Do preference reversals generalise? Results on ambiguity and loss aversion

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Do Preference Reversals Generalise? Results on Gamble Transparency and Reference Points*

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Abstract
Preference reversals are frequently observed in the lab, but almost all designs use completely transparent prospects, which are rarely features of decision making elsewhere. This raises questions of external validity. We test the robustness of the phenomenon to gambles which incorporate realistic ambiguity in payoffs and probabilities. In addition, we test a recent rationalisation of preference reversals by Third Generation Prospect Theory, which would also restrict the incidence of reversals outside the lab. According to this account, reversals occur largely because the selling protocol generally used for the valuation task activates loss aversion, which is excluded by the free gift protocol of the choice task. We find that reversals are not dependent on these procedures, though they seem to be encouraged by transparency.

Keywords: Preference Reversals, External Validity, Ambiguity, Loss Aversion

1. Introduction

Psychologists often, perhaps typically, interpret the behavioural economics literature as providing copious evidence that people do not exhibit the rational consistency in behaviour that mainstream economic theory attributes to them. But many researchers, especially in economics, have been reluctant to endorse this interpretation. It is common instead to question aspects of the lab, to run designs which attempt to make the troublesome results disappear, and to devise modifications of decision theory which might rationalise them. This pattern of responses is particularly evident over the "preference reversal" phenomenon, whereby a subject in an experiment chooses "P" over "$", but attaches a higher monetary value to $ than to P. P is usually a safe bet, and $ is usually a risky bet offering a higher prize but a lower probability of winning it. We aim to shed light on the external validity of the findings and to test an apparently compelling, recent attempt to rationalise them.

Lichtenstein and Slovic's favoured interpretation of the phenomenon was in terms of the compatibility between gamble attributes and the tasks involved in the design. When asked to give monetary valuations of the bets, subjects' attention may be drawn to the monetary prizes involved, to a greater extent than when subjects have to choose a gamble. Whilst this is an explanation of the phenomenon that is still standing after extensive testing, one might question the extent to which the phenomenon is informative about decisions in general under this account. It is possible in this context that subjects are "anchoring" on the specific higher prize figure in the risky bet, in which case the reversals might be a product of unusually transparent outcomes used in the laboratory tasks. In the real world, one might argue, we are not often called upon to value chances of prizes of money, outside of specific arenas like gambling. Rather, we have to assess the values of goods and services; the role of money is to enable commensurable valuations of non-money items. It is debatable philosophically whether a test of a theory is undermined by a lack of external validity, and the contrary view, defended for example by Schram (2005), is perhaps prevalent amongst experimental economists. Independently of this issue, though, we find it natural to probe how the results might extend to other contexts than the lab, given the large amount of research attention they have attracted over recent decades.[1] Our position is that even if theory testing designs do not have to have external validity, we learn more from them if they do, so it is important to ask the question.

Bohm (1994) is perhaps the best known test of whether preference reversals generalise to tasks which are more realistic, in that the probabilities and consequences are not transparent. Bohm used real goods in an auction, namely two used cars, to conduct the valuation task of his experiment, finding no evidence of preference reversals. However, although this clearly departed from transparency, it is perhaps too uncertain how subjects interpreted the options available. A possible interpretation of the results is that subjects had very firm preferences over the cars, which were reflected in both choice and valuation procedures. Even using transparent prospects, it is easy to observe zero reversals by making one of the gambles very much more attractive, either by making its expected value much greater or making it stochastically dominant. But that does not alter the fact that reversals are common when the two gambles are closer in attractiveness.

We adopt a different strategy to Bohm, suggested by Bardsley *et al.* (2010). This is to explore external validity *in the lab*. We do so by modifying the attributes of the gambles which can be regarded as most artificial, but retaining a degree of control over the gamble characteristics that is difficult to achieve in a field experiment. We retain the essential characteristics of the P and $ bets, described above, but explore the introduction of ambiguity in both payoffs and probabilities. Analytically, it seems plausible that ambiguity reduces to uncertainty over probabilities.[2] However, it does not follow that experimenters should use only tasks that are transparent in consequences. For decision processes, and potential heuristics and biases, may be different when subjects have to construct for themselves the set of consequences over which probabilities range. We vary the degree of vagueness of the tasks to explore the robustness of preference reversals to departures from transparency. Our use of prospects which are non-transparent in both probabilities and consequences extends work by Trautmann *et al.* (2009), which found reversals for gambles which were ambiguous in probabilities only.

