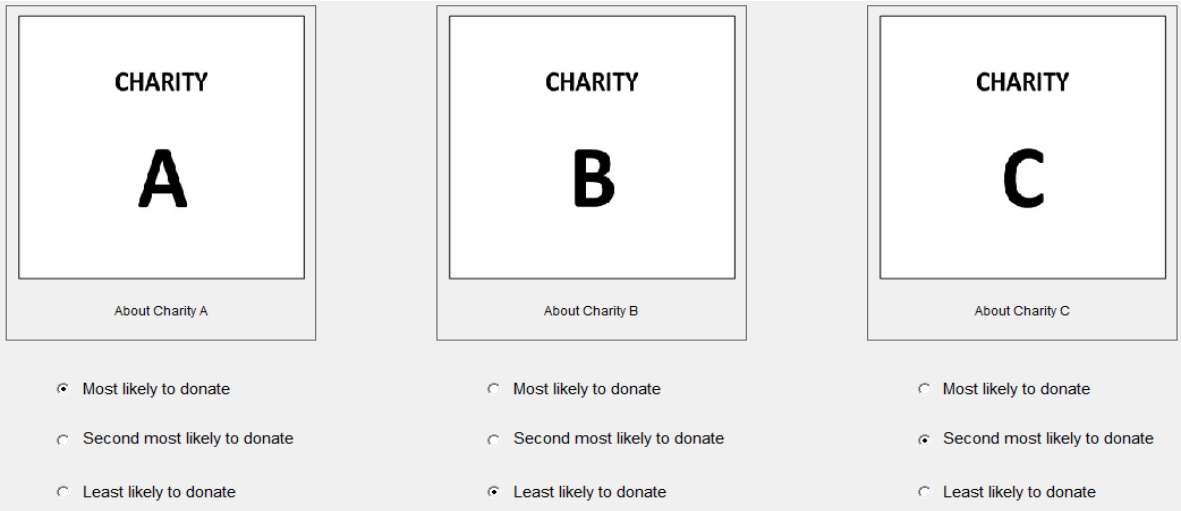
Part 1

The following instructions are simple, but please follow them carefully. Part 1 consists of thirteen tasks. For completing Part 1, you will receive £5 at the end of the experiment.

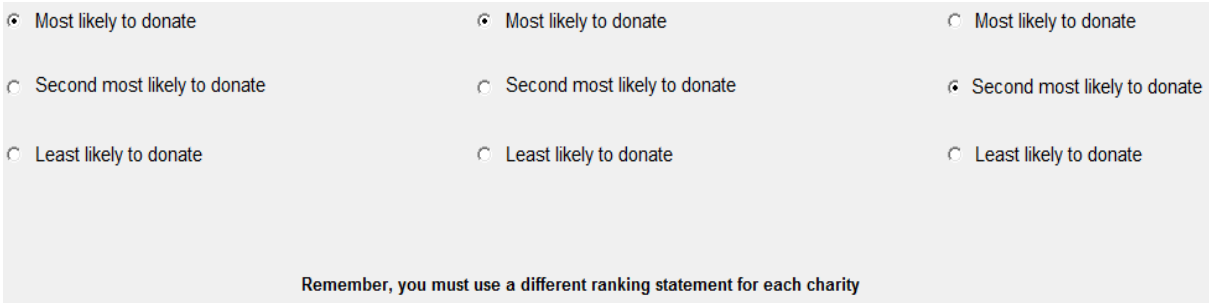
We are interested in your attitudes towards different charitable causes. In each task you will be shown three real charities. As well as the name and logo of each charity, you will be given a brief description of each charity, as well as information about how each charity may use the donations it receives. This is real information taken from the respective charity websites. Details of the charity websites will be made available at the end of the experiment.

In each task, you will be asked to rank the three charities, based on how likely you believe you would be to donate to each of them. Underneath each charity will be three ranking statements "Most likely to donate to", "Second most likely to donate to" and "Least likely to donate to". For each charity, you will be asked to select the ranking statement that you believe best reflects

your willingness to donate to that charity. To assist in the explanation of the ranking tasks, please see the example below.



Here there are three charities, A, B and C. The participant has ranked Charity A "Most likely to donate to", indicating that, of these three charities, this is the charity the participant would be *most* likely to donate to. The participant has ranked Charity C "Second most likely to donate", indicating that, of these three charities, this is the charity the participant would be *second most* likely to donate to. Finally, the participant has ranked Charity B "Least likely to donate", indicating that, of these three charities, this is the charity the participant would be *least* likely to donate to.



Remember, this task does not require you to part with any real money, but it is asking you to rank the charities by how willing you *would* be to donate to these charities, *as if* you were being asked to part with real money, and so you should take these decisions seriously.

Please note, there are no right or wrong answers to these ranking tasks, and your decisions in all tasks will be completely anonymous. When ranking each charity, you must use a different ranking statement for each charity (one "Most likely to donate to", one "Second most likely to

donate" and one "Least likely to donate") - there can be no ties. You will not be able to confirm your ranking and leave the task until each charity has a different ranking statement. Until you give each charity a different ranking statement a reminder message will appear on your screen and you will have to edit your rankings to make each one different before being able to continue. Below is an example of an identical ranking and the error message that would be shown below the ranking statements.

You will complete a ranking exercise for each of the thirteen tasks. Please note you may see the same charity in more than one task. After everyone has completed all thirteen tasks, Part 1 will finish. At the end of the experiment, all participants will receive £5 for completing Part 1. Part 2 will be explained once Part 1 is complete.

Before we proceed, I ask you to answer the short quiz that will follow shortly on your screens, to ensure you understand the tasks in Part 1. Please attempt these and feel free to re-read the instructions as you do so. If you have any queries please raise your hand and an experimenter will come to assist you.

Now we are ready to start Part 1.

Part 2

I will now read aloud the instructions for Part 2. What you earn in Part 2 will be added to the £5 you have already earned in Part 1. The amount you earn in Part 2 will be determined only by the decisions you make in Part 2. Part 2 consists of two tasks, the *choice* task, and the *donation* task.

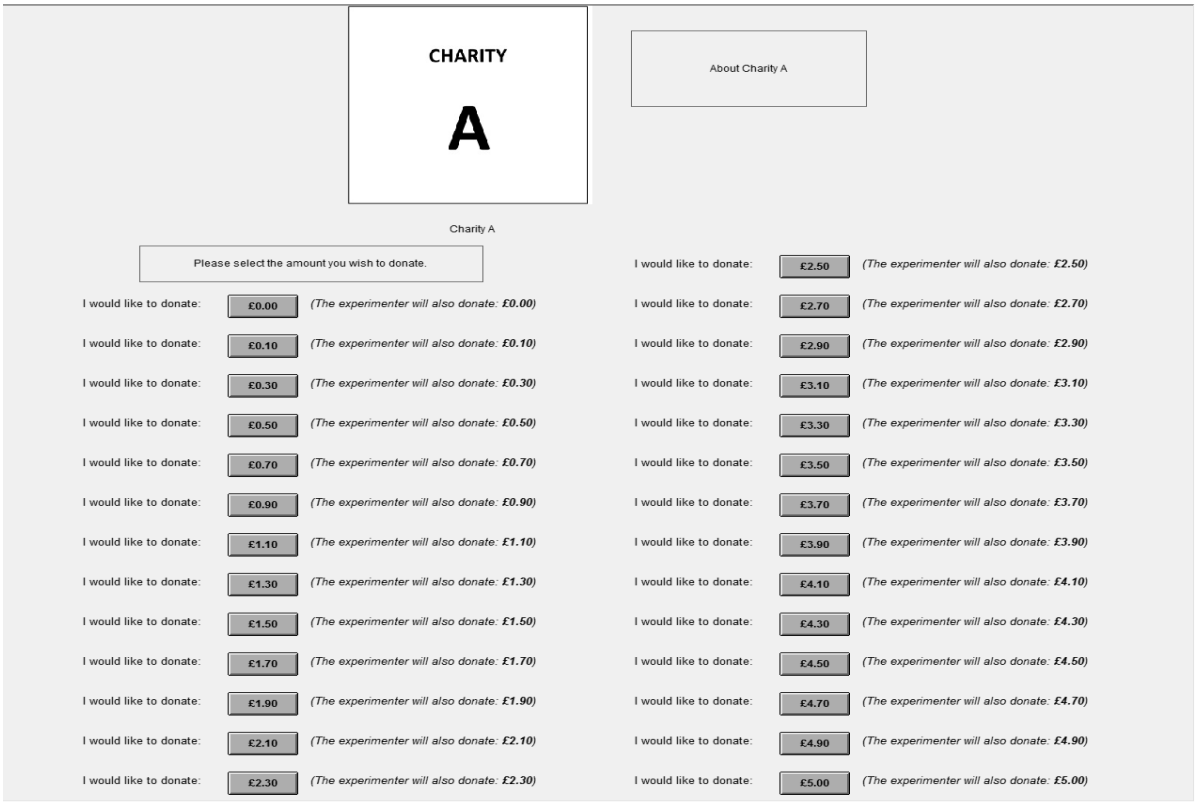
Choice Task

In the choice task, you will be shown on your screens three of the charities you previously ranked in Part 1, as well as the same information you received in Part 1 about these charities. Of these three charities, you will be asked to choose one. The charity that you choose will be the charity you have the opportunity to donate real money to. Once you have chosen this charity, you will be shown on your screens only the charity you have chosen.

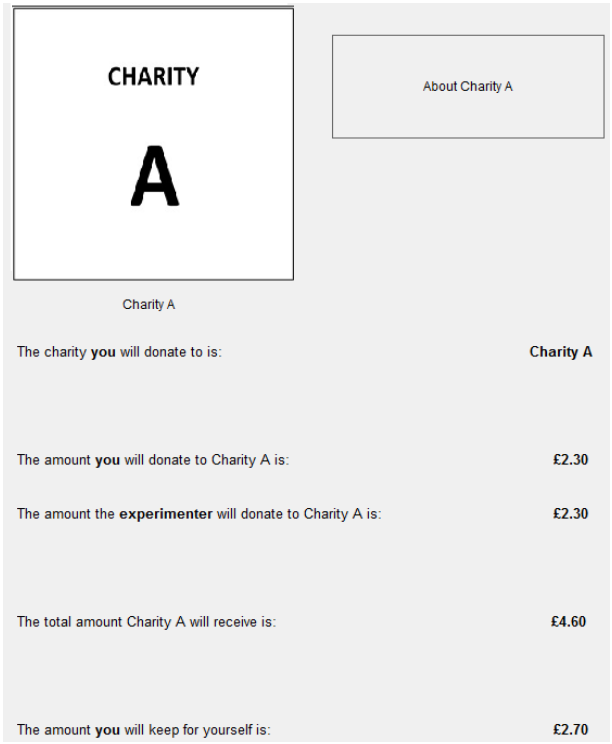
Donation Task

At the beginning of the donation task, you will be allocated £5. You will be shown the donation page for the charity you have chosen. The donation page lists 27 possible donation amounts ranging from £0.00-£5.00. You will be asked to select which of these amounts you would like to donate to that charity. The amount you choose to donate will be taken from your £5 allocation. However much you decide to donate to the charity, the experimenter will donate the same amount to the same charity, using the research funds provided for this experiment. If you choose to not donate all of your £5, the amount not donated will be added to the £5 you earned in Part 1 to make your final earnings.

To assist in the explanation of the donation task, please see the example donation page below. The donation page gives you the information of the charity you are donating to, as well as the list of all possible donation amounts, from £0.00-£5.00. This means it is possible to donate all, or nothing, of the £5 you are given at the beginning of Part 2. The donation page also gives the corresponding donation that the experimenter will match.



Once an amount to donate is chosen, you will be shown a confirmation page. To assist in the explanation of the confirmation page, please see below. Here is an example of someone who has chosen to donate £2.30 of their £5 to Charity A.



On the confirmation page, you will be able to see the amount you will donate, the amount the experimenter will donate, the total (combined) amount the charity will receive and the amount you will take away with you (this will be added to the £5 you earned in Part 1). At this point you will have to confirm your donation amount or return to the donation page and revise your donation amount. Please note, once you have confirmed the decisions you make in the donation stage, these will become real donations to real charities. All donation decisions will be kept completely anonymous.

Once you have confirmed your donation decision, you will be asked to complete a questionnaire. After completing this you will be shown your final earnings. The experiment will end when everyone has completed the donation decision and questionnaire. Payment of your earnings will be made, in private, upon leaving this experiment.

Once all donations from these experiments are received, the total amounts to be donated to each charity will be added together and sent as a single donation to each of these charities. For example, we will add the total amounts to be donated to Charity A, and send that total amount to Charity A as one donation. We will do this for every charity. When you receive your payment for this experiment, you will also receive a slip of paper with the address of a website. Once all donations have been received by each charity, the confirmation and acknowledgement of these donations by each charity will be uploaded to this website, which you will be able to view yourself.

Before we proceed, I ask you to answer the short quiz that will follow shortly on your screens, to ensure you understand the tasks in Part 2. Please attempt these and feel free to re-read the instructions as you do so. If you have any queries please raise your hand and an experimenter will come to assist you.

Now we are ready to start Part 2.

Notes: These experimental instructions were for participants in Choice. For participants in No-Choice, the 'Choice Task' section was removed and terminology replaced to explain a charity would be selected for participants by their computer.

Appendix 3.8. Pre-experimental quiz (correct answer in **bold**)

Part One

Question 1: How many tasks will you complete in Part 1?

a) 1
b) 3
c) 13

Question 2: How much will you earn for completing Part 1?

a) £5.00
b) £13.00
c) It depends

Question 3: Imagine three charities, Charity A, Charity B and Charity C. Suppose you thought that Charity A was the charity that you believe you would be *most* willing to donate to. What ranking would you give this charity?

d) Most likely to donate
e) Second most likely to donate
f) Least likely to donate

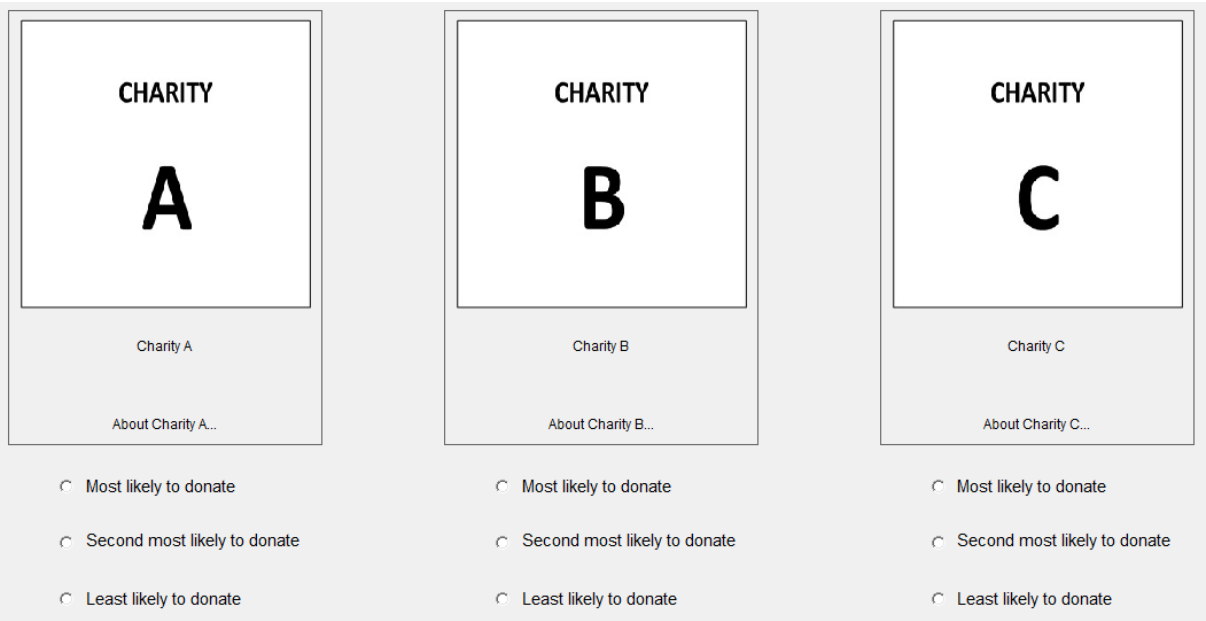
Question 4: Imagine three charities, Charity A, Charity B and Charity C. Suppose you thought that Charity C was the charity that you believe you would be *second most* willing to donate to. What ranking would you give this charity?

a) Most likely to donate
b) Second most likely to donate
c) Least likely to donate

Question 5: Imagine three charities, Charity A, Charity B and Charity C. Suppose you thought that Charity B was the charity that you believe you would be *least* willing to donate to. What ranking would you give this charity?

a) Most likely to donate
b) Second most likely to donate
c) Least likely to donate

Question 6: Take a look at these three charities below. Suppose you thought that Charity C was the charity that you believe you would be *most* willing to donate to, Charity A was the charity that you believe you would be *second most* willing to donate to, and Charity B was the charity that you believe you would be *least* willing to donate to. Please select the ranking statements below that correspond to these preferences.



Correct ranking preference statements:

Charity A: Second most likely to donate

Charity B: Least likely to donate

Charity C: Most likely to donate

Part Two

Question 1: How much will you be allocated at the beginning of the donation task in Part 2?

a) £0.20
b) £5.00
c) £10.00

Question 2: Suppose you have chosen to donate £2.70 (£2.30) to the chosen charity. How much will the experimenter match to give to this charity?

a) £2.30 (**£2.30**)
b) **£2.70** (£2.70)
c) £5.40 (£4.60)

Question 3: Suppose you have chosen to donate £2.50 to the chosen charity. How much in total (including the matched experimenter donation) will the charity receive?

a) £2.50
b) £5.00
c) £7.50

Question 4: Suppose you have chosen to donate £2.30 (£2.70) to the chosen charity. How much will you keep for yourself from Part 2?

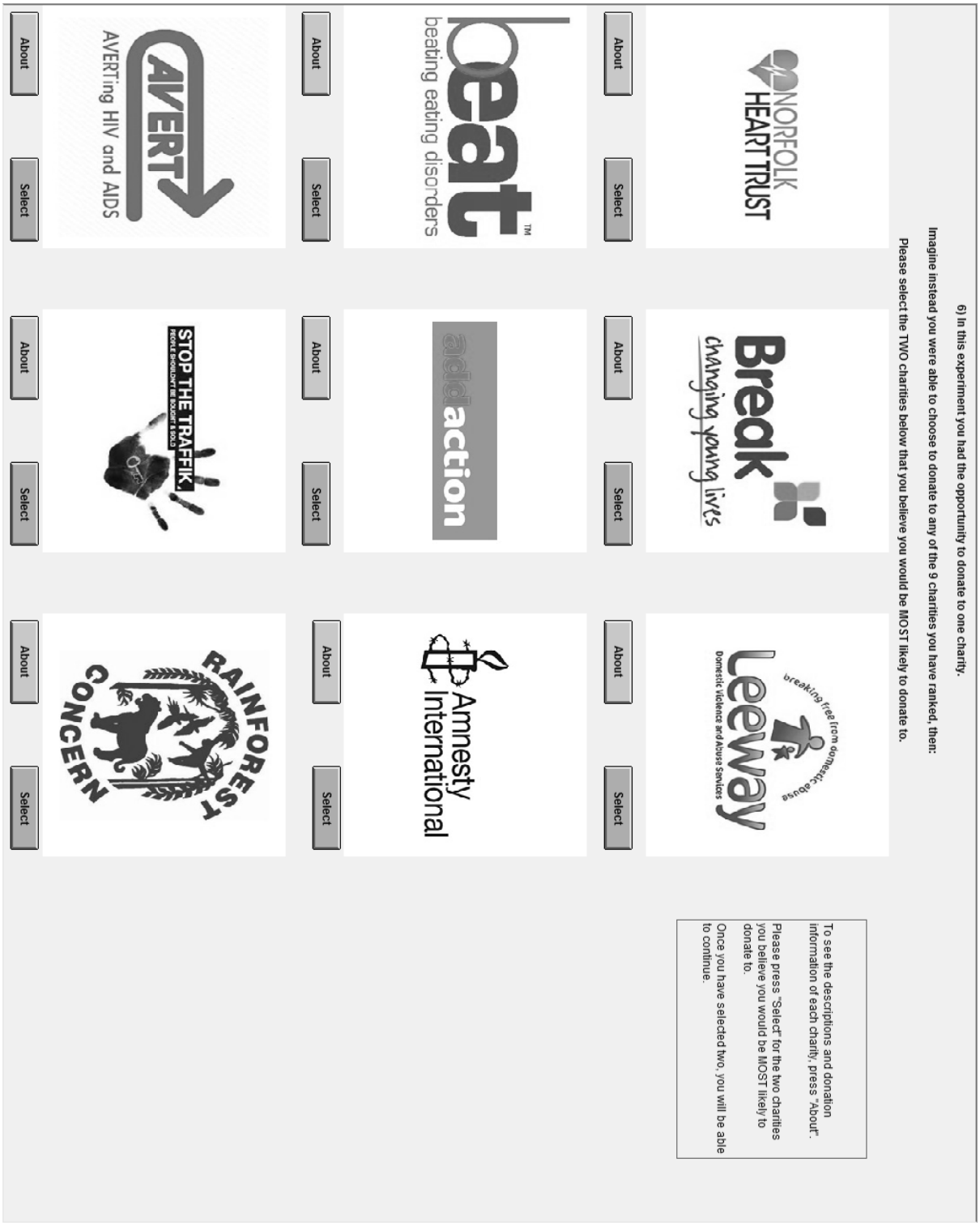
a) £2.30 (**£2.30**)
b) **£2.70** (£2.70)
c) £4.60 (£5.40)

Notes: Questions and answers with parentheses were randomised between the two possible values for participants, to control for any potential framing effects from these example valuations.

Appendix 3.9. Post-experimental questionnaire

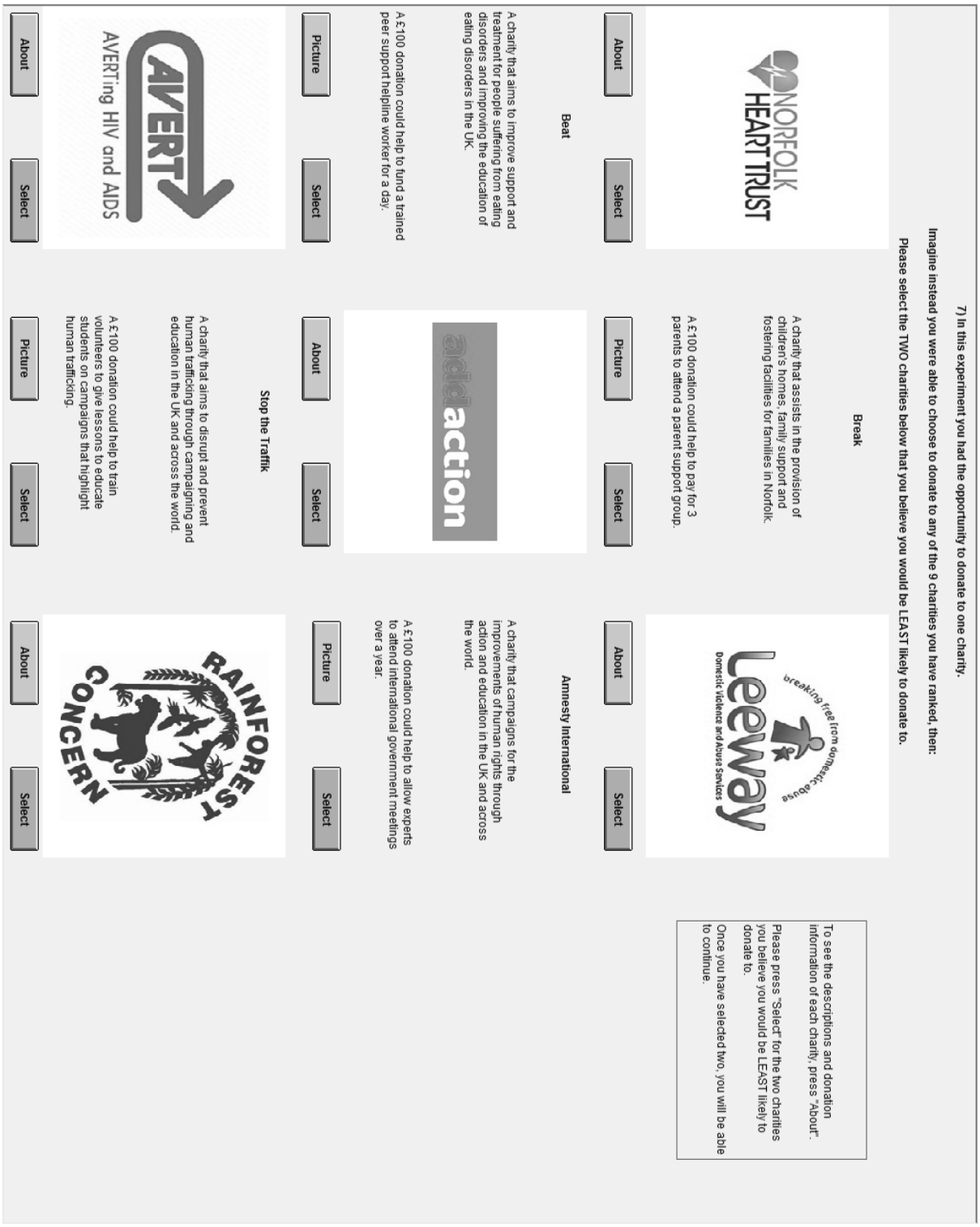
3.9.1. Demographic information questionnaire

3.9.2. Post-experimental consistency check for *most* preferred charities



Notes: Participants could select 'About' on each charity to review the information they had previously received about that charity.

3.9.3. Post-experimental consistency check for *least* preferred charities



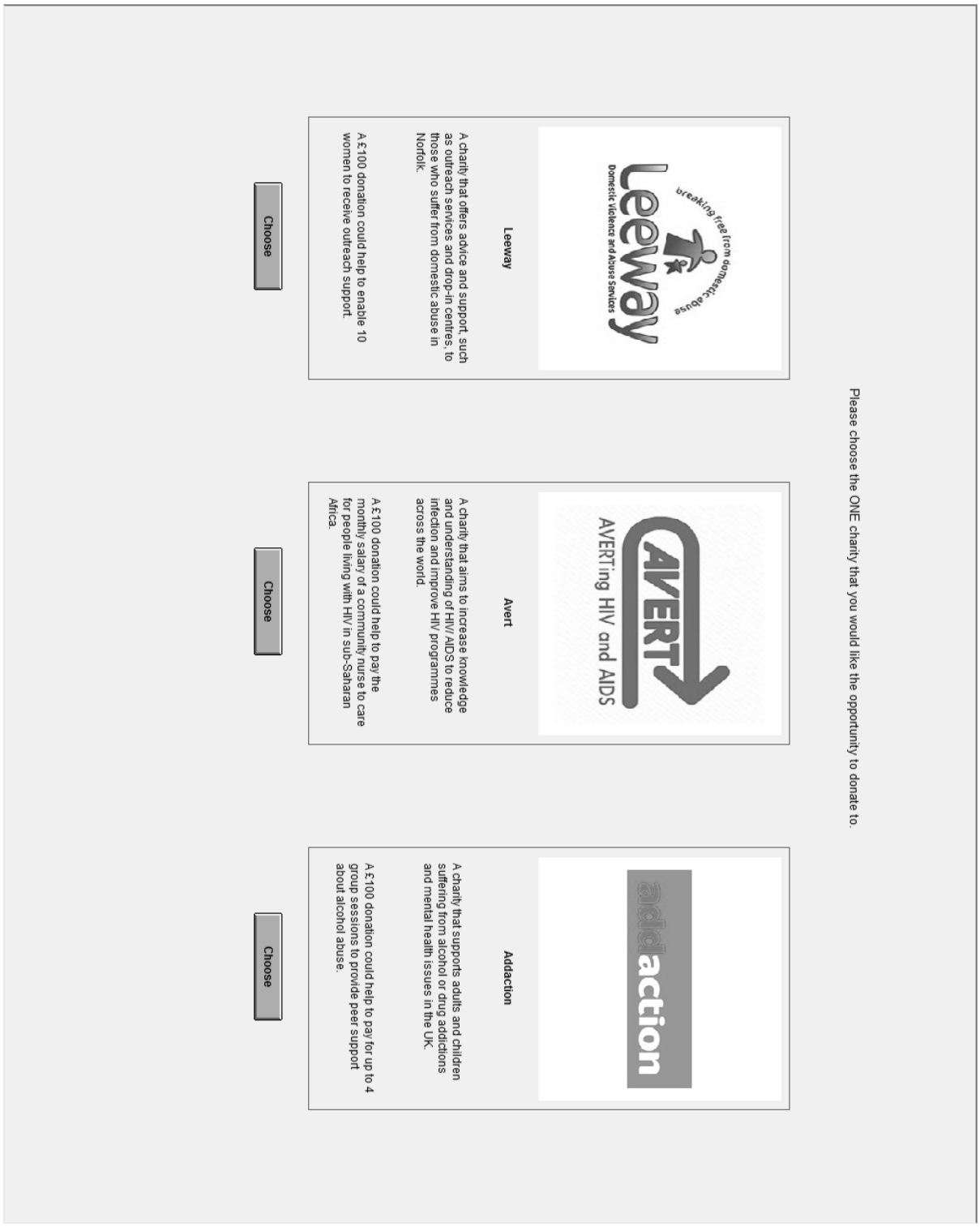
Notes: This screenshot shows the way information was presented if participants selected 'About' on any charity. Selecting 'Picture' returns the original charity image.

Appendix 3.10. Experimental screenshots

3.10.1. Ranking task in Part One

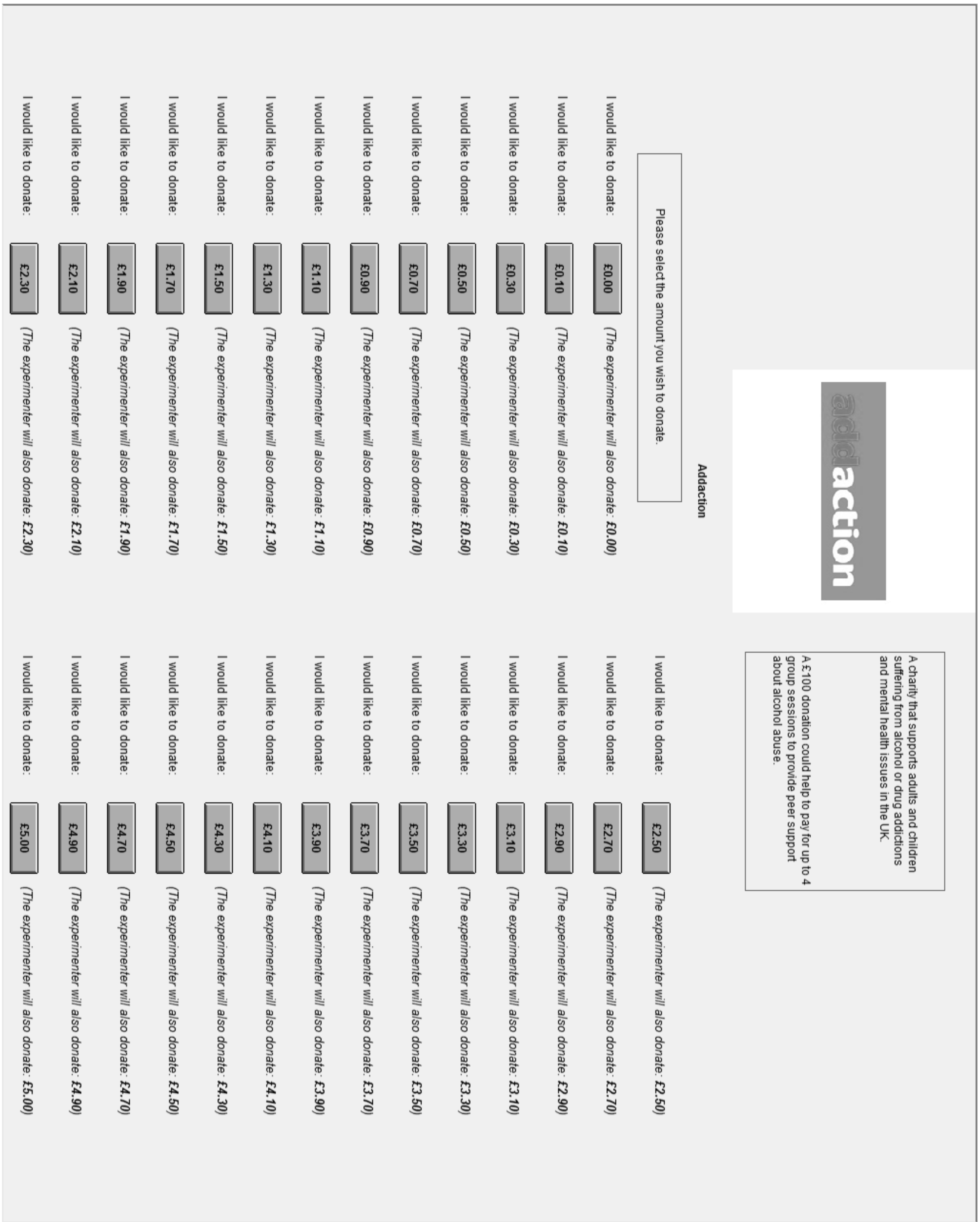
3.10.2. Ranking task confirmation in Part One