Using ambiguous prospects is a probe of the extent to which the results might generalise, inspired *inter alia* by the compatibility hypothesis. For it is possible that reversals are dependent upon transparency, depending on the mechanisms involved, if that hypothesis is true. Two of the mechanisms that have been suggested for the compatibility hypothesis, namely anchoring and adjustment, and imprecision of preferences, are typically expounded with reference to precise monetary values for the prizes. See for example Butler and Loomes (2007), Cox (2008) and Blavatskyy (2008).

A second aim of the experiment was to test the loss aversion explanation of preference reversal, first stated by Sugden (2003), and recently incorporated into Schmidt *et al.*'s (2008) "Third Generation Prospect Theory" (PT3). The idea is that, in the choice task, the reference point from which gambles are evaluated is pre-experiment wealth, so that both gambles are regarded as improvements on the status quo. Whereas in the valuation task, since this is generally conducted under a selling protocol, subjects are placed in the position of already possessing the gamble. The theoretical innovation is a reworking of prospect theory to model changes in the reference point, and a key motivation of the authors is to better explain preference reversals. Using this framework, Schmidt *et al.* show that the standard pattern of reversals can be attributed largely to loss aversion, with sellers of gambles being averse to the prospect of having sold a $-bet in the event that it would have paid out, thus "losing" the high payoff. Because of the protocol of the choice task, in which subjects choose between potential gains, such a prospect would not be evaluated as a loss there. Given the asymmetry of the valuation function specified in Prospect Theory, one would expect to observe many preference reversals of the standard kind if the P and $ gambles are close enough in attractiveness.

Testing the loss aversion explanation of preference reversals is of interest in its own right, but our two research questions are not entirely unrelated. For if a reference point shift is responsible for the results, one might expect behaviour outside the lab to have greater apparent consistency.[3] The reason for this is related to Bohm's (1993, 1994) work. Bohm notes that the prospects used in preference reversal designs are unusual in that they are such good bets. If a casino used such gambles in its daily business, it would be showering its pundits with money; thus, such bets cannot exist in a market. Although that is certainly true, for the criticism to hold a reason still needs to be found why reversals are especially likely with unusually good bets, since the choice and valuation tasks use the same bets. We share Bohm's intuition that the standard design might be too rosy to be representative, but we think that the more likely culprit is the choice task. The reference point implemented by the choice task seems unusual, because in normal life choices between free gifts are, sadly, rare. Presents aside, we tend to be giving something up to gain something else. Indeed, on an influential view associated in particular with Robbins (1932), economics is basically coextensive with such "scarcity."

We introduce ambiguity in both prizes and probabilities to test the robustness of reversals to departures from transparency. The PT3, loss aversion, account of reversals is tested by holding the reference point constant between choice and valuation tasks. We run both tasks with a free gift protocol comparing this with what happens in the usual protocol where there is gift selection in choice tasks and selling in the valuation task. We use the terms "gift treatment" and "selling treatment" respectively to denote these conditions.

2. Tasks

The task set consisted of twelve gambles, divided into 6 pairs (*a-f*) for the choice tasks, but presented in randomised order for the valuation tasks. The information about payoffs and probabilities were chosen, on the basis of a pilot study, to produce a mixture of P- and $-choosers. This was necessary because if either bet is too attractive it will be chosen in both choice and valuation tasks and the preference reversal phenomenon will excluded by the imbalanced bet parameters.

The paired gambles were as shown in Table 1 below, with a * rating to indicate, roughly, the degree of intended ambiguity. A zero-star rating means nothing is ambiguous, so that the gamble is an 'industry-standard' prospect, with transparency in both probabilities and payoffs. A one-star rating implies that there is one attribute of the gamble which is ambiguous, a two-star rating that there are two ambiguous attributes, and so on. For the three-star gamble, neither the probability, type nor value of the prize is specified.

Thus, only for P1 and $1 were the gambles transparent. The underlying probabilities and payoffs for the other gambles were not revealed to subjects until after the experiment. Payoffs and probabilities for each gamble were in fact as shown in Table 2 below.

3. Procedures

3.1. General

Experiments were run in May and October 2009 in the Psychology department at the University of Lancaster, UK. 104 subjects took part, 52 in the selling treatment and 52 in the gift treatment. Subjects were undergraduates and postgraduates drawn from across the University. Subjects received a show-up fee of £4 in addition to anything they earned from the preference reversal tasks. Expected earnings were approximately £10 in total, consisting of the show-up fee and around £6 from the preference reversal tasks. The experiment lasted around 40 minutes in total, imparting a wage rate comparable to that obtainable for *ad hoc* postgraduate research assistance.

a) P1 A 90% chance of winning. The prize is £6.67

$1 A 20% chance of winning. The prize is £30.00

b) P2 A fairly good chance of winning £10*

$2 A pretty low chance of winning £60*

c) P3 A very high probability of winning. The prize is a modest sum of money.**

$3 A low probability of winning. The prize is a very generous sum of money.**

d)[1] P4 A reasonable probability of winning a bag of coins.**

$4 A low probability of winning a roll of notes.**

e) P5 A large sum of money has been divided equally into a large number of equal shares, which are represented by scrabble tiles in a bag. If you draw any of these tiles you will get one of these shares. There are six tiles that do not win: J, K, Q, X and Z (there are two Ks).**

$5 A large sum of money has been divided into six equal shares, which are represented by scrabble chips in a bag. If you draw any of these tiles you will get one of these shares. The six tiles that win are: J, K, Q, X and Z (there are two Ks).**

f) P6 A high probability of winning. The prize (not cash) is modest, but we are confident that most people would be happy to get this.***

$6 A low probability of winning. The prize (not cash) is something extremely desirable, we are confident that most people would be very happy indeed to get this.***

Table 1: Gambles As Presented to Subjects

Note: For gamble pair d) the actual bag of coins and roll of notes were used as stimuli.

Gamble	Prize	Probability
P1	£6.67	0.90
\$1	£30	0.20
P2	£10	0.70
\$2	£60	0.10
P3	£7	0.95
\$3	£100	0.05
P4	£14	0.45
\$4	£35	0.18
P5	£7.95	0.80
\$5	£58.30	0.12
P6	£10 store voucher[4]	0.70
\$6	£50 store voucher	0.10

Table 2: Underlying Gamble Payoffs and Probabilities

Instructions were administered both in written form and verbally, and the procedure determining payouts was demonstrated, both for choice and valuation tasks. Tasks were completed by subjects completing a booklet of answers, recording the choices and valuations in each task. Separate booklets were completed for choices and valuations, and tasks were counterbalanced across sessions.

The information for the ambiguous gambles was necessarily incomplete, but the instructions emphasised that the expected payouts were "similar" in terms of their monetary value. This served two purposes: to make the comparison between the transparent and ambiguous gambles meaningful, and to ensure that all tasks were taken with roughly equal seriousness. The instructions are given in the Appendix.

3.2. Gamble Realisation and Payments

The sequence proceeded as follows. Before the valuation (choice) tasks were completed, a valuation (choice) task was demonstrated, then the subjects completed the valuation (choice) tasks. At that point the answers to the valuation (choice) tasks were collected. A choice (valuation) task was then demonstrated, and the subjects went on to complete the choice (valuation) tasks. After these answers had been collected, the paid task was selected and played out: a coin flip decided whether this would be a choice or valuation task, then a separate die roll determined which gamble would be paid out. The same gamble (or gamble pair, if a choice task paid out) was selected for everyone, but was realised with an independent roll of the die for each subject.