3.10.3. Choice task in Part Two (Choice treatment only)

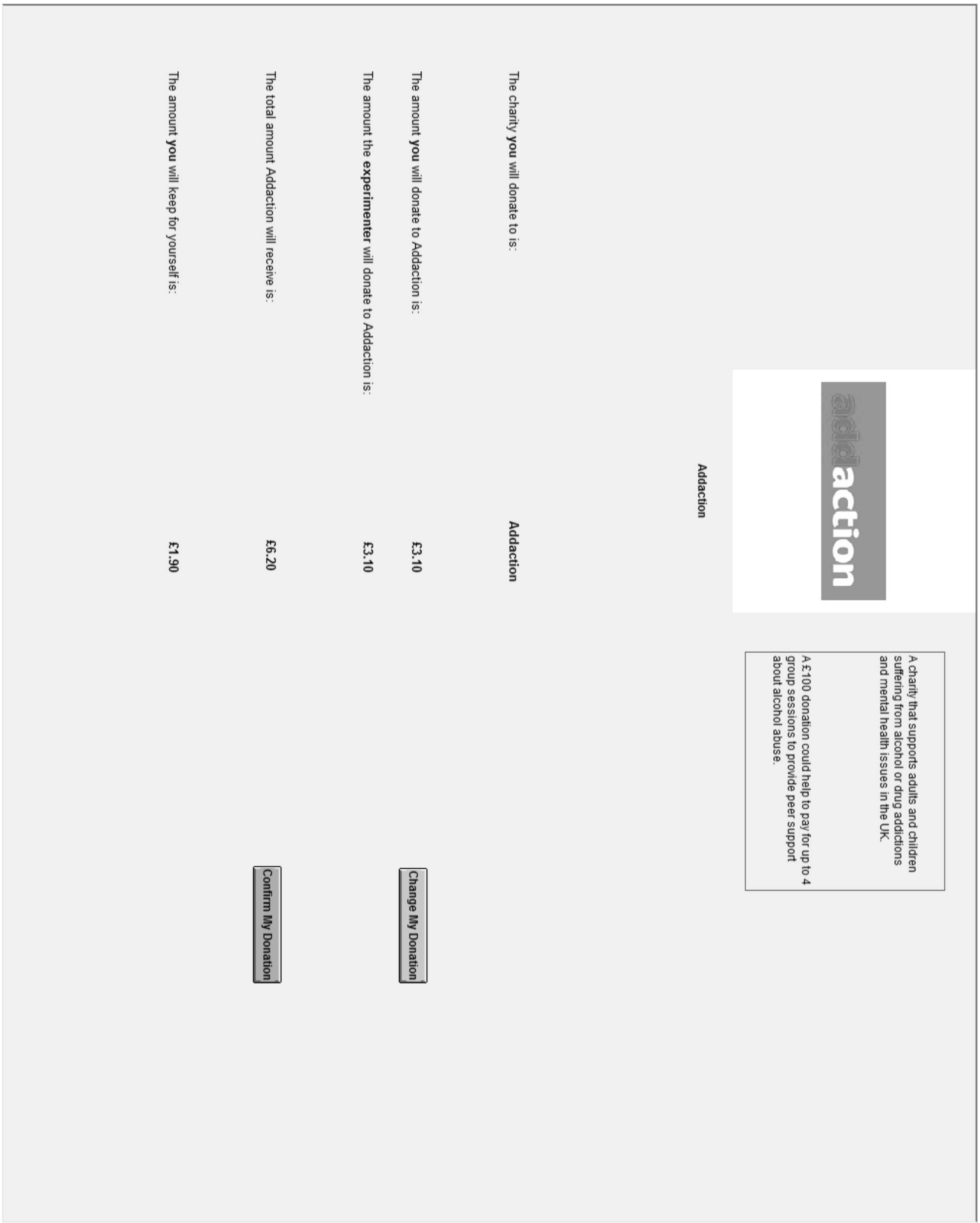


3.10.4. Chosen (Choice treatment) or selected (No-Choice) charity confirmation

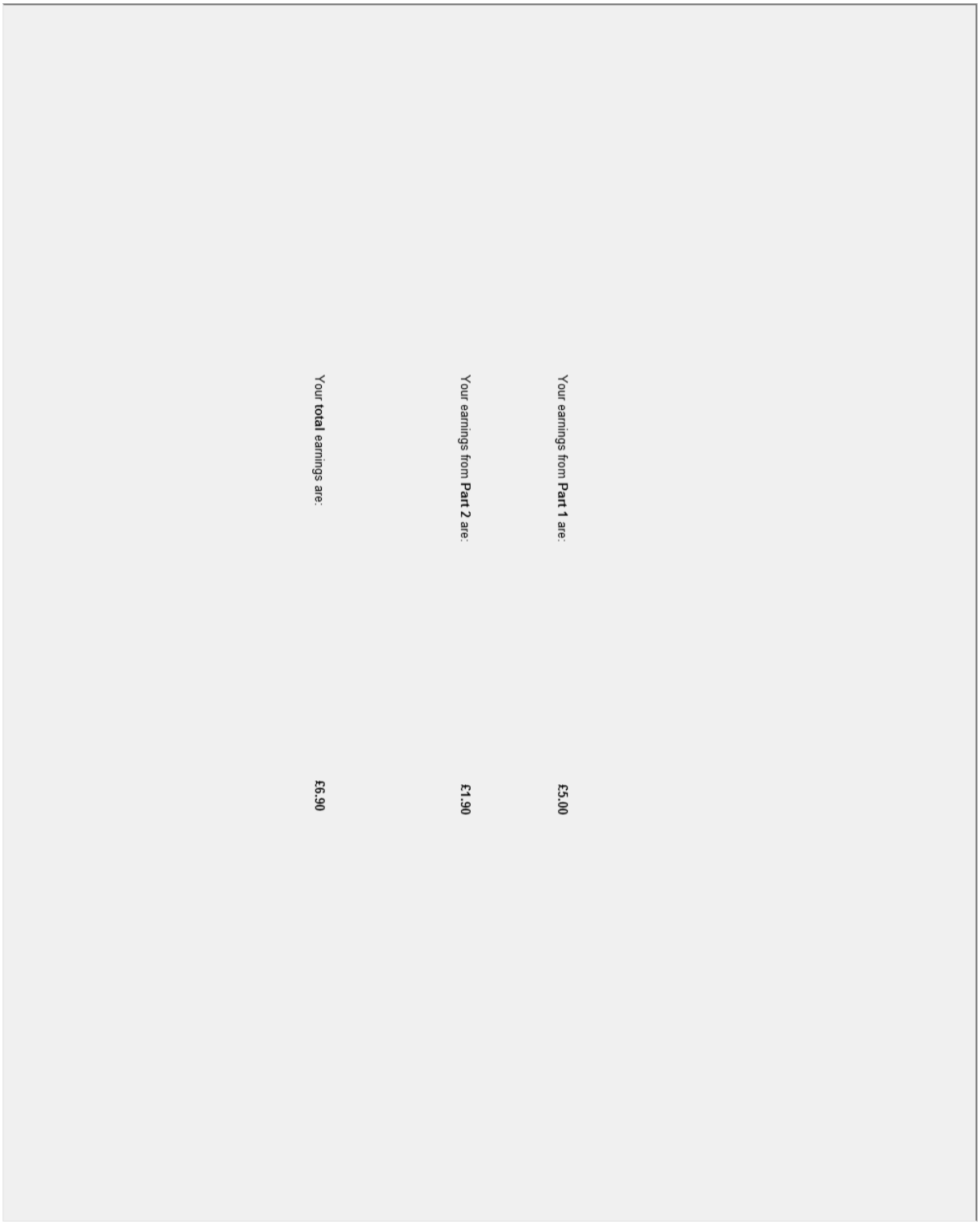
3.10.5. Charity donation page



3.10.6. Charity donation confirmation page



3.10.7. Final earnings page



Chapter 4

The Value of Choice in Consumable Goods Valuation

1. Introduction

The vast majority of experimental studies that consider the effect of choice versus no choice do so by using consumable goods to be assessed. This seems intuitive, given that choice is such a fundamental facet of our purchasing decisions in day-to-day life; whether that be choice between goods of different objective values, or between goods which are identical in cost but differ in terms of their subjective worth (think, for example, of choosing a new shirt, offered in a variety of colours). Such studies have considered satisfaction and purchasing behaviour for chocolates (Iyengar and Lepper, 2000) or yoghurt flavours (Botti and Iyengar (2004), and real food consumption decisions across children (Zeinstra *et al*., 2010, Dominguez *et al*., 2013, de Wild *et al*., 2015) and adults (King *et al*., 2008, Parizel *et al*., 2017).

Whilst the study in the previous paper considered a novel experimental design testing for choice effects across charitable giving, since in reality individuals spend much more time choosing and purchasing a variety of consumable goods, it is prudent to test its effectiveness in capturing a choice effect for these more conventional consumable goods. Thus, this paper reports the results of an experiment designed to test for a real monetary value of the positive effect of choice on valuations for real consumable goods- patterned mugs. It also differs from previous designs in the literature by testing for a quantifiable increase in valuation for consumable goods as a result of choice effects; previous designs have measured other metrics, such as binary decisions of likelihood to purchase, or satisfaction ratings. Measuring a change in valuation hopes to place a tangible monetary value on the act of choice.

The motivations of valuations of consumable goods may differ to willingness-to-donate to a charitable cause, and as such, the size of choice effects may differ across an experimental design testing this effect in both scenarios. Decisions involving consumable goods, where it is the individual making the valuation decision who directly benefits from consumption, are perhaps easier to explain from a rational, utility-maximising consumer perspective, than

donations to a charitable cause whereby simple models of individual rationality could predict that no one would want to donate anything to charity (e.g. Sugden, 1982, Dawes and Thaler, 1988).

The results from this experiment find some evidence of a choice effect for consumable goods, although this is not as strong or statistically robust as choice effects for charitable donations. In particular there is no evidence to suggest that choice effects increase the likelihood that consumers will place any positive valuation on a good. However, of those who do value the good at some positive value, choice has a strong and significant effect on increasing valuations. Although this experiment was not designed to be directly compared with the findings of choice effects in charitable giving, since the underlying motivations of why individuals value a good or donate to a charity may differ, it is of interest to consider why any differences in results might be a result of this.

The rest of the paper proceeds as follows. Section 2 describes the experimental design in the context of this experiment for private goods, and section 3 outlines the key hypothesis to be investigated. Section 4 presents the results and section 5 provides a discussion of the implications of these findings. Section 6 concludes.

2. Experimental Design

The design of this experiment was identical to the design in Chapter 3. That is, the experiment consisted of two treatments *Choice* and *No-Choice*, in a between-subject experimental design, and both consisted of two Parts, a preference-elicitation stage in Part One, and a valuation stage in Part Two. The only differences between the two experiments were the goods used (and so the terminology used to describe these) and the mechanism required in Part Two to elicit valuations.

The motivation for the design was just as in the previous study, to elicit a pure choice effect for the valuation of patterned mugs, whilst holding constant any preference effects of choosers and non-choosers; the design of *No-Choice* aimed to ensure that non-choosers received the mug they *would have* chosen, had they instead been given the opportunity to choose.

The consumable goods in this experiment were patterned mugs. The mugs themselves were white ceramic, and the patterns adorning were printed black. As with the experiment in Chapter 3, there were a total of nine different goods available. The designs on each of these nine

patterned mugs were custom designed to differ across two possible dimensions *size-number* (large-one, medium-five and small-nine) and *shape* (squares, circles, triangles)[15].

2.1. Part One – Preference Elicitation Stage

The intention of Part One was to establish preferences for different designs of mugs across a series of thirteen preference eliciting ranking tasks. The design of Part One is almost identical to that of the previous study, differing only in the precise methodology of eliciting preferences for consumable goods, mugs. In both treatments, Part One was identical. In these thirteen ranking tasks participants were shown different combinations of three mugs, and asked to rank these patterned mugs, based on their desirability relative to each other (by selecting one of three ranking statements, "*most*" "*second most*" and "*least*" desirable). Specific answers to each ranking task were non-incentivised and participants received a flat rate of £5 for completing this task. The first twelve tasks were designed in such a way that each mug was ranked once and only once against all other eight. The thirteenth task was a randomly repeated version of one of the first twelve tasks, as a test of consistency between ranking preferences across these identical tasks.

2.2. Part Two – Valuation Stage

Once Part One was complete, Part Two began. In Part Two, the differences in design for treatments Choice and No-Choice were as in the previous study. In the Choice treatment, participants were shown the same three mugs from one randomly determined ranking task in Part One and asked to choose which of the three they wished to have the opportunity to take away with them. In the No-Choice treatment, one task from Part One was randomly determined and the mug ranked most highly in that task by the participant was shown to them as the mug they had the opportunity to take away with them. Again, in this experiment it was predicted that participants in Choice would choose the good they ranked most highly in the corresponding ranking task in Part One. As such, selecting the mug a participant in No-Choice ranked most highly in a randomly selected task in Part One is a good indicator of the good they would have chosen in that equivalent task, had they instead been given a choice. Once choices were made (or participants had been shown which mug had been selected), the remainder of Part Two was identical for both treatments.

[15] Images and details of each mug may be found in Appendix 4.1. The mugs used in this experiment were based on the original design of mugs used in Chapters 1 and 2.

The methodology required for valuations of mugs differed substantially from the donation decisions in the previous study. The mug that participants had the opportunity to take away with them was distributed to them. This is in line with findings which suggest that physical exposure towards good can increase its perceived value (Knetsch and Wong, 2009). Participants were informed that they owned the mug, but that they could sell it back to the experimenter if they wished. The decision to treat participants as sellers was deliberate. Explanations of choice effects stem from an attention-based hypothesis, and Carmon and Ariely (2000) suggest that sellers focus their attention more readily on attributes of a good than buyers. Therefore it would be reasonable to believe that manipulations of attention through choice would be more readily captured by sellers.

In order to incentivise participant's valuations of the goods, a Becker-DeGroot-Marschak (henceforth BDM) (1964) was used. For 27 values, ranging from £0.01-£5.00, participants were asked if they would be willing to 'sell', or 'not sell' (and so keep the mug) at these given values. Prior studies suggested that similar patterned mugs would be valued between approximately £2.00-£3.00, suggesting that a maximum valuation of £5.00 would reduce the possibility that participants' valuations might be framed by the upper and lower bounds of the BDM mechanism (Bohm *et al*., 1997). The fact that truthful valuations were the optimal response in the valuation mechanism was made clear to participants in both the instructions and pre-experimental quizzes.

A participant who acted on consistent preferences between money and goods would report at most one *preference switch*, from 'not sell' at relatively low prices to 'sell' at relatively high prices. (There would be no preference switch for a participant who would 'not sell' at every price or would 'sell' at every price). If a participant's decisions implied more than one preference switch, these switches were highlighted on the participant's screen, together with the relevant material from the experimental instructions which reminded them of the workings of the valuation mechanism, and gave them the opportunity to revise their decision, if they wished to. Participants were free to resubmit decisions with two or more preference switches, but this revision stage allowed errors to be corrected.

If a participant reported no more than one preference switch for a good, the location of that switch (or its absence) locates one of twenty-eight points on an ordinal valuation scale. For participants with exactly one preference switch, recorded *valuations* were defined as the mean of the highest price at which they would 'not sell' and the lowest price at which they would

'sell' (or, equivalently, the lowest price at which they would 'sell', *minus* £0.10). For example, a participant who would 'not sell' at £2.30, but would 'sell' at £2.50 was determined to have a valuation of £2.40, since that true value must lie somewhere greater than £2.30 but less than £2.50. Not all valuations were in £0.20 increments. If a preference switch occurred between £0.01 and £0.10 the valuation was determined at £0.05, and if a preference switch occurred between £4.90 and £5.00, the valuation was determined at £4.95. The decision to use a minimum value of £0.01 was deliberate. Anyone who would 'sell' at all values (i.e. including £0.01) was determined to have a value of £0.00, suggesting they had no interest in taking a mug away. Anyone who would 'not sell' at all values was indicated to have a valuation of £5.10, as it indicates their valuation of the good exceeded the maximum possible £5.00.

In any statistical analysis of results, these extreme parameters must be accounted for. First, non-parametric tests (such as Wilcoxon rank-sum tests) only require a ranking of values, and so this actual upper valuation does not affect statistical outcomes. In any regression analysis, censoring at lower and upper limits (using a Tobit model) accounts for any valuation above this upper limit, so this actual valuation does not affect statistical outcomes. In fact, no participants valued their good at the maximum value in this experiment.

2.3. Payment to Participants

Once decisions were made at all possible values, participants were shown 27 *valuation boxes* on their screens, and were asked to select one. Each of these boxes contained one of the possible 27 sell/not sell values. The value contained in the box selected by each participant was determined to be the price that the experiment "offered" the participant for the mug.

If the participant had declared at that value that they would 'sell' the mug, they received the price the experimenter offered in addition to the £5 earned in Part One, but did not take a mug away. If the participant had declared at that value that they would 'not sell' the mug, they took the mug away with them, and received no money from Part Two (but still received the £5 earned in Part One).

2.4. Implementation

This experiment was conducted at the same time as the experiment in Chapter 3, late 2016-early 2017 at the University of East Anglia's Centre for Behavioural and Experimental Social Science (CBESS), but using different participants. Participants were recruited through the centre's online recruitment system and were relatively inexperienced; due to concerns that

participants who had taken part in many prior experiments would be less likely to value their good positively, all participants had participated in three or fewer experiments, and none had previously participated in experiments of this type. The experiment was conducted using experimental software package z-Tree (Zurich Toolbox for Ready-made Economic Experiments) (Fischbacher, 2007), and participants undertook the experiment in isolated computer booths. Since there was no a-priori reason to suspect cultural difference in attitudes towards the valuation of mugs, the nationality of participants was not a factor for participation in this experiment, though all were students from the university.

Both treatments included pre-experimental quizzes, where participants were tested on their understanding of experimental procedures. Participants who answered incorrectly were asked to review the relevant instructions before attempting the question again. 87.5% of all questions were answered correctly at the first attempt, and 97.1% were answered correctly after two attempts, suggesting participants understood the mechanisms of the experimental design.

3. Hypotheses

The design of these experiments allows for a simple measure of choice effect, perceived as differences in valuations across Choice and No-Choice treatments. The hypothesis of a positive effect of choice is not a new one, although the design of this experiment is likely to yield a more conservative estimate than previous findings, given the control in design.

This paper does not attempt to isolate one specific cause of a choice effect, but for all a null hypothesis predicts no difference between valuation with or without an effect of choice, given an experimental design that ensures all participants receive the mug they would have (or actually had) chosen from a given set. An alternative hypothesis predicts the following difference in average valuations of individual (i) (V_i) for treatments No-Choice (NC) and Choice (C):

Alternative Hypothesis (H1) – Choice Effect – Mug Valuations

$$\textit{For all } i \qquad\qquad V_{iNC} < V_{iC}$$

4. Results

A total of 124 participants took part in this experiment, although five participants recorded inconsistent valuation decisions in the BDM mechanism such that unambiguous intended

valuations could not be inferred, and so these were dropped from the analysis[16]. This left 119 participants with usable data. Table 4.1 below outlines the summary statistics for both treatments.

Mug valuations in No-Choice were on average £2.23, and in Choice valuations were on average £2.39, an increase of approximately 7.17%, although this difference in average valuations was not statistically significant (*t*-stat= 0.681, *p*= 0.497). Median results report similar findings and significance levels. Additional summary statistics in Table 4.1 reveal that more participants in Choice valued their mug at £0.00 than did in No-Choice (six and two participants, respectively), although this finding is not statistically significant (*p*= 0.150). No participants in either treatment valued the mug at the maximum level.

	Mug Valuations	
	No-Choice	**Choice**
N	60	59
Mean (£)	**£2.23**	**£2.39**
t-Test (*t*-stat)	0.681	
p-value	0.497	
Median (£)	**£2.00**	**£2.40**
Mann-Whitney (*z*-stat)	0.872	
p-value	0.383	
Min (£0.00) (n)	**2/60**	**6/59**
Chi-Squared (χ^2-stat)	-2.068	
p-value	0.150	
Max (£5.10) (n)	**0/60**	**0/59**
Chi-Squared (χ^2-stat)	N/A	
p-value	N/A	

Table 4.1. Summary statistics by treatment

4.1. Preference Score Effects

The design of these goods were selected so as to reduce large disparities in valuations and preferences between different goods. Whilst this was not important to the experimental outcome of choice effects it is of interest to observe whether this held true. Kruskal-Wallis tests

[16] Recall that participants were given an opportunity to rectify apparently inconsistent decisions in the valuation stage. As with previous chapters utilising this valuation elicitation technique, if participants still revealed inconsistency after this, efforts were made to allow for human error and still record an intended valuation. This was achieved through the following rule: if consistency could be achieved through the rectification of *one* valuation decision, and it was obvious which valuation decision was erroneous, then this one valuation decision was rectified and valuation was inferred from these consistent valuation decisions. However, no participants were inconsistent in such a way. Participants either rectified their valuations to become consistent or required more than one valuation decision to become consistent, or it was not obvious which valuation decision was erroneous, as with the 5 participants dropped from the analysis.

reveal that, for both No-Choice ($\chi^2(8)= 8.794$, $p= 0.360$), and Choice ($\chi^2(8)= 6.423$, $p= 0.600$), actual valuations did not significantly differ across specific goods (see Appendix 4.2).