In the event that a gamble paid out, a separate die roll for each subject determined the outcome of the selected lottery, excepting P5 and \$5 which were resolved by independent draws of a chip from a bag. Up to 2 rolls of a 10 sided-die, were used, giving a range of numbers from 0 – 99, with the first roll determining the first digit. Thus, for example, for P4, if a subject rolled higher than a 4 on the first roll she lost; if she rolled lower than 4 she won. If she rolled a 4, she rolled again, with numbers 0-4 paying out. In this way, numbers 0-44 paid out and 45-99 lost.

For valuation tasks, 12 sealed envelopes were picked from a box by a subject at the beginning of the experiment and assigned as they chose to numbers 1-12, which represented valuation tasks. These envelopes were stuck to a board at the front of the room. Each envelope specified a sum of money which was to serve as an offer which could be exchanged for a lottery depending on the subject's reserve price. In the event that a valuation task paid out, a subject drew once from tickets numbered 1-12 to determine which task, for all subjects, was chosen. The number was taken from the envelope to determine whether each subject played the gamble, if their reserve prices was lower than the offer, or received the sum of money.

3.3. Selling Versus Gift Manipulation

In the selling treatment, instructions for the valuation tasks were couched in the standard language of prices and offers. That is, subjects were told they had been given a gamble and were asked to specify the price for which they would be willing to sell it. It was explained that an offer had been made for each gamble, contained in the sealed envelopes at the front of the room. If a particular gamble was selected to be played out, then if the offer exceeded their price they would exchange the gamble opportunity for the cash, otherwise they would "keep" and play the gamble. The logic underpinning this procedure is exactly the logic of the more usual Becker Degroot Marshak mechanism, but without the complicating factor of a randomising device to determine the offers.

In the gift treatment, both the gamble and the amount of money in the envelope were described as gains, and the instructions avoided the language of buying, selling, offers, keeping or exchanging. Thus, subjects were asked to consider a gamble, the offer was described simply as a number and they were asked to say how much they thought the gamble was worth. In other words, the task was to specify an equivalent gain. Both outcomes were described as prizes – either a subject would receive a gamble to play or they would receive some cash, depending on whether the number in the envelope was less or greater than their valuation respectively.

In the gift treatment, therefore, the reference point against which possible outcomes are judged is the same in the choice and valuation tasks. Whilst in the selling treatment it differs. There, subjects in the valuation task are asked to put a selling price on something they have already been given, whilst in the choice tasks they choose to receive one of two gambles.

4. Results

Results are set out in Table 3 below. We report the result of a 2-sided binomial test, where the null hypothesis is that standard and nonstandard reversals are equiprobable. This allows us to distinguish between random and systematic reversals. An exposition of why this is necessary is given in the appendix, where the reader can also find the data reported in the classic preference reversal matrix form.[5]

The proportion of responses that constitute standard preference reversals, that is, the percentage of subjects in each task choosing P but valuing $ more highly, is shown in the "reversals" entry. This is calculated excluding equal valuations. Summary statistics on valuations are shown in Table 3 below.

Task	a		b		c		d		e		f	
Treatment	Selling	Gift	Selling	Gift	Selling	Gift	Selling	Gift	Selling	Gift	Selling	Gift
sample size	52	52	52	51	51	52	51	52	51	52	52	51
Choice												
% choosing P	0.37	0.29	0.46	0.37	0.45	0.58	0.20	0.17	0.41	0.38	0.31	0.39
% choosing $	0.10	0.15	0.25	0.14	0.08	0.12	0.39	0.50	0.18	0.19	0.17	0.20
Choice & Valuation												
Consistent, prefer P	19	15	24	19	23	30	10	9	21	20	16	20
Consistent, prefer $	5	8	13	7	4	6	20	26	9	10	9	10
Standard Reversal	25	27	13	18	20	16	17	12	12	11	22	17
Counter Reversal	3	2	2	7	4	0	4	5	9	11	5	4
Binomial p-value	0	0	0.01	0.04	0	0	0.01	0.14	0.66	1	0	0.01
Standard reversals	0.48	0.52	0.25	0.35	0.38	0.31	0.33	0.23	0.23	0.21	0.42	0.32

Table 3 Results

Notes: i) 'consistent, prefer P ($)' denotes P ($) was chosen and valued at least as highly as $ (P).

ii) different sample sizes reflect missing data

	Selling		Gift	
	P	$	P	$
Mean	£7.43	£9.39	£7.06	£8.64
Median	£6.58	£8.38	£6.25	£7.29
Sample Standard Deviation	£2.89	£5.39	£3.20	£4.87

Table 3: Valuations

Note: median and standard deviation are shown for each subject's mean valuation over all 12 gambles.

5. Analysis

5.1. By Task

In all tasks, the binomial test is highly significant except for task *e*, for which the test statistic is non-significant as reversals are symmetric. In every case where the statistic is significant, standard reversals outnumber nonstandard reversals. Thus, for all tasks except *e* there is a statistically significant asymmetry between behaviour in the choice and valuation tasks, which is definitive of the preference reversal phenomenon.

There is a significant difference between the percentage of standard reversals for the transparent gambles and across the other tasks (50% versus a mean of 30%; 2-tailed Z-test, $p < 0.01$). This comparison uses subjects' decisions averaged over the other gambles.[6]

5.2. By Treatment

Asymmetric preference reversals are a prominent feature of the results in both treatments, as indicated by the binomial test. The standard pattern of preference reversal results is generally reproduced in both treatments, meaning that there is a greater propensity for subjects to choose P in the choice task than in the valuation task. Differences in the proportions of standard reversals across treatments are not significant for any task (2-tailed Z-test ($p > 0.10$)).

Differences in valuations across treatments are non-significant according to standard tests (2-tailed T-test; Mann-Whitney U test; $p > 0.10$).

6. Discussion

The results analysed by task show conclusively that the preference reversal phenomenon is not an artefact of the transparency of prospects, as established in section 5.1. Even for gambles which were vaguely specified in both probabilities and outcomes, a significant asymmetry between choice and valuation results occurred, conforming to the classic preference reversal pattern. The only gamble pair for which this is not the case, pair *e*, still records a high incidence of reversals. There, standard and nonstandard reversals became equally frequent. Thus, for pair *e* and pair *e* only, the evidence is consistent with a symmetric stochastic process of preference reversal production, whereby subjects deviate with some probability from an underlying disposition to choose either P or $.

Although this was not an intended treatment manipulation, we find it likely that pair *e* involved more cognitive difficulty than the other gambles. This may have caused the difference observed, since it may have undermined the basis for definite preferences between P5 and $5, resulting in a greater stochastic element in decision making. A further possibility is that the explicit use of frequency probabilities underlies this result. For the other tasks, no frequency information was given and it would have been unclear to subjects how the prospects were to be resolved.

The transparency of gambles may nonetheless encourage reversals. Our results are consistent with this hypothesis, since the transparent gamble pair in task *a* produced significantly more reversals than the average across the other pairs. We draw this conclusion tentatively since there is considerable variation in the frequency of reversals between tasks, and it may be the case that differently specified vague gambles could match the frequency of reversals in transparent gambles. However, it seems to us unlikely that the difference is down to the specific gamble used, since i) the rate of standard reversals observed was not particularly high relative to other designs, and ii) at least one gamble pair, *f*, produced a similar split to pair *a* between P and $ in the choice task but fewer standard reversals.

It is likely given such results that preference reversals arise through a combination of mechanisms, some of which are reinforced by transparent prospects, but others of which also obtain for ambiguous prospects. Likely candidates for these include anchoring and adjustment and preference imprecision. Likely candidates for the latter include distinct mental processes involved in choice and valuation.

Essentially the same pattern of results obtains in both selling and gift treatments, as established in section 5.2. This is evidence against the PT3 account of reversals in terms of loss aversion, since we ought to have found a marked decrease in reversals in the gift treatment.