As with the prior study, the design of Part One allowed all mugs to be ranked once with each and every alternative mug in pairwise comparisons in the first twelve tasks. From these pairwise comparisons, a preference score can be elicited. If a mug is preferred to all other eight options, it scores a preference score of eight, and if it is not preferred to any of the other eight options, it scores a preference score of zero, and so on- with a higher score implying a higher overall preference.

Table 4.2 outlines, for each preference score, the average valuations of all mugs which were assigned that preference score by participants. An extension of a Wilcoxon rank-sum (two-tailed) test, a Cuzick Trend test tests for a consistent trend of valuation decisions as preference score increases. A lack of significance for both treatments suggests that overall preference score has little effect on final valuation decisions, consistent with findings in Chapter 3.

	No-Choice		**Choice**	
Score	Mean (£)	(n)	Mean (£)	(n)
0	----	0	£3.80	1/59
1	----	0	£1.73	3/59
2	£2.40	3	£1.50	2/59
3	£2.40	1	£1.00	1/59
4	£2.20	4/60	£3.27	3/59
5	£2.60	9/60	£2.70	6/59
6	£2.45	8/60	£2.41	14/59
7	£2.01	16/60	£2.32	10/59
8	£2.12	19/60	£2.37	19/59
Cuzick Trend Test				
	z-stat	0.730	*z*-stat	-0.450
	p-value	0.466	*p*-value	0.650

Table 4.2. Mean valuations by preference score

4.2. Consistency Effects

As with the experiment in Chapter 3, the effectiveness of this experimental design in measuring a pure choice effect is predicated on the assumption that the choices made by participants in Part Two of the Choice treatment were consistent with the decisions made in the corresponding ranking task in Part One. Table 4.3 below outlines results of a number of consistency measures which tested whether participants were consistent in their ranking and choice decisions throughout the experiment. The table also reports the results of chi-squared tests for each

consistency measure, which compare observed consistency levels with a baseline expected consistency, if participants simply responded randomly to all tasks in this experiment. For more details of consistency tests, see section 5.2 in Chapter 3.

The consistency tests in this experiment are remarkably consistent to the results of the experiment in Chapter 3; that is, in general, participants were remarkably consistent independent of which consistency measure was used. Importantly, over 80% of participants in Choice revealed *Task Choice consistency* (where random responses would predict a consistency of 33.33%). This infers that, for the preference decisions of mugs, the No-Choice design of this experiment was successful in determining that the mug a non-chooser receives can be reasonably predicted as the mug they ranked most desirable in any corresponding ranking task.

Actual Consistency (n) *Expected Consistency (n)* **Actual Consistency (%)** *Expected Consistency (%)* χ^2-statistic ***p*-value**	**Task Choice Consistency**	**Task 13 Consistency**	**Questionnaire Consistency**	**Transitivity Violations**
Mug Valuations	48/59 *≈20/59* 81.36% *33.33%* χ^2= 61.230 ***p*< 0.001***	105/119 *≈40/119* 88.24% *33.33%* χ^2= 161.415 ***p*< 0.001***	111/119 *≈26/119* 93.28% *22.22%* χ^2= 347.616 ***p*< 0.001***	312/9996 *=2499/9996* 3.12% *25.00%* χ^2= 2551.938 ***p*< 0.001***

Table 4.3. Results of tests for a number of experimental consistency checks

A number of other consistency measures were implemented in this experiment also. For all additional consistency measures, participants revealed remarkably high levels of consistency, suggesting that they did take the ranking tasks seriously and were consistent in their preference decisions. *Task 13 consistency*, *Questionnaire consistency* and *Transitivity violations* all occurred at levels significantly different from the levels expected by random decision making (all at a level of $p< 0.001$).

4.3. Regression Analysis

Summary statistics suggest that, whilst valuations in Choice are on average greater than in No-Choice, these differences are not statistically significant. The use of parametric analysis enables the inclusion of a number of other factors including demographic effects, preference score and consistency measures, to better isolate the effect of choice on valuations. These are reported in Table 4.4.

Running a regression analysis requires a recognition that the lower censoring of results may impact the dependent variable (valuation amount). Participants were unable to value the mug at less than £0.00 (although this is a natural censoring), but it is possible that those who indicated a value at £0.00 may have true valuations that fall below this (i.e. they may have required a payment to warrant taking the mug away with them). Analysis of the distributions of valuations (see Appendix 4.3) suggests that the occurrence of £0.00 valuation is greater than would typically be expected by normal distribution; recall no participants indicated a maximum (upper limit) valuation. Thus any regression analysis would need to account for this censored lower limit.

A Tobit model is used in Models 1 to 4 to account for this lower censoring. Models 1 and 2 measure the effect of a variety of demographic and experimental effects for both No-Choice and Choice treatments separately. Models 3 and 4 pool both treatments to include the effect of choice on valuation amount. In these models, the dependent variable is the valuation reported by a participant. The following independent variables are used:

Choice: takes the value 1 if the participant was in Choice treatment, 0 if in No-Choice.

Age: takes the value of the participant's reported age, with the minimum reported age standardised to 0 (i.e. the participants reported age, minus 18).

Age^2: takes the square of variable *Age*, to measure for a quadratic effect of age.

Female: takes the value 1 if the participant's reported gender is female, 0 otherwise.

Economics: takes the value 1 if the participant's reported field of study is Economics, 0 otherwise.

Non-UK/ Irish Nationality: takes the value 1 if the participant's reported nationality is from outside the UK and Ireland, 0 otherwise.

Preference Score: takes the value of the preference score (0, 1, …, 8) of the mug that participant values.

Task Choice Consistency (Choice only): takes the value 1 if participants in Choice choose the mug they ranked most highly in the corresponding task in Part One, 0 if otherwise.

All Consistency: takes the value 1 if participants are consistent in all other consistency measures (Task 13 consistency, Questionnaire consistency, no Transitivity violations), 0 otherwise.

*Economics*Choice*: takes the value 1 if the participant's reported field of study is Economics *and* if the participant was in Choice treatment, 0 otherwise.

In Models 3 and 4 in Table 4.4, in line with non-parametric analysis, whilst the effect of choice increases valuations on average, this is not statistically significant, when including other explanatory variables (p= 0.629, p= 0.190, respectively).

Mug Valuations	**Tobit Model** (Lower Limit- £0.00, Upper Limit- £5.00)			
	Model 1 **No-Choice**	Model 2 **Choice**	Model 3 **All**	Model 4 **All**
Choice	-------	-------	0.1213	0.3601
	-------	-------	*(0.250)*	*(0.273)*
Age	0.0962	-0.0069	0.0310	0.0240
	(0.132)	*(0.170)*	*(0.104)*	*(0.102)*
Age^2	-0.0045	-0.0013	-0.0022	-0.0019
	(0.008)	*(0.013)*	*(0.007)*	*(0.007)*
Female	-0.2715	0.5926	-0.0852	-0.0404
	(0.318)	*(0.505)*	*(0.274)*	*(0.270)*
Economics	0.5389	-0.6816	-0.0499	0.5399
	(0.390)	*(0.511)*	*(0.321)*	*(0.432)*
Non-UK/ Irish Nationality	0.3860	0.2395	0.3697	0.4379
	(0.405)	*(0.561)*	*(0.334)*	*(0.330)*
Preference Score	-0.1071	0.0032	-0.0052	-0.0204
	(0.093)	*(0.123)*	*(0.068)*	*(0.067)*
Task Choice Consistency	-------	0.4282	-------	-------
	-------	*(0.619)*	-------	-------
All Consistency	0.0519	-0.5781	-0.2409	-0.2065
	(0.363)	*(0.449)*	*(0.281)*	*(0.277)*
Economics*Choice	-------	-------	-------	-1.2294**
	-------	-------	-------	*(0.614)*
Constant	2.4127***	1.7921**	2.0638***	1.9809***
	(0.676)	*(0.785)*	*(0.520)*	*(0.513)*
Sigma	1.1388***	1.4149***	1.3288***	1.3066***
	(0.107)	*(0.142)*	*(0.091)*	*(0.090)*
# Obs	60	59	119	119

Standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1

Table 4.4. Tobit models of choice, demographics, consistency and preference score on mug valuations

It is possible that the effect of choice is being generated by different categories of experimental participants. Participants must first decide whether or not they wish to value the mug at any positive amount. Table 4.5 reports models which account for these differences. Model 5 utilises

a Probit model, which observes the effect of a number of variables on the probability that participants will value the mug at a positive amount (i.e. an amount greater than £0.00).

This model suggests that belonging to Choice treatment increases the likelihood that a participant would value their mug at £0.00, although this is not statistically significant (*p*= 0.105).

(Likelihood of valuation > £0.00)	**Probit Model** Model 5 All
Choice	-0.7568 *(0.467)*
Age	-0.2589 *(0.299)*
Age^2	0.0335 *(0.032)*
Female	0.9021* *(0.462)*
Economics	0.1883 *(0.538)*
Non-UK Nationality	-1.0863* *(0.640)*
Preference Score	-0.0023 *(0.118)*
All Consistency	-0.5653 *(0.449)*
Constant	2.7631** *(1.098)*
Sigma	------- -------
# Obs	119

Table 4.5. Probit model of choice, demographics, consistency and preference score on likelihood to assign a valuation greater than £0.00

If participants do decide they wish to value the mug positively, they then must decide how highly to value the mug. Table 4.6 utilises a Truncated Regression model, truncated at £0.00 (i.e. it only includes in the model the participants who did value their mug at a positive amount), and observes the effects of a number of additional variables, on valuation amount, conditional on participants valuing at a positive amount. Of participants who did assign a positive value to their mug, the effect of choice was positive and significant (*p*= 0.071).

Mug Valuation (if valuation > £0.00)	Model 6 **Truncated Regression** **All**
Choice	0.4267* *(0.237)*
Age	0.0572 *(0.095)*
Age^2	-0.0057 *(0.007)*
Female	-0.5003* *(0.263)*
Economics	-0.1088 *(0.300)*
Non-UK Nationality	0.8790*** *(0.319)*
Preference Score	-0.0197 *(0.064)*
All Consistency	-0.0198 *(0.266)*
Constant	2.0239*** *(0.493)*
Sigma	1.1416*** *(0.091)*
# Obs	111

Table 4.6. Truncated Regression model of choice, demographics, consistency and preference score on mug valuations

In general, demographic effects appear to have some influence on overall valuations. Interaction variable *Economics*Choice* in Model 4 suggests that Economics study in Choice significantly reduces valuations (p= 0.048). Tables 4.5 and 4.6 suggest that females are significantly more likely to assign a positive value to their mug, but of those who do assign a positive value, females value these significantly less than male participants. Similarly, those who identified as being non-UK/ Irish nationals were significantly less likely to assign a positive value to their mug, but of those who did, non-UK/ Irish nationals on average valued their mugs more highly. This switching of financial decision-making preferences across demographics has been noted in previous studies (e.g. for genders in Andreoni and Vesterlund, 2001) suggesting that such effects may be genuine.

A lack of significance of preference score on mug valuation is reflected in the econometric models. As with the findings in Chapter 3, participants were remarkably consistent in their preferences for different mugs, but an increased preference does not equate to an increase in valuation for a mug. The purchase of a mug may be separated into the attributes of owning a

mug, and the specific printed pattern is one such attribute. Whilst a participant may have preferred, for example, squares over circles, and large over small shapes, this does not appear to translate to an increase in valuation. This may be because other attributes, for example size, apparent quality, or perceived practical use (all of which are constant across all nine options) may be much more important in determining valuation. Therefore, it is not necessarily a surprise that increasing the absolute preferences of different patterned mugs is not met with an increase in valuation.

If goods are evaluated by considering the attributes of a good, and choice increases a focus on the attributes that differ (Basu and Savani, 2017), then participants in Choice should give additional weight to these different patterns. However, the Choice treatment allowed participants to choose the mug they preferred among three possible alternatives, irrespective of its absolute ranking across the full set of nine mugs. This suggests that a pure choice effect is successful in increasing valuations by making salient to participants that that chosen good is the preferred option from a certain subset.

It is of interest to consider the effect of consistency on valuation decisions. 11 participants in Choice did not choose the mug they ranked most desirable in the corresponding ranking task in Part One, and on average these valued their chosen mugs more highly, but not significantly so, as seen in Model 2. For the remaining consistency measures, all three were pooled, to measure the effect of being fully consistent (i.e. indicating *Task 13 consistency*, *Questionnaire consistency* and no occurrence of *Transitivity violation*). In total, 29.4% of participants were fully consistent in these three measures, although there is no evidence that this consistency had any sizeable or significant effect on mug valuation.

5. Discussion

The results from this experiment offer some support of pure choice effects for the valuation of patterned mugs, although this is not as substantial as in the prior study on charitable giving. On average, valuations increase by only 7.2%. It appears in this experiment that participants who value their mug at £0.00 are largely influencing this finding. A truncated regression, which only considers the effect of choice amongst participants who assigned some positive value to their mug, found that choice does have a positive and statistically significant effect on mug valuations, conditional on giving some positive value. One must question whether it is valid to exclude such £0.00 valuing participants.

The motivation of providing a valuation decision of £0.01 in the valuation eliciting BDM mechanism was such that when a participants decided to 'sell' at that value, they indicate they value the good at less than £0.01- which intuitively suggests they assign a valuation of £0.00 (or even that they would require some positive payment in order to be willing to take the good away). Whilst the objective financial value of the mugs was never revealed to participants, it must be apparent that the goods do cost something, and thus suggests that any participant valuing at £0.00 is doing so to reveal their disinterest in receiving the good whatsoever (analogously- this could be interpreted as revealing a *personal* valuation of £0.00, but the consequence is the same).

In the prior study measuring choice in charitable giving, choice had a positive effect in increasing the likelihood that a participant would donate something. This is not the case in this current study. Evidently, there exists some participants for whom a mug was simply not desirable, and choice effects were not sufficient in changing this belief. Since participants were randomly assigned to their treatment, those participants who have no desire to take away a mug may belong to either Choice or No-Choice treatment with equal probability. As seen in the summary statistics, 8/119 participants valued the good at £0.00, 2 from No-Choice and 6 from Choice, a difference which is not statistically significant and as such cannot be rejected as simply random distribution of 'zero-valuing participants' across the two treatments.

The truncated regression, therefore, is simply measuring the effect of choice amongst participants who do have some interest in taking the mug away with them. Amongst these participants, choice does have a positive and significant effect. The issues of 'zero-valuing participants' may be one common to consumable goods in general, or may be simply a response to the type of good used in question. The goods used in this experiment, patterned mugs, were relatively plain with basic geometric patterns (but deliberately designed to be so). A mug is a fairly standard good, which participants may own several of already, or have little use for. Perhaps using a good of higher value, or one of more obvious use to all participants, may suffer less from the issue of 'zero-valuing participants' and as such may see stronger evidence of choice effects across the entire participant population.

The experiment in this paper was designed to test a novel experimental design which could measure a pure effect of choice, whilst holding preferences constant between both Choice and No-Choice treatments. This offers a more conservative measure of choice effects, relative to the prior literature in which the preferences of non-choosers are not considered. A natural

concern in designing this experiment was that participants in Choice would not choose the mug they ranked most highly in the corresponding ranking task. Given that a high level of consistency has been observed in this experiment for the valuation of patterned mugs, *and* at a similarly high level for a different and distinct type of good in the prior study, charitable causes, then one can say with some confidence that this method of experimental design is successful in its aim to make preferences constant across both Choice and No-Choice treatments.

It is possible that this limited evidence of positive effect of choice for consumable goods is also in part a result of this experimental design. Recall, the design intended to better isolate evidence of pure choice effects, holding preferences constant, argued to be an improvement on previous designs in the literature. It would be of potential future interest to test experimentally whether receiving a preferred good does indeed increase valuations. The measure of preference score in this experiment suggests that increasing overall preference does not generate increasing valuations, but the experimental design was such that participants were always receiving a mug they had ranked a most desirable across at least one ranking task. Whilst this study was concerned with a measure of choice when potential preference effects were absent, an experimental design in which the good assigned to participants was in no way related to preferences of participants (as with the designs of previous experiments in the literature) could provide a quantifiable measure of preference effects.

6. Conclusion

The aim of this experiment was to utilise a novel experimental design to measure a pure choice effect in which preferences were constant across both Choice and No-Choice treatments. Whilst this experimental design was primarily intended to measure the effect of choice on donations to charitable causes, since the vast majority of prior experiments in the literature measure choice effects on consumable goods, it was important to use this design for the valuation of consumable goods- patterned mugs.

The results of this experiment suggest some evidence of a pure choice effect, but this effect is smaller than other experiments using consumable goods. This may be a result of two factors. The first is that preference effects, which are made constant across both treatments in this experiment, may be a large contributing factor to choice effects in the previous experiments in the literature. Second, choice effects in this experiment were found to be significantly stronger when only considering participants who assigned some positive value to their mug. Potential

causes of this finding, and possible future research to better account for these factors, has been discussed.

Acknowledgements:

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Appendices

Appendix 4.1. Descriptions of goods used

A plain white ceramic mug with small printed black squares. Dishwasher and microwave safe.

A plain white ceramic mug with small printed black circles. Dishwasher and microwave safe.

A plain white ceramic mug with small printed black triangles. Dishwasher and microwave safe.

A plain white ceramic mug with medium printed black squares. Dishwasher and microwave safe.

A plain white ceramic mug with medium printed black circles. Dishwasher and microwave safe.

A plain white ceramic mug with medium printed black triangles. Dishwasher and microwave safe

A plain white ceramic mug with large printed black square. Dishwasher and microwave safe.