Given that we found null results across the selling and gift treatments, one might question whether our treatment manipulation was actually effective. However, in the present case it is important to note that the factor being manipulated is the specification of the reference point, as determined by the instructions. These instructions are entirely at the discretion of the experimenter, and the PT3 explanation assumes the efficacy of the instructions to implement different reference points. It may be the case that the selling treatment did not give rise to a marked differential in loss aversion, as predicted by PT3, but if so this counts against loss aversion as an explanation of preference reversal. The valuations data shown in Table 4 are consistent with a small difference in loss aversion across treatments in the direction predicted by the PT3 account, but the observed difference is not statistically significant. Another explanation of the small observed difference, assuming for the sake of argument that it is a real difference, is that subjects are applying a heuristic adapted to real life bargaining situations of asking for a higher price than one's actual reserve price.

The logic of the PT3 account of preference reversals is impeccable. However, it seems to us that it may be psychologically implausible. For it requires that subjects view selling a gamble as risking a loss, even if its resolution is to remain unknown, and to weight that potential loss as they would an experienced departure from a current endowment. This is, we suspect, especially unlikely given that the subject is only fleetingly endowed with the gamble, for the purposes of one task.

7. Conclusions

Our results indicate that preference reversals are not artefacts of the gamble transparency, nor of the shifting reference point, deployed in the standard laboratory design. Firstly, classic preference reversal results were obtained with ambiguous prospects. Explanations of reversals which have been stated with reference to precise valuations, including anchoring and adjustment, and preference imprecision, may need to be revised to accommodate this case. We do find some evidence, however, that reversals may be encouraged by transparency.

Secondly, reversals were undiminished when choice and valuation tasks deployed the same reference point. This contradicts the PT3 explanation of reversals, and indeed any explanation based on loss aversion. The reason may be that in the preference reversal context, loss aversion may be psychologically implausible because the loss concerned is a departure from a hypothetical, rather than an experienced, monetary holding. Thus, the latest attempt to reconcile preference reversals with decision theory seems to be rejected by the lab. The failure of the reference point manipulation to impact on reversals may be considered further good news for the robustness of experimental results on choice under risk. For what is arguably a rather artificial aspect of the standard design is found to have no discernible impact on reversals.

Notes

[1] Lichtenstein and Sovic (1973) reproduced the phenomenon in a casino. This is somewhat encouraging for external validity, but only concerning the subject pool and a naturally-occurring setting. Casino gambling has in common with the lab an unusual degree of prospect transparency compared to the generality of normal decisions. The gambles used in the casino replication were regular lab gambles, in that they were fully transparent in both probabilities and consequences.
[2] We owe this point to professor Peter Wakker (personal correspondence).
[3] It should be noted that there is more than one respect in which preference reversal experiments may have or lack external validity. One is the extent to which reversals are likely to occur naturally, outside the lab. This matter motivates Bohm's (1993, 1994) investigations. Another is the extent to which the factors responsible for preference reversals also underlie behaviour elsewhere. If preference reversal experiments reveal loss aversion, and loss aversion is important in other domains, this would be an important kind of external validity even if there were very few naturally-occurring preference reversals, or none at all. We are concerned here exclusively with the former matter. We owe this point to professor Chris Starmer (personal correspondence).
[4] The store vouchers used were for HMV stores, which are exchangeable for CDs, DVDs, computer games and so on.
[5] We owe this point to professor Robert Sugden (personal correspondence).
[6] Two subjects were dropped for purposes of this comparison for whom missing data had been recorded.

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Appendix

Detecting Preference Reversals

Preference reversals generate results of the form shown in the table below:

Task		Choice	
	Outcome	P	\$
Valuation	P>\$	A	B
	\$>P	C	D

Following Lichtenstein and Slovic (1971), the preference reversal literature frequently cites a difference between the proportions B/(A+B) and C/(C+D) as evidence of an asymmetry in reversals. However, this is a mistake, from a stochastic point of view. Suppose that all subjects prefer P to \$, but choose \$ with probability q. Then A, B, C, and D expressed as a proportion of total choices would become $(1\text{-}q)^2$, q(1-q), q(1-q) and q^2 respectively. It follows that B/(A+B) and C/(C+D) would equal q and 1-q respectively. Thus, although choices and valuations would be consistent in the sense that under either procedure P is selected with probability 1-q, the measure would report an 'asymmetry in reversals.' The binomial test, or an approximate version such as McNemar's (1947) test of marginal homogeneity, is suitable to detect an imbalance in the off diagonal elements of such a matrix.

Instructions

Welcome to this experiment. Each of you has been given a show-up fee of £4 for attending today. In addition to this, you each have the chance to win a further prize. The details of how this will happen are as follows. The experiment consists of two parts, a "choice" part and a "valuation" part. In each case you have to judge the attractiveness of possible lotteries. At the end of the experiment a coin will be tossed to determine which part is played out for real, and one of the tasks will be chosen at random. Which prize you receive depends on which task is chosen and which decision you made in that task, plus a purely random element.

The tasks in both parts involve lotteries. Each lottery refers to a chance of winning a prize. The chances and prizes involved are different in each lottery. You have to say what you would choose to do in each situation below, based on how attractive you think the lotteries are. All the lotteries would cost the experimenter a similar amount of money if everyone chose to play them. All the lotteries will be revealed at the end, after your decisions have been recorded.

<u>Choice Part</u>

In this part of the experiment you have to choose between two lottery tickets in each task. Indicate your choice by circling letter A or B. What you decide here will affect your potential prize from the experiment if a task from the choice part is paid out at the end. (Recall that one task from the whole experiment is selected at random and played out for real.)

[A practice choice task using transparent gambles was played out before subjects received their answer booklets, but not paid. The booklets presented the tasks in the following form:]

1. Choose between
    A. A 90% chance of winning. The prize is £6.67, and
    B. A 20% chance of winning. The prize is £30.00

2. Choose between
    A. A fairly good chance of winning £10*
    B. A pretty low chance of winning £60*

[And so on. The order of gamble pairs was counterbalanced across subjects.]

<u>Valuation Part</u>

<u>1. Selling Treatment</u>

In this part of the experiment we would like you to tell us how much you would be willing to sell a lottery ticket for. We appreciate that for some of the lotteries this may be quite difficult. However, we think that it is in your interests to take this seriously. What you decide here will affect your potential prize from the experiment if a task from the valuation part is paid out at the end. (Recall that one task from the whole experiment is selected at random and played out for real.)

In each task you are given a lottery ticket. For each ticket we would like you to state a price for which you would be just willing to sell it. We've constructed this part of the experiment to give you a reason to do just that – though you are quite free to do what you like!

The envelope which was selected (at random, by a participant) at the start of this experiment contains a number of pounds (and pence), which we will call the “offer.” This number, which is different for each ticket, will only be revealed after all decisions have been made.

If the number you gave as the price of the ticket is less than or equal to the offer number, you will be given that offer in pounds if this task is paid out. Otherwise you will keep the lottery ticket, and that will determine your earnings by a play of the lottery. The higher the price you specify, the more likely it is that you will keep the lottery ticket. The lower the price, the more likely it is that you get the money instead.

Suppose you think the ticket is worth £x. If you say that your price is more than £x, then you risk that the offer is *higher* than £x, but *less than* your price. In that case you will keep the lottery ticket, whereas you could have received more than £x by stating the price as £x. If, alternatively, you say that your price is less than £x, you risk that the offer is less than £x but more than your price. In that case you will receive a sum of money which you think is inferior to the ticket, whereas you could have kept the ticket by stating a price of £x.

(Verbal) Write down any number, large or small. Imagine someone who owns an antique. He’s not sure what the market value is but probably it’s not worth much. But there’s a small chance it’s worth a lot. He decides, taking this into account, that it’s worth £30 to him. Now imagine he’s playing the same game described in the instructions, with your number as the offer.