A plain white ceramic mug with large printed black circle. Dishwasher and microwave safe.

A plain white ceramic mug with large printed black triangle. Dishwasher and microwave safe.

Appendix 4.2. Statistics of tasks and mugs observed in Part Two

4.2.1. Tests of non-random distribution of mean valuation and frequency by mug selected or chosen in Part Two, across treatments

	No-Choice		**Choice**	
Mug	Mean (£)	(n)	Mean (£)	(n)
Small Squares	£2.29	13	£2.69	11
Small Circles	£1.71	14	£2.47	5
Small Triangles	£2.84	11	£2.03	10
Medium Squares	£2.63	6	£1.98	3
Medium Circles	£0.60	1	£1.80	7
Medium Triangles	£2.01	3	£3.00	6
Large Squares	£1.87	6	£2.96	5
Large Circles	£2.60	3	£3.20	1
Large Triangles	£2.40	3	£2.20	11
Mean (£)	**Kruskal-Wallis Test**		**Kruskal-Wallis Test**	
	$\chi^2(8)$-stat	8.794	$\chi^2(8)$-stat	6.423
	p-value	0.360	*p*-value	0.600
(n)	**χ^2-Test**		**χ^2-Test**	
	$\chi^2(8)$-stat	27.900	$\chi^2(8)$-stat	15.288
	p-value	**< 0.001*** **	*p*-value	**0.054***

4.2.2. Tests of non-random distribution of mean valuation and frequency by task selected in Part Two, across treatments

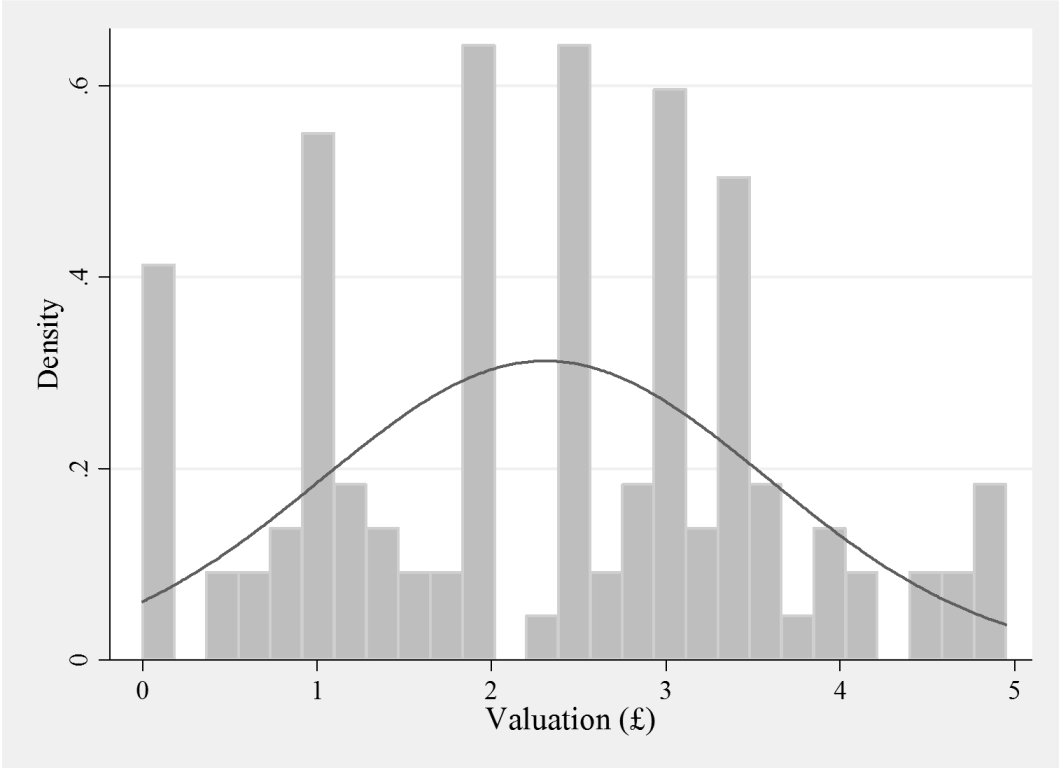
	No-Choice		**Choice**	
Task	Mean (£)	(n)	Mean (£)	(n)
1	£2.38	8	£2.60	4
2	£2.00	4	£2.50	10
3	£1.60	2	£2.12	5
4	£1.55	4	£2.39	5
5	£1.64	5	----	0
6	£2.60	5	£3.40	1
7	£2.45	7	£2.60	2
8	£3.00	5	£1.76	4
9	£1.88	5	£3.20	3
10	£2.00	4	£2.51	7
11	£2.10	6	£2.26	9
12	£2.80	5	£2.20	9
Mean (£)	**Kruskal-Wallis Test**		**Kruskal-Wallis Test**	
	$\chi^2(11)$ -stat	7.383	$\chi^2(11)$ -stat	4.267
	p-value	0.767	*p*-value	0.935
(n)	**χ^2-Test**		**χ^2-Test**	
	$\chi^2(11)$-stat	5.200	$\chi^2(11)$-stat	23.780
	p-value	0.921	*p*-value	**0.014****

Appendix 4.3. Distribution of valuation amounts

4.3.1. Summary statistics of distribution of grouped valuation

Valuation (*n*)	**All**	**No-Choice**	**Choice**
£0.00	8	2	6
£0.05-£0.95	8	6	2
£1.00 - £1.95	23	14	9
£2.00 - £2.95	35	19	16
£3.00 - £3.95	32	14	18
£4.00 - £4.95	13	5	8
£5.00	0	0	0

4.3.2. Histogram of distribution of actual valuation



Appendix 4.4. List of ranking task combinations

Task Number	Task Name	Mug Type		
1	**Small**	Small Squares	Small Circles	Small Triangles
2	**Medium**	Medium Squares	Medium Circles	Medium Triangles
3	**Large**	Large Squares	Large Circles	Large Triangles
4	**Squares**	Small Squares	Medium Squares	Large Squares
5	**Circles**	Small Circles	Medium Circles	Large Circles
6	**Triangles**	Small Triangles	Medium Triangles	Large Triangles
7	**Mix 1**	Small Squares	Medium Circles	Large Triangles
8	**Mix 2**	Medium Squares	Large Circles	Small Triangles
9	**Mix 3**	Large Squares	Small Circles	Medium Triangles
10	**Mix 4**	Small Squares	Large Circles	Medium Triangles
11	**Mix 5**	Medium Squares	Small Circles	Large Triangles
12	**Mix 6**	Large Squares	Medium Circles	Small Triangles

Notes: Whilst a task number was assigned to each specific ranking task, the randomisation of task order meant that participants completed these tasks in different orders.

Appendix 4.5. Experimental instructions

Introduction

Welcome to this experiment on decision making. Thank you for coming. Please follow along as I read through the instructions. If you have a question, please raise your hand and I will come to answer your question privately.

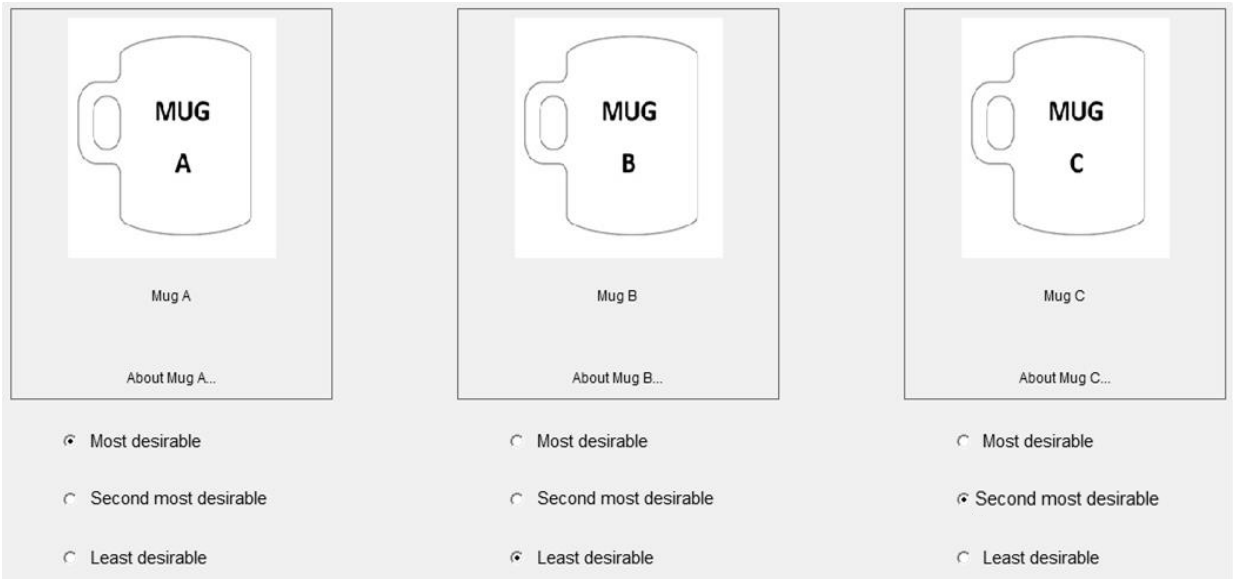
Your decisions in this experiment are private, and we ask you not to communicate with others or react verbally to outcomes during this experiment. If you have any questions during the experiment please raise your hand and an experimenter will come to assist you. Please do not eat or use any electronic devices whilst you are taking part in this experiment. Please keep to these simple rules, as anyone breaking them may be asked to leave without payment. Various agencies have provided funding for this experiment.

This experiment consists of two parts, Part 1 and Part 2. I will shortly read the instructions for Part 1. You will receive the instructions for Part 2 after Part 1 has finished. In completing Part 1, you will receive £5. What you will earn in Part 2 will be explained in more detail after Part 1 has finished. I will now read aloud the instructions for Part 1.

Part 1

The following instructions are simple, but please follow them carefully. Part 1 consists of thirteen tasks. For completing Part 1, you will receive £5 at the end of the experiment.

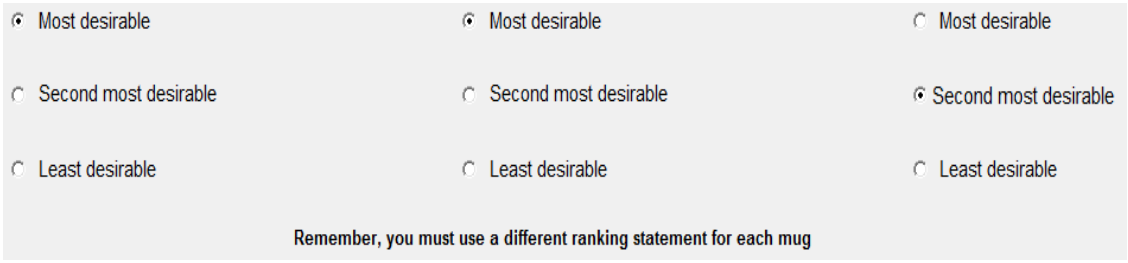
We are interested in your attitudes towards different consumable goods. In each task you will be shown images of three real consumable goods- patterned mugs. In each task, you will be asked to rank the three mugs, based on how desirable you believe them to be. Underneath each mug will be three ranking statements “Most desirable”, “Second most desirable” and “Least desirable”. For each mug, you will be asked to select the ranking statement that you believe best reflects your beliefs about the desirability of the mugs. To assist in the explanation of the ranking tasks, please see the example below.



Here there are three mugs, A, B and C. The participant has ranked Mug A "Most desirable", indicating that, of these three mugs, this is the mug the participant believes to be *most* desirable. The participant has ranked Mug C "Second most desirable", indicating that, of these three mugs, this is the mug the participant believes to be *second most* desirable. Finally, the participant has ranked Mug B "Least desirable", indicating that, of these three mugs, this is the mug the participant believes to be *least* desirable.

The tasks are asking you to rank the mugs by how desirable you believe them to be, *as if* you were being asked to consider purchasing them, and so you should take these decisions seriously.

Please note, there are no right or wrong answers to these ranking tasks, and your decisions in all tasks will be completely anonymous. When ranking each mug, you must use a different ranking statement for each mug (one "Most desirable", one "Second most desirable" and one "Least desirable") - there can be no ties. You will not be able to confirm your ranking and leave the task until each mug has a different ranking statement. Until you give each mug a different ranking statement a reminder message will appear on your screen and you will have to edit your rankings to make each one different before being able to continue. Below is an example of an identical ranking and the error message that would be shown below the ranking statements.



You will complete a ranking exercise for each of the thirteen tasks. Please note you may see the same mug in more than one task. After everyone has completed all thirteen tasks, Part 1 will finish. At the end of the experiment, all participants will receive £5 for completing Part 1. Part 2 will be explained once Part 1 is complete.

Before we proceed, I ask you to answer the short quiz that will follow shortly on your screens, to ensure you understand the tasks in Part 1. Please attempt these and feel free to re-read the instructions as you do so. If you have any queries please raise your hand and an experimenter will come to assist you.

Now we are ready to start Part 1.

Part 2

I will now read aloud the instructions for Part 2. What you earn in Part 2 will be added to the £5 you have already earned in Part 1. The amount you earn in Part 2 will be determined only by the decisions you make in Part 2. Part 2 consists of two tasks, the *choice* task, and the *price* task.

Choice Task

In the choice task, you will be shown on your screens three of the mugs you previously ranked in Part 1. Of these three mugs, you will be asked to choose one. The mug that you choose will be the mug you have the opportunity to take away with you. Once you have chosen this mug, you will be shown on your screens only the mug you have chosen.

Price Task

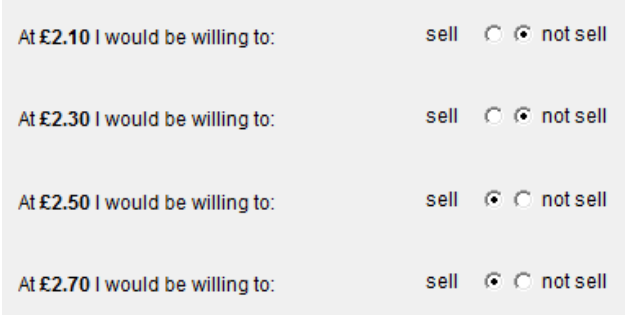
At the beginning of the price task, you will be given your chosen mug. You will have the opportunity to either take this mug away with you, or to sell it back to the experimenter. You will be shown the pricing page for the mug you have chosen. The pricing page lists 27 possible monetary amounts ranging from £0.01-£5.00. At each of these prices, you will be asked if you would be willing to sell the mug back to the experimenter at that price, or not.

Once you have made a decision to 'sell' or 'not sell' at each of these 27 values, you will be shown a page with 27 grey pricing boxes. Behind these boxes are the 27 values from the pricing task. You will be asked to select one of these boxes. The price behind the box you select will be the price that the experimenter offers for your mug.

If you have indicated you would be willing to sell the mug at this price, you will give up the mug and receive that price in addition to the £5 you earned in Part 1. If you have indicated you would not be willing to sell the mug at this price, you will take the mug away with you and receive no additional money (but you will still receive the £5 you earned in Part 1.)

Please note that your decisions in the price task cannot affect which price the experimenter offers. So when deciding whether or not you are willing to sell at the listed prices, it is in your interest to think about each price separately.

To assist in the explanation of the price task, here is an example. Suppose a participant in the experiment is asked whether or not they would be willing to sell their mug at the following prices:



An individual with consistent preferences would switch from 'not sell' to 'sell' no more than once. This is because they would not want to sell at any price less than their personal valuation of the mug and would want to sell at prices greater than this. Here the participant has indicated that they are not willing to sell at prices £2.10 and £2.30, but that they are willing to sell at prices £2.50 and £2.70. This suggests that the personal valuation of the mug by the participant is somewhere greater than £2.30, but less than £2.50. If one of these four prices was behind a pricing box selected by the participant, to be the offer made by the experimenter, the participant's decision at that price would be made binding.

Once you have selected a pricing box and your decision at that price has been made binding, you will be asked to complete a questionnaire. After completing this you will be shown your final earnings, including whether or not you will take a mug away with you. The experiment will end when everyone has completed the price task and questionnaire. Payment of your earnings and the giving of any mugs will be made, in private, upon leaving this experiment.

Before we proceed, I ask you to answer the short quiz that will follow shortly on your screens, to ensure you understand the tasks in Part 2. Please attempt these and feel free to re-read the instructions as you do so. If you have any queries please raise your hand and an experimenter will come to assist you.

Now we are ready to start Part 2.

Notes: These experimental instructions were for participants in Choice. For participants in No-Choice, the 'Choice Task' section was removed and terminology replaced to explain a mug would be selected for participants by their computer.