Suppose he said £1000 as the price. What could go wrong? … (answer elicited)
Suppose he said £100 as the price. What could go wrong? … (answer elicited)
It’s the same problem for any price higher than £30.
Now suppose he said £2 as the price. What could go wrong? … (answer elicited)
Suppose he said £20 as the price. What could go wrong? … (answer elicited)
It’s the same problem for any price lower than £30.
This shows that the best thing you can do is state your true price.

2. Neutral Treatment

In this part of the experiment we would like you to tell us how valuable a lottery ticket is to you. We appreciate that for some of the lotteries this may be quite difficult. However, we think that it is in your interests to take this seriously. What you decide here will affect your potential prize from the experiment if a task from the valuation part is paid out at the end. (Recall that one task from the whole experiment is selected at random and played out for real.)

In each task you are required to consider a lottery ticket. For each ticket we would like you to state a sum of money which you think is as good as it but no better. We've constructed this part of the experiment to give you a reason to do just that – though you are quite free to do what you like!

The envelope which was selected (at random, by a participant) at the start of this experiment contains a number of pounds (and pence), which we will call the "deciding number." This number, which is different for each ticket, will only be revealed after all decisions have been made.

If the number you gave as the value of the ticket is less than or equal to the deciding number, you will be given that number in pounds if this task is paid out. Otherwise you will be given the lottery ticket, and that will determine your earnings by a play of the lottery. The higher the value you specify, the more likely it is that you will get the lottery ticket. The lower the value, the more likely it is that you get the money instead.

Suppose you think the ticket is worth £x. If you say that the value is more than £x, then you risk that the deciding number is *higher* than £x, but *less than* your stated value. In that case you will be given the lottery ticket, whereas you could have received more than £x by stating the value as £x. If, alternatively, you say that the value is less than £x, you risk that the deciding number is less than £x but more than your stated value. In that case you will receive a sum of money which you think is inferior to the ticket, whereas you could have received the ticket by stating a value of £x.

(Verbal) Write down any number, large or small. Imagine someone who owns an antique. He's not sure what the market value is but probably it's not worth much. But there's a small chance it's worth a lot. He decides, taking this into account, that it's worth £30 to him. Now imagine he's playing the same game described in the instructions, with your number as the deciding number.

Suppose he said £1000 as the value. What could go wrong? … (answer elicited)
Suppose he said £100 as the value. What could go wrong? … (answer elicited)

It’s the same problem for any value higher than £30.
Now suppose he said £2 as the value. What could go wrong? … (answer elicited)
Suppose he said £20 as the value. What could go wrong? … (answer elicited)
It’s the same problem for any value lower than £30.
This shows that the best thing you can do is state your true value.

[After the valuation instructions, subjects valued two practice lotteries with transparent prospects, which were not paid out. They then valued the 12 lotteries, the order being counterbalanced across subjects.]

Pair		Combined Treatments P>$	$>P	Binomial P value	standard-reversals		Selling Treatment P>$	$>P	Binomial P value	standard-reversals		Gift Treatment P>$	$>P	Binomial P value	standard-reversals
a	P1	20	52	0.00	0.58	P1	10	25	0.00	0.58	P1	10	27	0.00	0.59
	$1	5	12			$1	3	5			$1	2	7		
b	P2	34	31	0.00	0.33	P2	20	13	0.01	0.28	P2	14	18	0.04	0.39
	$2	9	19			$2	2	12			$2	7	7		
c	P3	47	36	0.00	0.37	P3	19	20	0.00	0.43	P3	28	16	0.00	0.32
	$3	4	10			$3	4	4			$3	0	6		
d	P4	12	29	0.00	0.33	P4	5	17	0.01	0.39	P4	7	12	0.14	0.27
	$4	9	38			$4	4	18			$4	5	20		
e	P5	34	23	0.76	0.25	P5	18	12	0.66	0.25	P5	16	11	1	0.25
	$5	20	15			$5	9	9			$5	11	6		
f	P6	29	39	0.00	0.44	P6	14	22	0.00	0.48	P6	15	17	0.01	0.40
	$6	9	12			$6	5	5			$6	4	7		