Appendix 4.6. Pre-experimental quiz (correct answer in **bold**)

Part One

Question 1: How many tasks will you complete in Part 1?

g) 1
h) 3
i) 13

Question 2: How much will you earn for completing Part 1?

d) £5.00
e) £13.00
f) It depends

Question 3: Imagine three mugs, Mug A, Mug B and Mug C. Suppose you thought that Mug A was the mug that you believe to be *most* desirable. What ranking would you give this mug?

a) Most desirable
b) Second most desirable
c) Least desirable

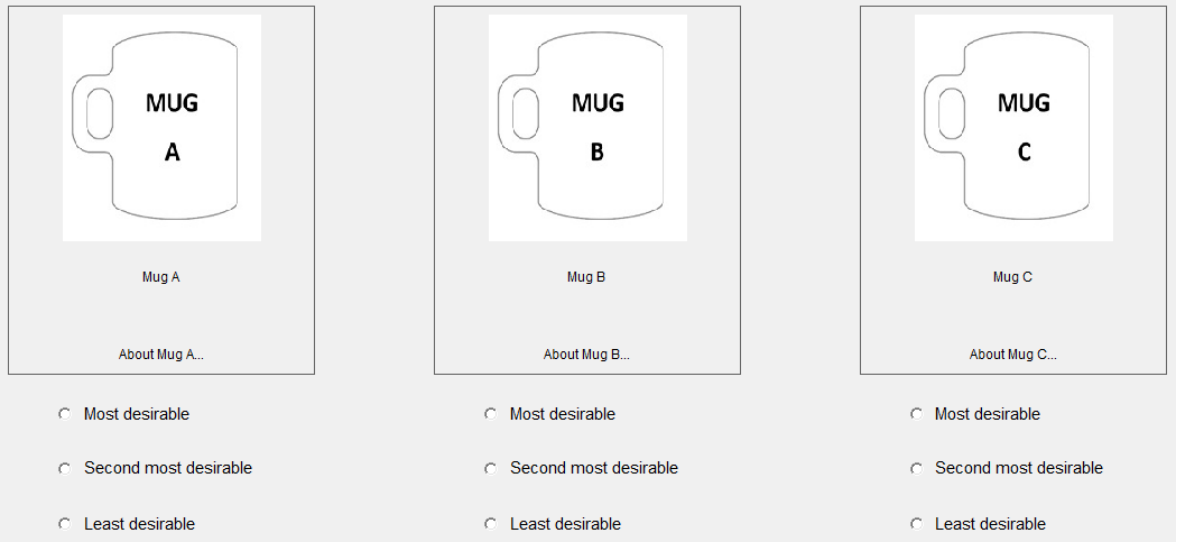
Question 4: Imagine three mugs, Mug A, Mug B and Mug C. Suppose you thought that Mug C was the mug that you believe to be *second most* desirable. What ranking would you give this mug?

a) Most desirable
b) Second most desirable
c) Least desirable

Question 5: Imagine three mugs, Mug A, Mug B and Mug C. Suppose you thought that Mug B was the mug that you believe to be *least* desirable. What ranking would you give this mug?

a) Most desirable
b) Second most desirable
c) Least desirable

Question 6: Take a look at these three mugs below. Suppose you thought that Mug C was the mug that you believe to be *most* desirable, Mug A was the mug that you believe to be *second most* desirable, and Mug B was the mug that you believe to be *least* desirable. Please select the ranking statements below that correspond to these preferences.



Correct ranking preference statements:

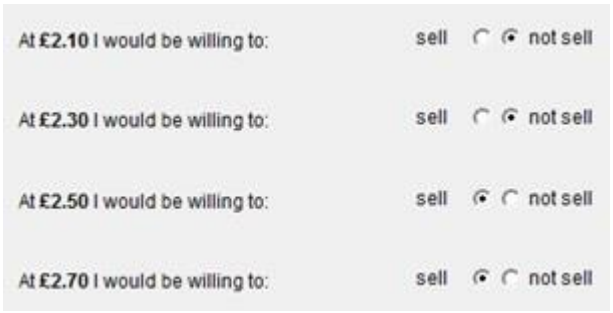
Mug A: Second most desirable

Mug B: Least desirable

Mug C: Most desirable

Part Two

Question 1: Using the example given below, suppose a price of £2.30 is selected. What does the participant do?

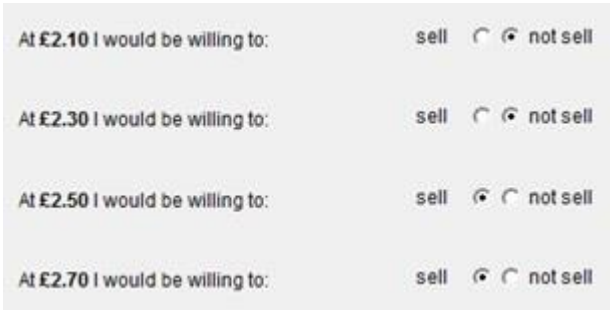


a) Not sell the mug and pay £2.30
b) Not sell the mug and receive no money
c) Sell the mug and receive £2.30

Question 2: Using the example given below, suppose a price of £2.90 is selected. What does the participant do?

a) Not sell the mug and pay £2.90
b) Not sell the mug and receive no money
c) Sell the mug and receive £2.90

Question 3: Using the example given below, what can we say about the preferences of this participant?



a) The participant has consistent preferences

b) The participant has inconsistent preferences

c) We cannot say at this stage

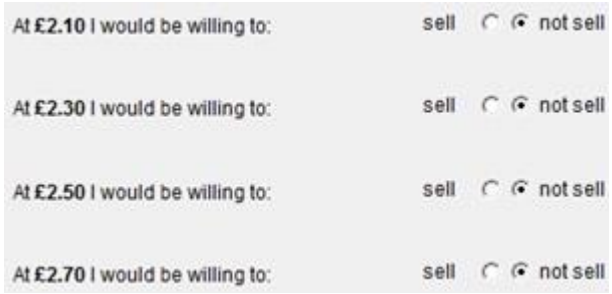
Question 4: Using the example given below, what can we say about the preferences of this participant?

a) The participant has consistent preferences

b) The participant has inconsistent preferences

c) We cannot say at this stage

Question 5: Using the example given below, what can we say about the preferences of this participant?



a) The participant has consistent preferences

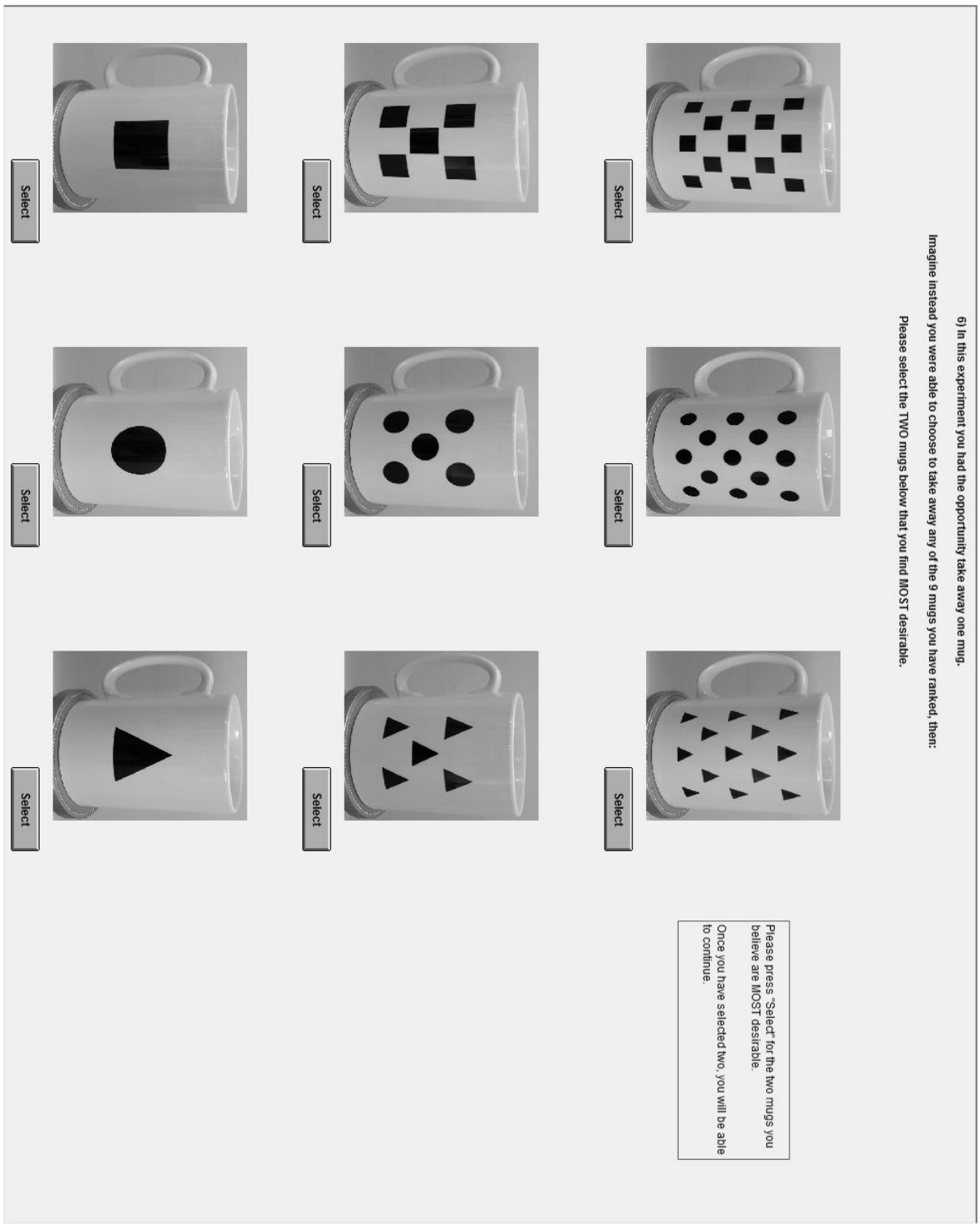
b) The participant has inconsistent preferences

c) We cannot say at this stage

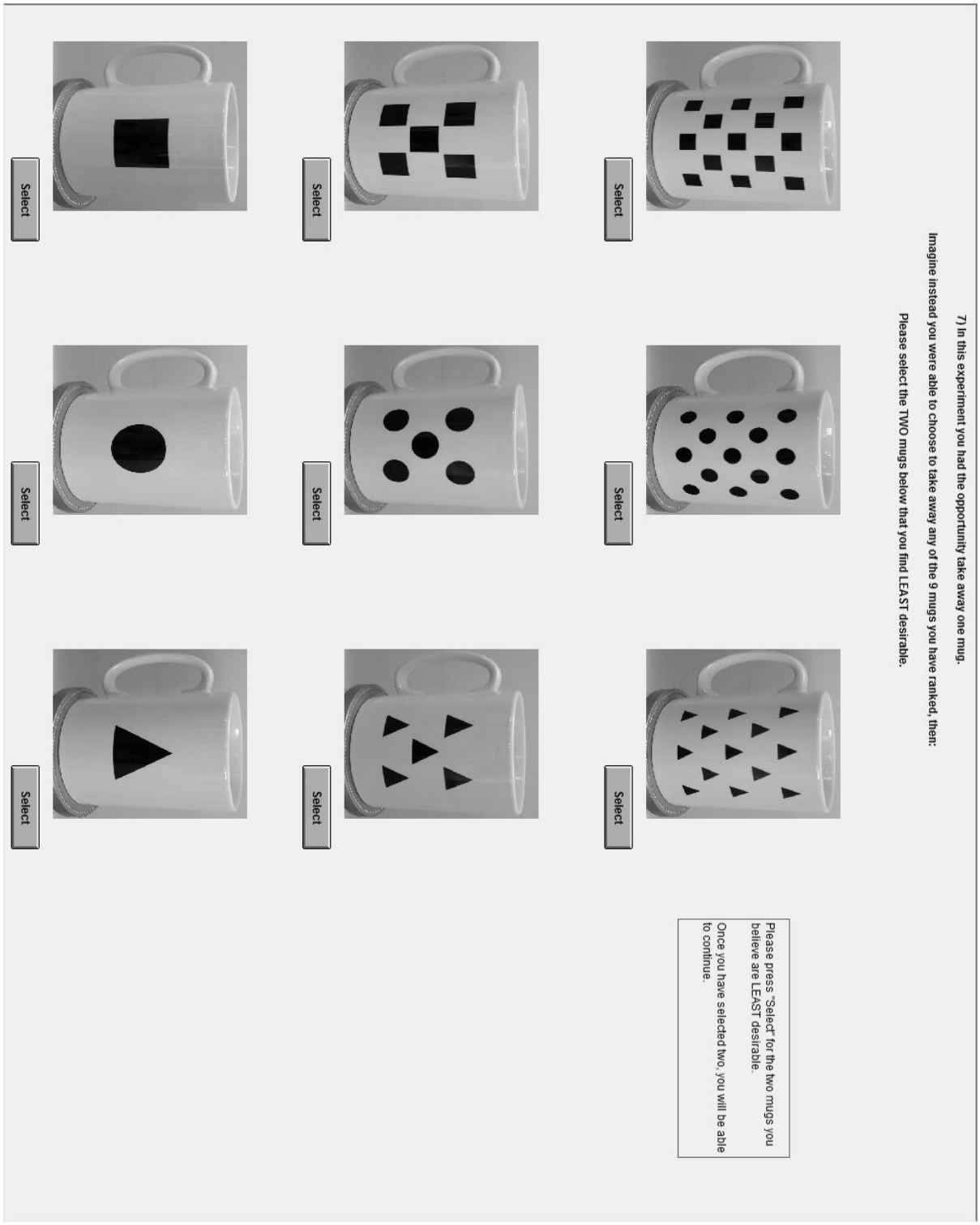
Appendix 4.7. Post-experimental questionnaire

4.7.1. Demographic information questionnaire

4.7.2. Post-experimental consistency check for *most* preferred mugs

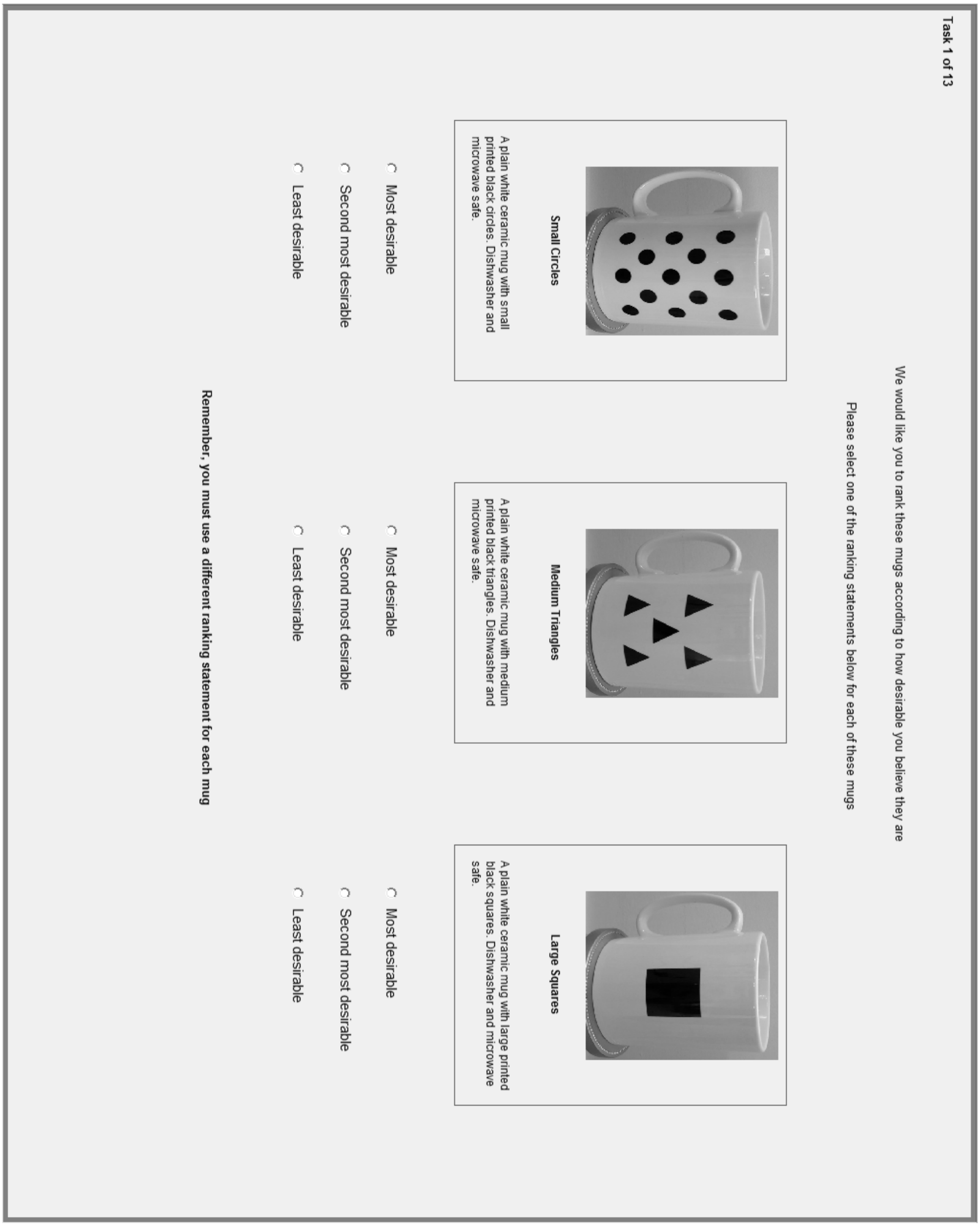


4.7.3. Post-experimental consistency check for *least* preferred mugs

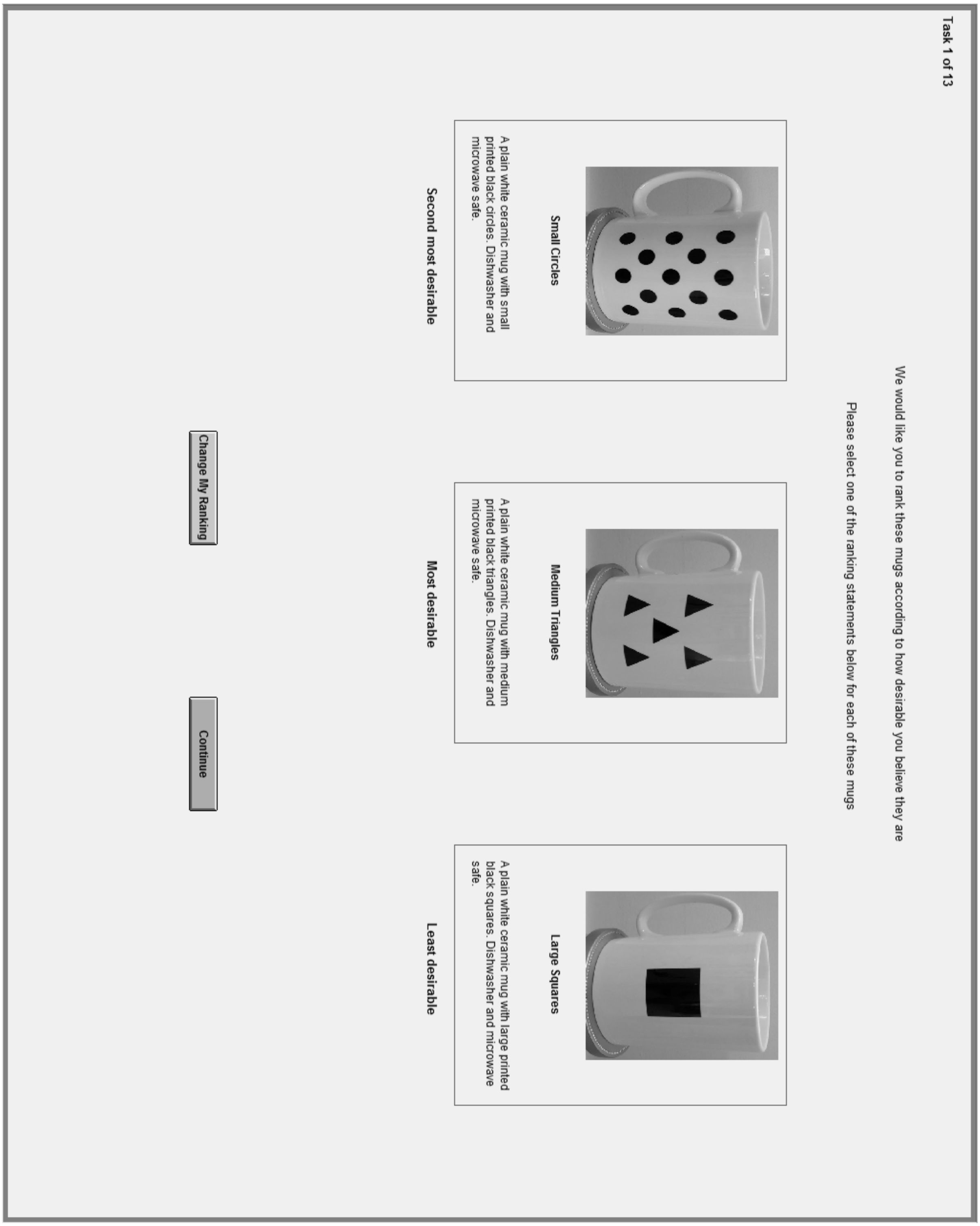


Appendix 4.8. Experimental screenshots

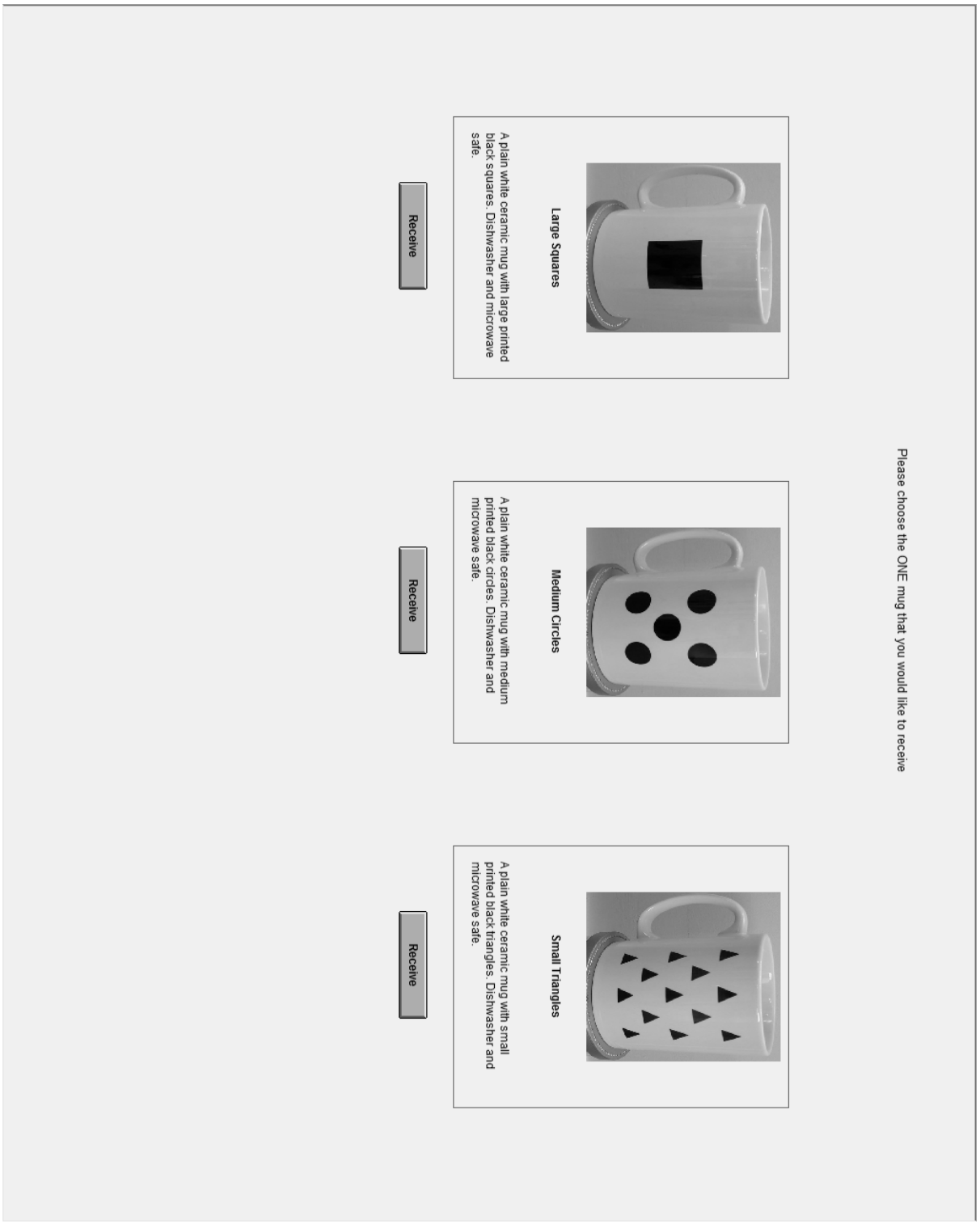
4.8.1. Ranking task in Part One



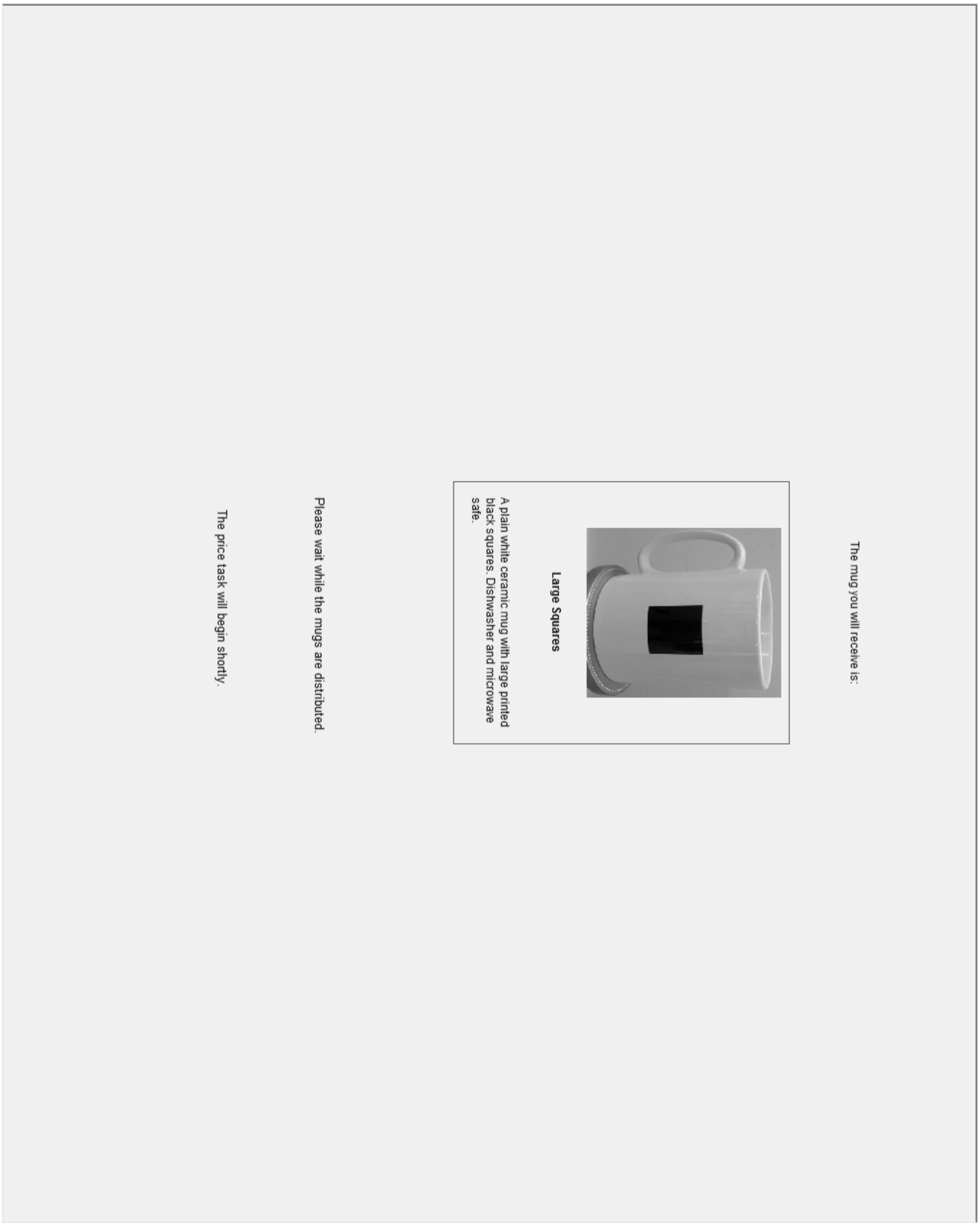
4.8.2. Ranking task confirmation in Part One



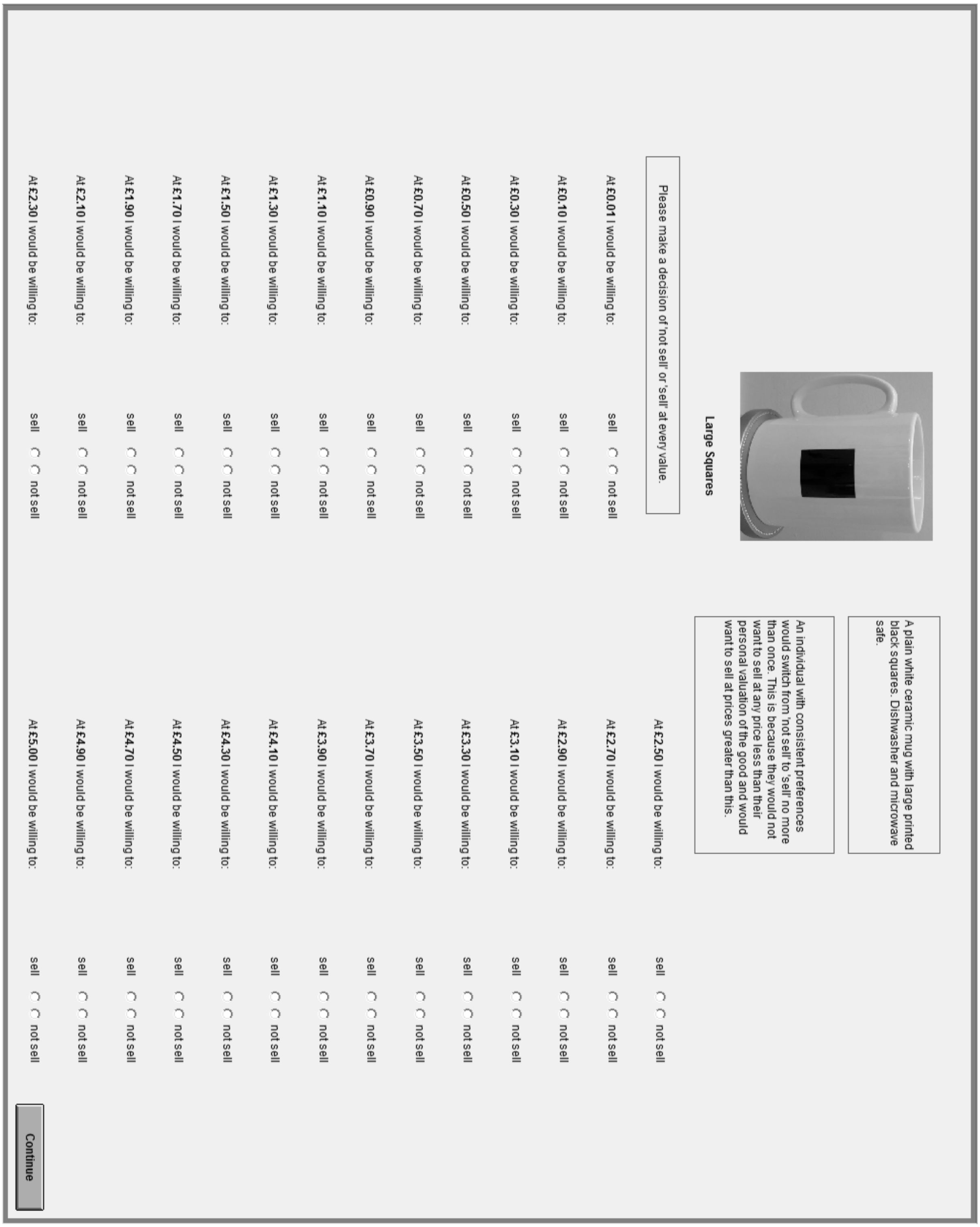
4.8.3. Choice task in Part Two (Choice treatment only)



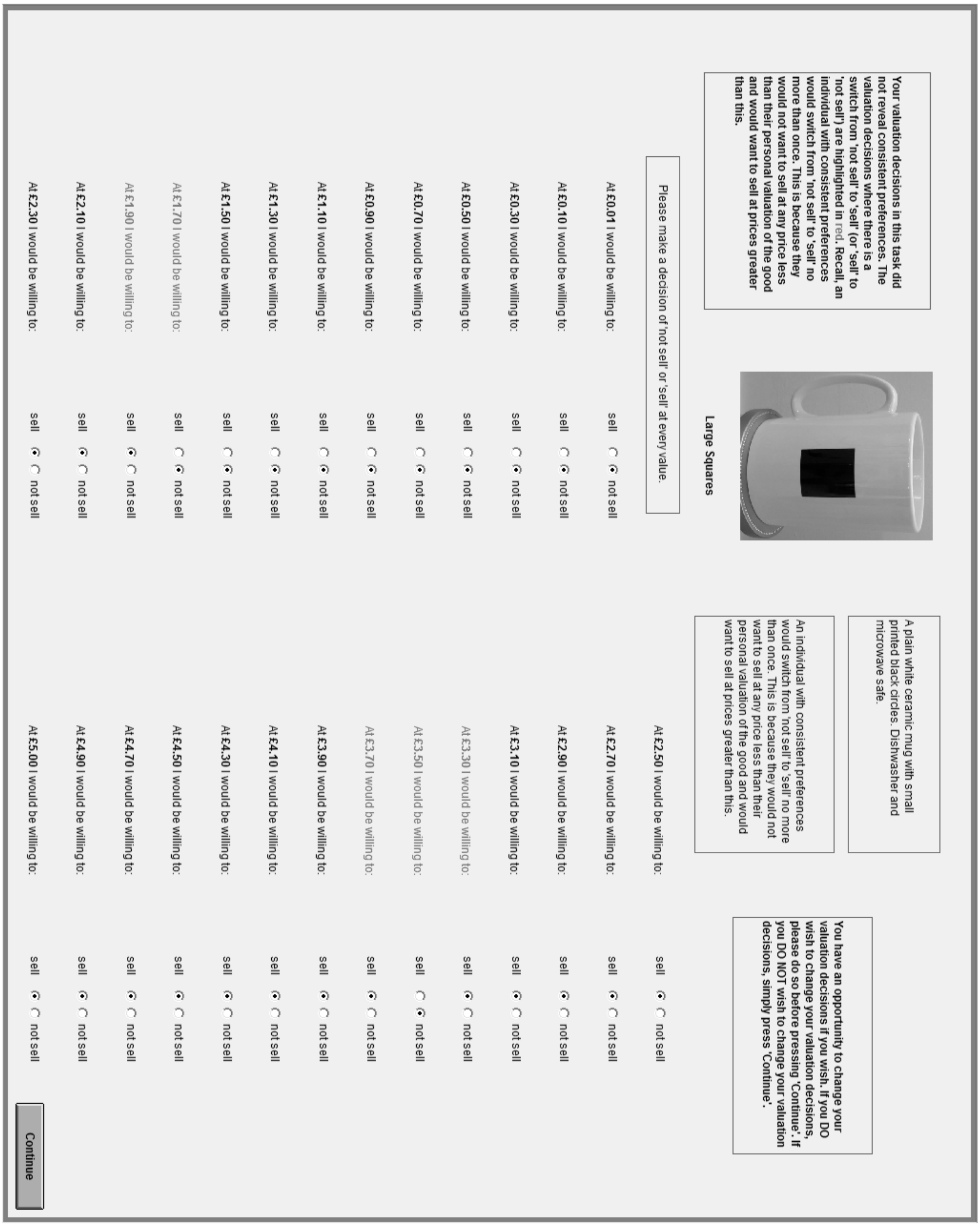
4.8.4. Chosen (Choice treatment) or selected (No-Choice) mug confirmation



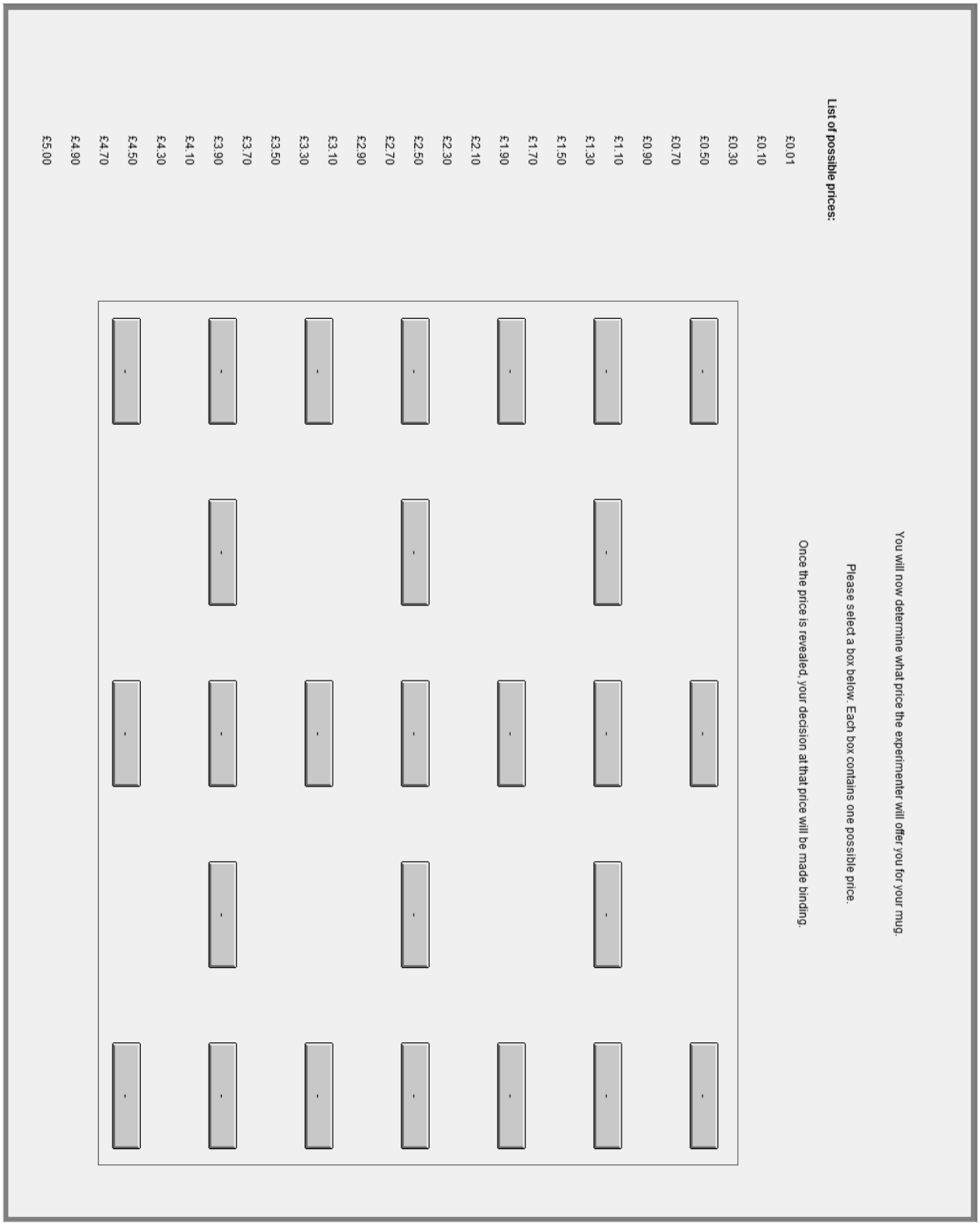
4.8.5. BDM valuation elicitation page



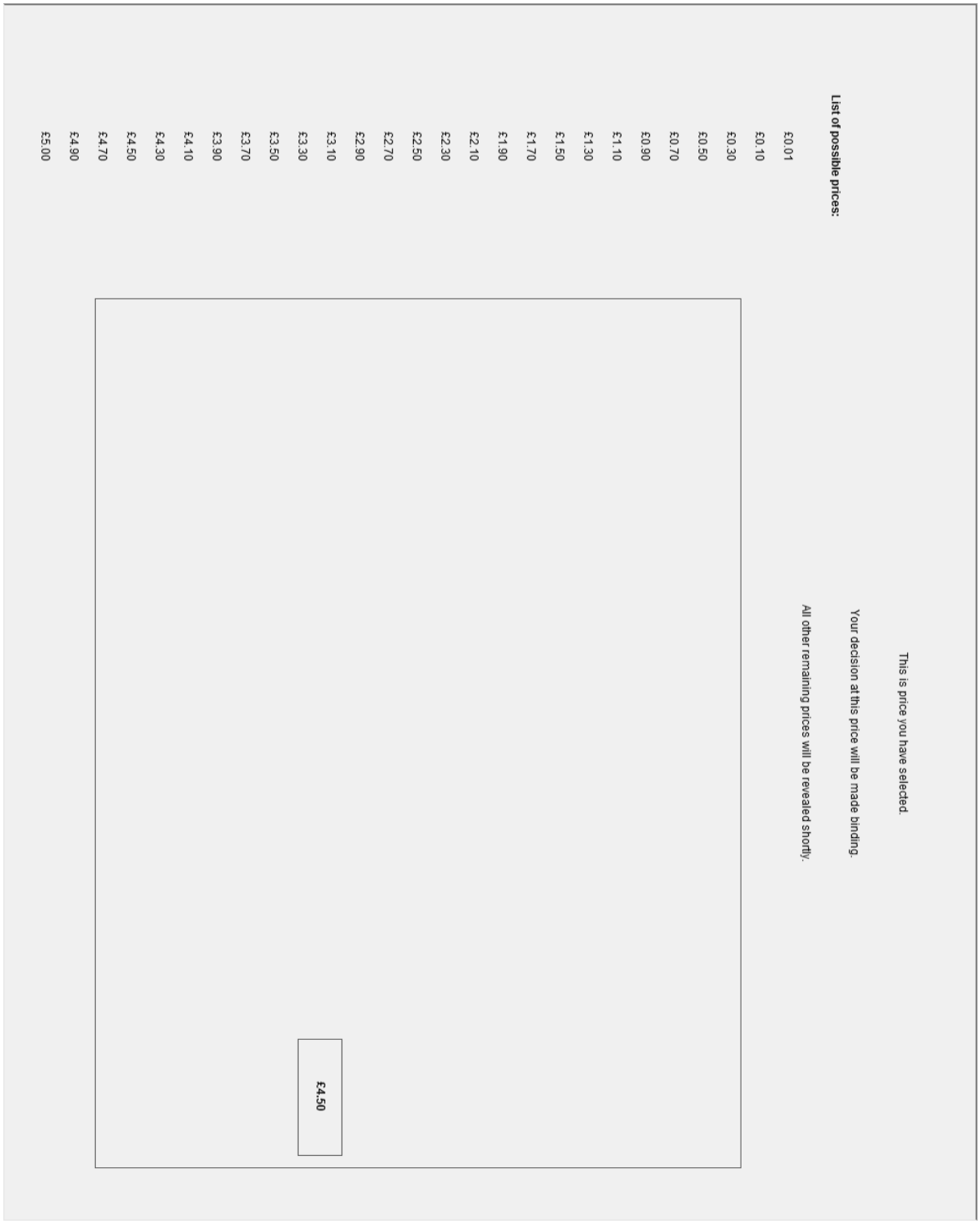
4.8.6. BDM valuation elicitation page- inconsistent preferences revision opportunity



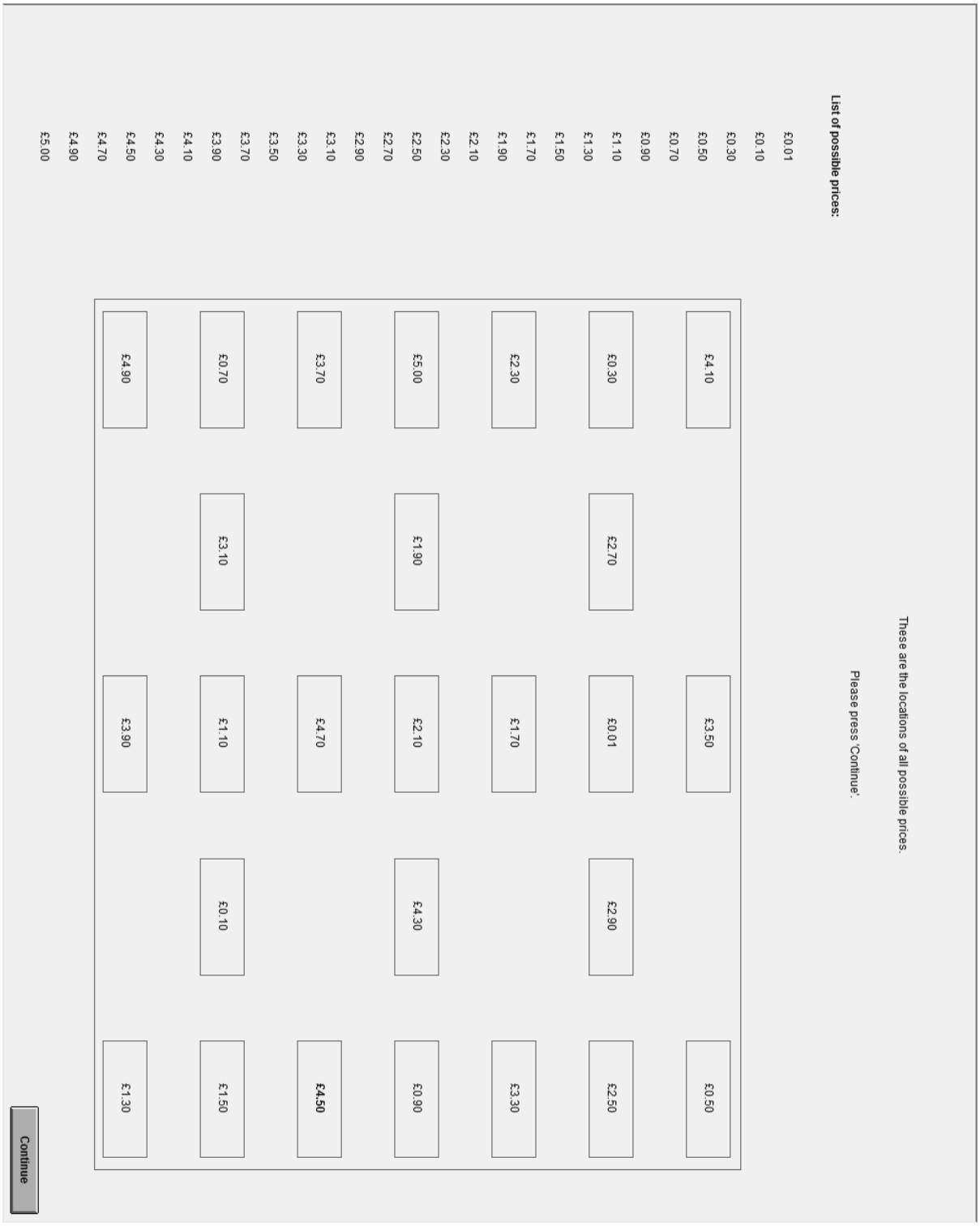
4.8.7. Valuation box choice page



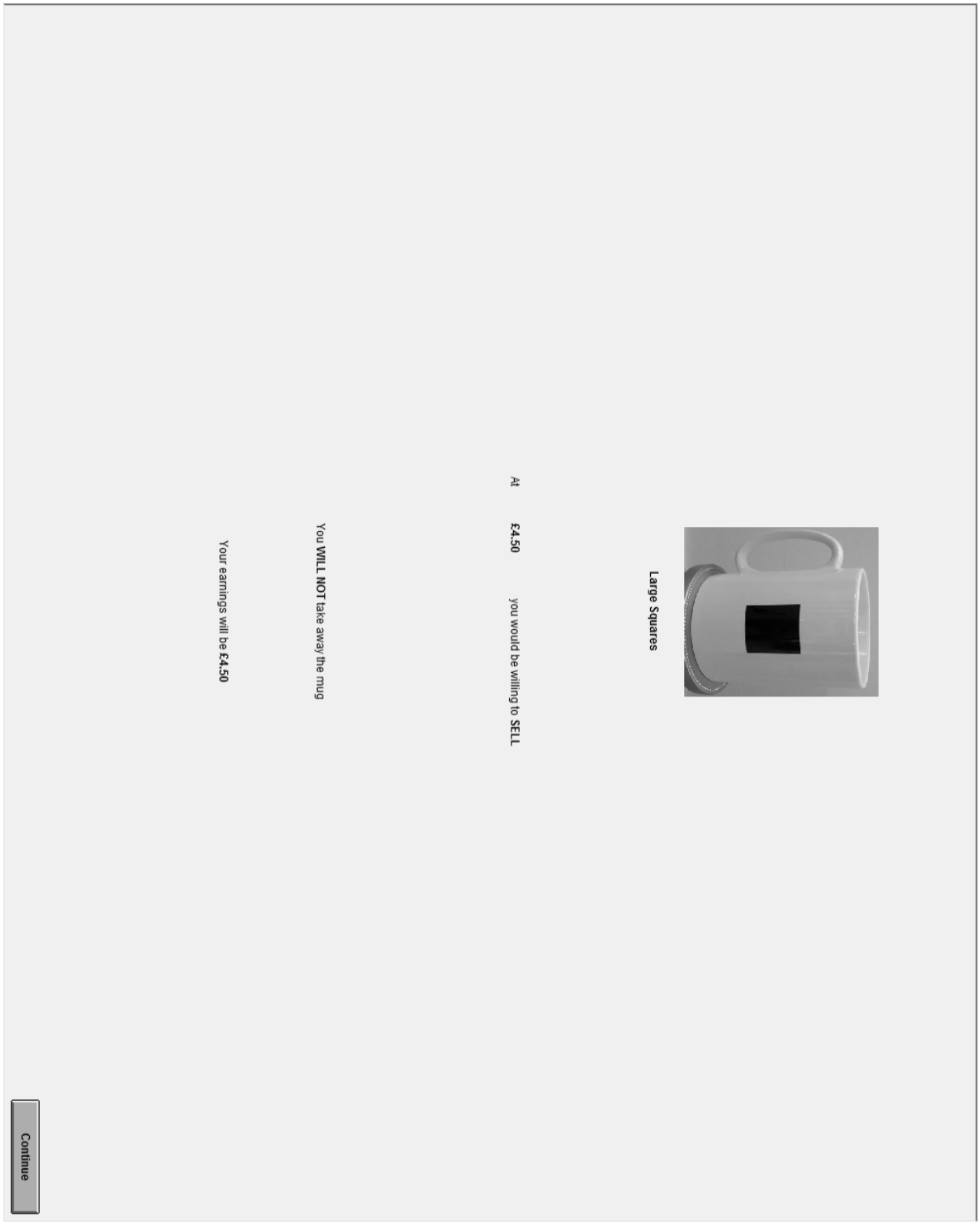
4.8.8. Valuation box choice page- selected choice



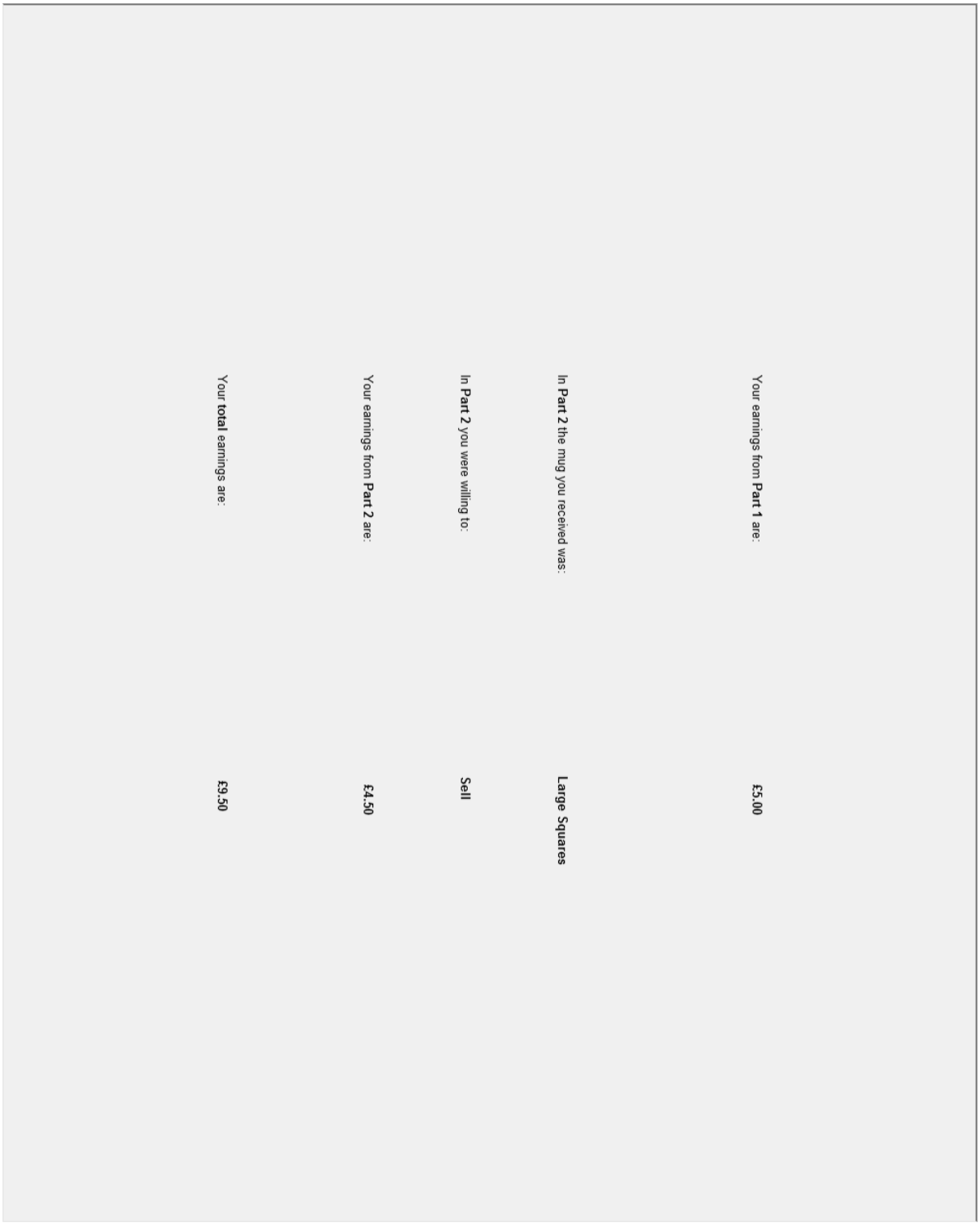
4.8.9. Valuation box choice page- location of all prices revealed



4.8.10. Experiment outcome summary page



4.8.11. Final earnings page



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